Student Mobility and Migration: Evidence from the Erasmus Programme

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Abstract

International labor mobility remains limited even where workers are free to move. I study whether exposure to foreign destinations before labor-market entry can raise it, using Erasmus, which organized student exchanges through cross-country partnerships between university departments in Europe. I digitize the program's first-decade partnership networks and match them to LinkedIn profiles of about 2 million graduates. Exploiting staggered department entry and link formation, I find that entry raises graduates' employment abroad by about 10 percent, and a new link raises their employment in the linked subnational region by a further 11 percent. The link effect appears at career start, remains similar 25 years later, and does not spill over to other regions of the linked country. On net, links reallocate graduates toward higher-income regions. Recent administrative Erasmus data reveal spillovers. Receiving an exchange student in the graduation year raises local graduates' employment in the student's home region by about 6 percent. Sending students abroad raises employment in their host regions among classmates with no exchange recorded on LinkedIn. Participants are also more likely to work in their exchange peers' home regions. These patterns suggest the effects operate through region-specific information and social ties rather than country-specific skills such as language.

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1 Introduction

Labor mobility often remains limited even when labor markets are formally integrated. In the European Union, workers have been free to live and work across member states for decades. Yet in 2023, only 4.0% of EU residents were born in another member state, compared with 27.5% of US residents born in another state (Figure 1). This contrast suggests that removing legal barriers alone may not be enough to raise geographic mobility.

Limited labor mobility hinders adjustment to regional economic shocks (Mundell, 1961; Blanchard and Katz, 1992), allowing adverse local labor market effects to persist (Topalova, 2010; Autor et al., 2013). It also prevents workers from sorting into the places where they would be most productive, reducing aggregate output (Bryan and Morten, 2019; Caliendo et al., 2021). What, then, keeps workers from moving? Beyond legal barriers, migration decisions are shaped by limited information about destination labor markets (McKenzie et al., 2013), linguistic and cultural distance (Adserà and Pytliková, 2015), and the absence of social ties (Bailey et al., 2018; Koenen and Johnston, 2024). Student exchange programs may affect these factors by helping students become familiar with another country's language and culture, gain access to local information, and build social connections before entering the workforce, and they may also change where students would like to live. Do student exchange programs change where graduates, including non-participants, work?

I study these questions in the context of Erasmus, the European Union's student exchange program. Launched in 1987 to foster familiarity with other European countries, strengthen ties among their citizens, and promote economic and social integration, the program has supported more than 4 million study exchanges in EU member states and the United Kingdom, allowing university students to spend three months to an academic year at a partner institution abroad. The program reduced the costs of studying abroad through financial grants, tuition waivers at host universities, and recognition of coursework toward home degrees. During its first decade, Erasmus connected more than 1,700 higher-education institutions through subject-specific partnerships between departments in different countries, which determined the destinations available to their students.

Assessing whether these opportunities translate into lasting changes in labor mobility has been constrained by the difficulty of tracking graduates across national borders. National administrative records typically provide little information on employment after emigration, while international migration data often lack the geographic detail and longitudinal coverage needed to trace careers across subnational regions.[1] Linking these trajectories to the universities and departments where graduates studied poses a further challenge (European Commission, 2017b;

[1] I focus on NUTS2 regions, the EU's standard subnational classification, corresponding to Italian regions such as Tuscany or Spanish autonomous communities such as Catalonia, with about 2 million inhabitants on average.

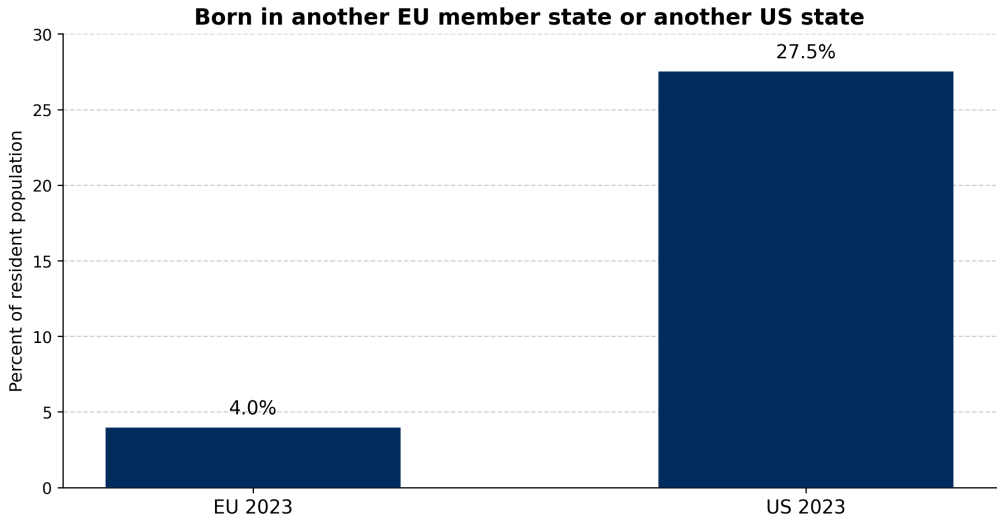


Figure 1: Residents living outside their country or state of birth, EU and US

Notes: Both series use the total resident population as the denominator. The EU measure counts residents born in another member state, using Eurostat population-by-country-of-birth statistics (Eurostat, 2025b). The US measure counts residents born in another US state, using the American Community Survey (U.S. Census Bureau, 2023).

Batsaikhan et al., 2018). I make progress on this issue using LinkedIn education and career histories, which record where graduates studied alongside where they subsequently worked. These data allow me to follow employment across countries and subnational regions over several decades, for exchange participants and non-participants alike. The underlying data cover about 32 million graduates from universities in EU member states and the United Kingdom, spanning graduation cohorts from 1980 to 2025.

To link these career histories to the expansion of Erasmus, I digitize nine Inter-University Cooperation Programme (ICP) directories from the program's first decade, harmonize LinkedIn education records across languages and national degree systems, and match the two sources. The directories cover Erasmus's first decade, when exchanges were organized through subject-specific university networks, before their replacement by university-wide institutional contracts in 1997/98, after which subject-specific partnerships were no longer centrally documented. During this ICP phase, Erasmus supported about 450,000 student exchanges. I use university–subject pairs as proxies for academic departments. Each ICP network brought together departments of the same subject area from several countries. I treat two departments in different countries as linked once they belong to the same network approved for student mobility. For the employment analysis, each department is linked to the subnational region of each of its foreign partners. Treatment therefore captures access through an approved student-mobility network. The merged sample follows about 2 million graduates of the 1980–1997 cohorts, each observed over a common 28-year post-graduation career window. They studied in 15 subject areas at about 1,100 higher-education institutions in the countries that were EU members by 1997. The sample contains about

176,000 treated department–region links.

I estimate the impact of Erasmus access on graduates' employment locations by exploiting the staggered expansion of Erasmus links across departments and foreign destinations. The framework is a gravity model derived from graduates' location choices, in which an Erasmus link shifts the attractiveness of a destination for a department's graduates.[2] I take this model to the data by estimating a difference-in-differences specification with Poisson Pseudo-Maximum Likelihood (PPML) on department–destination–cohort cells. Treatment effects are allowed to vary across link-opening and graduation cohorts, which avoids the biases of two-way fixed effects under staggered adoption (Wooldridge, 2023, 2025; Nagengast and Yotov, 2025). Pair fixed effects absorb persistent bilateral ties, and department–cohort and destination–cohort fixed effects absorb time-varying shocks to departments and destinations. The design then compares changes in employment of graduates along newly linked department–region pairs with changes along not-yet- and never-linked pairs. Since treatment is defined by access rather than participation, the estimates capture effects on all exposed graduates, including those who never study abroad. Students who go abroad are selected twice: they choose to apply, and home universities often select among applicants on grades and language skills. They also choose their destination, possibly where they already plan to live or work. Comparing participants with non-participants, or participants' destinations with other destinations, would therefore confound the effect of the exchange with these traits and preferences. Comparing entire cohorts of a department before and after a link opens avoids this. Identification requires that, absent Erasmus links, expected employment of graduates along treated and comparison pairs would have followed parallel trends, conditional on the fixed effects. The event studies show no evidence of differential pre-trends, lending support to this assumption.

The results yield three main findings. First, opening an Erasmus link increases subsequent employment of a department's graduates in the linked region. The effect appears at labor-market entry and persists for decades. Second, beyond the linked region, Erasmus entry raises graduates' employment across all foreign destinations, but has no additional effect on other regions of the linked country. Third, complementary evidence from modern Erasmus flows shows spillovers beyond students who travel: receiving exchange students increases local graduates' subsequent employment in the incoming students' home regions, while outgoing exchanges are followed by higher employment in those destinations among graduates with no recorded exchange on LinkedIn.

Before examining these findings in detail, I verify that the department links reconstructed from the ICP directories generated additional student mobility. I proxy for exchanges using temporary foreign education spells recorded on LinkedIn that last less than one year and occur

[2]The choice set includes all NUTS2 subnational regions in the countries that were EU members by 1997, together with aggregate alternatives for EFTA, countries that joined Erasmus after the ICP phase, and the rest of the world.

during a student's main degree. Opening a bilateral department link sharply increases exchange flows between the linked departments, with no noticeable differential pre-trends. The effect on exchange flows grows as the partnership matures.

Having established that these links generated additional student mobility, I turn to their effects on graduates' subsequent employment. First, Erasmus links create persistent employment corridors. Opening a link raises the number of a department's graduates working in the linked region by about 11%, relative to department–region pairs not yet or never linked. The effect appears near labor-market entry, remains of similar magnitude 25 years after graduation, and grows as the partnership matures, paralleling the increase in exchange participation. In levels, the estimated reallocation corresponds to the same amount of employment as if roughly 4,200 additional graduates in my sample spent their entire 28-year careers in linked regions. Under population scaling, this rises to about 32,000, or roughly one for every 14 Erasmus exchanges. The result remains robust across specifications that add country-pair–cohort, university–destination-region–cohort, or department–destination-country–cohort fixed effects, absorbing bilateral country shocks, university-wide changes in ties to particular regions, and department-specific changes in ties to destination countries, respectively. The effect extends beyond the program's earliest links, appears across five treatment cohorts and remains stable when the sample is restricted to pairs that eventually become linked. It is robust to alternative destination sets, weighting and inference choices, and leave-one-out exclusions of countries and fields.

Second, Erasmus affects both whether graduates work abroad and where abroad they work. Departments enter the program at different times and establish links to particular countries and regions. I use this variation to distinguish the broad foreign-employment effect of entry from additional effects specific to linked countries and regions. Erasmus entry raises employment across foreign destinations by about 10%, while directly linked regions receive an additional increase of similar magnitude. The 11% linked-region effect reported above therefore captures an increase relative to the department's other foreign destinations, rather than the program's total effect on employment in the linked region. There is no further effect elsewhere in the linked country, which is difficult to reconcile with mechanisms operating primarily through language or other host-country-specific human capital. In levels, under the same population scaling, the two margins imply about 82,000 additional graduate-career equivalents in EU destinations, or roughly one for every five Erasmus exchanges. This ratio describes the aggregate employment reallocation among all graduates of exposed departments, including potential spillovers to non-participants. The broad foreign-employment effect remains stable after absorbing university-wide internationalization trends, making broader university policies promoting foreign employment unlikely to explain the result.

The historical link-opening estimates combine three forms of exposure within the multilateral ICP networks: studying in a linked region, meeting incoming students from that region at home,

and meeting students from that region at a third partner university. To distinguish these channels, I turn to complementary administrative Erasmus records available since 2008, which do not cover the ICP period. These records separately measure incoming and outgoing exchanges for each university–subject pair and academic year and identify the origins of students attending a common host institution. They therefore allow me to examine both exposure through exchanges at home and connections formed with peers abroad.

This complementary analysis yields the third main result: Erasmus generates spillovers beyond students who themselves go abroad. I link incoming and outgoing flows to LinkedIn employment outcomes within the same gravity design, exploiting year-to-year variation in their timing and direction. Receiving at least one student from a region in the graduation year raises local graduates' subsequent employment there by about 6%, with the response concentrated among graduates with no recorded exchange. Sending at least one student to a region in any of the final three academic years before graduation is associated with 2–4% higher subsequent employment there among local graduates, with positive effects also among graduates with no recorded exchange on LinkedIn. Neither response appears in placebo years immediately outside the relevant exposure window. Incoming exchanges provide the clearest evidence of spillovers: they expose local students without requiring travel, and their timing is plausibly exogenous to local graduates' destination preferences, conditional on the controls. Outgoing exchanges also suggest spillovers through classmates, but shared destination preferences and potentially underreported exchange participation on LinkedIn complicate their interpretation.

To study the third channel, in which students from different regions meet at a common host university, I exploit year-to-year variation in the origins of Erasmus students at that host. Participants are more likely to subsequently work in the home regions of same-field peers they met abroad, including regions they never visited for exchanges themselves. Erasmus can therefore connect regions through encounters at a common host, even without direct student flows between them.

A separate set of results further clarifies the mechanisms. First, student mobility itself appears to drive the employment effects. During the ICP period, links with greater initial bilateral mobility capacity generate larger increases in both exchange and employment, while controlling for other forms of ICP cooperation leaves the link effect essentially unchanged. In the modern administrative data, employment effects likewise increase with the number of incoming and outgoing students. Second, proportional effects are larger for more distant destinations and along initially thinner employment corridors, consistent with Erasmus mattering most where departments and destinations were least connected to begin with. Third, the effect is close to zero for Law graduates, whose cross-border practice remained heavily restricted, but similar to the baseline in the other large fields, suggesting that student exchange facilitates mobility mainly where legal barriers have already been removed. Fourth, the proportional employment effect is

similar whether the linked destination is richer or poorer than the origin. But because baseline flows are larger toward richer regions, the same proportional effect implies larger absolute reallocations toward higher-income destinations, suggestive of worker reallocation toward more productive areas.

The interpretation of these findings also depends on selection into LinkedIn, which may affect both the estimated effects and their generalizability. Coverage is nevertheless unusually broad for this setting: the ICP-era sample contains roughly 13% of first-degree graduates in the relevant EU cohorts, with substantially higher coverage in later cohorts. The linked-region effect is similar in the best-covered groups, including UK departments and the over-represented Business, Social Sciences, and Engineering fields. The sample also reproduces administrative migration patterns: the number of graduates working in each EU-15 destination closely matches official stocks of tertiary-educated migrants, and arrivals across Spanish regions match the population register. The inflow–outflow analysis also yields similar employment responses for the 2013–2020 cohorts, where the sample covers more than 35% of graduates.

Beyond overall coverage, a more specific concern is that Erasmus itself changes who appears in the LinkedIn data and who reports employment locations, rather than where graduates work. For example, exchange students may be more likely to create profiles or report their jobs. The fixed effects and controls address most such stories. Department-by-cohort effects absorb any change in how many graduates of a given department and cohort appear on LinkedIn, and department-by-destination-country-by-cohort effects absorb changes in a department's propensity to place graduates in a given foreign country. A spurious linked-region effect would therefore require Erasmus to shift the sample, within a specific department, toward graduates who would have worked in the newly linked region rather than in other regions of the same country, and to do so exactly when the link opens. Such a narrowly targeted change in sample composition seems unlikely, although it cannot be ruled out. If, however, if Erasmus raises LinkedIn use only by inducing graduates to work in the linked region, selection could not create a response where none exists. It could amplify a genuine one only slightly, since induced movers are a small fraction of each cohort. I test one selection channel directly and find no evidence that Erasmus entry changes the probability of reporting a locatable job.

In the final part of the paper, I perform back-of-the-envelope calculations of the economic magnitude of the estimates, using the random-utility location-choice model underlying the gravity equation. Using the estimated distance gradient, the linked-region effect is equivalent to making the destination about 15% closer. Using an elasticity of location choices with respect to destination income of 1.63, the median estimate compiled by Head and Mayer (2021), the model implies a gain of about 0.2% of annual consumption per graduate of an exposed department from the linked-region component, and about 1.6% when all foreign-employment margins are included. Over a 28-year career, these gains are worth about €1,900 and €14,000 per graduate.

These calculations rely on the location-choice model, value gains only for graduates of exposed departments, and hold destination incomes and amenities fixed, but they indicate that the effects are economically meaningful.

Contribution to the literature. First, the paper contributes to research on study abroad and subsequent labor-market mobility. I show how Erasmus shapes employment across Europe over several decades, distinguishing effects in linked regions, elsewhere in linked countries, and other foreign destinations, and documenting spillovers beyond exchange participants. Parey and Waldinger (2011) use a nationally representative longitudinal survey of German graduates and instrument study-abroad participation with department-level Erasmus availability to estimate its effect on later work abroad. Related work shows that studying abroad raises the probability of later living abroad (Oosterbeek and Webbink, 2011) and examines the labor-market returns to Erasmus participation (De Benedetto et al., 2025). I build on this literature by combining reconstructed bilateral Erasmus links with geographically detailed career histories of graduates from across Europe and administrative exchange records. These complementary data allow me to examine the long-run geographic effects of exchange opportunities, different geographic margins and the role of exposure to incoming students, outgoing classmates, and peers met abroad.

Second, the paper contributes to research on how migration networks form and shape destination choices. I show that public policy can create new cross-border networks: temporary, voluntary educational exchanges generate lasting employment connections between partner regions and, through peers who meet at a common host university, between regions without direct student flows. Existing studies, largely of low-skilled migrants, show that people are more likely to move where they have social ties or where earlier migrants from the same origin have settled (Munshi, 2003; Stuart and Taylor, 2021; Blumenstock et al., 2025). Because such networks are usually inherited from past migration, much of the evidence on how they form comes from historical and forced movements, which created ties whose effects persist for decades (Burchardi and Hassan, 2013; Kinnan et al., 2018; Burchardi et al., 2019). Erasmus links, by contrast, opened at known dates between specific departments and regions, allowing me to trace ties among future high-skilled workers from the moment they form.

Third, the paper contributes more broadly to migration research by connecting work on temporary migration with work on the determinants of high-skilled mobility. Migration decisions are dynamic: return plans shape how much migrants invest in skills at the destination (Dustmann and Görlach, 2016; Adda et al., 2022), and even a single subsidized migration episode can raise later migration (Bryan et al., 2014). Evidence on high-skilled migrants instead emphasizes taxes (Kleven et al., 2013; Akcigit et al., 2016; Kleven et al., 2020), earnings, and immigration policy (Kerr et al., 2016). I show that a temporary stay abroad during university shapes where graduates work for decades, including graduates who never went abroad themselves, so that early exposure

is itself a determinant of high-skilled mobility.

Fourth, the paper connects research on intergroup contact to the integration of a supranational union. I show that temporary educational contact across national borders shapes where people subsequently live. Existing studies show that structured contact across regions or groups within countries can shape identity, nation-building, social integration, and where people later live (Bazzi et al., 2019; Cáceres-Delpiano et al., 2021; Bagues and Roth, 2023; Okunogbe, 2024). I extend this evidence across national borders, measuring European integration by where people choose to work rather than how they identify.

Section 2 describes Erasmus and the ICP system. Section 3 presents the data, and Section 4 develops the location-choice framework and empirical strategy. Section 5 reports the link-level results and their geographic reach. Section 6 disentangles the channels underlying the link effect and examines spillovers to non-participants. Section 7 benchmarks the economic magnitude of the effects, and Section 8 concludes.

2 Institutional Background

Erasmus was established by Council Decision 87/327/EEC in 1987 (Council of the European Communities, 1987). The program allowed university students to spend three months to an academic year at a partner university in another Member State, with the study period recognized toward their home degree. The founding Decision sought to create a pool of graduates with first-hand experience of the economic and social conditions of other Member States, to strengthen interaction across countries, and to lay the basis for intensified cooperation within the Community (Council of the European Communities, 1987; European Commission, 1988). Contemporary Commission discussion also linked student mobility to the completion of the internal market. Erasmus was thus conceived not only as an educational exchange program, but also as an investment in a more integrated European economy and labor market.

The program transformed study abroad from a largely individual undertaking into an institutionally organized form of mobility. Participating institutions arranged recognition of coursework completed abroad, students paid no tuition to the host institution, and mobility grants covered part of the additional costs of travel and living abroad (European Commission, 1988; Teichler, 1996; Maiworm, 2001). Community funds also supported the administrative costs of organizing exchanges and, in some cases, language preparation (ERASMUS Bureau, 1988; Teichler, 1996). These arrangements reduced several important barriers to temporary foreign study, including admission, credit recognition, financing, and information about host institutions.[3]

[3]Recognition remained imperfect. From 1989/90 Erasmus piloted the European Community Course Credit Transfer System (ECTS). In 1990/91 participants reported that roughly 75% of coursework completed abroad was

The ICP system. During the period studied here, Erasmus operated through *Inter-University Cooperation Programmes* (ICPs): subject-specific networks linking higher-education institutions in different participating countries. Each ICP had a coordinating institution and partner institutions and could receive support for student mobility and related forms of cooperation.[4]

The subject-specific organization of ICPs is central to the empirical design. Participation was organized primarily by academic departments: departmental coordinators prepared applications, selected students, organized mobility, and arranged recognition of coursework (Teichler, 1996; Maiworm, 2001). I therefore use a university–subject unit as the empirical analogue of a department. Because departments could join different ICP networks and connect to different foreign destinations at different times, Erasmus expanded both across departments and across destinations within departments.

Support was competitive and budget constrained. Applications were evaluated centrally, and demand exceeded available resources in the program's early years. In 1987, 398 of 868 applications were funded, and the amount awarded covered only about one third of requested funding (European Commission, 1988). The timing of network expansion therefore reflected both institutional demand and the gradual allocation of centrally administered Erasmus support. Figure 2 shows the resulting expansion: links per department rose from fewer than one in nearly every region in 1987/88 to more than ten in most regions by 1995/96. Because departments added links to different destinations in different years, this expansion provides the treatment variation used in the empirical analysis.

recognized on average, rising to about 95% where ECTS applied (European Commission, 1993; Teichler et al., 2000).

[4]Community support could be awarded separately for network development and administration, student mobility, and teaching-staff mobility (ERASMUS Bureau, 1988).

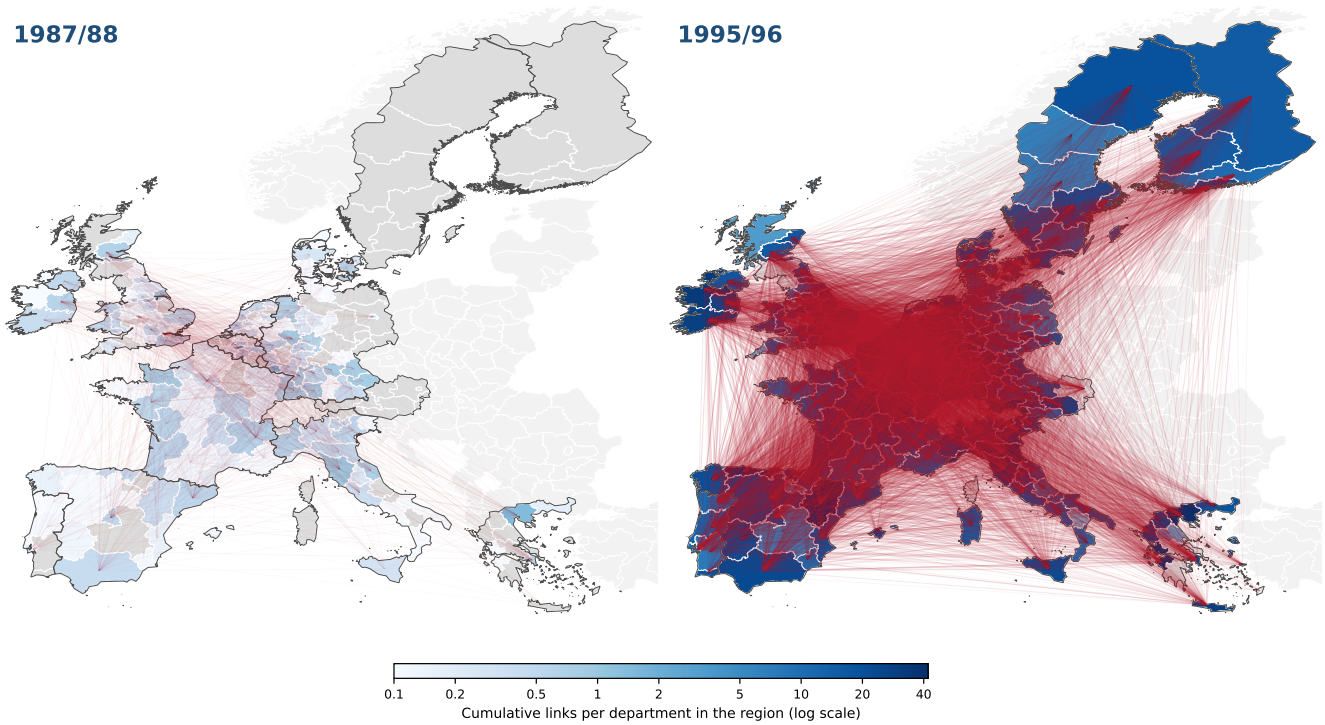


Figure 2: Expansion of Erasmus links during the ICP phase

Notes: Each panel shows cumulative ICP links established by the indicated academic year. Shading gives the number of department–region links per department in each subnational NUTS2 region, on a log scale. Lines connect pairs of regions with at least one pair of departments in a common ICP network. Grey regions have no links.

Erasmus was incorporated into the broader Socrates program in 1995, but the transition away from ICPs was gradual: the ICP system remained operative through academic year 1996/97, and from 1997/98 onward universities submitted consolidated Institutional Contracts that replaced the centralized subject-specific system (European Commission, 2017a; Teichler, 2002; Teichler et al., 2000). I refer to 1987/88–1996/97 as the *ICP phase*. Erasmus grew rapidly during this period. It began in 1987/88 with 3,244 student exchanges involving 11 of the then twelve European Community member states (European Commission, 1993; Teichler and Maiworm, 1997). Annual study mobility exceeded 50,000 students by 1992/93 and 80,000 by the end of the ICP phase and new member states joined, and continued to grow thereafter (Appendix Figure A.1).

The main link-level analysis is restricted to the ICP phase, for which centralized directories allow the timing of subject-specific department–destination access to be reconstructed, and to higher-education institutions in the EU-15 countries that participated during this phase.[5] Throughout, I focus on Erasmus *study mobility*: temporary study at a partner institution while remaining enrolled in a degree at the home institution. Later forms of Erasmus mobility, including work placements and Erasmus Plus activities are introduced under the Lifelong Learning Pro-

[5]Non-EU countries that participated in Erasmus at various points are excluded because the analysis focuses on mobility within the European political and economic union.

gramme, are not comparable to the ICP-era treatment and are excluded (European Commission, 2010; European Parliament and Council of the European Union, 2013).

3 Data

The analysis combines individual education and employment histories from LinkedIn, newly digitized Erasmus Inter-University Cooperation Programme (ICP) directories, European Commission administrative records on Erasmus mobility, and bilateral regional characteristics. This section describes the sources, the strengths and limitations of the LinkedIn data, the two estimation samples, and the construction of outcomes and treatment. Appendix A provides additional detail.

LinkedIn education and employment histories. Revelio Labs standardizes publicly available LinkedIn profiles into individual education and employment histories. Education records contain institutions, degree titles, fields of study, and enrollment dates. Employment records contain employers, occupations, seniority, locations, and start and end dates. The underlying data cover about 32 million graduates of universities in EU member states and the United Kingdom, spanning graduation cohorts 1980–2025 (Appendix Figure A.2). I harmonize degree titles across languages and national education systems to ISCED levels,[6] retain tertiary degrees, and map reported fields of study to subject areas. I refer to a university–subject unit as a *department*. The subject classification differs between the two analyses: the ICP analysis uses the 15 subject areas of the ICP directories, while the analysis of recent exchange flows uses eight broad ISCED fields, because the administrative subject classification changed in 2014 and differs from the ICP subject areas.

ICP directories. During its first decade, Erasmus operated through subject-specific ICP networks. I digitize the nine available ICP directories for 1987/88–1995/96. For each approved network, the directories report the participating institutions, the subject area, and the supported activities, and, for student mobility, the network's total quota of exchange students and their expected average duration of stay. Combining automated extraction with manual review, I recover all but 7 of the 16,227 approved ICP records. Because networks approved in 1995/96 remained in place for one additional academic year, I carry the final directory forward to 1996/97. Appendix A.4 describes the digitization.

[6] The International Standard Classification of Education (ISCED 2011) is UNESCO's framework for comparing education programs across national systems. Tertiary education comprises levels 5–8: short-cycle tertiary, bachelor's or equivalent, master's or equivalent, and doctoral.

Administrative Erasmus records. The European Commission provides open, anonymized individual-level records of Erasmus mobility for 2008/09–2023/24. These records report sending and receiving institutions, education level, field of study, stay dates, gender, nationality, and age. I use them to reconstruct annual inflows and outflows between departments.

Bilateral characteristics. Geographic distance is the great-circle distance between the centroids of the origin and destination NUTS2 regions. Regional GDP per capita in PPP terms for year 2000 comes from Rosés and Wolf (2019). Linguistic distance captures how far apart the languages of the origin and destination countries are, using the linguistic-tree distances of Pellegrino et al. (2025) weighted by the languages spoken in each country. Cultural distance captures differences in values and beliefs between the populations of the two countries, using the 2002 measure from Pellegrino et al. (2025), which is based on World Values Survey responses.

Estimation samples. The main analysis uses graduates of the 1980–1997 cohorts from EU-15 departments with at least ten matched graduates. Because the ICP directories and LinkedIn do not share a common institution identifier, I link them through the official Erasmus eligibility list using normalized institution names, geographic information, and manual review of unresolved cases. The resulting crosswalk reconciles historical name changes, institutional reorganizations, and mergers (Appendix A.6). The sample contains about 2 million graduates in 7,107 departments at 1,091 institutions,[7] and 219 destination locations.[8] Of about 7,000 departments, 5,270 open at least one link during the ICP phase, creating about 176,000 department–region links.

The analysis of recent exchange flows uses graduates of the 2013–2020 cohorts from departments in the EU-27 and the United Kingdom, for which the administrative records cover the full annual exposure window and earlier exchange activity. I match the sending and receiving institutions in these records to institutions in the LinkedIn data, retaining about 2.3 million exchange records. The sample contains about 6.8 million graduates in about 9,500 departments at 1,376 institutions, and 281 destination locations.[9] Appendix Table A.1 summarizes the two samples.

Employment outcomes. I reconstruct each graduate's employment location on January 1 of every calendar year from the underlying position spells and geocode locations to NUTS2 regions

[7] Institutions are counted after the crosswalk, which combines historical predecessors, renamed institutions, and separate campuses into a single institution, the unit used in the ICP directories. LinkedIn lists the same institutions under 1,553 separate institution pages.

[8] Destinations are the NUTS2 regions of the EU-15, with EFTA countries, countries that joined Erasmus after the ICP phase, and the rest of the world as three residual destinations.

[9] Destinations are the NUTS2 regions of the 28 countries that belonged to the EU during this period, with the other Erasmus program countries and the rest of the world as two residual destinations.

using the NUTS 2021 classification. Within the EU, destinations are NUTS2 regions,[10] and employment elsewhere is assigned to residual destinations. In the main analysis, the outcome for destination l is the share of a graduate's observed post-graduation career spent employed in l, over a balanced 28-year window, the longest horizon available for every cohort in the sample. The denominator consists of career years within this window for which employment can be assigned to a destination in the choice set, so destination shares sum to one. I also construct employment location at fixed years since graduation to study persistence over the career. Appendix Figure A.6 reports coverage over the career.

Exchange participation. I construct two measures of study abroad from LinkedIn education histories. The *direct Erasmus measure* identifies foreign education records that explicitly mention Erasmus name. The *inferred-exchange measure* identifies short periods of study at a foreign university that take place during a degree at a home university.[11] It captures temporary study abroad even when the profile does not identify the stay as Erasmus. It is my preferred measure because it reflects changes in short-term study abroad rather than in how students label those stays, and because it is the only measure with meaningful coverage of the ICP cohorts. The ICP-period sample contains about 18,000 inferred exchanges[12] with an identified destination region, roughly 4% of the exchanges the program recorded between 1987/88 and 1996/97. This is well below the 10% coverage of graduates, likely because older cohorts list their degrees more completely than their stays abroad. Section 3.1 shows how the measure responds to link openings.

ICP treatment. The directories record membership in subject-specific networks, not realized student flows. Let d denote a department and l a foreign NUTS2 region. Department d is linked to l when it belongs to an ICP approved for student mobility that includes at least one partner institution in l. A link therefore indicates access to a common approved network, not that students necessarily moved between the two institutions. ICPs supporting only other activities, such as teacher mobility or curriculum development, do not generate links, and several partners or ICPs connecting d to l count as a single link. Throughout, a *link* is this directed treatment, a *pair* is any department–destination combination whether linked or not, and a *corridor* is the corresponding flow of graduates.

For each pair (d, l), let $g(d, l)$ denote the first graduation cohort exposed to the link. A link first appearing in academic year $t/t+1$ first exposes graduation cohort $t+1$, and exposure is ab-

[10] NUTS2 provides a harmonized regional unit across European countries and is sufficiently granular to distinguish subnational labor-market destinations while remaining comparable across countries.

[11] A spell qualifies if its recorded end year is at most one year after its start year. It is attributed to the home degree whose enrollment period contains it.

[12] Across the 51 million degree records in the data, 1.4 million carry an inferred exchange and 0.7 million an explicit Erasmus mention.

sorbing thereafter. Treatment therefore captures first access to a link rather than contemporaneous membership in an active ICP.[13]

The student-mobility network expanded rapidly. Participation rose from 449 institutions and roughly 1,000 departments in 1987/88 to 1,754 institutions and more than 8,500 departments by 1995/96, and the average number of destination regions per participating department rose from roughly 4 to about 25. Treatment is therefore staggered both across departments and across destinations within departments, and subject areas within the same university typically enter at different times (Appendix A.5).

Properties of the LinkedIn data. LinkedIn education and career histories record where graduates studied alongside where they subsequently worked. This combination makes it possible to link a department's Erasmus partnerships to employment in specific foreign regions, and thus to estimate link effects at the subnational level. Official migration statistics are typically reported by country, are not linked to where migrants studied, and capture movements within the EU incompletely, because free movement removes border controls and relies on registrations that movers often do not make. To my knowledge, no other source follows graduates across countries and subnational regions over several decades, for exchange participants and non-participants alike, on a comparable scale.

These advantages come with selective coverage. The ICP-period sample contains about 10% of first degrees awarded by EU-15 universities to the 1980–1997 cohorts, and about 13% of those awarded to cohorts graduating after Erasmus began. Coverage rises steadily across cohorts, from 3.6% for 1980 to 17.7% for 1997, and reaches about 35% for the 2013–2020 cohorts used in the analysis of recent exchange flows. Pooling the 1980–1997 cohorts, coverage is highest in the Netherlands, Belgium, the United Kingdom, Sweden, and Ireland, and lowest, apart from Luxembourg, in Greece, France, Italy, Austria, and Germany. Business, engineering, and the social sciences are over-represented, and education, the humanities, medicine, and the arts are under-represented (Appendix A.3). Coverage is nonetheless substantial for data that follow graduates across national borders, and as a robustness check i will re-estimates the main specification in the largest and best-covered countries and fields.

Despite selective coverage, the LinkedIn sample reproduces official migration data at both levels at which the analysis operates (Appendix A.8). Across countries, the number of 1980–1997 graduates working in another EU-15 country in 2000 tracks the official stock of tertiary-educated migrants on the same country pair, with an elasticity of 0.82, or 0.62 within origin and destination countries. Within a country, the distribution of LinkedIn arrivals across Spanish regions in 1996–2000 reproduces the population register, with an elasticity of 0.74. The sample also tracks the

[13]The coding treats first access as durable because institutional knowledge and contacts may persist beyond formal renewal, while non-renewal does not undo the exposure received by earlier cohorts.

exchanges that generate the variation. Compared with European Commission records for 2008–2021, inferred exchanges match administrative participation rates one for one on average, and their allocation across host regions within departments follows the administrative records with a slope of 0.8. LinkedIn therefore measures where graduates work, across and within countries, and where they went on exchange, closely enough to study university graduate migration at the subnational level and to measure the first-stage response with recorded exchanges.

3.1 Effects on Exchange Participation

Before examining employment, I verify that Erasmus expanded exchange along the reconstructed links rather than relabeling existing stays. Figure 3 shows the raw data. Exchange between eventually linked department–institution pairs roughly doubles in the opening year, from about 0.006 stays per 100 students, and keeps growing, to about 0.025 three years later and 0.05 after nine. Exchange with never-linked institutions stays at or below 0.001 throughout. Linked pairs, however, already show rising exchange before opening, which could reflect departments or destinations that were becoming more internationally active in this period.

To absorb such trends, Appendix C.4 re-estimates the response at the department–destination–cohort level with the fixed effects of the gravity analysis below and exchange participation as the outcome.[14] Preopening coefficients are small relative to the post-opening response, and exchange then rises steadily, from 0.004 percentage points for the first exposed cohort to 0.038 nine cohorts later. The pooled estimate of 0.0149 percentage points is about four times the preopening mean among eventually linked corridors and ranges from 0.0120 to 0.0154 under richer fixed effects (Appendix Table C.9, Panel B). Because the outcome is inferred from short foreign education spells rather than from the Erasmus label, this increase reflects additional study abroad rather than relabeling. It also validates the historical link construction and the LinkedIn exchange measure.

[14] I estimate this specification in levels because links typically move exchange along a corridor from zero to positive values, so proportional effects are uninformative.

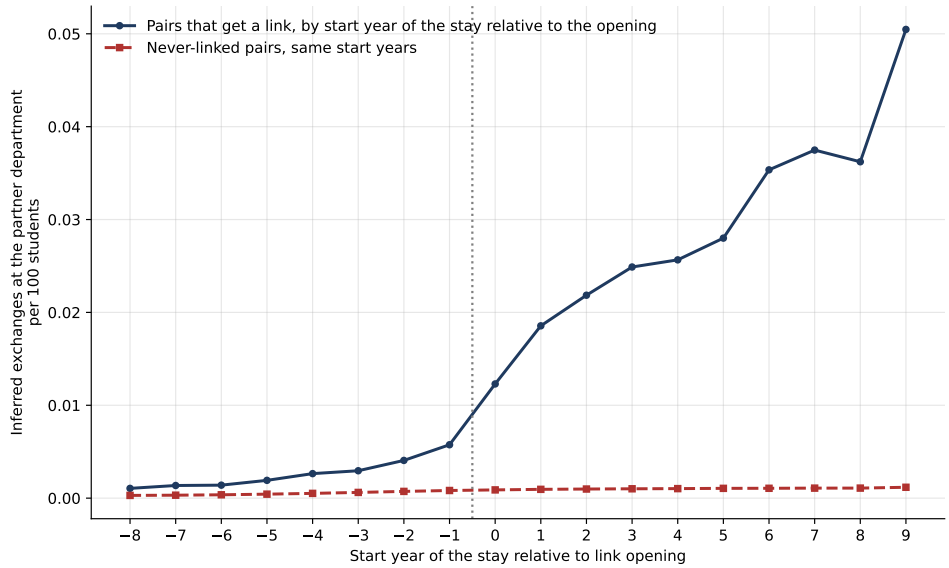


Figure 3: Exchange Participation by Year Relative to Link Opening

Notes: This figure plots inferred exchanges at the partner institution per 100 students of the home department, by start year of the stay relative to the first academic year in which the department pair is listed in a student-mobility ICP. Each pair combines an EU-15 department with a foreign EU-15 institution that has a department in the same subject area. The solid line averages over pairs first listed in 1987/88–1995/96, and the dashed line over pairs that never share an ICP, reweighted to the start years of the linked pairs. Year 0 is only partly exposed, since stays beginning in its spring precede the listing. Inferred exchanges are short foreign education spells attributed to a degree at the home institution. The sample covers stays starting in 1979–1996 by students of the 1980–1997 graduation cohorts.

4 Location Choice, Gravity, and Identification

This section presents the empirical framework used to estimate how Erasmus links affect graduates' employment destinations. I briefly introduce the location-choice model underlying the gravity equation, then describe estimation, the staggered-treatment design, and the identifying assumptions.

4.1 Location Choice and Gravity

The framework follows the random-utility tradition of McFadden (1974) and its application to migration gravity equations in Bertoli and Fernández-Huertas Moraga (2013), reviewed by Beine et al. (2016). Graduate i from department d and graduation cohort c decides on employment location post-graduation and derives utility from destination l according to

$$u_{i,d,l,c} = \ln w_{l,c} + a_{l,c} - \kappa_{d,l,c} + \frac{1}{\theta}\epsilon_{i,l}, \tag{1}$$

where $w_{l,c}$ is real income, $a_{l,c}$ captures destination amenities, and $\kappa_{d,l,c}$ is a bilateral utility component. It captures anything specific to the pair, including information, social connections, destination-specific returns to skills, and graduates' preferences for the destination. This bilateral term encompasses information frictions, social connections, and destination-specific returns to skills. The shocks $\epsilon_{i,l}$ are i.i.d. type-I extreme value, and $\theta > 0$ is the inverse scale of taste heterogeneity.

Under these assumptions, the probability that a graduate chooses destination l takes the multinomial-logit form. Let $Y_{d,l,c}$ denote the number of graduates from department d and cohort c residing in destination l. Its expectation equals this choice probability multiplied by the total number of graduates in the department–cohort, giving

$$\mathbb{E}[Y_{d,l,c}] = \exp(\mu_{d,c} + \lambda_{l,c} - \theta\kappa_{d,l,c}), \tag{2}$$

where $\mu_{d,c}$ combines the number of graduates with the choice-set denominator, $\lambda_{l,c} = \theta(\ln w_{l,c} + a_{l,c})$ captures destination attractiveness, and $-\theta\kappa_{d,l,c}$ is the bilateral utility component. Appendix B provides the derivation.

I treat the opening of an Erasmus link as a shock to this bilateral component. Exchanges may provide information, create social connections, develop destination-specific skills, or change graduates' preferences for the destination, making it more attractive to the department's graduates. The framework accommodates all of these channels without distinguishing among them. The next subsection specifies how link exposure enters the gravity equation and describes estimation and identification.

4.2 Estimation and Identification

Choice set and outcome. A destination l is a NUTS2 region in one of the EU-15 countries that participated in Erasmus during the ICP phase, or one of three aggregate outside options: EFTA, countries that entered Erasmus after the ICP phase, and the rest of the world. The choice set $\mathcal{L}$ also includes all regions of the graduate's home country. Let $K(d)$ and $K(l)$ denote the countries of department d and destination l, and let $r(d)$ denote the NUTS2 region of department d. Domestic destinations and the aggregate outside options are never directly treated.

Let $s_{i,l}$ be the share of graduate i's observed career years in the balanced 28-year window spent employed in l, with $\sum_{l\in\mathcal{L}} s_{i,l} = 1$. The outcome for department d, graduation cohort c, and destination l is the employment mass

$$Y_{d,l,c} = \sum_{i\in(d,c)} s_{i,l} \qquad N_{d,c} = \sum_{l\in\mathcal{L}} Y_{d,l,c} \tag{3}$$

where $N_{d,c}$ is the number of located graduates in the department–cohort.

Treatment and empirical specification. Let $G_{d,l}$ denote the first graduation cohort exposed to an Erasmus link from department d to destination l. Treatment begins when $c \geq G_{d,l}$ and is staggered across department–destination pairs. I model link exposure as a change in the bilateral component of equation (2). Allowing treatment effects to vary across link-creation cohorts g and graduation cohorts s, I estimate[15]

$$\mathbb{E}[Y_{d,l,c} \mid \cdot] = \exp\left(\sum_{g,s} \beta_{g,s} \mathbf{1}\{G_{d,l} = g\}\mathbf{1}\{c = s\} + \xi_{d,l} + \mu_{d,c} + \lambda_{l,c} + \varphi_c^K B_{d,l}^K + \varphi_c^R B_{d,l}^R\right), \quad (4)$$

with $s = g - 1$ omitted for each treatment cohort.

Department–cohort effects $\mu_{d,c}$ absorb changes common to a department's graduates, including the number observed in each cohort. Destination–cohort effects $\lambda_{l,c}$ absorb changes in destination attractiveness common across departments. Pair effects $\xi_{d,l}$ absorb persistent differences between department–destination pairs. The indicator $B_{d,l}^K$ equals one for destinations in the department's home country, and its cohort-specific coefficient φ_c^K absorbs the general rise in foreign relative to domestic employment that came with European integration and globalization. Similarly, $B_{d,l}^R$ equals one for the department's home region, and φ_c^R absorbs changes in graduates' propensity to stay where they studied.[16]

The treatment terms follow the extended two-way fixed-effects approach of Wooldridge (2023, 2025), allowing effects to vary by link-creation cohort and calendar cohort rather than imposing homogeneous effects under staggered adoption.[17] This avoids the negative weights issue that plagues staggered difference-in-differences designs (see Arkhangelsky and Imbens, 2024, for a review).

Estimation of the gravity equation. I estimate equation (4) using Poisson pseudo-maximum likelihood (PPML). PPML estimates the multiplicative conditional mean in levels, so zero employment flows within retained department–destination pairs remain in the sample and no transformation of zero outcomes is required. It also avoids the inconsistency of log-linearized OLS under heteroskedasticity (Santos Silva and Tenreyro, 2006).

Estimation is conducted on department–destination–cohort cells, but it is equivalent in slope

[15] In the common-effect case, the bilateral component is $-\theta\kappa_{d,l,c} = \xi_{d,l} + \varphi_c^K B_{d,l}^K + \varphi_c^R B_{d,l}^R + \beta D_{d,l,c}$, where $D_{d,l,c} = \mathbf{1}\{c \geq G_{d,l}\}$. The specification below replaces the common treatment coefficient with cohort-specific interactions. Appendix B provides the derivation.

[16] The home-country control follows the use of border-by-time effects in gravity models combining domestic and international flows (Bergstrand et al., 2015; Nagengast and Yotov, 2025).

[17] Nagengast and Yotov (2025) use the same PPML structure in a gravity setting.

coefficients to estimation on the underlying individual location choices. Because systematic utility varies only at the cell level, the individual data enter the likelihood only through the cell employment mass $Y_{d,l,c}$. With department $\times$ cohort fixed effects, the PPML score equations therefore coincide with those of the corresponding individual-level conditional-logit model (Guimarães et al., 2003; Schmidheiny and Brülhart, 2011). Pairs with zero employment mass in every cohort do not contribute to the PPML likelihood, while zero flows within retained pairs remain in the estimation.

For the event study figures, I define event time as $k = s - g$ and average the estimates across treatment cohorts, weighting each treated department–destination–cohort cell by its number of graduates. [18] Standard errors are clustered at the department–destination pair, the level of treatment assignment.

Identification. Identification comes from whether employment of a department's graduates rises disproportionately in a destination after the department opens a link to that destination, relative to the contemporaneous evolution of not-yet- and never-linked corridors. The comparison is conditional on the fixed effects and controls in equation (4). The identifying assumptions are parallel trends conditional on these terms and no anticipation. In the multiplicative specification, conditional parallel trends require that, absent the link, expected employment of the department's graduates in treated and comparison corridors would have followed the same proportional evolution once these effects are removed. The event-study leads provide a diagnostic for these assumptions.

Interpretation. The coefficient β captures the relative change in employment in the linked destination among graduates of a given department and graduation cohort. Its causal interpretation rests on the identifying assumptions discussed above and does not require the multinomial-logit structure.

Under the location-choice model introduced above, β also captures a change in bilateral utility, $\beta = -\theta \Delta\kappa_{d,l,c}$. Translating this change into consumption units requires calibrating θ, which I do in Section 7 to benchmark the economic magnitude of the estimates.

[18] Specifically,

$$\bar{\beta}_k = \sum_{g \in \mathcal{G}_k} \omega_{g,k} \beta_{g,g+k}, \qquad \omega_{g,k} = \frac{N_{g,k}}{\sum_{g' \in \mathcal{G}_k} N_{g',k}}, \qquad N_{g,k} = \sum_{(d,l):\, G_{d,l}=g} N_{d,g+k},$$

where $\mathcal{G}_k$ contains the treatment cohorts observed at event time k. I construct the pooled post-treatment coefficient analogously and calculate standard errors for these averages using the delta method. I also use alternative weighting schemes as robustness checks.

5 Link-Level Effects

This section estimates how establishing a student-mobility ICP link between a department and a foreign region affects where the department's graduates work. I first estimate the employment response in the linked region, trace it over the career, and test its robustness. I then examine how the response varies across corridors and how far it extends beyond the linked region.

5.1 Employment Effects Along Linked Corridors

Employment rises in the linked region. Figure 4 reports the preferred PPML event study. The outcome is the number of a department's graduates from a given cohort who work in a destination, with each graduate weighted by the share of their first 28 career years spent there, so it counts graduates in full-career equivalents. Event time measures how long the link had existed when a cohort graduated, with $k = 0$ the first cohort exposed to the link. Three features stand out. First, the pre-treatment coefficients are flat and close to zero, so employment along corridors that later received a link evolved like employment along comparison corridors before the link opened. Second, the effect is immediate: employment in the linked region rises by about 0.06 log points already for the first exposed cohort, which could use the link at most in its final year of study. Third, the effect grows with the age of the link, reaching about 0.19 log points for cohorts graduating eight to nine years after opening, with no sign of reversal. This growth mirrors the first stage, where exchange along a link expands as the partnership matures and later cohorts are exposed for more years of study. Pooling all post-opening cohorts gives an effect of 0.106 log points ($t = 6.2$), or an 11.1% increase in employment in the linked region.

The effect persists over the career. Because event time in Figure 4 refers to the age of the link, not to the age of the graduates, the career-window outcome cannot show whether graduates stay in the linked region or return home after a few years. I therefore re-estimate the model with employment measured at fixed years after graduation. Figure 5 shows the event studies for employment 1, 5, 10, and 25 years after graduation. At every horizon, the pre-treatment coefficients are flat and the post-opening coefficients are positive and of similar size, so the pattern in Figure 4 is not driven by a particular stage of the career. Appendix Table C.1 reports the pooled coefficients: 0.111, 0.135, 0.115, and 0.100 log points at the four horizons. Appendix Figure C.1 extends the exercise to every career year from one to 28. The estimates stay between 0.10 and 0.14, with a modest phase-in over the first five years and no later decline. The response thus appears at labor-market entry and remains a quarter century later, rather than reflecting temporary jobs near the exchange destination.

A concern with fixed-horizon outcomes is that the set of graduates observed at each career

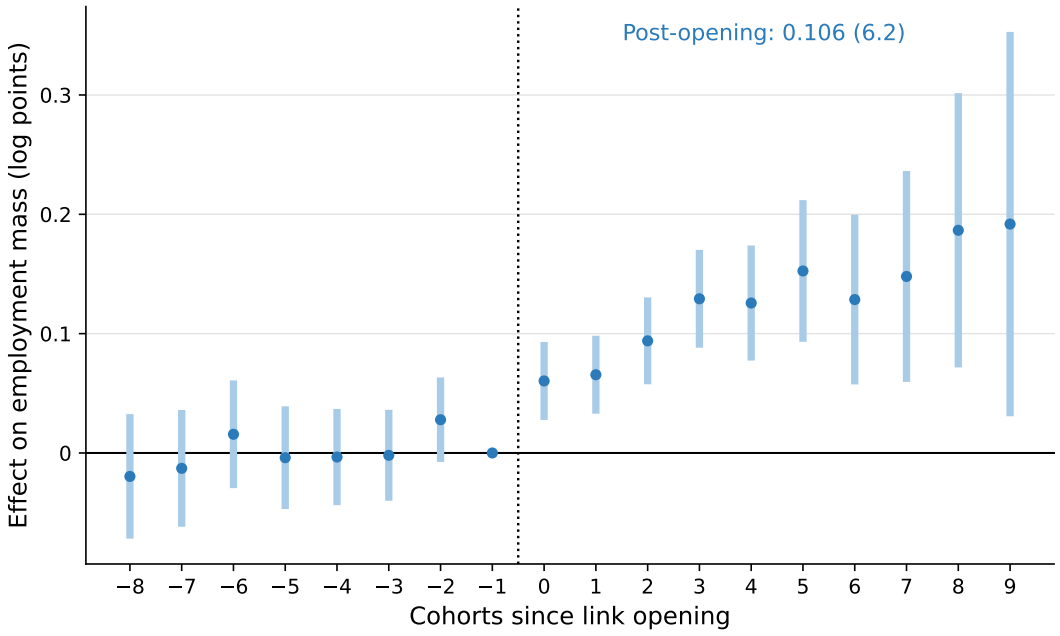


Figure 4: Establishing an ICP link increases employment in the linked region

Notes: This figure plots PPML event-study estimates from equation (4).The outcome is the number of graduates of department d and cohort c working in destination l, with each graduate weighted by the share of their observed career years in the first 28 years after graduation spent employed in l. Event time k is the graduation cohort minus the first cohort exposed to the department–destination link, so it measures how long the link had existed when the cohort graduated. Coefficients are in log points, estimated separately by link-opening cohort and graduation cohort and aggregated using graduate-mass weights. The specification includes department × destination, department × cohort, and destination × cohort fixed effects, together with home-country × cohort and home-region × cohort controls. Effects are relative to $k = -1$, the graduation cohort immediately preceding treatment. Bars show 95% confidence intervals. Standard errors are clustered by department–destination pair, the level at which treatment is assigned.

year could itself respond to Erasmus. If exposure changed which graduates have a recorded job at a given horizon, the profile would mix changes in where graduates work with changes in who is observed. Two pieces of evidence speak against this. First, Appendix Figure C.1 also reports the profile for a balanced sample of graduates with a located job in every career year up to each horizon, so that the same graduates are followed along the profile. This profile closely tracks the one for all located graduates. Second, Appendix Table C.2 shows that entering Erasmus does not systematically change the number of graduates with a located employment record at any of the four horizons.

These findings complement evidence from German graduate surveys that studying abroad raises the probability of later working abroad (Parey and Waldinger, 2011). The present design differs in scope and in the question it answers. It covers the whole program across all EU-15 countries, roughly two million graduates, and destinations at the level of subnational regions, and it follows careers for 25 years. It also estimates the effect of access to a link for all graduates of a department, not only for participants, and therefore captures responses among classmates who never went abroad. Next section discussess spillovers to non-participats in more details.

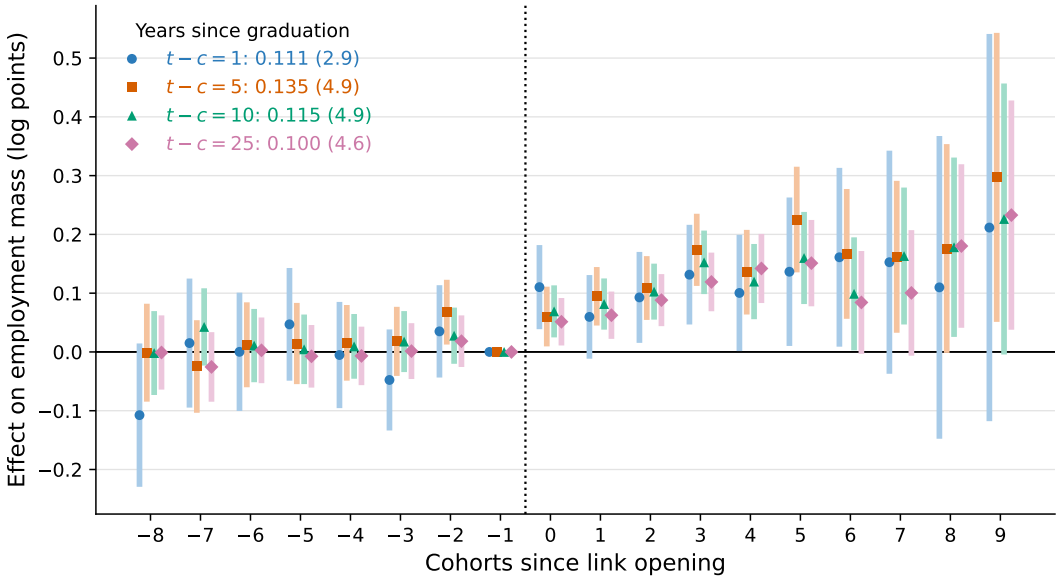


Figure 5: The linked-region employment effect persists over the career

Notes: This figure plots PPML event-study estimates of employment in the linked destination 1, 5, 10, and 25 years after graduation. For each horizon, the outcome is the number of a department's graduates from a given cohort employed in the destination in that year after graduation, and each horizon is estimated separately using equation (4) with the preferred fixed effects. Event time is defined as in Figure 4. Coefficients are in log points and aggregated across link-opening cohorts with weights proportional to the number of graduates. Effects are relative to $k = -1$. Bars show 95% confidence intervals. Standard errors are clustered by department–destination pair.

Robustness to richer fixed effects. The staggered expansion of ICP links generates variation across departments within universities and across regions within destination countries. This allows me to test whether the employment response was indeed due to the opening of a particular link rather than broader changes in bilateral mobility, university relationships, or departments' orientation toward foreign countries.

Column (2) of Table 1 addresses the possibility that European integration increased mobility more between some country pairs than others. Directional country-pair $\times$ cohort effects replace the common home-country control with unrestricted country-pair-specific changes. The link effect therefore cannot be attributed simply to graduates from one country becoming more likely to work in another as bilateral economic ties strengthen.

Column (3) addresses university-wide connections to particular destinations. For example, recruitment relationships or cooperation with institutions in a foreign region could increase employment there across several departments, independently of their own Erasmus links. University $\times$ destination $\times$ cohort fixed effects absorb the component of these changes shared across departments. Identification then compares departments within the same university for the same destination and graduation cohort.

Column (4) addresses department-specific changes that could increase employment throughout a destination country. For example, a department might introduce Spanish-language classes

Table 1: Employment effects under richer fixed-effect structures

	(1)	(2)	(3)	(4)	(5)
Post-opening	0.1057***	0.0926***	0.0786***	0.1103***	0.1039*
	(0.0171)	(0.0172)	(0.0241)	(0.0254)	(0.0550)
Mean of y, all cells	0.5412	0.5425	0.9926	0.7488	1.1442
Mean of y, foreign EU-15 cells	0.0693	0.0696	0.1966	0.1699	0.3257
Observations	3,674,907	3,665,921	1,966,182	2,220,372	1,399,427
Origin dept. × Dest. reg. FE	✓	✓	✓	✓	✓
Dest. reg. × Cohort FE	✓	✓		✓	
Origin dept. × Cohort FE	✓	✓	✓		
Home-region × Cohort effects	✓	✓	✓	✓	✓
Domestic-country × Cohort effects	✓				
Country pair × Cohort FE		✓	✓		
University × Dest. reg. × Cohort FE			✓		✓
Origin dept. × Dest. country × Cohort FE				✓	✓

Notes: Pooled post-opening PPML estimates using destination employment mass over the balanced 28-year career window. Coefficients are in log points and aggregated using graduate-mass weights. Column (1) reports the preferred specification. Columns (2)–(4) include directional country-pair × cohort, university × destination × cohort, and department × destination-country × cohort effects, respectively. Column (5) combines the restrictions in columns (3) and (4). All specifications retain the home-region × cohort control. Sample sizes can decline because richer effects generate additional all-zero groups subject to separation or singleton groups with no slope information. Standard errors in parentheses are clustered at the department–destination level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

or Spain-focused career guidance, making its graduates more likely to work across Spain independently of a particular link to Madrid. Department × destination-country × cohort fixed effects absorb the country-wide component of such changes. Identification therefore compares employment in Madrid with employment in other Spanish regions within the same department–cohort, rather than attributing a general increase in employment in Spain to the Madrid link. Column (5) combines the within-university and within-destination-country restrictions.[19]

The point estimates remain close to the baseline 0.106 log points: 0.093, 0.079, 0.110, and 0.104 across columns (2)–(5). The employment response thus remains associated with the particular department–region link after controlling for country-pair integration trends, university-wide destination ties, and department-specific shifts toward the destination country.

Because richer fixed effects also change the PPML estimation sample[20], Appendix Table C.3 repeats the comparison on column (5)'s estimation sample. Country-pair and department × destination-country × cohort controls leave the estimate essentially unchanged, so their stability does not reflect changes in the retained observations. Because the latter absorb a department's

[19]These controls can also absorb genuine Erasmus responses shared across departments or destinations. I therefore use them as robustness checks rather than as estimates of the link's full reduced-form response.

[20]Poisson regression drops fixed-effect groups with zero employment in every cell, which carry no information, as well as singleton groups. Finer fixed effects create more such groups, so the sample falls from 3.67 million cells in column (1) of Table 1 to 1.40 million in column (5). The estimates therefore condition on groups with some observed employment.

ties to the destination country as a whole, this also suggests that the effect operates at the regional rather than the country level. University × destination × cohort controls reduce the estimate, consistent with removing a component shared within universities, but leave a positive link-specific response.

Selection into link timing and cohort composition. A potential violation of conditional parallel trends arises if departments opened links in response to improving employment prospects in particular destinations. Appendix Figure C.2 shows that geographic proximity predicts openings mainly in the first two program years. At the same time income, linguistic, and cultural distance show no systematic relationship with timing. The employment response is not confined to these earliest openings: effects remain positive for the first four link-opening cohorts, extending beyond the period in which geographic proximity predicts link formation (Appendix Figure C.3). Moreover, the larger employment effects for earlier links parallel their larger effects on exchange participation (Appendix Figure C.4). This correspondence is consistent with link-induced student mobility being leading to greater employment response, rather than selection driving it.

Restricting the sample to eventually-linked pairs removes comparisons with never-linked corridors. The pooled effect remains 0.083 log points (Appendix Table C.5, column 6), so the result is also present when identification relies on variation in opening dates among selected corridors.

Prospective students may also choose departments partly for their Erasmus destinations. Such sorting can contribute to the link's reduced-form effect and complicates its interpretation as a response by a fixed student population. It cannot readily explain the immediate response, since students entering after a link opens would generally graduate several years later. Composition changes may nevertheless contribute at longer event times, with earlier changes possible in shorter postgraduate programs.

Selection into LinkedIn. LinkedIn users are not a representative sample of European graduates, which raises three questions: whom the estimates describe, whether a move itself makes a graduate more likely to appear in the data, and whether exposure to Erasmus changes who appears even among graduates who never move. Appendix D develops the intuition in a simplified setting.

The first question concerns external validity. Because coverage is higher in Northern Europe and in business, engineering, and the social sciences (Appendix A.3), the pooled estimate weights countries and fields by their presence on LinkedIn rather than by their size. This matters only if the effect differs across these groups, and it does not substantially: excluding any single country or field leaves the estimate unchanged (Appendix Figure C.5), and the large, best-covered fields give the same estimate as the full sample (Appendix Figure C.9).

A move may also raise the chance of appearing in the sample, for instance if graduates working in London or Madrid are more likely to have a profile or to fill employment location. This cannot create a response, since without a move nobody becomes more visible. Nor can it amplify one much. The graduates it adds to the sample are the induced movers themselves, who are very few relative to the cohort, so had they been in the sample in the counterfactual as well, the counterfactual mean would barely change.

Finally, exposure could make graduates who would have moved anyway more likely to appear in the sample. Department $\times$ cohort effects absorb changes in the number observed, and department $\times$ destination-country $\times$ cohort effects absorb shifts toward foreign work in general or toward a particular country. The estimate barely moves under these controls (Table 1). A selection explanation surviving these controls must therefore favor graduates whose counterfactual employment is concentrated in the newly linked region, in a way aligned with link timing. For example, a Sapienza graduate who would have moved to Madrid in any case would have to create a LinkedIn profile only because Sapienza opened a link to Madrid. It is difficult to think of a mechanism that would generate such a pattern.

Three further checks suggest that selection into LinkedIn is not a critical concern. First, among groups large enough for precise estimates, the best covered are departments in the United Kingdom and in business, engineering, and the social sciences, where LinkedIn covers 26–33% of the 1997 cohort against 18% overall. These groups give the same effect as the full sample (Appendix Table C.8). Second, entering Erasmus does not meaningfully change the share of observed graduates with a located employment record, about one per hundred records against a mean of 69 (Appendix Table C.2). Third, the designs in Section 6 use the 2013–2020 cohorts, with more than twice the LinkedIn coverage of the ICP cohorts, and find employment responses of comparable magnitude.

Additive counterpart. As a more transparent robustness check, I estimate the same treatment structure and fixed effects by least squares, with the share of career years spent in the destination as the outcome. Unlike PPML, the linear model keeps the full destination choice set, including department–destination pairs with no employment in any cohort, which PPML drops because they carry no information about a proportional effect. The career-window effect is 0.0140 percentage points, with similar estimates under richer fixed effects (Appendix Table C.9), so the response is robust to both the functional form and the sample restriction. At shorter career horizons, however, the linear event studies show some pre-treatment drift, so their dynamics are less clean than those of the PPML estimates (Appendix Figure C.11).

Other ICP activities. Besides student exchanges, the ICP directories record three other approved activities for each network: teacher mobility, joint curriculum development, and joint programs.

A corridor with a student-mobility link may therefore also carry these ties, which could raise employment without any student moving, for example through faculty contacts or shared courses. I add to the preferred specification an indicator for each activity, equal to one from the first directory year in which it is recorded on the same department–destination corridor. The student-mobility estimate is essentially unchanged, at 0.104 log points against the baseline 0.106 (Appendix Table C.4). The response is therefore tied to the student exchanges a link creates rather than to the other institutional ties that accompany it, consistent with the quota gradient in Section 5.2.

Additional robustness. The estimates are also robust to alternative destination choice sets (Appendix Table C.5), clustering schemes (Appendix Table C.6), aggregation weights (Appendix Table C.7), and excluding each country and field in turn (Appendix Figure C.5).

5.2 Heterogeneity in Linked-Corridor Effects

The average effect raises four questions about how links work and whom they move. First, is the response larger where links allowed more students to move, as it should be if it runs through student exchange? Second, does the induced mobility reallocate graduates toward richer and more productive regions? Third, do links matter most along corridors where information and contacts were initially scarce? Finally, do they matter less where formal barriers to working abroad remain? Appendix C.3 reports the supporting figures.

Initial student-mobility capacity. I classify linked pairs into terciles of student-mobility capacity using the quota apportioned to each pair in its opening directory year. Membership is fixed over the pair's history, and each tercile receives its own ETWFE treatment structure under the baseline fixed effects.[21]

Higher initial quotas predict larger increases in exchange participation and larger proportional employment responses (Appendix Figure C.6). The PPML coefficients rise from 0.049 log points in the low tercile to 0.079 in the middle and 0.150 in the high tercile, and their equality is rejected ($p = 0.025$). This gradient ties the employment response to the student mobility a link generates, rather than to the mere existence of an institutional connection.

Income gap and the direction of induced moves. A central motivation for labor mobility in a union is that it moves workers toward more productive places. I therefore ask whether Erasmus

[21]The directories report network-wide student quotas rather than bilateral allocations. I divide each network's total quota equally among its directed cross-country institution pairs. Because actual exchanges may be concentrated among particular partners, this is a noisy proxy for bilateral capacity. Fixing membership at opening avoids classifying links by later quota changes that may respond to their success.

links send graduates up or down the income gradient, dividing corridors into three groups by destination minus origin GDP per capita.[22] Figure 6(a) shows that the proportional effects are statistically indistinguishable across the three groups: 0.134, 0.118, and 0.108 log points toward poorer, similar, and richer destinations ($p = 0.84$). This pattern is consistent with the location-choice model, in which destination income enters a destination's attractiveness, whereas the link shifts the bilateral component, so a link raises employment by a similar proportion wherever it points. Because corridors toward richer regions are larger to begin with, the same proportional effect translates into more movers in absolute terms. Panel (b) rescales each effect by the size of its corridors: of the graduates whom links move, 44% go to richer destinations, 30% to destinations with similar income, and 27% to poorer ones. Erasmus thus reallocates graduates mainly toward richer regions, not because it changes how graduates respond to income differences, but because its links run along corridors where those differences already pull.

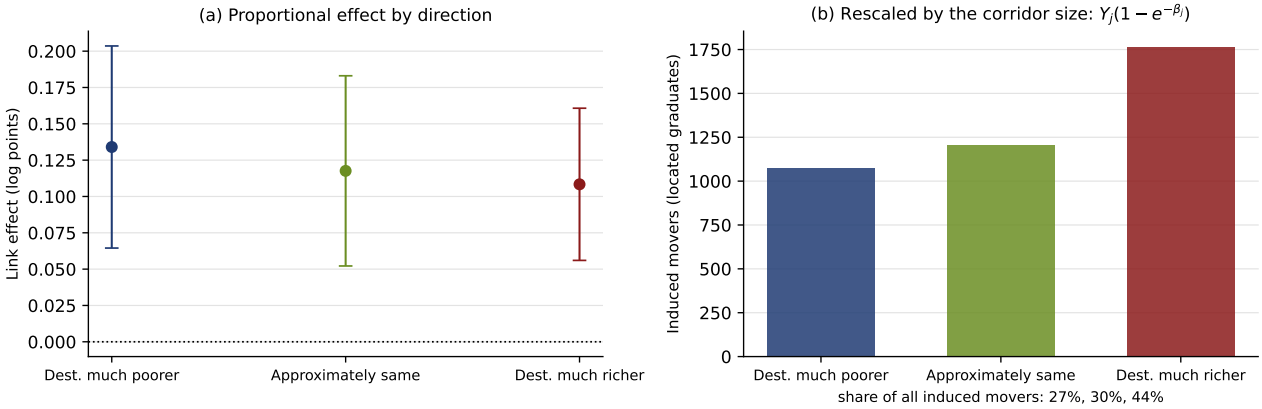


Figure 6: Links raise employment by a similar proportion in all directions but move most graduates toward richer regions

Notes: This figure reports employment effects by three groups of destination minus origin GDP per capita (PPP, 2000). Panel (a) reports pooled post-opening PPML effects in log points from a single regression interacting the treatment structure with group indicators, bars show 95% confidence intervals clustered at the department–destination level. Panel (b) converts each effect into induced movers, computed as the post-opening employment mass of treated corridors times $1 - e^{-\beta}$, and reports each group's share of all induced movers.

Initial connections. If links work by supplying information and contacts, they should matter most where departments and destinations knew little about each other: along unconnected corridors, each exchange student brings back contacts and information that did not exist before. I proxy initial connections with geographic distance and with the presence of the department's earlier graduates in the destination. Appendix Figure C.7 shows that the PPML effect rises with distance, from 0.065 log points in the nearest tercile to 0.157 and 0.248 in the middle and most distant ones. Appendix Figure C.8 shows a similar pattern for earlier presence. The effect is

[22] The cutoffs, at a gap of roughly ±4,700 euro or about 15% of median origin GDP per capita, split corridors into close to equal thirds.

1.85 log points, a large change from a very small base, where the department's pre-Erasmus graduates (1980–1987 cohorts) had no recorded presence in the destination, against 0.069 and 0.001 where their presence was below and above the median. In absolute terms both rankings reverse, because closer and better-connected corridors are larger to begin with. Links thus move the most graduates along established corridors but change employment the most where initial connections were weakest.

Fields. Information and contacts help graduates work abroad only if they are allowed to do so. Where formal barriers to practicing in another country remain, links that lower informational and social barriers should therefore raise employment abroad less. Law provides such a case: unlike doctors and architects, lawyers could not easily establish practice in another member state until the late 1990s.[23] Appendix Figure C.9 reports the effects for the seven largest fields. The six fields other than Law respond similarly to the pooled estimate, with effects between 0.06 and 0.14 log points whose equality cannot be rejected. Law is the exception. Despite having the largest exchange response, its employment effect is small and imprecise (0.03, s.e. 0.10). Because legal training is specific to a national jurisdiction, an exchange may be closer to consumption than to an investment in a foreign career for law students, which would explain why they take it up the most yet rarely move. The estimate is too imprecise to differ significantly from the other fields, but it fits evidence that occupational licensing limits geographic mobility (Johnson and Kleiner, 2020).

5.3 Geographic Reach

I next ask how geographically concentrated the employment response is. When a department establishes a link to Madrid, the effect may be confined to Madrid, extend to other Spanish regions, or reflect a broader increase in foreign employment. The data allow these margins to be separated: departments enter Erasmus in different years and continue adding links to countries and regions after entry, while employment is observed both at home and in foreign destinations that never receive a direct link.

Nested geographic margins. Let G_d^E denote the department's first cohort with access to Erasmus, $G_{d,K}^K$ its first cohort with a link to foreign country K, and $G_{d,l}$ the first cohort exposed to a direct link to destination l. Exposure dates are set to $+\infty$ for margins that are never treated. For an eventually linked destination, $G_d^E \leq G_{d,K(l)}^K \leq G_{d,l}$.

[23] Doctors obtained mutual recognition of diplomas in 1975 (Directives 75/362/EEC and 75/363/EEC) and architects in 1985 (Directive 85/384/EEC). Directive 89/48/EEC extended a general recognition system to many other regulated professions, but permanent cross-border establishment for lawyers was substantially facilitated only by Directive 98/5/EC in 1998.

I extend the normalized bilateral utility component in equation (14), written here in static form, to

$$-\theta\kappa_{d,l,c} = \xi_{d,l} + \varphi_c^K B_{d,l}^K + \varphi_c^R B_{d,l}^R + \beta^E D_{d,l,c}^E + \beta^K D_{d,l,c}^K + \beta^L D_{d,l,c}^L, \tag{5}$$

where $D_{d,l,c}^E = (1 - B_{d,l}^K)\mathbf{1}\{c \geq G_d^E\}$ indicates foreign destinations after department entry, $D_{d,l,c}^K = \mathbf{1}\{c \geq G_{d,K(l)}^K\}$ indicates regions of an eligible foreign country after the department's first link to that country, and $D_{d,l,c}^L = D_{d,l,c}$ is the direct-link indicator. The country and direct-link indicators are zero for domestic destinations and the aggregate outside options. The outside options can nevertheless receive the broad foreign-entry effect. For example, a department's first link to Madrid exposes other Spanish regions to the country and entry margins, while destinations in unlinked foreign countries receive only the entry margin. The specification therefore allows employment to respond even in destinations without a direct Erasmus link.[24]

Under this specification, β^E captures the common shift toward foreign relative to domestic destinations, β^K the additional shift toward regions of a linked country, and β^L the further increment in the directly linked region. Each treatment block receives its own treatment-cohort $\times$ calendar-cohort interactions, as in equation (4), omitting the cohort immediately before that margin's first exposure. Effects can therefore vary across treatment cohorts and over time within each margin. The joint specification nevertheless imposes additivity in the log conditional mean: a margin's effect does not additionally depend on exposure to the other margins (de Chaisemartin and D'Haultfœuille, 2023).

The identifying comparisons follow this geographic structure. Department entry is identified from changes in foreign relative to domestic employment across departments entering at different times, including departments that have not yet or never enter, net of the common cohort changes in home bias. The country increment compares unlinked regions of linked countries with destinations in countries to which the department is not yet linked, conditional on the broad entry response. The direct-link increment compares linked and unlinked regions within the same destination country.

The aggregate outside options, including the rest of the world, help separate the broad response from the country and regional increments: they can receive the entry effect but never a direct-link or linked-country effect. Domestic destinations provide the foreign-versus-home reference (Larch et al., 2025). Each margin requires conditional parallel trends and no anticipation for its corresponding comparison.[25]

[24] This distinction between direct treatment and indirect exposure is related to the spillover problem studied by Butts (2023). Here exposure follows department entry and country links rather than proximity to a treated location.

[25] Appendix Table C.12 documents the available support: 98% of linked departments reach some but not all foreign countries, 99% of linked department–country pairs leave at least one region unlinked, and the three exposure dates coincide for only 20% of linked pairs. In single-region destination countries, the country and direct-link indicators coincide, so their separate identification comes from multi-region countries.

Local and broad effects, with little country-level increment. Appendix Table C.11 reports the joint estimates. The directly linked region receives an additional effect of 0.1013 log points ($t = 5.3$), while department entry produces a broad foreign-destination effect of 0.1001 log points ($t = 4.0$). The additional linked-country estimate is close to zero and statistically indistinguishable from it: -0.0045 log points ($t = -0.2$).

The direct-region coefficient is nearly unchanged when the broader margins are introduced, falling from 0.1057 when estimated alone to 0.1013 in the joint specification. The response therefore has two distinct components: an increment in the directly linked region and a broader shift toward foreign employment. Other regions of the linked country share the broad foreign response, but show little additional increase specific to that country.

The broad coefficient measures a shift toward foreign rather than domestic employment after department entry, over and above the linked-country and linked-region increments. It therefore addresses whether Erasmus expands outward mobility, rather than only changing its foreign destinations. Because this margin relies on department-entry timing, a relevant concern is that entry coincides with a university-wide internationalization policy.

Column (4) addresses this concern by adding university $\times$ foreign-status $\times$ cohort fixed effects. These absorb changes in foreign relative to domestic employment shared across departments of the same university, including common effects of university-wide language provision, foreign-career services, or broader internationalization policies. Identification then comes from departments entering Erasmus at different times within the same university. The corridor increment remains broadly unchanged, while the entry estimate is 0.129 log points ($t = 4.5$). The broad response is therefore not explained by a common university-wide shift toward foreign employment.

Figure 7 reports the corresponding event studies, including the pre-exposure coefficients for each margin. The direct linked-region response builds as the link matures, while the broad foreign margin is flatter and less precisely estimated.

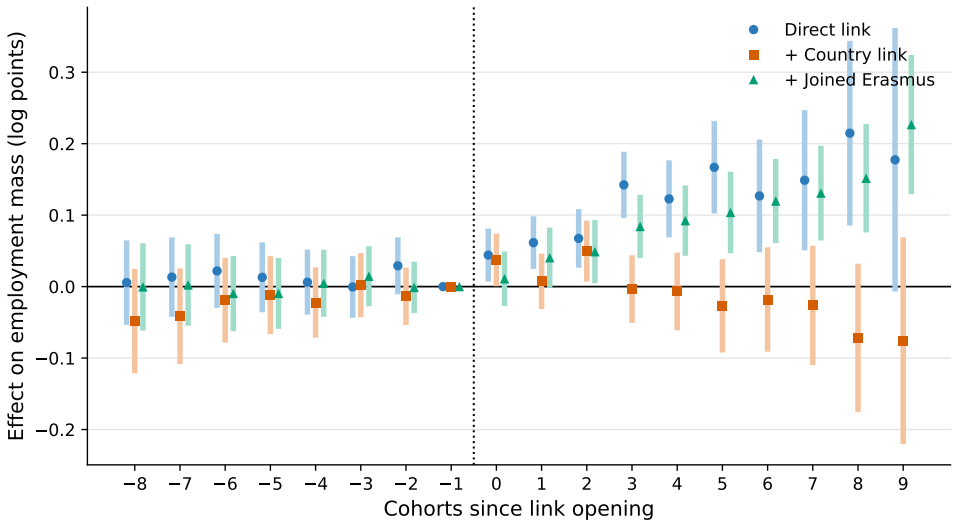


Figure 7: Dynamic paths of the three geographic margins

Notes: PPML event-study paths of the three jointly estimated treatment margins: the direct department–region link, any link to the destination country, and the department's first entry into Erasmus interacted with all foreign destinations. Each margin is measured relative to its own treatment date and enters with its own treatment-cohort × event-time structure. Coefficients are in log points. Bars report 95% confidence intervals based on standard errors clustered at the department–destination level.

The broad entry margin remains more exposed to department-specific changes in LinkedIn composition. Greater representation of internationally mobile graduates could raise observed foreign employment without changing individual location choices. University × foreign-status × cohort effects absorb such shifts when shared across departments of the same university, but not when they are specific to the department entering Erasmus. This is an additional reason to interpret the broad foreign margin more cautiously than the corridor effect identified within destination countries.

Interpretation. The linked-region increment is consistent with destination contacts facilitating access to local employment, alongside evidence that networks shape migrants' employment outcomes and destination choices (Munshi, 2003; Blumenstock et al., 2025). Contacts with students, faculty, and alumni at partner institutions could provide information or referrals specific to the surrounding region. The broad foreign response is consistent with information and confidence that facilitate pursuing careers abroad more generally. Both channels can benefit classmates without a recorded exchange, connecting the geographic pattern to the preceding results.

Language is also an established determinant of international migration (Adserà and Pytliková, 2015). A predominantly host-country-specific human-capital explanation would, however, predict an additional response in other regions of the linked country, where language skills and familiarity with national institutions should also be useful. The near-zero country increment provides little support for that geographic prediction. Taken together, the local and broad responses are more

consistent with region-specific connections and a wider international orientation than with an employment response centred on the host country as a whole.

The evidence thus points toward benefits extending through the department, while leaving the separate contributions of participating classmates, institutional contacts, and shared services unresolved.[26]

6 Unpacking the Link Effect and Spillovers to Non-Participants

The link effect estimated above may operate through three channels arising from the multilateral structure of ICPs. Consider a network linking departments A, B, and C. First, students from A may study in B's region and subsequently stay or return after graduation. Second, incoming students from B may provide local students at A with information and contacts that influence their location choices. Third, students from A and B may meet while on exchange at C. The second channel affects students not participating in exchanges, providing spillovers to non-participants, while the third can connect graduates to destinations they have never visited. Appendix Figure C.12 illustrates these possibilities. Since membership in the same ICP provides access to all three channels, the historical link-opening design cannot distinguish them.

I use administrative Erasmus records for academic years 2008/09–2023/24 to provide complementary evidence on these channels. The first exercise separates outgoing from incoming flows and compares graduates with and without a recorded exchange. The second examines participants' exposure to peers from other regions at a common host institution.These exercises exploit variation in realized exchanges and student overlap that is unavailable in the historical directories. Although they concern a later period, they may shed light on the mechanisms underlying the ICP-era effects, as exchanges continued to create similar opportunities for exposure and interpersonal contact.

6.1 Separating Outgoing and Incoming Student Flows

Data and outcome. The European Commission's website provides open data on Erasmus mobility through anonymized individual-level records covering 2008/09–2023/24. These records report sending and receiving institutions, education level, field of study, stay dates, gender, nationality, and age. I retain approximately 2 million exchange records for which both institutions can be identified and matched to institutions in the LinkedIn data. Using these records, I reconstruct annual inflows and outflows between departments and destination regions, as well as

[26]To the extent that participants generate utility gains for classmates that they do not internalize, exchange creates a positive externality and an additional rationale for subsidies. The present design does not separately quantify that externality or the optimal number of places.

the composition of exchange peers. As elsewhere in the paper, a department is a university–subject pair. [27]

The estimation sample covers 7.8 million graduates from approximately 9,500 departments in the EU-28, graduation cohorts 2013–2020, and 281 destination units[28], yielding about 26 million department–destination–cohort cells, of which 2.6 million carry identifying variation after the Poisson separation step. For these cohorts, the records cover the full annual exposure window and earlier exchange activity.

I retain the employment outcome used in the main analysis, restricting the career window to years one to five after graduation. Each located graduate contributes a total weight of one, allocated across destinations according to the fraction of their observed employment years spent in each. I estimate the model for all graduates and separately for those with and without an inferred exchange. The inferred-exchange measure identifies a foreign education spell lasting less than one year and occurring within enrollment in the home degree.

Exposure and specification. I distinguish exchanges by their timing relative to graduation. Let $E^{to}_{d,l,c;\tau}$ indicate whether any student from department d studied in region l at relative time τ, and let $E^{from}_{d,l,c;\tau}$ indicate whether any student from an institution in l studied at d. For $\tau \in \{-3,-2,-1,0,+1\}$, exposure refers to academic year $c+\tau-1/c+\tau$, so $\tau=0$ is the academic year ending in the graduation year. Two additional bins, <-3 and $>+1$, indicate any exchange in the observed years before and after this window, respectively.

I estimate the following specification by PPML:

$$\mathbb{E}[Y_{d,l,c} \mid \cdot] = \exp\left\{ \sum_{\tau\in\mathcal{T}} \left(\beta^{to}_{\tau} E^{to}_{d,l,c;\tau} + \beta^{from}_{\tau} E^{from}_{d,l,c;\tau}\right) + \xi_{d,l} + \mu_{d,c} + \lambda_{l,c} + \varphi^{K}_{c} B^{K}_{d,l} + \varphi^{R}_{c} B^{R}_{d,l} \right\}, \quad (6)$$

where $\mathcal{T} = \{<-3,-3,-2,-1,0,+1,>+1\}$. The fixed effects follow the main gravity specification: department–destination effects $\xi_{d,l}$ absorb persistent bilateral differences, department–cohort effects $\mu_{d,c}$ absorb factors common to each department's graduating cohort, and destination–cohort effects $\lambda_{l,c}$ absorb changes in destination attractiveness. The home-country indicator $B^{K}_{d,l} = \mathbf{1}\{K(l) = K(d)\}$ and home-region indicator $B^{R}_{d,l} = \mathbf{1}\{l = r(d)\}$ interact with cohort to allow home bias to evolve over time. I estimate the model separately for each sample, re-estimating all coefficients and fixed effects.

[27] Because the administrative subject classification changed in 2014 and differs from the ICP subject areas, subjects are harmonized to eight broad ISCED fields in this design.

[28] Destinations are the NUTS2 regions of the 28 countries that belonged to the EU during this period, with the other Erasmus program countries and the rest of the world as two residual destinations. Origin departments are in the EU-28.

Identification. The identifying assumption is that, conditional on department–destination, department–cohort, and destination–cohort fixed effects, home-country and home-region indicators interacted with cohort, and the other exchange exposures, the timing of inflows and outflows is unrelated to unobserved determinants of a cohort's employment in the destination. Both directions enter jointly, so their separate effects are identified by differences in timing and incidence within department–destination pairs. Conditional on persistent bilateral ties, year-to-year variation in inflows is plausibly exogenous to the home cohort's destination preferences, since it reflects decisions by students at partner institutions. Outflows are more susceptible to demand-driven selection, as students' destination preferences may influence both their exchanges and subsequent employment. This concern is weaker for non-participants, whose own choices do not directly determine outgoing exchanges, although preferences may be shared with participating classmates.

The outer bins also help distinguish exposure during enrollment from persistent exchange relationships. Controlling for earlier and later flows reduces the risk that the annual coefficients capture the broader exchange history rather than the timing of student overlap. The outer-bin coefficients themselves do not identify the effect of establishing a partnership.

The main remaining concern is a time-varying department–destination connection, such as an employment agreement, that increases both exchanges and subsequent hiring. Richer fixed effects, following the main ICP analysis (TBD), can absorb broader regional and institutional shocks but would not eliminate such bilateral developments. I assess this explanation using the timing, shape, and intensity of the response.

A persistent hiring arrangement should benefit both cohorts entering the labor market and recent graduates already working. It would therefore generally predict a response to exchanges before and after graduation, including at $\tau = +1$ and for later exchanges within the observed career window. Contact during university instead predicts a response concentrated among cohorts enrolled when the visitors arrive, making the post-graduation coefficients the key diagnostic.[29] Different timing profiles for inflows and outflows would provide further evidence against a common underlying shock, while a response increasing with the number of incoming students would be consistent with greater opportunities for contact. To explain all three patterns, an alternative hiring mechanism would need to coincide closely with exchange activity, disproportionately benefit enrolled cohorts relative to recent graduates, and vary with exchange volume while generating different timing profiles across the two directions.

Results and spillovers. Figure 8 presents the results separately for graduates with and without an inferred exchange. In the full sample, having at least one incoming student in the graduation

[29] Post-graduation exchanges are not a strict placebo for all contact channels, since alumni may remain connected to their department. Earlier exchanges may also matter for students enrolled for more than four years.

year is associated with approximately 6% higher subsequent employment in that student's region of origin. Outgoing exchanges are associated with increases of approximately 2–4% in each of the final three academic years, $\tau = -2, -1, 0$, rising toward graduation.

The inflow results provide the clearest evidence of spillovers. Conditional on persistent bilateral ties and the other controls, incoming exchanges are plausibly exogenous to the home cohort's destination preferences. The response is concentrated in the graduation year and among graduates without an inferred exchange, 6.2% against 2.7% for participants, with no response to arrivals after graduation in either group. This pattern supports a mechanism through contact with incoming students as local graduates prepare to enter the labor market.

Outflow responses extend across the final three academic years. Among participants they are largest two and one years before graduation, about 5%, when their own stays take place, and about 3% in the final year. Since outgoing exchanges may reflect destination preferences, these estimates primarily provide descriptive evidence that participants stay in or return to regions they visited. Among graduates without an inferred exchange, the response is about half as large in the two earlier years, 2%, and equal in the final year, 4%. Selection is less direct for this group because they did not choose an exchange in the destination.[30] A persistent department–destination demand shock should generally also generate responses outside the study window, particularly after graduation. The concentration around enrollment and the distinct inflow and outflow profiles therefore lend support to a spillover interpretation.

These spillovers amplify the program's reach beyond the small fraction of students who participate directly. Incoming students connect local graduates to foreign destinations, while outgoing students may pass information and contacts to classmates who remain at home.

Exchange volume. Appendix Figure C.13 replaces the annual any-flow indicators with categories for 1, 2–4, 5–9, and 10 or more students relative to none. The two outer bins remain binary any-flow indicators. For inflows in the graduation year, the implied increases in employment in the students' origin region are approximately 5%, 6%, 11%, and 15%, respectively. Outflow estimates in the graduation year rise from approximately 3% to 6% across these categories, with responses also present at $\tau = -2$ and $\tau = -1$.

Larger exchanges are thus associated with stronger responses, while the timing remains concentrated near graduation. Together, the volume and timing patterns support a role for contact generated by actual exchanges, beyond the existence of a partnership alone. They complement the historical evidence that links with greater initial mobility capacity produce larger exchange and employment responses.

[30] Unreported exchanges may contaminate this group, and non-participants may share destination preferences with participating classmates. Appendix A.8 bounds the share of unreported participants in this group at 0–3%.

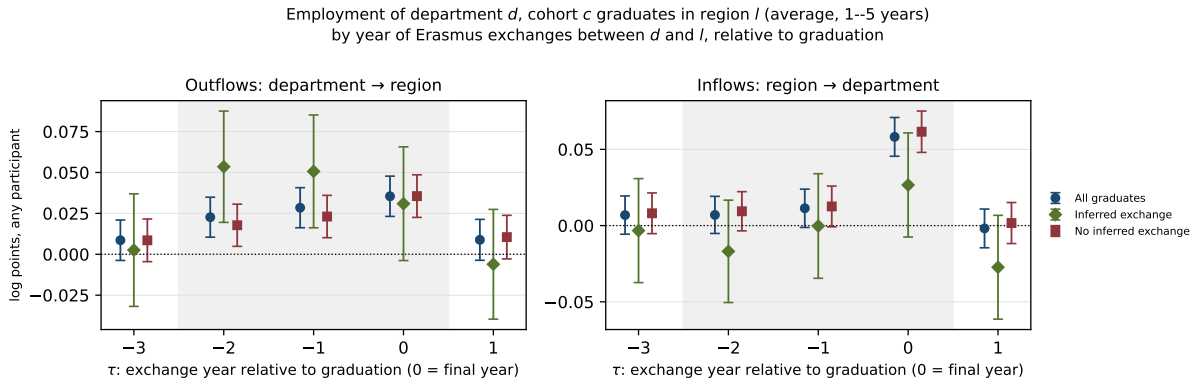


Figure 8: Outgoing and incoming exchanges: full sample and by inferred exchange

Notes: PPML estimates of equation (6) for the full sample and separately for graduates with and without an inferred exchange. An inferred exchange is a foreign education spell lasting less than one year within enrollment in the home degree. The outcome is the number of graduate-years that cohort c of department d spends in region l during years 1–5 after graduation. Outflows record students from department d studying in region l, while inflows record students from institutions in l studying at d. Both directions enter jointly. Annual indicators record any exchange at $\tau = -3, \ldots, +1$, where $\tau = 0$ is the academic year ending in graduation. The outer bins record any exchange in the observed years with $\tau < -3$ and $\tau > +1$, respectively. All specifications include department–destination, department–cohort, and destination–cohort fixed effects, together with home-country and home-region indicators interacted with cohort. Coefficients and fixed effects are re-estimated for each sample. Coefficients are in log points. Bars indicate 95% confidence intervals, with standard errors clustered by department–destination pair.

6.2 Exposure to Exchange Peers

I next examine whether Erasmus participants' subsequent employment locations respond to the regions from which their exchange peers originate. A stay abroad can introduce students to people from many destinations beyond the host region itself. These encounters may create friendships, personal relationships, and contacts that shape later mobility. Survey evidence motivates this channel. In the ESNsurvey 2008, 87.8% of exchange and international students reported spending much or most of their time with other foreign students, compared with 41.5% for local students (Bauwens et al., 2009, p. 27, Figure 14). These interactions can generate lasting personal ties.[31]

Sample and exposure. The sample contains about 468,000 LinkedIn users who report an Erasmus exchange, of whom about 410,000 have a located job in the first five years after graduation. The analysis uses the roughly 375,000 participants whose host institution and exchange years appear in the administrative inflow records, which begin in 2008/09.

For each participant i and destination region l, I measure exposure as the share of incoming Erasmus students at the participant's host institution who come from region l, averaged over the

[31]The European Commission's Erasmus Impact Study reports that 27% of Erasmus students met their long-term partner during their exchange (European Commission, 2014).

participant's exchange years. I compute this share separately for peers in the same field ($E^{dep}_{i,l}$), peers in other fields at the same host ($E^{hei}_{i,l}$), and students at other institutions in the host region ($E^{reg}_{i,l}$). I also measure the same-field share in the two academic years before the participant arrived ($E^{dep,\text{pre}}_{i,l}$) and the two years after they left ($E^{dep,\text{post}}_{i,l}$), averaged over each two-year window.[32] Peer origins come from administrative records, so they do not depend on peers' LinkedIn reporting. The participant's home region is excluded from the set of destinations.

Specification and identification. The outcome $P_{i,l}$ is the percentage of the participant's located years 1–5 after graduation spent working in region l. I estimate

$$\begin{aligned} P_{i,l} = \beta^{d} E^{dep}_{i,l} + \beta^{h} E^{hei}_{i,l} + \beta^{reg} E^{reg}_{i,l} + \beta^{d}_{\text{pre}} E^{dep,\text{pre}}_{i,l} + \beta^{d}_{\text{post}} E^{dep,\text{post}}_{i,l} \\ + \psi_{h,l} + \lambda_{y,l} + \omega_{h,y} + \xi_{d,l} + \mu_{c,l} + \varepsilon_{i,l}, \end{aligned} \tag{7}$$

where h is the host institution, y the exchange year, d the home department, and c the graduation cohort. The fixed effects absorb persistent ties between hosts and destinations ($\psi_{h,l}$) and between home departments and destinations ($\xi_{d,l}$), as well as shocks common to a destination in a given exchange year or cohort ($\lambda_{y,l}$, $\mu_{c,l}$) and to a host in a given year ($\omega_{h,y}$). Standard errors are clustered by host institution.

Identification comes from year-to-year changes in where a host's incoming students come from. Participants may choose hosts based on the composition they can observe: the host's typical mix of incoming students, which the host–destination effects absorb, and its composition in recent years, which $E^{dep,\text{pre}}_{i,l}$ controls for directly. What participants cannot observe when applying is which students will arrive during their own stay. Conditional on these controls, the composition of contemporaneous peers is therefore unlikely to reflect participants' own destination preferences. The post-departure share $E^{dep,\text{post}}_{i,l}$ provides a placebo: if the effect reflects contact during the stay rather than a host's persistent ties to particular regions, only contemporaneous peers should matter.

Results. Figure 9 reports the estimates. The same-field coefficient is 0.19 percentage points per unit exposure, with a standard error of 0.04. The baseline is small: averaged over all foreign regions in the choice set, a participant spends 0.07% of located career years 1–5 in a given region. Increasing a region's share of same-field peers from 5% to 15% therefore raises the share of career years spent in that region by 0.019 percentage points, about a quarter of this baseline. A one-standard-deviation increase in exposure, among participant–region pairs with positive exposure, raises it by about 9%.

[32] Same-field inflows from a given region to a given host are small in any single year, so pooling two years improves the precision of these shares and keeps the pre- and post-period measures symmetric.

The response is concentrated among same-field peers at the participant's host during the exchange. Estimates for peers in other fields at the same host and for peers at other institutions in the host region are close to zero, as is the estimate for same-field inflows in the two years after the participant left. The estimate for same-field inflows in the two years before arrival is positive but less than half the contemporaneous coefficient and imprecise, with a standard error of 0.05. This pattern is consistent with direct interaction during the stay shaping subsequent location choices, with some room for persistent field-specific ties at the host. Appendix Figure C.14 reports the estimates for employment at years 1, 5, and 10 separately. Results are similar using the broader inferred-exchange measure.

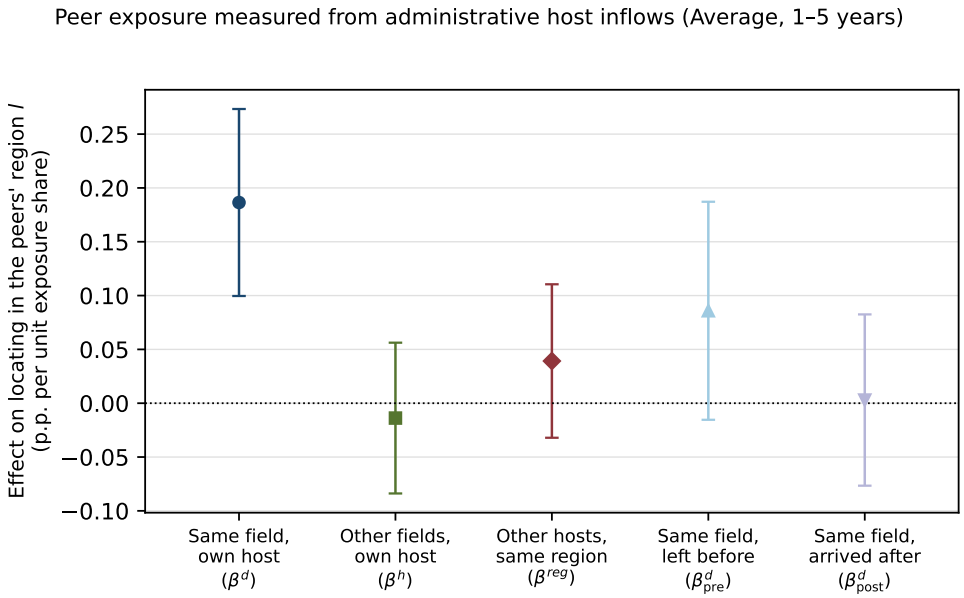


Figure 9: Exchange-peer origins and participants' subsequent employment

Notes: This figure reports linear estimates of equation (7) for LinkedIn users who report an Erasmus exchange at a host covered by the administrative inflow records (2008/09 onward). Exposure is the share of region l among incoming Erasmus students during the participant's exchange, measured separately for same-field students at the host, other-field students at the host, and students at other institutions in the host region. The timing checks use same-field inflows in the two years before arrival and after departure. The outcome is the share of career years 1–5 after graduation spent in l, in percentage points per unit of exposure. The specification includes host–destination, exchange-year–destination, host–year, home-department–destination, and cohort–destination fixed effects, and excludes the home region. Bars show 95% confidence intervals, with standard errors clustered by host institution.

These findings suggest that Erasmus may connect regions even when they exchange no students directly. A participant from region A studying in region C can acquire information and contacts related to region B through peers from B. If these ties subsequently influence location choices, a common host can create a connection between the two origin regions without a direct exchange flow between them. The program's geographic reach may therefore extend beyond the routes along which students themselves travel.

7 Economic Magnitudes

I assess the economic scale of the ICP phase estimates in three ways. First, I count the graduates the program moved, in the sample and scaled to the exposed population. Second, I express the utility shifts as equivalent reductions in bilateral distance, a benchmark that does not depend on the preference-dispersion parameter θ. Third, I use the location-choice model to express the utility gains in consumption and monetary units.

7.1 Induced Moves

Using the joint estimates of Section 5.3, I predict how the graduates of each exposed department and cohort would have been distributed across destinations without Erasmus, and how they are distributed given the department's actual links, linked countries, and entry date (Appendix Table C.15). Because shares sum to one within each department–cohort, the predictions move graduates between destinations rather than adding employment. For an individual graduate, the effect is a larger share of the career spent in a destination, so I express the reallocation as the equivalent number of graduates spending their entire 28-year career there. In the sample, links move the equivalent of about 4,200 graduates into the directly linked regions. Together, the three margins move about 22,900 graduates abroad, 10,700 of them to other EU-15 countries, almost all out of employment in their home country.

The sample contains 1.22 million exposed first-degree graduates, while exposed departments in the EU-15 awarded 9.37 million first degrees, 7.7 times as many (Appendix Table C.16). If the effects in the sample hold for all graduates of exposed departments, the program's first decade moved the equivalent of about 32,000 graduates into linked regions and about 176,000 abroad, 82,000 of them to other EU-15 countries. With 449,000 Erasmus exchanges between 1987/88 and 1996/97, this is one additional graduate in a linked region for every 14 exchanges, and one in another EU-15 country for roughly every five. These effects apply to all graduates of exposed departments, whether or not they went abroad. Appendix Table C.17 shows how the figures change under alternative population scalings.

7.2 Equivalent Distance

Distance provides a familiar benchmark for how strongly bilateral factors shape location choices (Beine et al., 2016). I regress the department–destination effects recovered from the joint PPML specification on log geodesic distance, restricting the sample to foreign pairs and including department and destination fixed effects. The regression weights each pair by the number of graduates behind it.

The distance gradient is -0.62 (s.e. 0.07). Adding directional country-pair fixed effects yields a similar -0.54 (s.e. 0.07), so the relationship remains when identified only from regional distance variation within country pairs (Appendix Table C.13).[33] At the preferred estimate, an effect of size Δv is equivalent to reducing log distance by $\Delta v/0.62$. The linked-region effect of 0.101 therefore corresponds to moving the destination about 15% closer, or roughly 120 kilometers at the median treated distance of 800 kilometers. The broad foreign-employment effect of 0.100 is equivalent to almost the same reduction. This benchmark relies only on estimates from this paper: the link effect and the distance gradient are measured on the same scale, so their ratio does not depend on the preference-dispersion parameter θ.

7.3 Utility Gains and Monetary Equivalents

The distance benchmark conveys the size of the effects but not their value to graduates. Valuing them requires the location-choice model and one parameter from outside the paper, the preference-dispersion parameter θ.

Let $U_{d,c} \equiv \mathbb{E}[\max_l u_{i,d,l,c}]$ denote expected location-choice utility per graduate and observed career year, measured in log-consumption units. The calculation is partial equilibrium: incomes, amenities, and non-treatment utility components remain fixed, while location probabilities adjust to the estimated utility shifts. Under the multinomial-logit model, a change in normalized destination utility of $\Delta v_{d,l,c}$ yields (Small and Rosen, 1981)

$$\Delta U_{d,c} = \frac{1}{\theta} \ln \left[\sum_l \pi^0_{d,l,c} \exp(\Delta v_{d,l,c}) \right], \tag{8}$$

where $\pi^0_{d,l,c}$ is the counterfactual destination share. Appendix C.11 derives this expression.

Estimated utility gains. The estimated utility shifts give the normalized gain $\theta \Delta U$ for each exposed department–cohort through equation (8). I then average these gains over all exposed department–cohorts, weighting each by its number of graduates, so the result is the gain for the average graduate of an exposed department. Converting this average into log-consumption units requires dividing by θ, which I set to 1.63, the median migration elasticity compiled by Head and Mayer (2021), with sensitivity across their range of 0.50–3.18 in Appendix Table C.17. I add the margins to $\Delta v_{d,l,c}$ one at a time, first the increment for directly linked regions, then the increment for other regions of linked countries, and finally the broad foreign increment, and recompute the gain at each step. The directly linked regions alone raise expected utility by the equivalent of 0.21% of annual consumption per graduate of an exposed department. Adding linked countries

[33] Standard errors are two-way clustered by department and destination. They condition on the recovered PPML fixed effects and do not propagate their estimation uncertainty.

Table 2: Utility gains and monetary equivalents

Measure	$\theta\,\Delta U \times 100$ (log pts)	$\Delta U \times 100$ (log pts)	PV per grad. (€)	Aggregate PV (€ bn)
Log-sum, all margins	2.51	1.54	14,122	117.8
Direct links	0.35	0.21	1,958	15.9
Country margin	-0.03	-0.02	-150	-1.2
Entry margin	2.19	1.34	12,314	103.1
EU programme expenditure, 1987–1996				1.19

Notes: The first row reports the gain from all geographic margins combined. The indented rows decompose it into sequential increments, adding direct links, then the other regions of linked countries, then broad foreign entry, and sum to the first row up to rounding. All columns are means over exposed department–cohorts, weighted by located graduates. The first column reports the normalized utility gain $\theta\Delta U$ and the second the gain in log-consumption units ΔU at $\theta = 1.63$, both multiplied by 100. Present values are computed for each department–cohort by applying the consumption-equivalent gain to EU-15 real compensation per employee in each of the 28 years after graduation, discounted to graduation at 3% and expressed in 2024 euros. Aggregates multiply each country–cohort's mean present value by its number of exposed first-degree graduates (ISCED-97 level 5A), 9.37 million in total. Program expenditure covers the EU budget only and is expressed in 2024 prices. Appendix Tables C.14, C.16, and C.17 report the construction and sensitivity analyses.

and the broad foreign margin raises the total gain to 1.56% per year, most of it from the broad margin (Table 2).

Monetary equivalents. To express these gains in euros, I treat each consumption-equivalent gain as a constant percentage of annual earnings. In each of the 28 years after graduation, I multiply this percentage by average EU-15 real compensation per employee in that year (European Commission, Directorate-General for Economic and Financial Affairs, 2026), which gives the euro value of the gain in that year. I then discount these annual values to the graduation year at 3% and sum them, expressing the result in 2024 euros. Equation (23) in the appendix states the calculation formally. The direct-link gain is worth about €1,900 per graduate of an exposed department, and the total gain about €14,000.

These figures allow a back-of-the-envelope comparison with the cost of the program. Scaled to the 9.37 million exposed graduates, the direct-link gain amounts to about €16 billion and the total gain to about €118 billion, compared with roughly €1.2 billion in EU Erasmus spending over 1987–1996[34]. Even the direct-link gain, the narrower and more tightly identified of the two, exceeds the EU budget for the program more than tenfold. The calculation rests on strong assumptions. It assumes that the sample estimates hold for all exposed graduates and leaves out resources from universities, students, and national agencies. It also holds wages and amenities fixed and ignores effects on other workers and regions, so it measures gains to exposed graduates

[34]EU spending on Erasmus totals ECU 657 million in nominal terms: ECU 85 million for 1987–1990 (Council of the European Communities, 1987), ECU 376.5 million for 1990–1994, and ECU 117.2 and 77.9 million in 1995 and 1996. In 2024 euros this is €1.19 billion. The total excludes national co-funding and the resources of universities and students.

within the model, including those who never went abroad, rather than the program's net social return. The comparison is therefore informative about orders of magnitude, not precise values: under any of these caveats, the gains to graduates are large relative to what the EU spent.

8 Conclusion

This paper shows that temporary student exchange can create durable cross-border labor-market links. I reconstruct the subject-specific ICP networks from the first decade of Erasmus and match their expansion to the education and career histories of about two million European graduates. Opening a link between two departments increases exchange participation and raises employment of the department's graduates in the linked region by about 11 percent. The effect appears at labor-market entry, persists for at least 25 years, and survives comparisons across departments within the same university and across regions within the same destination country.

Erasmus affects both whether and where graduates work abroad. It raises employment across foreign destinations in general and in the linked region in particular, but not in other regions of the linked country, a pattern difficult to reconcile with country-specific skills such as language. The response is larger for links with greater student-mobility capacity and, in proportional terms, for more distant destinations and thinner initial corridors, consistent with Erasmus mattering most where bilateral frictions were greatest. Because corridors toward richer regions are larger, most of the induced moves go toward destinations with higher income.

The effects reach beyond the students who travel. The link effect remains among graduates without a recorded exchange, suggesting that information and contacts spread within departments. Using administrative records of Erasmus flows since 2008, I show that incoming exchange students raise local graduates' employment in the students' home regions, and that participants are more likely to work in the home regions of peers they met abroad, including regions they never visited. Erasmus therefore creates connections that extend beyond direct participants and beyond the regions that exchanges directly connect.

Within the location-choice model, the linked-region effect is equivalent to bringing the destination about 15 percent closer and corresponds to a gain of about 0.2 percent of annual consumption per exposed graduate. Across the program's first decade, it amounts to about one additional graduate-career in a linked region for every 14 exchanges. These are partial-equilibrium benchmarks rather than earnings effects. Future work will examine whether the induced moves raise wages and productivity, how they affect workers and firms in origin and destination regions, and whether they improve adjustment to regional shocks.

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A Additional Details on Data

This appendix documents the data underlying the analysis. It places the ICP phase in the long-run growth of Erasmus, describes the LinkedIn education and employment histories, their coverage of graduates and of graduates' careers, and the measures of exchange participation, and then turns to the digitization of the ICP directories, the construction of treatment links, the expansion of the network, and the matching of ICP and LinkedIn institutions. It concludes by comparing the LinkedIn measures with administrative records.

A.1 Erasmus Expansion

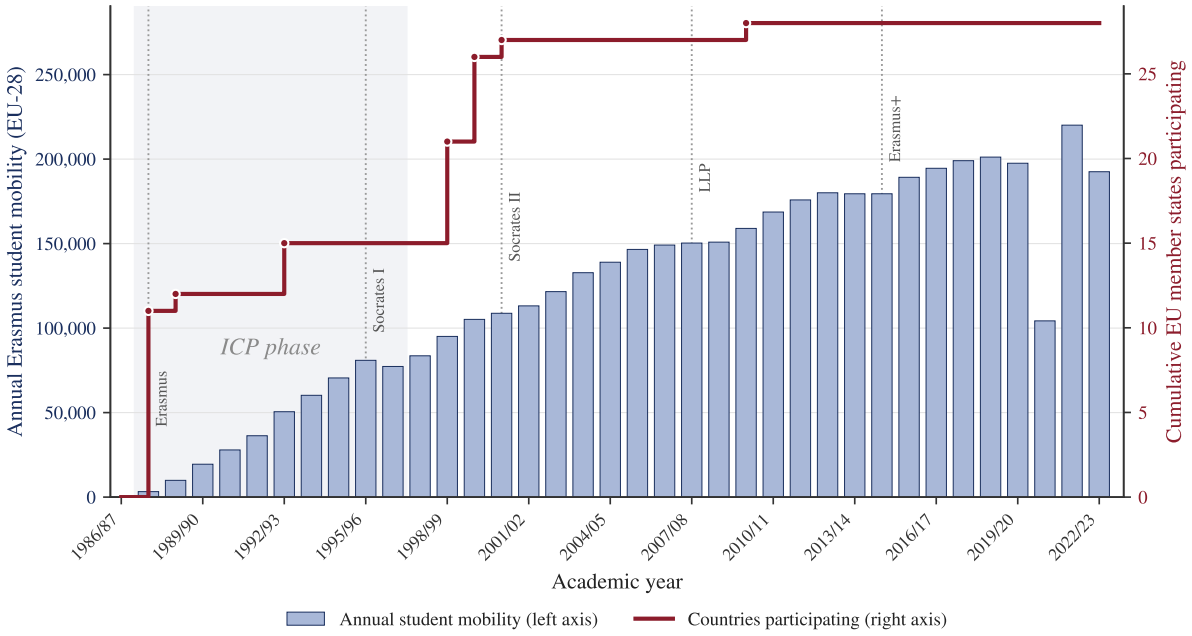


Figure A.1: Erasmus student mobility and geographic coverage, 1986/87–2022/23

Notes: Annual Erasmus study mobility (left axis) and cumulative participation of EU-28 member states (right axis), 1986/87–2022/23. The program began in 1987/88. Vertical lines mark the principal program phases: Erasmus (1987/88), Socrates I (1995/96), Socrates II (2000/01), the Lifelong Learning Programme (2007/08), and Erasmus+ (2014/15). The shaded region marks the ICP phase used in the main analysis, 1987/88–1996/97. Student-mobility figures for 1987/88–2007/08 are from European Commission, Directorate-General for Education and Culture (2012), restricted to the EU-28 universe shown in the figure. Figures for 2008/09–2022/23 are constructed from the European Commission administrative records described in Section 3.

A.2 LinkedIn Education and Employment Data

Degree harmonization. Degree titles in LinkedIn are reported as free text and vary substantially across countries, languages, and institutions. I construct a dictionary mapping each unique combination of awarding country and raw degree string to an International Standard Classifica-

tion of Education (ISCED) level. The dictionary covers 167,321 distinct country–degree-string combinations.

Classification proceeds in two stages. I first normalize degree strings and apply country-specific regular-expression rules that account for differences in degree terminology across national education systems. Each successful classification records the assigned ISCED level, a confidence score, and the rule generating the assignment. The rule engine also flags records that appear to describe temporary exchange stays rather than degree credentials.

Strings not classified by these rules are reviewed in frequency order, together with information on their country composition, with GPT assistance used for the most common residual cases. The remaining unresolved long tail is left unclassified. The stored rule identifiers and confidence scores provide an audit trail for the resulting dictionary.

I retain records classified as ISCED 6 (Bachelor's or equivalent), ISCED 7 (Master's or equivalent), or ISCED 8 (Doctoral or equivalent). Sub-tertiary credentials, non-degree records, and degree strings that cannot be assigned reliably to an ISCED level are excluded.

Field identification and subject-area coding. The ICP directories classify student mobility into 15 subject areas. To assign LinkedIn graduates to the corresponding university–subject units, I map free-text field-of-study labels to these categories.

I embed the 38,430 distinct LinkedIn field-of-study strings and descriptions of the 15 ICP subject categories in a common vector space using OpenAI's `text-embedding-3-large` model. Each LinkedIn field string is assigned to the ICP category with the highest cosine similarity. Mappings with a maximum similarity below 0.5 are left unclassified.

I manually review the mappings of the 200 most frequent LinkedIn field labels and correct misclassifications. The final procedure assigns approximately 95% of education records at institutions represented in the ICP data to one of the 15 subject areas.

Employment location construction. LinkedIn employment histories consist of position spells with reported start and end dates and location information, typically at the city or settlement level. I standardize raw location strings by country and match 30,803 distinct city–country combinations to geographic coordinates. European locations are assigned to NUTS3 regions under the NUTS 2021 classification and then aggregated to NUTS2, the destination level used in the analysis. Position records without a start date or usable geographic information are excluded.

I reconstruct each graduate's employment location on January 1 of every calendar year. A position is active if its reported spell includes that date, with missing end dates treated as ongoing. When multiple positions are active, I assign the location of the position with the longest total spell duration. These annual snapshots are used to construct both career-average destination shares and employment outcomes at fixed years since graduation.

A.3 LinkedIn Coverage and Representativeness

This subsection documents the size and coverage of the LinkedIn data. Figure A.2 shows the number of degree records by graduation year. Figures A.3 and A.4 compare the samples used in the ICP-period analysis and in the analysis of recent exchange flows with official graduate counts, and Figure A.5 compares the field composition of the ICP-period sample with that of first degrees. Figure A.7 shows the geography of the ICP-period sample.

Coverage is the number of graduates in the estimation sample divided by the number of first degrees awarded by universities in the same country to the same graduation cohort. Eurostat reports first degrees only from 1998, so graduate counts for the ICP cohorts have to be approximated. For each country, I take the number of first degrees at ISCED 1997 level 5A in the earliest reported year and extrapolate it backward at 2.1% per year, the average annual growth of first degrees between 1998 and 2001 in the eleven countries observed in both years.[35] The resulting denominators assume that graduate numbers in every country grew at this common rate before 1998. Where higher education expanded faster than average, they overstate earlier cohorts and therefore understate coverage, and where it expanded more slowly, they do the opposite. Coverage of the 2013–2020 cohorts uses observed Eurostat counts of bachelor's graduates by country and year.

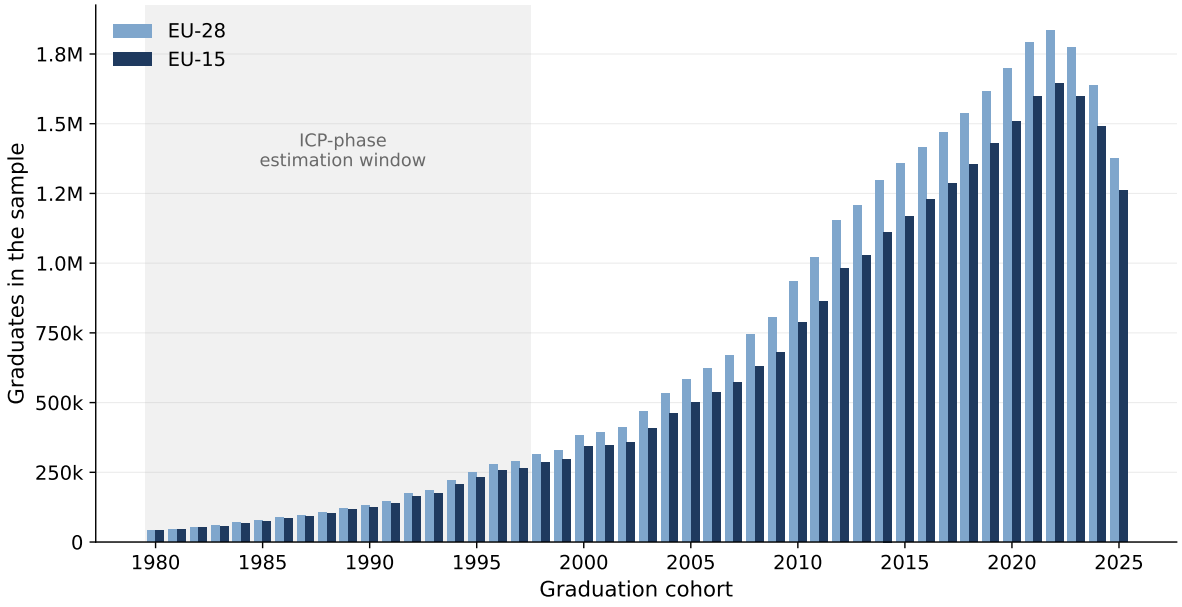
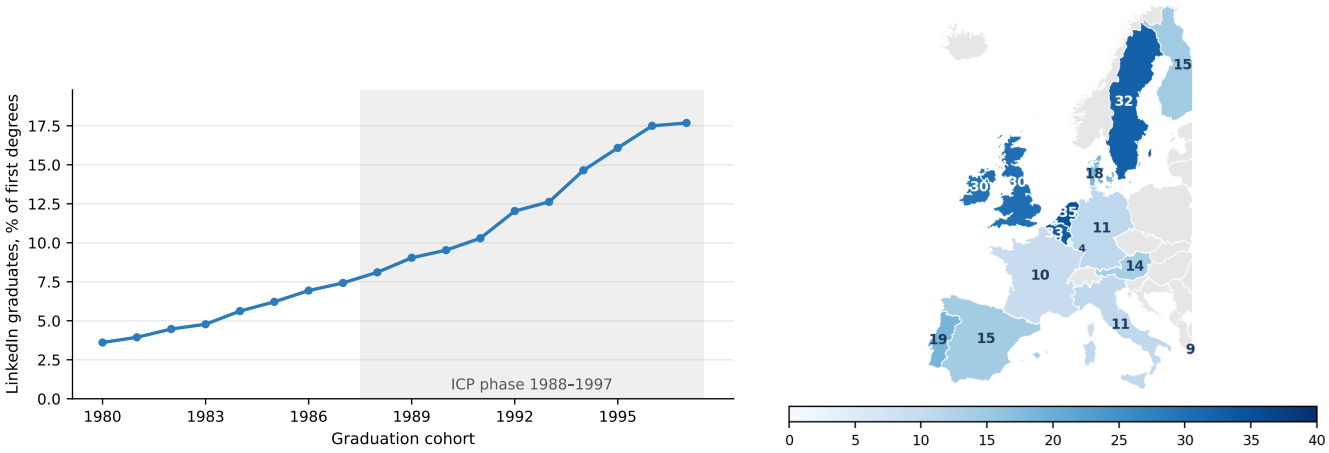


Figure A.2: LinkedIn degree records by graduation year

Notes: Tertiary degree records (ISCED levels 6–8) with a mapped field of study at universities in the EU-15 and in the EU-28 (EU-27 and the United Kingdom), by year of graduation. A graduate with two degrees appears twice. The shaded area marks the 1980–1997 cohorts of the ICP-period analysis.

[35] Eurostat reports first degrees at ISCED 1997 level 5A (table educ_grad4) from 1998 for eleven of the EU-15 countries. Belgium and Luxembourg first report in 2000 and Greece in 2001. Denmark's 1998 and 1999 values reflect a series break, so its series starts in 2000.

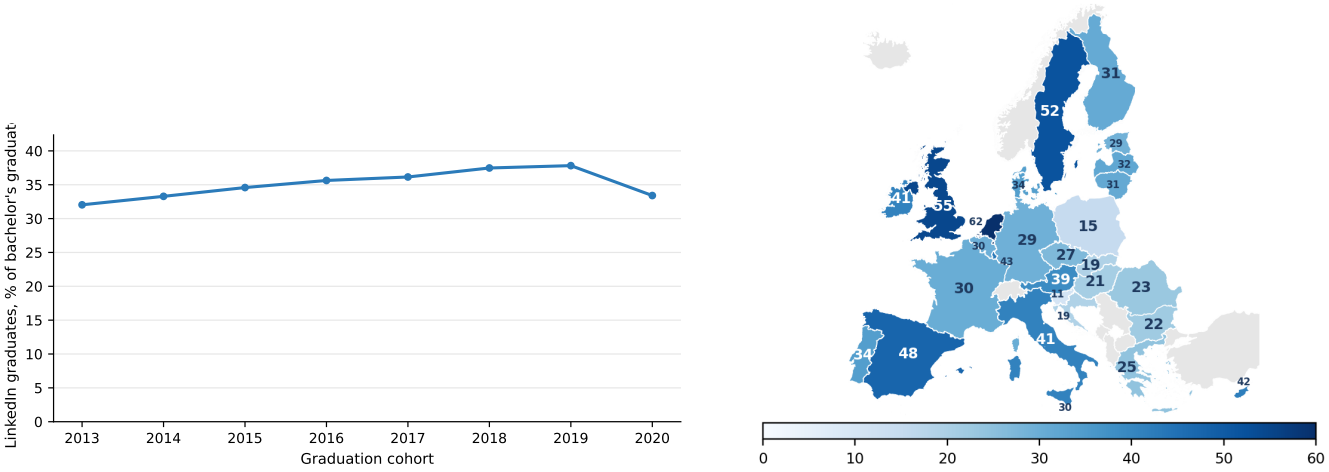


(a) By graduation cohort, EU-15

(b) 1997 cohort, by country of the university

Figure A.3: LinkedIn coverage of first university degrees, 1980–1997 cohorts

Notes: Graduates in the estimation sample as a percentage of first university degrees awarded in the same country and year. First degrees are each country's earliest Eurostat count of ISCED 1997 level 5A degrees, back-cast at 2.1% per year. The shaded area in panel (a) marks the graduation cohorts exposed to the ICP phase.



(a) By graduation cohort, EU-27 and the United Kingdom

(b) 2013–2020 cohorts, by country of the university

Figure A.4: LinkedIn coverage of bachelor's degrees, 2013–2020 cohorts

Notes: Graduates in the analysis of recent exchange flows, each counted once at the first retained degree, divided by Eurostat counts of bachelor's graduates (ISCED 6) in the same country and year. Countries are the EU-27 and the United Kingdom. The 2020 value excludes the United Kingdom, for which Eurostat reports no graduates in that year. About a quarter of the counted graduates list only a master's degree, so the ratio overstates coverage of bachelor's graduates. Counting only persons whose first degree is a bachelor's gives 25%.

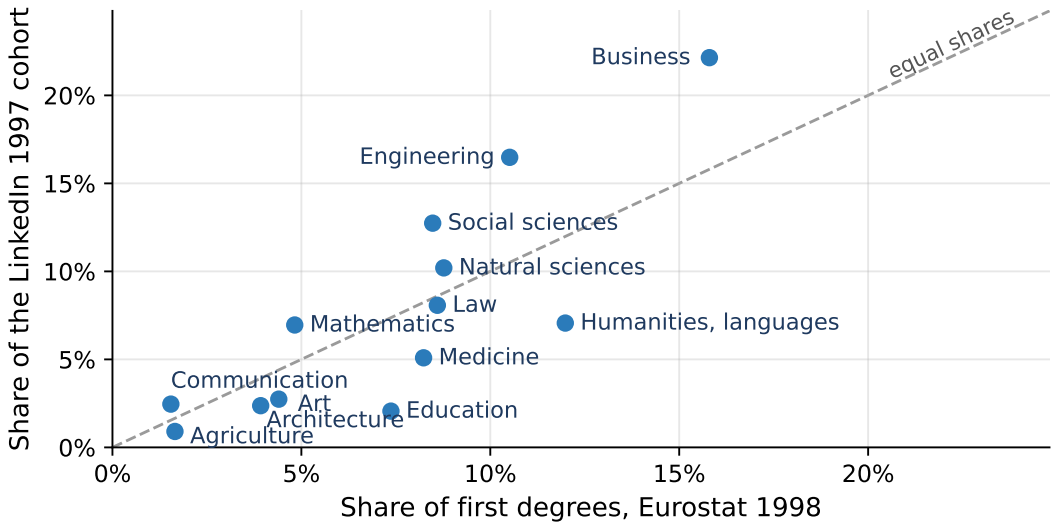


Figure A.5: Field composition of the LinkedIn sample and of first degrees

Notes: Each point is a field. The horizontal axis is its share of first degrees in 1998, the first year with a field breakdown in Eurostat. The vertical axis is its share of the 1997 cohort in the estimation sample. Both cover the EU-15 except Belgium, Greece, and Luxembourg, which lack field data. ICP subject areas are merged into thirteen fields. Points above the dashed line are over-represented in the sample.

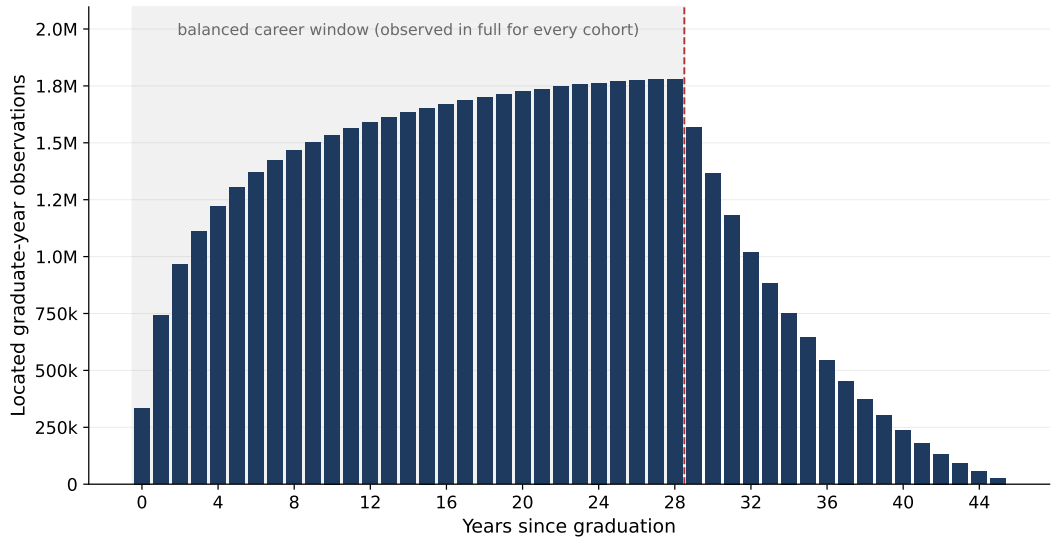
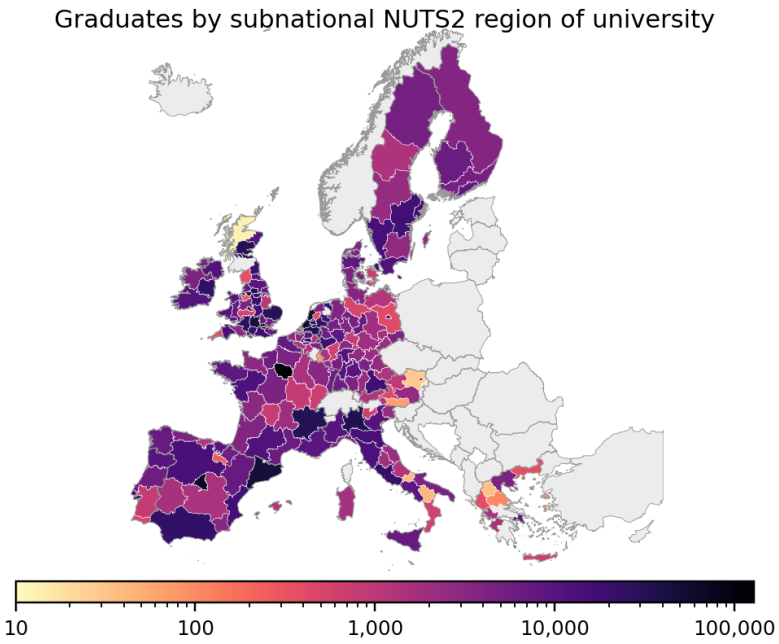
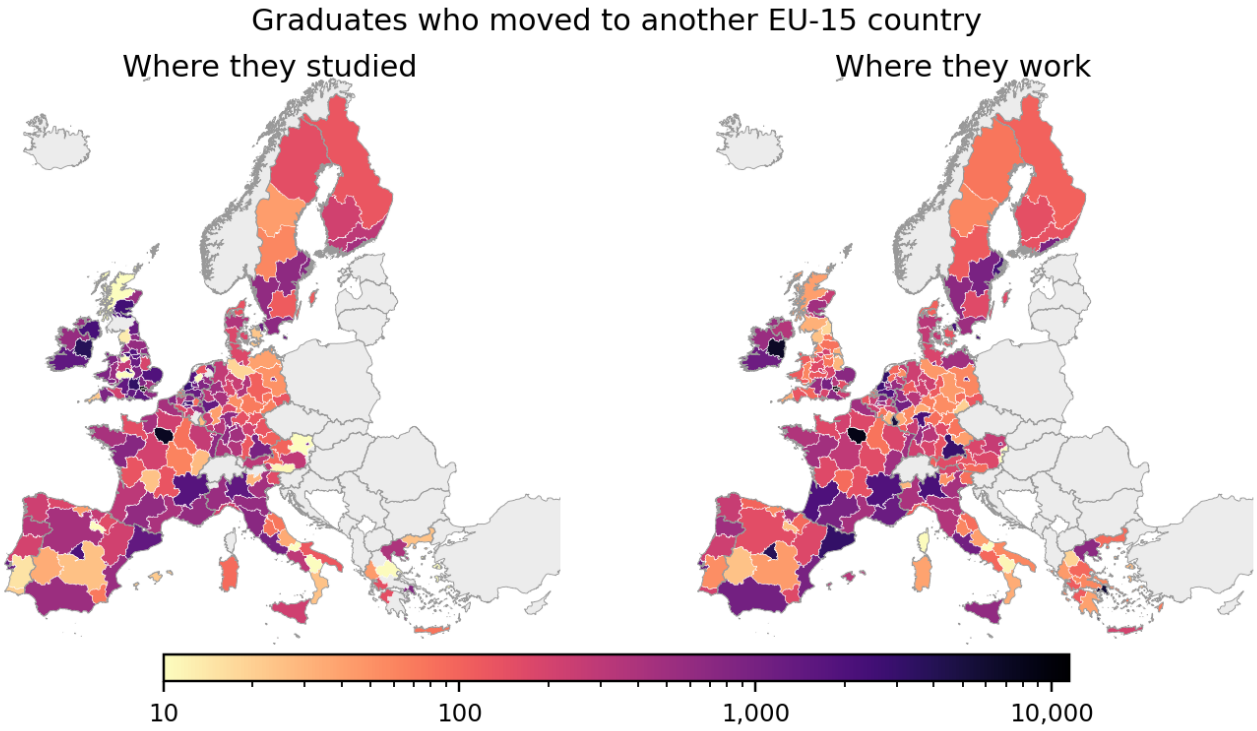


Figure A.6: Career coverage and the balanced 28-year window

Notes: Number of graduates of the 1980–1997 cohorts with a usable employment location, by years since graduation. Missing graduate-years have no active reported position or no usable location. All 18 cohorts are observed through career year 28 (shaded), the window used in the main analysis, and a usable location is observed in about 85–86% of graduate-years within it. Beyond that horizon, the count declines mechanically as later cohorts reach the end of the data.



(a) All graduates, by region of the university



(b) Graduates employed in another EU-15 country: region of the university (left) and region of the job (right)

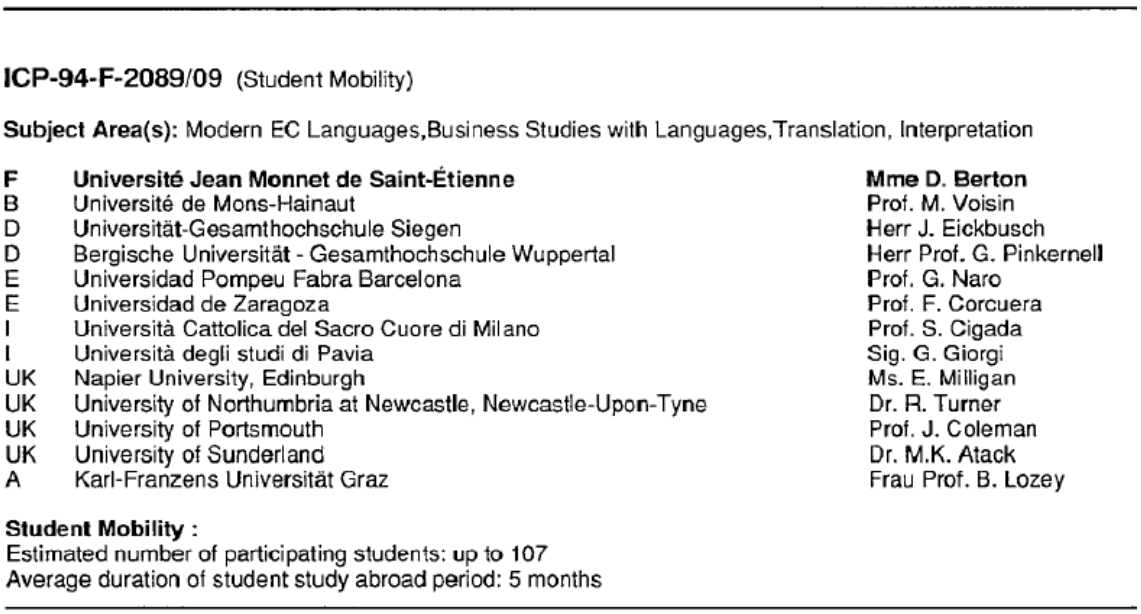
Figure A.7: Geography of the estimation sample

Notes: Panel (a) counts graduates of the estimation sample with at least one located career year (EU-15 departments with at least ten matched graduates of the 1980–1997 cohorts, 2,004,572 graduates) by NUTS2 region of the university. Panel (b) weights each graduate by the share of observed career years employed in an EU-15 country other than that of the university (125,940 graduate equivalents). Color scales are logarithmic. Grey regions have no observations. London is aggregated to a single region.

A.4 ICP Directories and Treatment Construction

Digitization. Each of the nine ICP directories covering academic years 1987/88–1995/96 was digitized by extracting text page by page and parsing it into structured agreement-level records. The main body of the nine directories contains approximately 7,900 pages. Automated parsing was followed by a systematic recovery step: agreements identified in each directory's own summary count but absent from the initial parse were individually investigated and, where the underlying pages were available, manually restored.

Across all nine directories, the final cleaned dataset recovers all but 7 of approximately 16,227 approved ICP agreements, a missing-record rate below 0.05%. All 7 unrecoverable agreements occur in the 1987/88 directory. A small number of additional restored records are missing particular fields, such as mobility type, but the agreement itself is retained. Figure A.8 shows a representative directory entry.



ICP-94-F-2089/09 (Student Mobility)

Subject Area(s): Modern EC Languages,Business Studies with Languages,Translation, Interpretation

F	**Université Jean Monnet de Saint-Étienne**	**Mme D. Berton**
B	Université de Mons-Hainaut	Prof. M. Voisin
D	Universität-Gesamthochschule Siegen	Herr J. Eickbusch
D	Bergische Universität - Gesamthochschule Wuppertal	Herr Prof. G. Pinkernell
E	Universidad Pompeu Fabra Barcelona	Prof. G. Naro
E	Universidad de Zaragoza	Prof. F. Corcuera
I	Università Cattolica del Sacro Cuore di Milano	Prof. S. Cigada
I	Università degli studi di Pavia	Sig. G. Giorgi
UK	Napier University, Edinburgh	Ms. E. Milligan
UK	University of Northumbria at Newcastle, Newcastle-Upon-Tyne	Dr. R. Turner
UK	University of Portsmouth	Prof. J. Coleman
UK	University of Sunderland	Dr. M.K. Atack
A	Karl-Franzens Universität Graz	Frau Prof. B. Lozey

Student Mobility :
Estimated number of participating students: up to 107
Average duration of student study abroad period: 5 months

Figure A.8: Example ICP directory entry

Notes: Representative entry from the 1990/91 ICP directory. Entries report the ICP network identifier, participating institutions and departments, subject area, and approved activities, including student mobility.

Institution-name harmonization. Raw institution names vary across directories because of OCR artifacts, abbreviations, differences between original-language and English names, and genuine renamings, status changes, or mergers. I first harmonize institution identities internally across the nine ICP directories before matching the resulting historical institutions to LinkedIn.

The resulting master file maps 2,518 raw institution-name variants across 19 countries to 2,007 distinct historical institutions. Harmonization is anchored on the 1995/96 directory, the final and most complete cross-section, and extended to institutions appearing only in earlier years. Of

the 2,007 historical institutions, 194 do not appear under any name variant in the final directory, largely because they ceased participating before the end of the ICP period.

Subject-area harmonization. The 1987/88 directory uses broader subject labels than the coding scheme introduced from 1988/89 onward. A single early category can therefore map to multiple later subject codes. I construct a correspondence table mapping each raw subject-area string into the harmonized coding scheme used in the 1995/96 directory.

LINGUA (Action II, code 20) is recoded into Languages and Philological Sciences at the department level used in the main analysis, since it funds language-specific mobility that is substantively part of the same field.

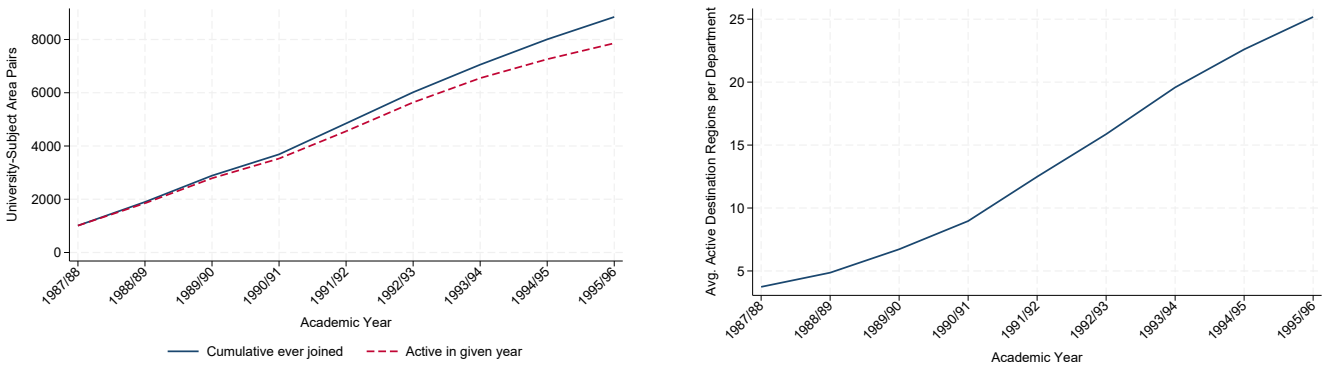
Construction of directed student-mobility links. Before constructing links, I restrict agreement-year records to ICPs approved for student mobility. Networks supporting only other activities, including program development or teaching-staff mobility, remain in the digitized archive but do not generate treatment.

Within each eligible ICP-year, I construct all directed cross-country institution pairs among participants in the same subject-specific network. A constructed link therefore indicates common membership in an approved student-mobility ICP, not a realized bilateral student flow. Self-pairs and within-country institution pairs are excluded.

Institution-level links are then collapsed to the university–subject–destination level used in the empirical analysis. Multiple partner institutions or ICPs connecting the same department to the same NUTS2 destination constitute a single department–destination treatment link.

A.5 Expansion and Identifying Variation

The student-mobility ICP network expanded rapidly. Participation rose from 449 institutions and roughly 1,000 university–subject units in 1987/88 to 1,754 institutions and more than 8,500 such units by 1995/96. Expansion also continued within participating departments: the average number of foreign NUTS2 destinations reached by a department rose from roughly 4 to about 25. Figure A.9 summarizes these two margins.



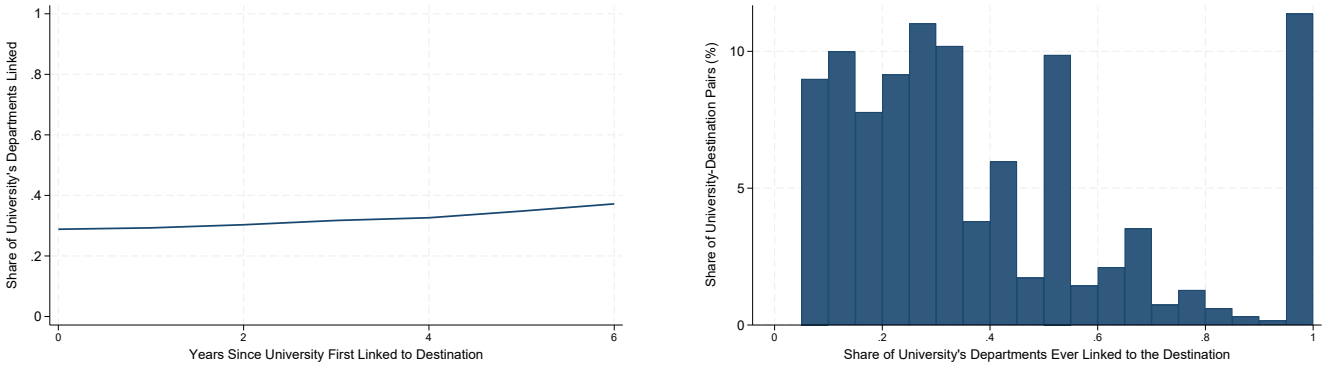
(a) University–subject participation

(b) Foreign destinations per department

Figure A.9: Expansion of the student-mobility ICP network

Notes: Panel (a) plots cumulative entry into the student-mobility ICP network together with the number of university–subject units active in each academic year. Panel (b) plots the average number of foreign NUTS2 destinations reached by participating departments in each year. LINGUA is recoded into Languages and Philological Sciences, consistent with the subject classification used in the main analysis.

Coverage remained incomplete as the network expanded, preserving the within-university and within-country variation used by the richer fixed-effect specifications. Only about 11% of university–destination pairs ever connect every observed subject area at the university to that destination, while only 1–2% of department–destination-country pairs ever connect to every NUTS2 region in the destination country. Figures A.10 and A.11 document this variation.

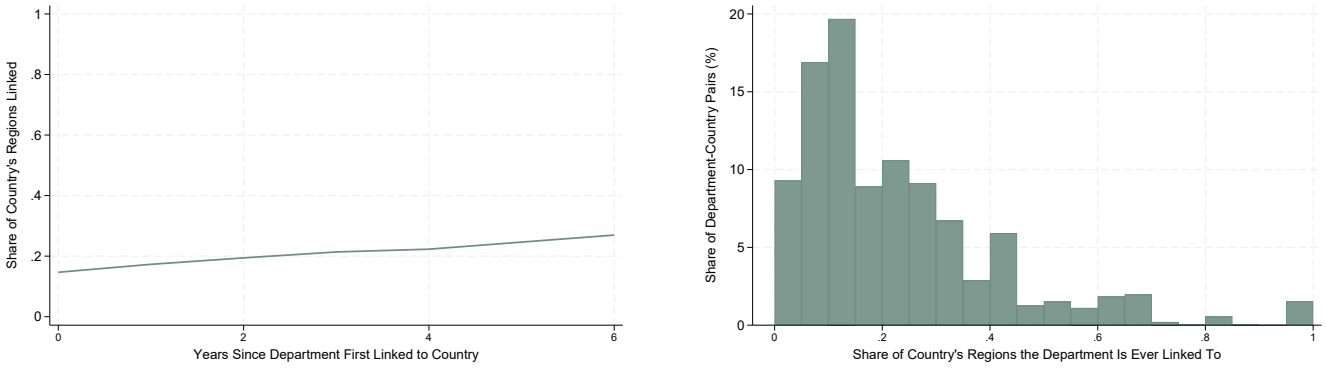


(a) Event-time path

(b) Pooled distribution

Figure A.10: Within-university variation in destination coverage across subject areas

Notes: Panel (a) plots, by years since a university first establishes an ICP link to a destination, the average share of that university's subject areas with an active link to the same destination. Panel (b) reports the distribution of the share of subject areas ever linked among university–destination pairs with at least one ICP link. The denominator is the university's total number of subject areas ever active in the ICP system. The figure documents the variation retained by university × destination × cohort fixed effects.



(a) Event-time path

(b) Pooled distribution

Figure A.11: Within-department variation in destination-country coverage across regions

Notes: Panel (a) plots, by years since a department first establishes an ICP link to any NUTS2 region in a destination country, the average share of that country's NUTS2 regions to which the department has an active link. Panel (b) reports the distribution of the share of regions ever linked among department–destination-country pairs with at least one ICP link. The denominator is the total number of NUTS2 regions in the destination country under the NUTS 2021 classification. The figure documents the variation retained by department $\times$ destination-country $\times$ cohort fixed effects.

A.6 Institution Matching and Geographic Harmonization

The ICP directories and LinkedIn records do not share a common institution identifier. I therefore route both datasets through the official Erasmus eligibility list, which provides standardized institution identifiers, names, and locations and serves as a common reference across the two sources.

Name and location matching. I generate candidate matches using normalized institution names and fuzzy string similarity, supplemented with geographic information. Candidates with similar names are accepted automatically only when their reported locations are consistent with the location of the corresponding eligibility-list institution. ICP institutions are matched using the same procedure, exploiting city information reported in the historical directories.

Because the eligibility list post-dates the ICP period, automated matching alone cannot resolve many historical institutions that subsequently changed name, merged, split, closed, or were otherwise reorganized. I manually review unresolved cases using historical institution names, locations, and documented institutional changes.

Institutional mergers and reorganizations. When several historical institutions later merge into a common university, I map their identifiers to a common institutional umbrella while retaining the subject-area dimension separately. This allows graduates reporting either a historical predecessor or a modern successor institution to be assigned consistently without combining unrelated subject areas.

Geographic harmonization. I geocode institutions to NUTS3 regions under the NUTS 2021 classification and aggregate them to NUTS2 for the empirical analysis. LinkedIn employment locations are harmonized to the same geography. Using a single NUTS vintage avoids mechanical changes in destination definitions over time and ensures that ICP partner locations and subsequent employment outcomes refer to the same spatial units.

Institution-level measurement error. Institution identifiers in LinkedIn remain imperfect. Graduates of renamed or reorganized universities may report either historical or modern names, while Revelio Labs' own harmonization introduces an additional source of classification error. Such errors weaken the correspondence between graduates and the ICP treatment assigned to their departments.

A.7 Estimation Samples

Table A.1 summarizes the two estimation samples.

Table A.1: Estimation samples

	ICP analysis	Recent exchange flows
Graduation cohorts	1980–1997	2013–2020
Origin countries	EU-15	EU-27 and the United Kingdom
Institutions	1,091	1,376
Subject areas	15 (ICP)	8 (ISCED-F based)
Graduates	2.1 million	6.8 million
Destination units	219	281
Coverage of first degrees	10.0%	about 35%
Treatment	ICP link opening	Annual exchange flows

Notes: Departments are university–subject pairs. Institutions count merged and renamed universities once. Destination units are NUTS2 regions plus residual destinations for other Erasmus program countries and the rest of the world. Coverage is the number of graduates with a located career relative to first degrees for the ICP analysis (Appendix A.3) and the number of graduates relative to bachelor's degrees for recent flows.

A.8 Validation Against Administrative Data

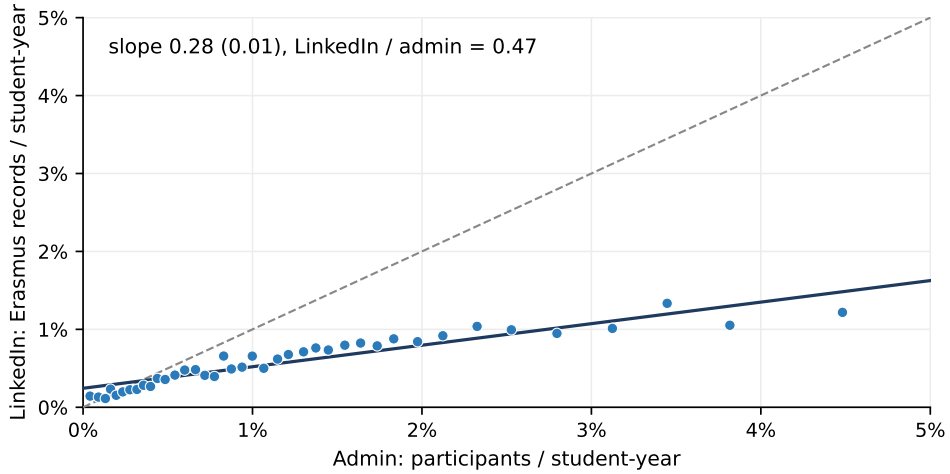
This appendix compares the LinkedIn measures with administrative records on three margins: exchange participation, cross-country migration of graduates, and the distribution of migrants within a destination country. LinkedIn observes only a fraction of graduates and of their ex-

changes, so the comparisons ask whether it reproduces relative participation and relative destinations rather than levels.

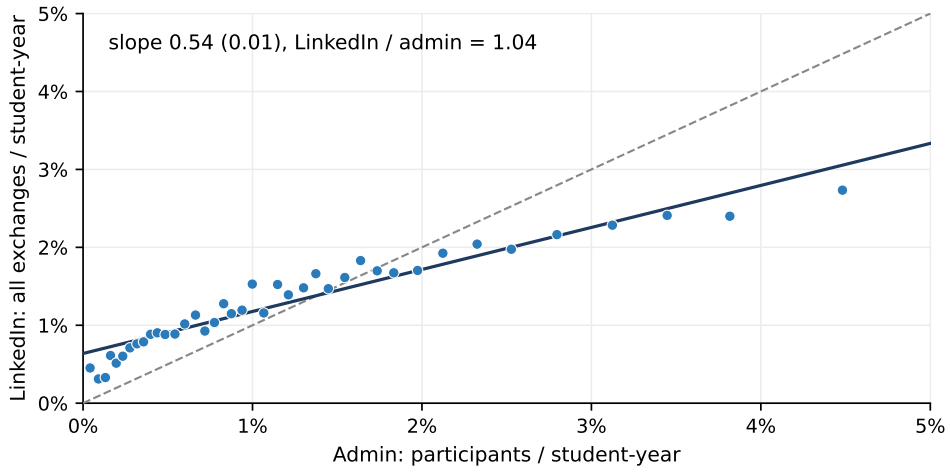
Exchange participation. The European Commission's individual-level Erasmus mobility records begin in academic year 2008/09 and report the sending institution, the field of study, and the host institution of every participant. I compare them with the LinkedIn exchange records of the same years at the department level, where a department is a university–field pair and hosts are aggregated to NUTS2 regions.

Figure A.12 compares participation rates. For each department, the administrative rate is the number of participants per enrolled student-year, with enrolment from the European Tertiary Education Register; the LinkedIn rate is the number of exchange records per enrolled student-year of LinkedIn users at the department. Explicit Erasmus mentions capture 47% of the administrative rate on average: a department whose students participate at 1.3% of student-years has an explicit-record rate of 0.6% (panel a). Inferred exchanges, which add short foreign education spells without an Erasmus label, match the administrative rate on average, with a ratio of 1.04 (panel b). In both panels the slope in levels is below the ratio of means because records also appear in departments with near-zero administrative rates, reflecting field misclassification and non-Erasmus stays. Figure A.13(a) reports detection by year at the university–host-region level: LinkedIn records one inferred exchange for every three to five administrative participants, rising from 0.18 in 2008/09 to 0.34 in 2019/20 as coverage of younger cohorts improves, and 0.09 to 0.16 for explicit mentions.

Figure A.13(b) asks whether the records go to the right places. For each department and host region, it plots the host region's share of the department's LinkedIn records against its share of the department's administrative participants, both demeaned within the department and pooled over 2008–2021. The slope is 0.80: a host region that receives ten percentage points more of a department's participants receives eight percentage points more of its records. Much of the gap to one is classification rather than reporting. The slope rises to 0.90 when fields are pooled within the university and to 0.88 when hosts are aggregated to countries, and it is the same for explicit Erasmus records. The remainder reflects a tilt of LinkedIn records toward Northern European hosts.



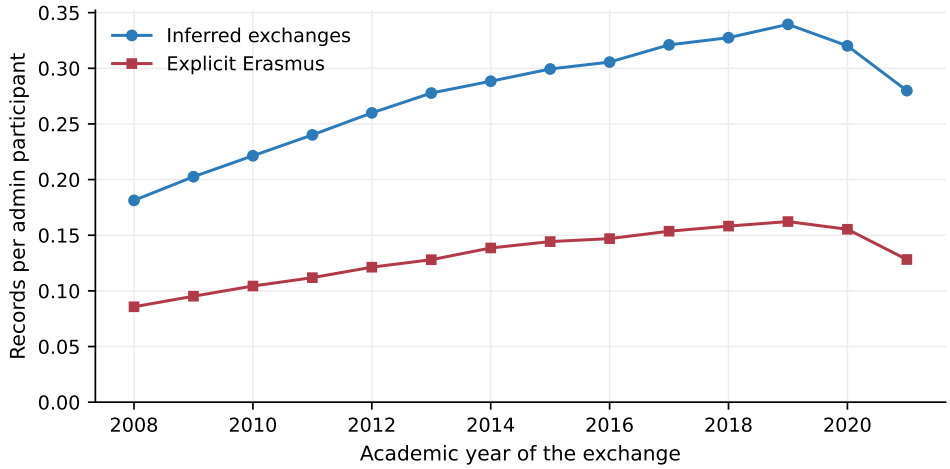
(a) Explicit Erasmus records



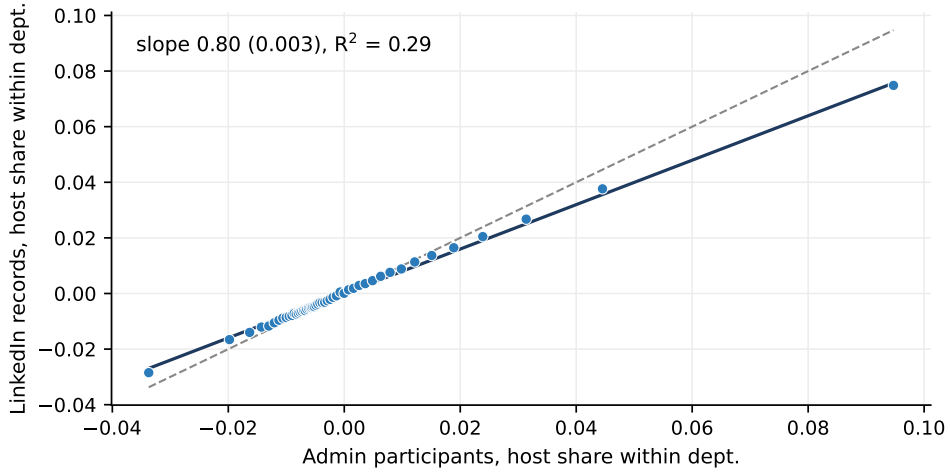
(b) Inferred exchanges

Figure A.12: Exchange participation rates: LinkedIn and administrative records by department, 2008–2021

Notes: Binned scatter over 3,621 departments (university × field) of EU-28 universities with at least 30 administrative participants in 2008–2021 and an administrative participation rate below 5%. The horizontal axis is Erasmus participants per enrolled student-year in the administrative records, with enrolment from the European Tertiary Education Register. The vertical axis is LinkedIn exchange records per enrolled student-year of LinkedIn users at the department, counting explicit Erasmus mentions in panel (a) and all inferred exchanges in panel (b). Forty bins of equal enrolment weight; the solid line is the enrolment-weighted fit and the dashed line the 45-degree line. The ratio reported is the ratio of weighted means.



(a) Records per administrative participant, by year



(b) Host regions within departments

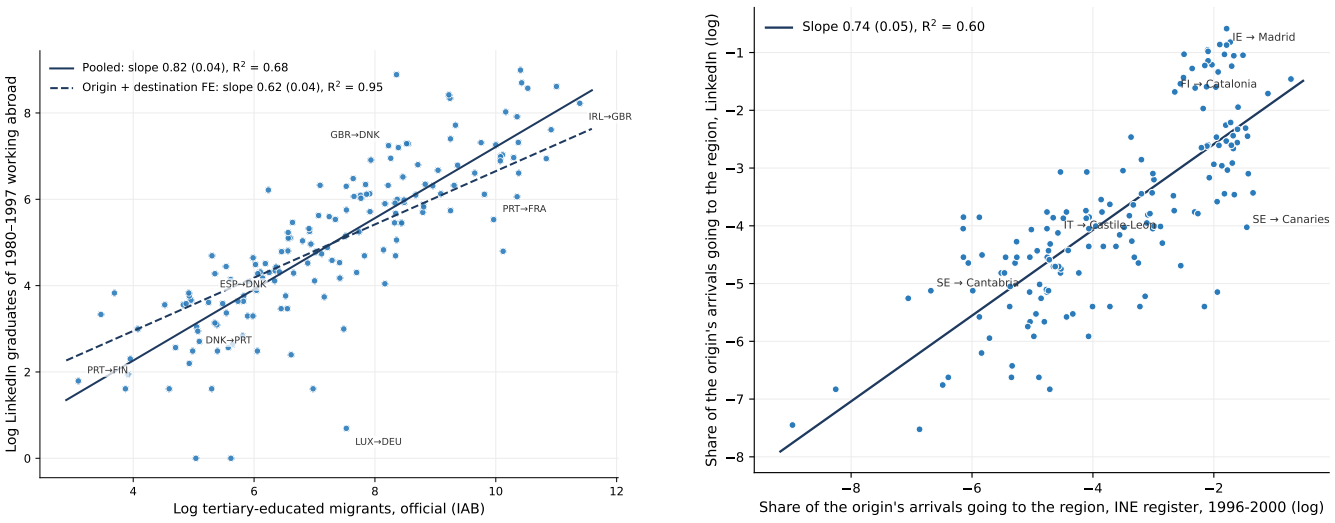
Figure A.13: Detection of exchanges over time and allocation across host regions

Notes: Panel (a): LinkedIn exchange records per administrative participant, summed over university × host-region cells, by academic year of the exchange; stays are dated by their start year. Panel (b): binned scatter over 212,397 department × host-region pairs with at least one LinkedIn record, pooled over 2008–2021. The horizontal axis is the host region's share of the department's administrative participants and the vertical axis its share of the department's LinkedIn records, both demeaned within the department. Pairs are weighted by the department's participants; the solid line is the weighted fit and the dashed line the 45-degree line.

Cross-country migration. Figure A.14(a) compares the number of 1980–1997 graduates of each EU-15 country's universities working in another EU-15 country in 2000 with the official stock of tertiary-educated migrants born in the origin and living in the destination, from the IAB brain-drain data (Brücker et al., 2013). Across the 162 country pairs with both measures, the elasticity of the LinkedIn count with respect to the official stock is 0.82 and the official stock explains 68% of the variation. With origin and destination fixed effects, which absorb country-level coverage and the sizes of sending and receiving countries, the elasticity is 0.62 and the fit rises to 95%. LinkedIn covers 11% of the official stock at the median pair. Figure A.15 adds the 2005, 2010, and

2020 census rounds of the OECD Database on Immigrants (OECD, 2023) and compares changes within country pairs. Among the 146 corridors with an average official stock of at least 2,000 tertiary-educated migrants, the within-corridor elasticity is 0.41, net of corridor, origin–year, and destination–year effects. Within-corridor changes are measured with error on both sides, since the official rounds themselves disagree on corridor-level changes while agreeing on the cross-section, so the cross-sectional comparison is the informative one.

Destinations within a country. Figure A.14(b) compares, for each EU-15 origin country, the share of its arrivals in Spain settling in each of the nineteen Spanish regions, in LinkedIn and in the Spanish residential variation register of the National Statistics Institute (INE), for arrivals aged 25–39 in 1996–2000. LinkedIn arrivals are graduates of universities in the origin country who were located outside Spain in 1995 and in Spain in 2000. Across 163 origin–region pairs, the elasticity of the LinkedIn share with respect to the register share is 0.74 and the register explains 60% of the variation in log shares. The subnational distribution of movers, the margin on which the main results are identified, is therefore reproduced in the mid-1990s, when LinkedIn covers less than a tenth of graduates.



(a) Graduates working abroad, by country pair, 2000

(b) Arrivals in Spain, by origin and region, 1996–2000

Figure A.14: Graduate migration in LinkedIn and in official statistics

Notes: Panel (a): each point is an EU-15 country pair. The horizontal axis is the log stock of tertiary-educated migrants aged 25 and over born in the origin and living in the destination in 2000 the vertical axis is the log number of 1980–1997 graduates of universities in the origin working in the destination in 2000. Belgium and Italy are absent as destinations from the benchmark. The solid line is the pooled fit and the dashed line the fit with origin and destination fixed effects. Panel (b): each point is an EU-15 origin country and a Spanish NUTS2 region. The horizontal axis is the log share of the origin's arrivals from abroad aged 25–39 in 1996–2000 that settled in the region, from the INE residential variation register, by previous country of residence; the vertical axis is the log share of graduates of the origin's universities located outside Spain in 1995 and in the region in 2000.

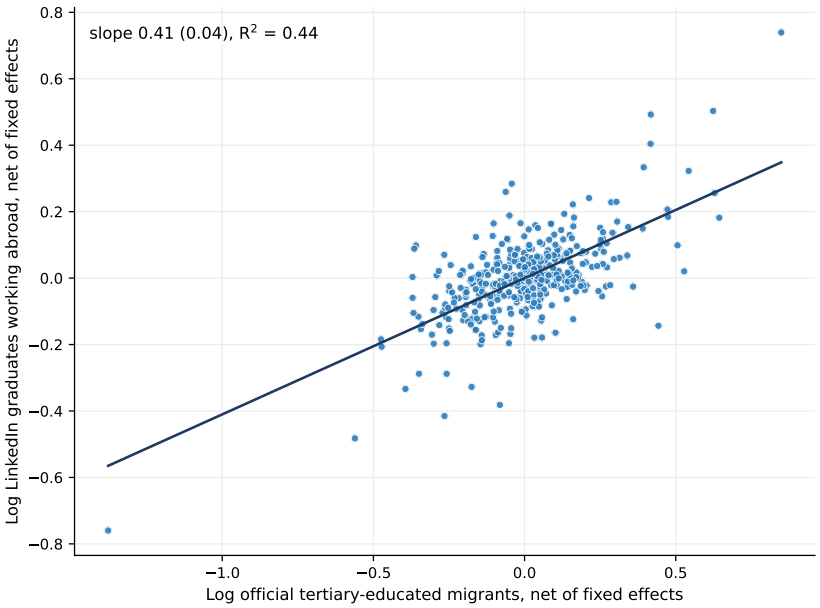


Figure A.15: Changes within country pairs: LinkedIn and official stocks, 2000–2020

Notes: Each point is a country pair in one of the years 2000, 2005, 2010, and 2020, for the 146 EU-28 pairs whose average official stock of tertiary-educated migrants is at least 2,000. The official stock is the IAB brain-drain count for 2000 and the OECD DIOC count for the later rounds (aged 25–64). The LinkedIn count is the number of graduates of universities in the origin, from the cohorts graduating 2 to 41 years earlier, working in the destination in that year. Both variables are residualized on pair, origin × year, and destination × year fixed effects. The standard error is clustered by pair.

B Derivation of the Gravity Equation

This appendix derives equation (2) from the location-choice model in Section 4 and explains its connection to the estimating equation.

Choice probabilities and normalization. Under equation (1), graduate i chooses the destination offering the highest utility from the choice set $\mathcal{L}$. Utility has two components: systematic utility, $\ln w_{l,c} + a_{l,c} - \kappa_{d,l,c}$, which captures the destination's attractiveness common to graduates from department d and cohort c, and an individual taste shock.

Choices depend on differences in systematic utility relative to the dispersion of individual tastes. Multiplying utility by θ leaves choices unchanged and gives the taste shocks unit scale. Define

$$v_{d,l,c} \equiv \theta \left(\ln w_{l,c} + a_{l,c} - \kappa_{d,l,c} \right) \tag{9}$$

as systematic utility in these normalized units. Since the taste shocks are i.i.d. type-I extreme value, the probability of choosing destination l is

$$\pi_{d,l,c} = \frac{\exp(v_{d,l,c})}{\sum_{m \in \mathcal{L}} \exp(v_{d,m,c})}. \tag{10}$$

The same difference in systematic utility produces a larger response when individual tastes are less dispersed, that is, when θ is larger. Location choices identify these normalized utility differences; separating their magnitude from θ requires additional information or restrictions.

Because income enters utility logarithmically with coefficient one, θ also measures the elasticity of the odds of choosing l over m with respect to income in l, holding other determinants fixed:

$$\ln\left(\frac{\pi_{d,l,c}}{\pi_{d,m,c}}\right) = \theta\left[\ln\left(\frac{w_{l,c}}{w_{m,c}}\right) + a_{l,c} - a_{m,c} - \kappa_{d,l,c} + \kappa_{d,m,c}\right]. \tag{11}$$

Section 7 uses an external calibration of θ to translate the estimated changes in normalized utility into income or consumption units.

From location choices to gravity. Let $N_{d,c}$ denote the number of graduates from department d and cohort c, and let $Y_{d,l,c}$ denote the number choosing destination l. The expected number choosing l is

$$\mathbb{E}[Y_{d,l,c}] = N_{d,c}\pi_{d,l,c} = N_{d,c}\frac{\exp[\theta(\ln w_{l,c} + a_{l,c} - \kappa_{d,l,c})]}{\sum_{m\in\mathcal{L}}\exp[\theta(\ln w_{m,c} + a_{m,c} - \kappa_{d,m,c})]}. \tag{12}$$

Taking logs gives

$$\ln\mathbb{E}[Y_{d,l,c}] = \underbrace{\ln N_{d,c} - \ln\sum_{m\in\mathcal{L}}\exp(v_{d,m,c})}_{\mu_{d,c}} + \underbrace{\theta(\ln w_{l,c} + a_{l,c})}_{\lambda_{l,c}} - \theta\kappa_{d,l,c}. \tag{13}$$

Exponentiating yields equation (2) in the main text.

The department–cohort component $\mu_{d,c}$ combines the number of graduates with the choice-set denominator. The latter captures the attractiveness of all alternative destinations and is the migration analogue of multilateral resistance (Bertoli and Fernández-Huertas Moraga, 2013). The destination–cohort component $\lambda_{l,c}$ captures income and amenities in destination l. These components are absorbed by department–cohort and destination–cohort fixed effects, respectively.

Bilateral utility and Erasmus exposure. Let $G_{d,l}$ denote the first cohort exposed to an Erasmus link from department d to destination l, and define $D_{d,l,c} = \mathbf{1}\{c \geq G_{d,l}\}$. To connect the model to the empirical specification, parameterize normalized bilateral utility as

$$-\theta\kappa_{d,l,c} = \xi_{d,l} + \varphi_c^K B_{d,l}^K + \varphi_c^R B_{d,l}^R + \beta D_{d,l,c}, \tag{14}$$

where $B_{d,l}^K$ indicates a destination in the department's home country and $B_{d,l}^R$ indicates its home region.

Substituting equation (14) into equation (13) gives

$$\mathbb{E}[Y_{d,l,c} \mid D_{d,l,c}, X_{d,l,c}] = \exp\left(\mu_{d,c} + \lambda_{l,c} + \xi_{d,l} + \varphi_c^K B_{d,l}^K + \varphi_c^R B_{d,l}^R + \beta D_{d,l,c}\right), \tag{15}$$

where $X_{d,l,c}$ collects the fixed-effect controls. Thus, β captures the change in normalized bilateral utility associated with an Erasmus link: $\beta = -\theta\Delta\kappa_{d,l,c}$.

C Additional Link-Level Results

This appendix reports supplementary estimates for the link-level analysis: the exchange first stage, dynamic and identification checks for the preferred PPML specification, additional robustness, the linear counterpart for employment, and heterogeneity.

C.1 Dynamics and Identification Checks

Effects at fixed career horizons. Table C.1 reports the linked-region employment effect at 1, 5, 10, and 25 years after graduation, while Figure C.1 reports estimates for every career year from one to 28. The proportional effect remains close to its career-window magnitude throughout.

Because the number of graduates with a located employment record varies over the career, Figure C.1 also reports the profile for a balanced sample of graduates located in every career year up to each horizon, and Table C.2 tests whether department entry into Erasmus changes observation rates at the corresponding horizons. The estimates are small and mostly statistically indistinguishable from zero, providing no evidence of treatment-induced differential observation.

Table C.1: Linked-region employment effects at fixed career horizons: PPML

	$k = 1$	$k = 5$	$k = 10$	$k = 25$
Post-opening	0.1108***	0.1346***	0.1151***	0.0997***
	(0.0384)	(0.0272)	(0.0236)	(0.0215)
Outcome mean, p.p.	3.0297	2.4200	2.2097	2.0443
Outcome SD, p.p.	10.3791	8.8950	8.2727	7.7211
Outcome mean (foreign EU-15), p.p.	0.3638	0.2891	0.2662	0.2409
Outcome SD (foreign EU-15), p.p.	2.0388	1.4974	1.3649	1.2418
Observations (cells)	1,262,697	1,883,735	2,144,865	2,416,821
Cohorts	18	18	18	18
Origin dept. × Dest. reg. FE	✓	✓	✓	✓
Dest. reg. × Cohort FE	✓	✓	✓	✓
Origin dept. × Cohort FE	✓	✓	✓	✓
Own ctry. + own reg. × Cohort dummies	✓	✓	✓	✓

Notes: Pooled post-opening PPML estimates with employment in the linked destination measured at the indicated number of years after graduation. Coefficients are in log points and are aggregated across link-opening cohorts using graduate-mass weights. Each horizon is estimated under the preferred specification and is subject to its own PPML separation pass. Standard errors are clustered at the department–destination level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

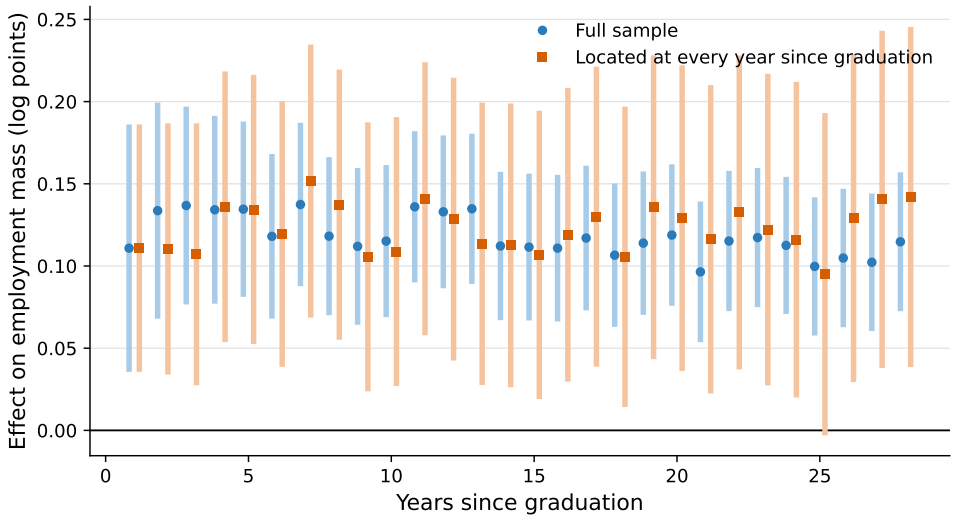


Figure C.1: Linked-region employment effect at every year since graduation

Notes: Pooled post-opening PPML estimates with employment in the linked destination measured at each career year from one to 28 after graduation. The first series uses all located graduates at each horizon; the second uses a balanced sample of graduates with a located employment record in every career year up to the horizon, so that the composition of the sample does not change along the profile. Each horizon is estimated separately under the preferred specification and is subject to its own PPML separation pass. Coefficients are in log points and are aggregated across link-opening cohorts using graduate-mass weights. Bars report 95% confidence intervals based on standard errors clustered at the department–destination level.

Table C.2: Attrition check at fixed career horizons

	$k = 1$	$k = 5$	$k = 10$	$k = 25$
Department entry, dept. + cohort FE	-0.461	-0.339	-0.717^{*}	-0.441
	(0.401)	(0.425)	(0.372)	(0.320)
Department entry, + HEI × cohort, field × cohort FE	-0.667	-0.358	-1.072^{**}	-0.740
	(0.512)	(0.567)	(0.522)	(0.459)
Located graduates per 100 records (mean)	33.25	58.78	69.12	79.78
Department–cohort cells	100,338	100,338	100,338	100,338

Notes: Department-level ETWFE estimates of the effect of a department's first entry into Erasmus on the number of graduates with a located employment record at the indicated career horizon, per 100 education records in the department–cohort. The denominator includes all graduates matched to the department–cohort, whether or not a located employment record is observed at that horizon. Cells are weighted by the number of education records. Standard errors are clustered by department. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Robustness to richer fixed effects on a common sample. The richer fixed-effect specifications in Table 1 differ slightly in their PPML estimation samples because separation is specification-specific. Table C.3 repeats the comparison on the estimation sample of the most saturated specification, separating changes due to fixed effects from changes in sample composition.

Table C.3: Common-sample robustness to richer fixed effects

	(1)	(2)	(3)	(4)	(5)
Post-opening	0.2354^{***}	0.2348^{***}	0.1414^{***}	0.2329^{***}	0.1039^{*}
	(0.0262)	(0.0265)	(0.0350)	(0.0346)	(0.0550)
Mean of y, all cells	1.1607	1.1607	1.1607	1.1607	1.1442
Mean of y, foreign EU-15 cells	0.3445	0.3445	0.3445	0.3445	0.3257
Observations	1,375,984	1,375,984	1,375,984	1,375,984	1,399,427
Origin dept. × Dest. reg. FE	✓	✓	✓	✓	✓
Dest. reg. × Cohort FE	✓	✓		✓	
Origin dept. × Cohort FE	✓	✓	✓		
Home-region × Cohort effects	✓	✓	✓	✓	✓
Domestic-country × Cohort effects	✓				
Country pair × Cohort FE		✓	✓		
University × Dest. reg. × Cohort FE			✓		✓
Origin dept. × Dest. country × Cohort FE				✓	✓

Notes: Columns (1)–(4) re-estimate the corresponding specifications of Table 1 on the estimation sample of its most saturated specification; column (5) reports the estimate from that specification. Small differences in cell counts can remain because Poisson separation depends on the fixed-effect structure. Standard errors are clustered at the department–destination level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Selection into link timing and opening cohorts. Figure C.2 relates new ICP link formation to predetermined bilateral characteristics. Figures C.3 and C.4 report employment and exchange responses separately by link-opening cohort. Column (6) of Table C.5 additionally restricts the preferred PPML specification to pairs that eventually receive a link.

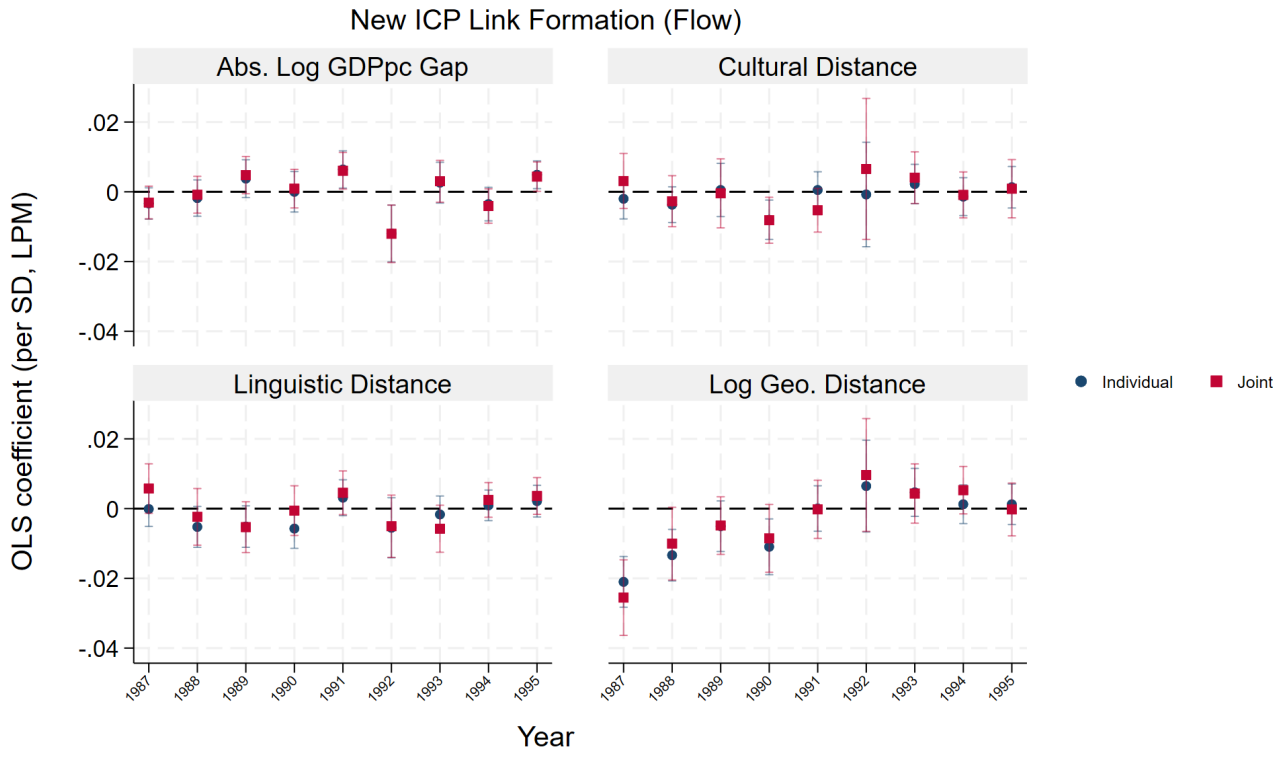
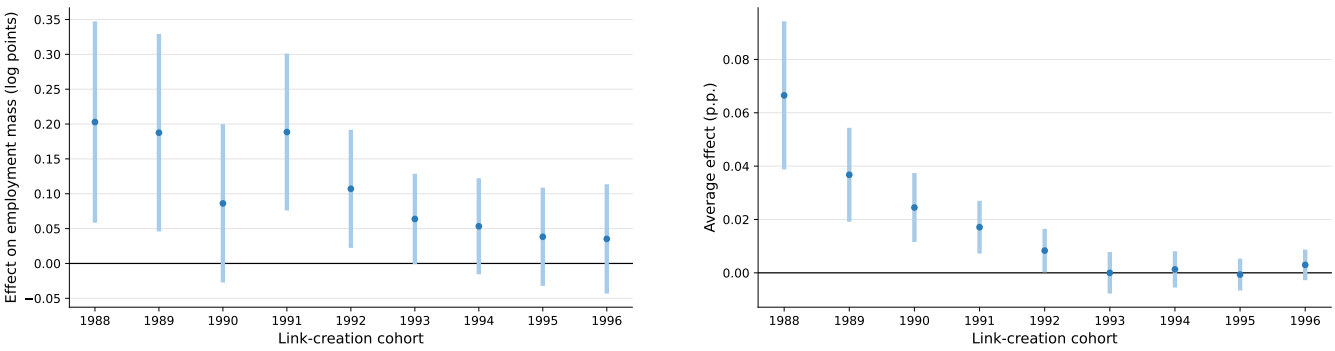


Figure C.2: Bilateral characteristics and new ICP link formation, 1987–1995

Notes: Year-specific OLS coefficients from regressions of a new-link indicator on standardized bilateral characteristics, entered separately (circles) and jointly (squares). Regressions include origin-NUTS2 and destination-NUTS2 fixed effects. The unit of observation is an undirected cross-country NUTS2-region pair; the outcome equals one only in the first year in which an ICP connection between the two regions appears. Bars report 95% confidence intervals based on standard errors clustered at the origin-NUTS2 level.



(a) Employment, PPML

(b) Employment, linear

Figure C.3: Employment effect by link-opening cohort

Notes: Pooled post-opening employment effects estimated separately by the first graduation cohort exposed to a link. Panel (a) reports PPML estimates in log points; panel (b) reports linear estimates in percentage points. Both use the preferred fixed-effect structure. Bars report 95% confidence intervals based on standard errors clustered at the department–destination level.

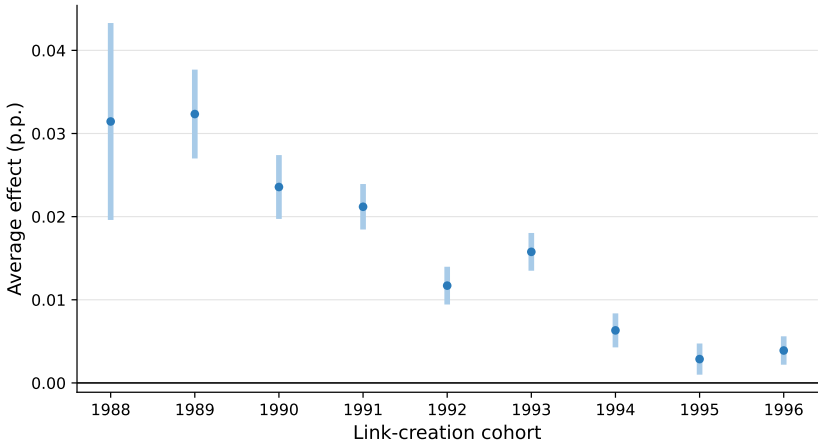


Figure C.4: Exchange participation by link-opening cohort

Notes: Pooled post-opening effects on inferred exchange participation estimated separately by the first graduation cohort exposed to a link. Estimates use the preferred linear specification and are expressed in percentage points. Bars report 95% confidence intervals based on standard errors clustered at the department–destination level.

C.2 Additional PPML Robustness

Other ICP activities. Table C.4 adds post-opening indicators for teacher mobility, curriculum development, and joint programs recorded on the same corridor. The student-mobility coefficient is essentially unchanged. Because these activities are themselves features of the broader ICP relationship, I interpret the exercise as showing that the result is insensitive to conditioning on their recorded presence rather than as isolating a unique student-mobility mechanism.

Table C.4: Robustness to other ICP activities on the same corridor

	Baseline	+ Other-activity controls
	(1)	(2)
Post-opening	0.1057***	0.1044***
	(0.0171)	(0.0188)
Teacher mobility (post)		0.0033
		(0.0141)
Curriculum development (post)		0.0213
		(0.0167)
Joint programme (post)		−0.0214
		(0.0149)
Observations (cells)	3,674,907	3,674,907
Origin dept. × Dest. reg. FE	✓	✓
Dest. reg. × Cohort FE	✓	✓
Origin dept. × Cohort FE	✓	✓
Domestic-country × Cohort effects	✓	✓
Home-region × Cohort effects	✓	✓

Notes: PPML estimates of the pooled post-opening linked-region employment effect under the preferred specification. Column (2) adds indicators equal to one from the pair's first recorded year of teacher mobility, curriculum development, and joint-program activity, respectively. These activities enter as controls rather than as separate event-study treatments. Coefficients are in log points. Standard errors are clustered at the department–destination level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Choice set and comparison group. Table C.5 varies the destination choice set and restricts the comparison group to eventually linked corridors. The latter specification drops never-linked pairs, so untreated observations come only from corridors that have not yet opened.

Table C.5: PPML robustness to the choice set and comparison group

	Baseline (1)	No aggregate dest. (2)	No home region (3)	No home country (4)	Foreign EU-15+EFTA (5)	Excl. never-linked (6)
Post-opening	0.1057***	0.1224***	0.0990***	0.0851***	0.0936***	0.0829***
	(0.0171)	(0.0181)	(0.0172)	(0.0180)	(0.0191)	(0.0300)
Outcome mean, p.p.	1.3834	1.2184	0.8964	0.4404	0.1670	0.2104
Observations (cells)	3,674,907	3,407,864	3,474,977	1,815,672	1,467,406	537,817
Origin dept. × Dest. reg. FE	✓	✓	✓	✓	✓	✓
Dest. reg. × Cohort FE	✓	✓	✓	✓	✓	✓
Origin dept. × Cohort FE	✓	✓	✓	✓	✓	✓
Domestic-country × Cohort effects	✓	✓	✓	✓	✓	✓
Home-region × Cohort effects	✓	✓	✓	✓	✓	✓

Notes: PPML estimates of the pooled post-opening linked-region employment effect under the preferred specification, with Poisson separation re-evaluated for each restricted panel. Column (2) drops the EFTA, rest-of-Erasmus, and rest-of-world aggregate alternatives; column (3) drops the department's own NUTS2 region; column (4) drops all home-country destinations; column (5) retains only foreign EU-15 regions and EFTA; column (6) drops never-linked pairs. Coefficients are in log points. Standard errors are clustered at the department–destination level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Alternative inference and aggregation. Table C.6 reports alternative clustering schemes, while Table C.7 varies the weights used to aggregate the group–time treatment effects. In particular, the equal-treated-observation scheme follows the aggregation used by Nagengast and Yotov (2025) in their gravity application.

Table C.6: Robustness to alternative clustering

	Pair (baseline)	Two-way: Dept × Dest.	Two-way: HEI × Dest.
Post-opening	0.1057***	0.1057***	0.1057***
	(0.0171)	(0.0173)	(0.0189)
Observations (cells)	3,674,907	3,674,907	3,674,907
Origin dept. × Dest. reg. FE	✓	✓	✓
Dest. reg. × Cohort FE	✓	✓	✓
Origin dept. × Cohort FE	✓	✓	✓
Domestic-country × Cohort effects	✓	✓	✓
Home-region × Cohort effects	✓	✓	✓

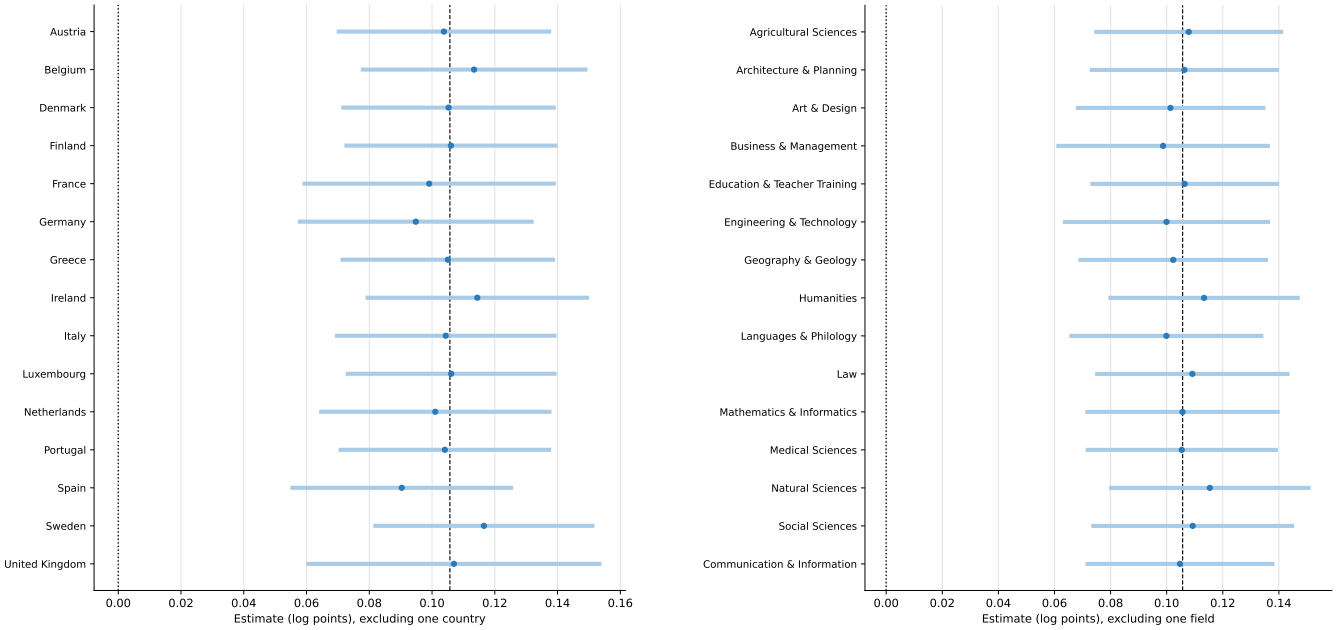
Notes: Preferred PPML estimate with the baseline pair-level clustering and the indicated two-way clustering schemes. Two-way specifications cluster by department and destination, or by university and destination. The coefficient is unchanged across columns; only the estimated variance changes. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table C.7: Pooled PPML effect under alternative aggregation weights

	Graduate mass (baseline)	Equal cells	Equal opening cohorts	Equal event times
	(1)	(2)	(3)	(4)
Post-opening	0.1057***	0.1298***	0.1070***	0.1282***
	(0.0171)	(0.0224)	(0.0175)	(0.0235)

Notes: Treatment coefficients from the preferred PPML specification aggregated under alternative weighting schemes. Column (1) uses the baseline graduate-mass weights; column (2) gives equal weight to each treated observation, corresponding to the aggregation used by Nagengast and Yotov (2025); column (3) uses graduate-mass weights within link-opening cohorts and equal weights across cohorts; column (4) uses graduate-mass weights within event times and equal weights across event times. Standard errors for the aggregates are obtained by the delta method from the full variance–covariance matrix clustered at the department–destination level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Leave-one-out robustness. Figure C.5 re-estimates the preferred PPML specification after excluding each EU-15 country and each subject area in turn. No individual country or field drives the proportional corridor effect.



(a) Excluding one country

(b) Excluding one subject area

Figure C.5: Leave-one-out robustness: PPML

Notes: Pooled PPML estimates excluding one EU-15 country at a time, simultaneously from the origin and destination sets, or one subject area at a time. Coefficients are in log points. Each restricted sample is subject to a new PPML separation pass. Dashed lines mark the full-sample estimate and dotted lines zero. Bars report 95% confidence intervals based on standard errors clustered at the department–destination level.

Best-covered countries and fields. Table C.8 re-estimates the preferred specification on the departments of the United Kingdom and on business, engineering, and social-science departments, the groups in which LinkedIn covers 26–33% of the 1997 cohort against 18% overall

(Appendix A.3). Each column restricts the origin departments and keeps all destinations. The estimates lie within one standard error of the baseline, so the response is not an artefact of thinly covered groups.

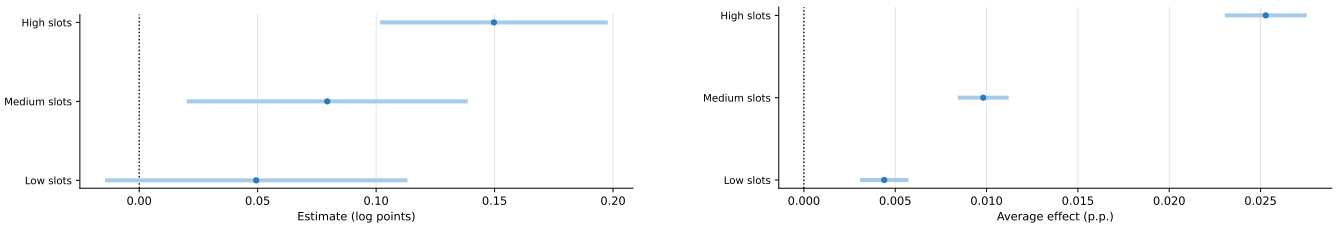
Table C.8: The linked-region effect in the best-covered countries and fields

	Baseline	UK	Business	Engineering	Social sciences
	(1)	(2)	(3)	(4)	(5)
Post-opening	0.1057***	0.1001***	0.1273***	0.1263***	0.0991**
	(0.0171)	(0.0308)	(0.0373)	(0.0410)	(0.0466)
Mean of y, all cells	0.5412	0.5230	0.6687	0.6904	0.6743
Mean of y, foreign EU-15 cells	0.0693	0.0876	0.0918	0.0698	0.0825
Observations	3,674,907	1,268,442	559,189	539,573	373,470
LinkedIn coverage, 1997 cohort (%)	18	32	26	29	28
Origin dept. × Dest. reg. FE	✓	✓	✓	✓	✓
Dest. reg. × Cohort FE	✓	✓	✓	✓	✓
Origin dept. × Cohort FE	✓	✓	✓	✓	✓
Domestic-country × Cohort effects	✓	✓	✓	✓	✓
Home-region × Cohort effects	✓	✓	✓	✓	✓

Notes: PPML estimates of the pooled post-opening linked-region employment effect under the preferred specification. Column (1) is the full sample. Columns (2)–(5) restrict the origin departments to the United Kingdom and to the business, engineering, and social-science subject areas, keeping all destinations, each with its own separation pass. The coverage row reports graduates of the 1997 cohort in the estimation sample as a percentage of first degrees (countries: Eurostat 2001 back-cast; fields: Eurostat 1998, twelve EU-15 countries). Standard errors in parentheses are clustered at the department–destination level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

C.3 Heterogeneity

Student-mobility capacity. Figure C.6 reports employment and exchange-participation responses by tercile of constructed link capacity, as discussed in Section 5.2.



(a) Employment, PPML

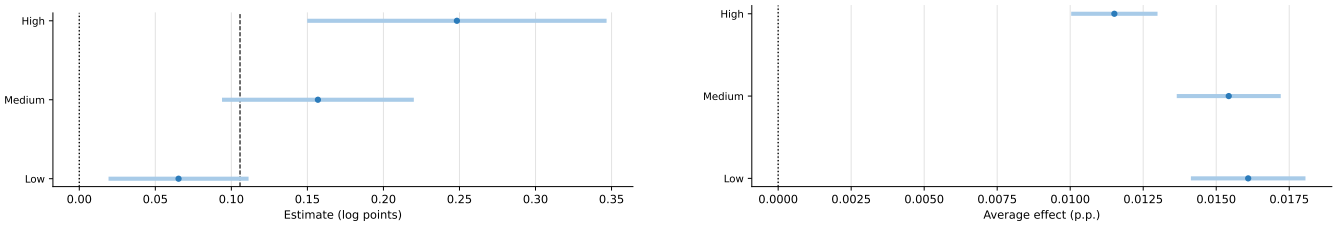
(b) Exchange participation, first stage

Figure C.6: Employment and exchange responses by student-mobility capacity

Notes: Pooled post-opening effects by tercile of initial student-mobility capacity. Capacity is measured using the quota apportioned to a department–destination pair in the first directory year in which it appears. Tercile membership is fixed throughout the pair's history, regardless of subsequent quota changes. Group effects are estimated jointly by interacting the full treatment-cohort $\times$ calendar-cohort structure with the fixed tercile indicators, with group-specific pre-treatment bins and common fixed-effect controls. Panel (a) reports PPML employment effects in log points; panel (b) reports the linear first-stage effect on inferred exchange participation in percentage points. Estimates are aggregated using graduate-mass weights. Bars report 95% confidence intervals based on standard errors clustered at the department–destination level.

The following figures report the heterogeneity exercises discussed in Section 5.2. Each reports the PPML employment effect in log points next to the linear first-stage effect on inferred exchange participation in percentage points.

Geographic distance. Figure C.7 reports effects by terciles of predetermined geographic distance between the department anchor and destination.



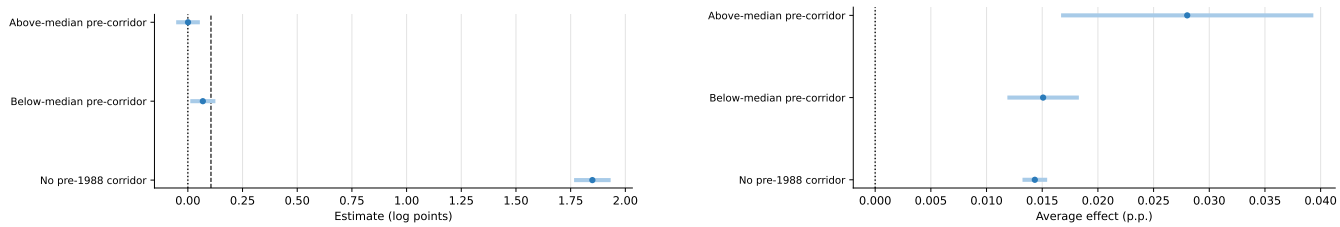
(a) Employment, PPML

(b) Exchange participation, first stage

Figure C.7: Heterogeneity by geographic distance

Notes: Corridors are divided into terciles of geographic distance between the department anchor and destination. Cutoffs are computed once over the universe of cross-border department-anchor–destination pairs using predetermined bilateral data and are common to the employment and exchange specifications. Group effects are estimated jointly by interacting the ETWFE treatment structure with tercile indicators under the preferred fixed effects. Panel (a) reports PPML employment effects in log points; panel (b) reports the linear first-stage effect on inferred exchange participation in percentage points. The dashed line in panel (a) marks the full-sample PPML estimate and the dotted line zero. Bars report 95% confidence intervals based on standard errors clustered at the department–destination level.

Pre-existing corridor networks. Figure C.8 groups corridors by employment patterns among the never-exposed 1980–1987 graduation cohorts.



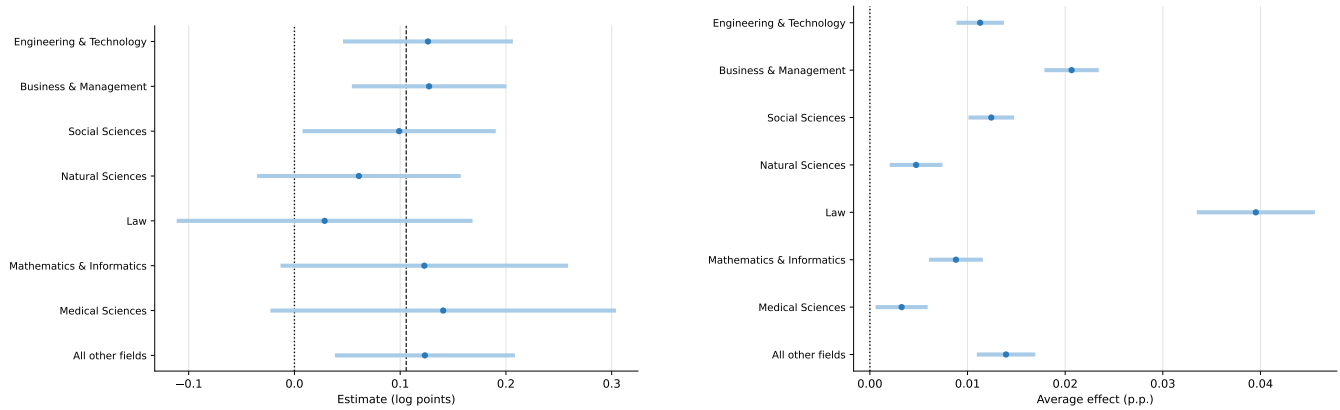
(a) Employment, PPML

(b) Exchange participation, first stage

Figure C.8: Heterogeneity by pre-1988 corridor networks

Notes: Pre-existing corridors are measured from the never-exposed 1980–1987 graduation cohorts as the share of a department's located graduates employed in the destination. Pairs with no such employment form the zero group; positive foreign corridors are split at the median, with each pair counted once. The measure is predetermined with respect to link openings. Group effects are estimated jointly by interacting the ETWFE treatment structure with group indicators under the preferred fixed effects. Panel (a) reports PPML employment effects in log points; panel (b) reports the linear first-stage effect on inferred exchange participation in percentage points. The dashed line in panel (a) marks the full-sample PPML estimate and the dotted line zero. Bars report 95% confidence intervals based on standard errors clustered at the department–destination level.

Fields. Figure C.9 reports the employment effect and exchange first stage separately for the seven fields with more than 100,000 graduates, with the remaining fields pooled.



(a) Employment, PPML

(b) Exchange participation, first stage

Figure C.9: Heterogeneity by field

Notes: Fields with more than 100,000 graduates are reported separately and ordered by sample size; "All other fields" pools the remaining subject areas. Panel (a) reports pooled PPML employment effects in log points, and panel (b) reports pooled linear effects on inferred exchange participation in percentage points. Each estimate comes from a separate split-sample regression under the preferred fixed effects. The dashed line in panel (a) marks the full-sample PPML estimate and the dotted line zero. Bars report 95% confidence intervals based on standard errors clustered at the department–destination level.

C.4 Exchange First Stage

I estimate the first stage with a linear version of equation (4) that keeps its treatment structure, fixed effects, and controls:

$$\bar{X}_{d,l,c} = \sum_{g,s} b_{g,s} \mathbf{1}\{G_{d,l} = g\}\mathbf{1}\{c = s\} + \xi_{d,l} + \mu_{d,c} + \lambda_{l,c} + \varphi_c^K B_{d,l}^K + \varphi_c^R B_{d,l}^R + \varepsilon_{d,l,c}, \tag{16}$$

where $\bar{X}_{d,l,c} = 100\, X_{d,l,c}/N_{d,c}$ and $X_{d,l,c}$ is the number of graduates of department d and cohort c with an inferred exchange in destination l. Observations are weighted by $N_{d,c}$, coefficients are aggregated with the same graduate-mass weights as the PPML estimates, and standard errors are clustered by department–destination pair. I use levels rather than PPML for two reasons. Links typically move exchange along a corridor from zero to positive values, so proportional effects have little economic content. And exchange is not chosen over the full destination set that motivates the gravity form, since it is available only through partner institutions.

Figure C.10 shows the event study. Exchange to the linked destination rises from about 0.004 percentage points for the first exposed cohort to 0.018 three cohorts later and 0.038 after nine. Part of this build-up is mechanical, since later cohorts were exposed to the link for more years of study, and the longest horizons rest on the earliest links, which have the largest exchange responses (Figure C.4). Pre-opening coefficients are small, between -0.003 and -0.001 percentage points, but statistically significant and drifting upward toward the opening, consistent with some exchange predating formal approval. The pooled effect is 0.0149 percentage points (s.e. 0.0006) and ranges from 0.0120 to 0.0154 under the richer fixed-effect specifications (Table C.9, Panel B).

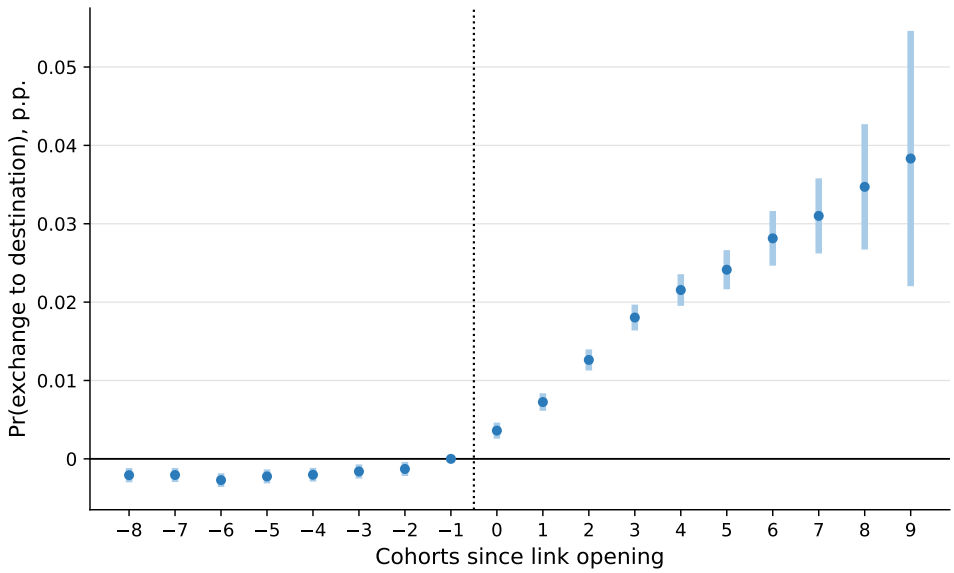


Figure C.10: Exchange participation along linked corridors: linear event study

Notes: Event-study estimates of equation (16), in percentage points. The sample consists of department $\times$ destination $\times$ graduation cohort cells for cohorts 1980–1997, weighted by the number of graduates in the department–cohort. Effects are relative to $k = -1$; event times earlier than $k = -8$ are binned. The coefficient at $k = 9$ is identified only from links first exposing the 1988 graduation cohort. Bars show 95% confidence intervals; standard errors are clustered by department–destination pair.

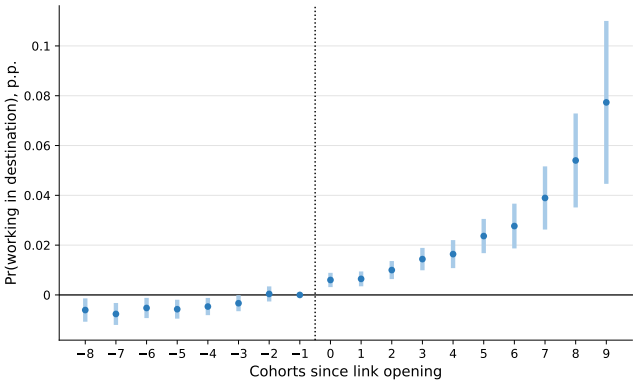
Table C.9: Linear effects of a student-mobility ICP link

	(1)	(2)	(3)	(4)	(5)
Panel A: Pr(working in destination), p.p.					
Post-opening	0.0140***	0.0143***	0.0096***	0.0132***	0.0086***
	(0.0019)	(0.0018)	(0.0017)	(0.0017)	(0.0014)
Outcome mean	0.4566	0.4566	0.4566	0.4566	0.4566
Outcome SD	3.6675	3.6675	3.6675	3.6675	3.6675
Outcome mean (foreign EU-15)	0.0332	0.0332	0.0332	0.0332	0.0332
Outcome SD (foreign EU-15)	0.4014	0.4014	0.4014	0.4014	0.4014
Panel B: Pr(exchange to destination), p.p.					
Post-opening	0.0149***	0.0149***	0.0120***	0.0154***	0.0124***
	(0.0006)	(0.0006)	(0.0006)	(0.0006)	(0.0006)
Outcome mean	0.0042	0.0042	0.0042	0.0042	0.0042
Outcome SD	0.1313	0.1313	0.1313	0.1313	0.1313
Observations	435,562,432	435,562,432	435,562,432	435,562,432	435,562,432
Origin dept. × Dest. reg. FE	✓	✓	✓	✓	✓
Dest. reg. × Cohort FE	✓	✓		✓	
Origin dept. × Cohort FE	✓	✓	✓		
Own region × Cohort dummies	✓	✓	✓	✓	✓
Own country × Cohort dummies	✓				
Country pair × Cohort FE		✓	✓		
HEI × Dest. reg. × Cohort FE			✓		✓
Origin dept. × Dest. country × Cohort FE				✓	✓

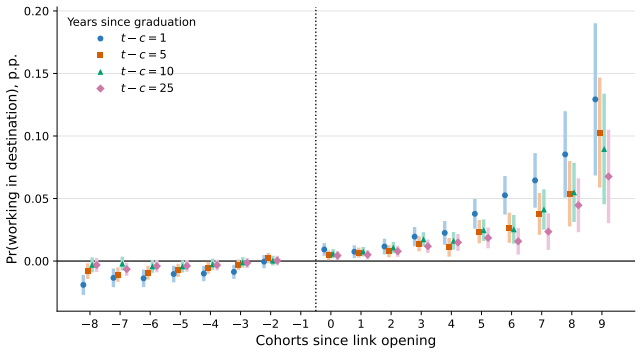
Notes: Pooled linear ETWFE estimates of equation (16). Panel A reports linked-region employment over the balanced 28-year career window (Appendix C.5); Panel B reports inferred exchange participation in the linked destination. Both outcomes are scaled by 100, so coefficients are percentage points. The columns reproduce the fixed-effect specifications of Table 1. Cells are weighted by the number of graduates in the department–cohort. Standard errors are clustered by department–destination pair. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

C.5 Linear Counterpart for Employment

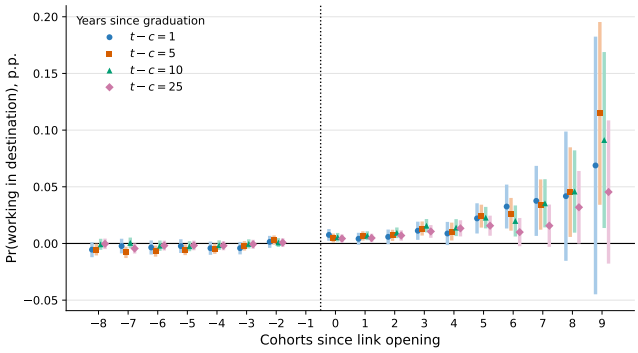
Estimating equation (16) with the employment outcome $\bar{Y}_{d,l,c} = 100\, Y_{d,l,c} / N_{d,c}$, the average share of career years that graduates of department d and cohort c spend in destination l, gives an additive counterpart to the PPML estimates that retains pairs with no employment in any cohort. Table C.9, Panel A, reports the pooled effects. Figure C.11 shows the event studies for the career window and at fixed horizons, and Table C.10 the pooled horizon estimates. At shorter horizons the leads show some drift; removing department–destination-specific linear trends estimated on untreated observations flattens the pre-treatment paths (panel c).



(a) Balanced 28-year career window



(b) Fixed career horizons



(c) Fixed horizons, pre-trends removed

Figure C.11: Linear event studies of employment in the linked region

Notes: Linear ETWFE estimates of equation (16) with the employment outcome, in percentage points. Panel (a) uses the balanced 28-year career window. Panel (b) measures employment at 1, 5, 10, and 25 years after graduation. Panel (c) repeats panel (b) after removing department–destination-specific linear cohort trends estimated on untreated observations. Effects are relative to $k = -1$. Bars show 95% confidence intervals; standard errors are clustered by department–destination pair.

Table C.10: Linked-region employment effects at fixed career horizons: linear

	$k = 1$	$k = 5$	$k = 10$	$k = 25$
Post-opening	0.0216***	0.0133***	0.0153***	0.0111***
	(0.0032)	(0.0025)	(0.0023)	(0.0022)
Outcome mean	0.4566	0.4566	0.4566	0.4566
Outcome SD	4.1787	3.9801	3.8666	3.7477
Outcome mean (foreign EU-15)	0.0276	0.0312	0.0329	0.0332
Outcome SD (foreign EU-15)	0.5706	0.5002	0.4877	0.4681
Observations	156,136,304	276,031,264	324,615,552	374,692,224
Cohorts	18	18	18	18
Origin dept. × Dest. reg. FE	✓	✓	✓	✓
Dest. reg. × Cohort FE	✓	✓	✓	✓
Origin dept. × Cohort FE	✓	✓	✓	✓
Own ctry. + own reg. × Cohort dummies	✓	✓	✓	✓

Notes: Pooled linear ETWFE estimates with employment measured at the indicated number of years after graduation, in percentage points. Cells are weighted by the number of graduates in the department–cohort. Standard errors are clustered by department–destination pair.

C.6 Three Channels Within an ICP Network

Figure C.12 illustrates how a common ICP network can connect graduates to a partner region through their own exchange, incoming students, or contact at another partner institution. The historical treatment records access through the network. The modern administrative records instead identify realized flows and dates, allowing the analysis to distinguish these forms of exposure.

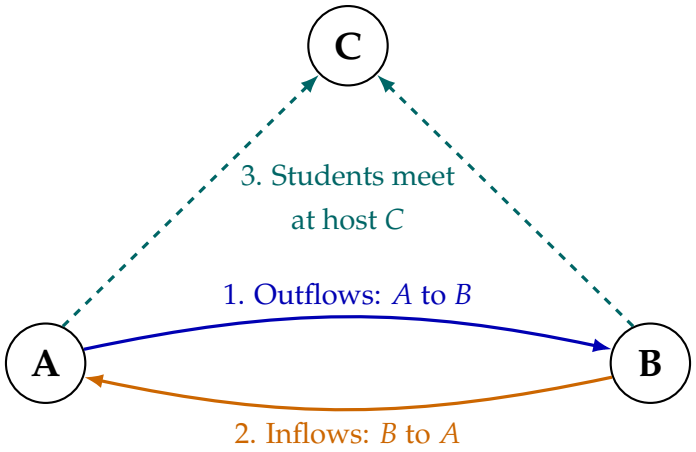


Figure C.12: Three channels connecting graduates of A to B's region

Notes: The diagram shows three departments in the same ICP, located in different countries. The historical network construction generates bilateral links among all three departments. Employment of A's graduates in B's region may increase because students from A study at B (outflows), students from B visit A and affect local students (inflows), or students from both departments meet during overlapping exchanges at C (common-host contact). Arrows represent possible realized exchange flows, not estimated effect sizes. The inflow channel can reach students at A who never study abroad; common-host contact can connect participants to regions they have not visited.

C.7 Variation with Exchange Volume

Figure C.13 distinguishes annual exposures by the number of exchange students. Annual categories are mutually exclusive within each direction and year, with no flow as the reference category. The early and late bins retain the binary any-flow coding from the baseline specification. These estimates compare flow-size categories rather than measuring a constant marginal effect per additional student.

C.8 Exposure to Exchange Peers

Figure C.14 re-estimates equation (7) with the employment indicator for region l at years 1, 5, and 10 after graduation in place of the 1–5 year average used in Figure 9. Each panel uses the participants with a located employment record at that horizon.

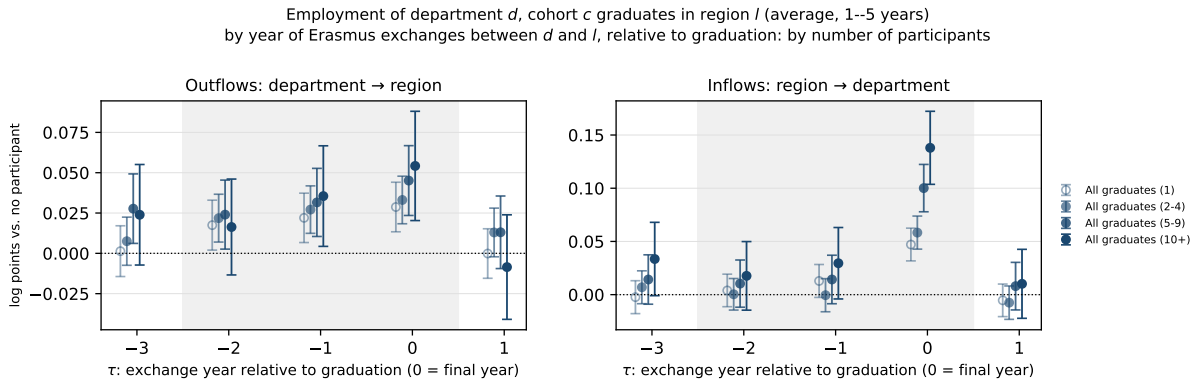


Figure C.13: Outgoing and incoming exchanges by flow size

Notes: PPML estimates for all graduates, replacing the annual any-flow indicators in equation (6) with indicators for one, two to four, five to nine, and at least ten students, relative to no flow. Incoming and outgoing exposures enter jointly for $\tau = -3, -2, -1, 0, +1$, with $\tau = 0$ denoting the academic year ending in graduation. The two outer bins remain binary indicators for any flow in observed years with $\tau < -3$ or $\tau > +1$. The outcome is employment in the destination over observed career years 1–5, with each located graduate contributing a total weight of one across destinations. The sample, fixed effects, home-bias controls, and graduate-mass aggregation weights are the same as in Figure 8. Coefficients are in log points. Bars report 95% confidence intervals based on standard errors clustered at the department–destination level.

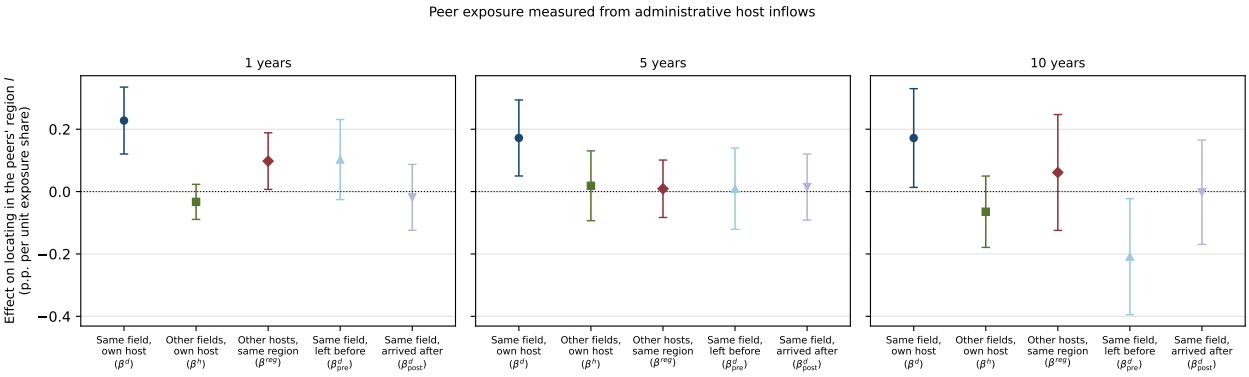


Figure C.14: Exchange-peer origins and participants' employment at years 1, 5, and 10

Notes: Linear estimates of equation (7) for LinkedIn users who explicitly report an Erasmus exchange and whose host appears in the administrative inflow records (exchanges from 2008/09). The outcome is an indicator for employment in region l at the stated year after graduation, scaled by 100. Exposures, timing checks, fixed effects, and clustering are as in Figure 9. Bars indicate 95% confidence intervals.

C.9 Geographic Margins

Table C.11: Geographic margins of the Erasmus employment effect

	(1)	(2)	(3)	(4)
Direct link to destination region	0.1057***	0.1031***	0.1013***	0.0840***
	(0.0171)	(0.0191)	(0.0191)	(0.0193)
+ any link to destination country		0.0265	−0.0045	0.0111
		(0.0239)	(0.0252)	(0.0259)
+ joined Erasmus (× foreign dest.)			0.1001***	0.1286***
			(0.0248)	(0.0287)
Observations (cells)	3,674,907	3,674,907	3,674,907	3,644,054
Origin dept. × Dest. reg. FE	✓	✓	✓	✓
Dest. reg. × Cohort FE	✓	✓	✓	✓
Origin dept. × Cohort FE	✓	✓	✓	✓
Own ctry. + own reg. × Cohort dummies	✓	✓	✓	✓
University × Foreign × Cohort FE				✓

Notes: PPML estimates extending equation (4) to the three treatment margins in equation (5). The outcome is department–cohort employment mass allocated to the destination over the balanced 28-year career window. Column (1) includes the direct-link margin; column (2) adds the linked-country margin; column (3) adds department entry interacted with foreign destinations; column (4) additionally includes university × foreign-status × cohort fixed effects. Each margin has its own treatment-cohort × calendar-cohort interactions. Coefficients are pooled incremental effects in log points, aggregated using graduate-mass weights. Each specification is subject to its own PPML separation check; zero-flow observations within retained pairs remain in the estimation. Standard errors in parentheses are clustered at the department–destination level. *** $p < 0.01$, ** $p < 0.05$.

Table C.12: Identifying variation for the nested decomposition

	Count	Share
EU-15 departments in the estimation sample	7,107	
Departments with at least one EU-15 link	5,270	
linked to some but not all 14 foreign countries	5,184	98.4%
Connected department–country pairs	48,643	
in multi-region countries	48,468	
with at least one non-linked region	47,993	99.0%
in single-region countries (corridor = country increment)	175	0.4%
first country link later than department entry	28,165	57.9%
Connected department–region pairs	168,127	
corridor opening later than first link to the country	93,577	55.7%
all three exposure clocks coincide	33,313	19.8%

Notes: Counts on the estimation sample (EU-15 departments with at least ten individuals; EU-15 NUTS2 destinations). The corridor, country, and entry increments of equation (5) are separately identified when departments are linked to some but not all foreign countries, linked countries retain non-linked regions, and the three exposure clocks $g^E(d) \leq g^K(d, K(l)) \leq g(d, l)$ differ. Shares are relative to the preceding unindented row (for the country rows, to all linked department–country pairs). In single-region destination countries the corridor and country increments coincide and only their sum is identified.

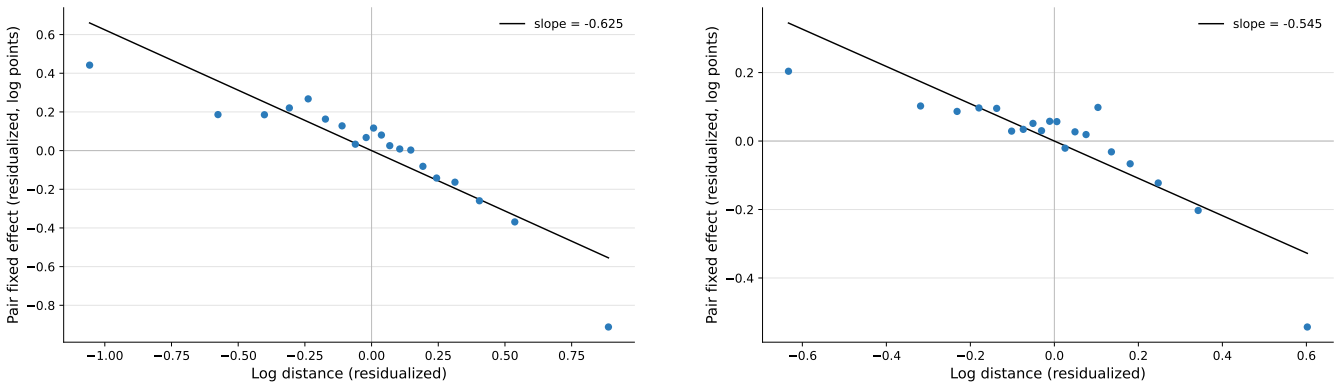
C.10 Gravity Benchmarking Regressions

Table C.13 reports the distance gradient used in Section 7, and Figure C.15 illustrates the residualized relationship.

Table C.13: Distance gradient of the estimated pair effects

	(1)	(2)	(3)	(4)
Log distance	-0.6246^{***}	-0.3840^{***}	-0.5447^{***}	-0.3703^{***}
	(0.0708)	(0.0491)	(0.0691)	(0.1422)
Sample	Foreign	Foreign	Foreign	All pairs
Cross-border control	–	–	abs.	Yes
Pair-mass weights	Yes	No	Yes	Yes
Pairs	107,334	107,334	107,334	205,585
Origin-department FE	✓	✓	✓	✓
Destination FE	✓	✓	✓	✓
Country-pair FE			✓	

Notes: Regressions of department–destination effects recovered from the joint PPML specification on log geodesic distance. Columns (1)–(3) use foreign pairs and include department and destination fixed effects. Column (1) uses graduate-mass weights; column (2) is unweighted; column (3) adds directional country-pair effects. Column (4) includes domestic pairs and a cross-border indicator. Standard errors are two-way clustered by department and destination and condition on the estimated PPML effects, without propagating their estimation uncertainty. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.



(a) Foreign pairs

(b) Within country pairs

Figure C.15: Distance and estimated bilateral employment affinity

Notes: Binned scatter of estimated pair effects against log distance on foreign pairs, weighted by graduate mass. Dots report equal-mass bins and lines show fitted relationships. Panel (a) residualizes both variables on department and destination effects; panel (b) additionally absorbs directional country-pair effects, retaining only regional variation within country pairs.

C.11 Utility Gains and Monetary Implementation

This subsection derives the utility-gain formulas in Section 7 and documents their empirical construction, income valuation, population scaling, and expenditure comparison.

Expected utility and the log-sum. Using the normalized systematic utility defined in Section 4, equation (1) can be written as

$$u_{i,d,l,c} = \frac{1}{\theta}\left(v_{d,l,c} + \epsilon_{i,l}\right). \tag{17}$$

Each graduate chooses the destination that maximizes utility given their destination-specific preferences. Expected maximum utility averages this maximized utility over the taste shocks, holding systematic utilities fixed. For i.i.d. unit-scale type-I extreme-value shocks (Small and Rosen, 1981),

$$U_{d,c} \equiv \mathbb{E}_\epsilon\left[\max_{l\in\mathcal{L}} u_{i,d,l,c}\right] = \frac{1}{\theta}\left[\ln \sum_{l\in\mathcal{L}} \exp(v_{d,l,c}) + \gamma_E\right], \tag{18}$$

where γ_E is the Euler–Mascheroni constant, which cancels when comparing policy counterfactuals. Thus $U_{d,c}$ measures the value of the available destination choices, allowing each graduate to select their preferred location. As in the main text, it is expressed per graduate and observed career year in log-consumption units.

Let $v^1_{d,l,c} = v^0_{d,l,c} + \Delta v_{d,l,c}$ and $\pi^0_{d,l,c} = \exp(v^0_{d,l,c}) / \sum_m \exp(v^0_{d,m,c})$. The additive constant

cancels when taking differences:

$$\begin{aligned}\Delta U_{d,c} &= \frac{1}{\theta}\ln\frac{\sum_l \exp(v^1_{d,l,c})}{\sum_l \exp(v^0_{d,l,c})} \\ &= \frac{1}{\theta}\ln\left[\sum_l \pi^0_{d,l,c}\exp(\Delta v_{d,l,c})\right].\end{aligned} \tag{19}$$

This gives equation (8). For small shifts, $\theta\Delta U_{d,c} \approx \sum_l \pi^0_{d,l,c}\Delta v_{d,l,c}$: destinations contribute in proportion to their counterfactual choice shares. The calculation uses the total utility shift and does not need to separate its sources, as long as links change the options graduates face, through lower costs, better information, or higher destination-specific returns. If links instead changed graduates' preferences, utility before and after would rest on different preferences, and their difference would not have a standard welfare interpretation. Incomes, amenities, and non-treatment utility components are held fixed across counterfactuals, while destination probabilities are renormalized.

Geographic implementation. Table C.14 combines the pooled coefficients from the joint PPML specification with each department's actual link portfolio and exposure dates. The constructed utility shift is

$$\widehat{\Delta v}_{d,l,c} = \widehat{\beta}^E D^E_{d,l,c} + \widehat{\beta}^K D^K_{d,l,c} + \widehat{\beta}^L D^L_{d,l,c}, \tag{20}$$

where $\widehat{\beta}^E$, $\widehat{\beta}^K$, and $\widehat{\beta}^L$ are the pooled entry, country, and direct-link coefficients. The indicators follow the exposure dates defined in equation (5); the coefficients remain fixed rather than following their estimated dynamic paths.

Counterfactual shares are constructed from the estimated destination component of normalized utility, setting all treatment terms to zero:

$$\widehat{v}^0_{d,l,c} = \widehat{\xi}_{d,l} + \widehat{\lambda}_{l,c} + \widehat{\varphi}^K_c B^K_{d,l} + \widehat{\varphi}^R_c B^R_{d,l}, \qquad \widehat{\pi}^0_{d,l,c} = \frac{\exp(\widehat{v}^0_{d,l,c})}{\sum_m \exp(\widehat{v}^0_{d,m,c})}. \tag{21}$$

The department $\times$ cohort effect $\mu_{d,c}$ is excluded. By equation (13) it is the log of the choice-set denominator together with $\ln N_{d,c}$, not a component of any destination's utility; being common across destinations, it cancels in equation (21) in any case. The individual fixed-effect components are identified only up to offsetting shifts between them, but their sum $\widehat{v}^0_{d,l,c}$ is invariant to the normalization, and a remaining department–cohort constant is absorbed by the share normalization.

Panel B instead uses pooled pre-entry career shares on the same evaluation sample. For

either set of shares, I calculate the gain within each department–cohort and then average:

$$\widehat{\Delta U}_{d,c} = \frac{1}{\theta} \ln \left[\sum_{l} \widehat{\pi}^{0}_{d,l,c} \exp(\widehat{\Delta v}_{d,l,c}) \right], \qquad \overline{\widehat{\Delta U}} = \frac{\sum_{(d,c)\in\mathcal{E}} N_{d,c} \widehat{\Delta U}_{d,c}}{\sum_{(d,c)\in\mathcal{E}} N_{d,c}}, \tag{22}$$

where $\mathcal{E}$ contains exposed department–cohorts and $N_{d,c}$ is their number of located graduates. Each department–cohort enters the average once, regardless of its number of links. The table reports $100\theta\overline{\widehat{\Delta U}}$.

For the sequential decomposition, I repeat the calculation adding direct links, country links, and broad foreign entry in that order. Differences between successive totals give each increment; the first therefore values direct links alone. Shapley contributions average these increments over all orderings.

Table C.14: Utility-gain construction by geographic margin

	ΔW (log points)		
	Cumulative	Increment	Shapley
Panel A: model-implied counterfactual shares			
Direct links only	0.35	0.35	0.36
+ country margin	0.32	-0.03	-0.03
+ entry margin (all margins)	2.51	2.19	2.18
First-order approximation (all margins)	2.40		
Panel B: pre-entry-share approximation			
Direct links only	0.29	0.29	0.30
+ country margin	0.27	-0.02	-0.02
+ entry margin (all margins)	2.46	2.19	2.18
First-order approximation (all margins)	2.35		
Sufficient statistic, basic specification	1.76		
Sufficient statistic, most demanding	1.27		

Notes: Graduate-weighted normalized utility gains, multiplied by 100, constructed using pooled geographic coefficients. Panel A uses fitted no-treatment destination shares; Panel B uses pooled pre-entry shares on the same evaluation sample. Sequential increments add direct links, country links, and broad foreign entry; Shapley contributions average increments across all orderings. Both decompositions sum in normalized utility units, but nonlinear consumption conversions need not be additive. The first-order row averages $100 \sum_l \widehat{\pi}^{0}_{d,l,c} \widehat{\Delta v}_{d,l,c}$ using the same graduate weights.

Table C.15 reports the graduate-careers behind the counts in Section 7: the predicted distribution of exposed department–cohorts across destination categories without Erasmus, and the distribution after the link, country, and entry shifts are applied cumulatively to each department's

actual links and timing.

Table C.15: Graduate-careers of exposed department–cohorts by destination under cumulative counterfactuals

	No Erasmus	Increment			All margins	Total change
		+ links	+ country	+ entry		
Directly linked region	42,061	4,157	-188	3,491	49,522	7,460
Other regions of a linked country	29,752	-122	-122	2,239	31,747	1,994
Other EU-15 destinations	17,072	-40	4	1,275	18,311	1,239
Other foreign destinations	178,613	-685	54	12,788	190,770	12,157
Home country	1,012,707	-3,310	253	-19,793	989,856	-22,851

Notes: The table reports graduate-careers of exposed department–cohorts by destination category, where one graduate-career is the full observed career of one located graduate. The first column uses the destination shares that the joint PPML specification predicts without Erasmus. Each subsequent column adds one margin in turn (the direct link, the linked country, and department entry), applied to each department's actual links and timing. Shares are renormalized within each department–cohort and multiplied by its number of located graduates, so every column sums to the same total. The last column reports the change from no Erasmus to all three margins. Population figures in the text multiply these changes by 7.7, the ratio of first degrees awarded by exposed departments (9.4 mln Table C.16) to exposed first-degree graduates in the sample (1.2 mln).

Income numeraire and discounting. I construct an employee-weighted EU-15 real-compensation path from AMECO (European Commission, Directorate-General for Economic and Financial Affairs, 2026). Each country's real-compensation index, deflated by the private-consumption deflator, is rebased to its 2024 euro level before aggregation. All years are therefore expressed in 2024 euros at 2024 exchange rates. The EU-15 average rises from approximately €40,200 in 1988 to €52,400 in 2024, and its exposure-weighted average over the relevant careers is approximately €48,600. Compensation includes employer social contributions and is an economy-wide income numeraire, not graduates' disposable consumption.

For a reported average annual gain ΔU and graduation cohort c, the present value at graduation is

$$PV_c = \left[\exp(\Delta U) - 1\right] \sum_{a=1}^{28} \frac{y_{c+a}}{(1.03)^a}, \tag{23}$$

where y_{c+a} is real compensation in career year a. This applies the estimated career-window average gain throughout the observed 28 years, using a 3% real discount rate (European Commission, 2023). For the expenditure ratios, benefits and expenditure are both discounted to 1987. The headline per-graduate values are instead discounted to graduation.

Population scaling. Exposure applies to graduates of participating departments, not only to exchange participants. For country j and graduation cohort c, let $p_{j,c}$ be the graduate-weighted

share of the LinkedIn sample whose department has entered Erasmus, $c \geq G_d^E$. I apply these shares to historical graduate counts $G_{j,c}$, giving a total exposed population of $\sum_{j,c} G_{j,c} p_{j,c}$.

The population reconstruction takes each country's number of first degrees from the earliest usable year in Eurostat and back-casts it to earlier cohorts at 2.1% per year, the growth of first degrees between 1998 and 2001 in the eleven countries reported in both years. The base year is 1998 for eleven countries, 2000 for Belgium and Luxembourg, which first report in that year, and for Denmark, whose 1998 and 1999 figures exclude bachelor's degrees, and 2001 for Greece. The same construction underlies the coverage rates in Appendix A.3. Table C.16 reports two bases, first university degrees (ISCED-97 level 5A) and all tertiary completions (ISCED-97 levels 5–6), which yield about 9.4 and 14.6 million exposed graduates over 1988–1997. The main text uses first degrees to avoid counting the same person at several degree levels.

This extrapolation requires the sample exposure shares to apply within population country–cohort cells and the estimated utility gains to generalize beyond LinkedIn. For scale, the administrative series records 448,813 Erasmus mobilities during the ICP phase (European Commission, Directorate-General for Education and Culture, 2012).

Table C.16: Graduates exposed to department entry, by cohort

Cohort	Exposed	Population (000s)		Exposed (000s)	
	share	5A	5–6	5A	5–6
1988	0.338	1,101	1,716	380	590
1989	0.523	1,124	1,752	599	930
1990	0.661	1,147	1,789	769	1,194
1991	0.735	1,171	1,827	871	1,356
1992	0.804	1,196	1,865	966	1,508
1993	0.868	1,221	1,905	1,059	1,649
1994	0.895	1,247	1,945	1,114	1,737
1995	0.919	1,273	1,985	1,166	1,817
1996	0.933	1,300	2,027	1,211	1,885
1997	0.929	1,327	2,070	1,231	1,917
Total		12,108	18,882	9,367	14,584

Notes: Country–cohort exposure shares are calculated with graduate weights in the matched LinkedIn sample. The displayed shares are aggregated with LinkedIn weights. Population counts back-cast each country's earliest usable Eurostat count of first degrees (1998–2001) at 2.1% per year, and exposed counts apply the country–cohort shares to these totals. The two bases are first university degrees (ISCED-97 level 5A) and all tertiary completions (ISCED-97 levels 5–6). Counts are in thousands.

Program expenditure and sensitivity. The expenditure construction combines ECU 85 million for 1987–1990 (Council of the European Communities, 1987), ECU 376.5 million for 1990–1994, and ECU 117.2 million and 77.9 million for 1995 and 1996, respectively, for a nominal total of

approximately ECU 657 million. Allocating the multi-year envelopes evenly and converting with the EU-15 private-consumption deflator gives €1.19 billion in 2024 prices, or €1.02 billion discounted to 1987. These inputs combine allocated and ex-post expenditure and exclude resources supplied by universities, national agencies, and students.

Table C.17 reports sensitivity to the elasticity, population base, horizon, and income numeraire. The 40-year extrapolation holds the gain and real income constant beyond the observed window, and the alternative numeraire is the 2024 EU average full-time salary of €39,800 (Eurostat, 2025a). A participant-count rescaling is also reported as an illustration, but does not correspond to the department-level estimand. Monetary values per euro of EU expenditure are scale comparisons, not net social returns: the budget includes transfers and omits other resources, while the valuation excludes migration-induced wage adjustments and effects on other workers, firms, and communities.

Table C.17: Sensitivity of monetary equivalents and expenditure comparisons

	$\theta = 0.50$	$\theta = 1.63$	$\theta = 3.18$
Panel A: Sensitivity to the migration elasticity			
Domestic-share gain, CE (%)	3.58	1.08	0.55
PV per graduate (€)	32,241	9,770	4,995
Direct-link gain, CE (%)	0.70	0.21	0.11
PV per graduate (€)	6,413	1,958	1,002
All-margins gain, CE (%)	5.20	1.56	0.79
PV per graduate (€)	47,135	14,122	7,202

Panel B: Alternative valuation assumptions, domestic-share gain, $\theta = 1.63$

	PV per graduate (€)
28-year historical-income benchmark	9,770
40-year extrapolation	12,268
28 years at the 2024 average salary	8,098

Notes: Panel A varies θ over the minimum, median, and maximum of the Head and Mayer (2021) compilation. Consumption equivalents are $\exp(\Delta U) - 1$, and normalized gains $\theta \Delta U$ are displayed multiplied by 100. Present values use the historical real-compensation path over 28 years, discounted to graduation at 3%, in 2024 euros. Expenditure ratios discount both benefits and costs to 1987. Panel B converts sequential geographic increments, Panel C compares first-degree and all-tertiary population bases, and Panel D varies the horizon and income numeraire. The main aggregate uses 9.37 million exposed first-degree graduates, and the participant-count illustration uses 448,813 administrative mobilities.

C.12 Comparison with Parey and Waldinger (2011)

Applying the department-entry design of Parey and Waldinger (2011) to the German LinkedIn subsample gives a reduced-form foreign-employment effect of 0.50 percentage points, close to

the 0.60 implied by their survey estimates (Table C.18). The comparison supports the scale of the LinkedIn employment response without requiring the two samples to have identical composition. I emphasize the reduced form, which compares the employment response to institutional access rather than interpreting the ratio of employment and participation responses as a private return to exchange.

Table C.18: The Parey–Waldinger design in the German LinkedIn sample

	Parey–Waldinger specification			ETWFE	
	First stage	Reduced form	IV	First stage	Reduced form
	(1)	(2)	(3)	(4)	(5)
ERASMUS / post-joining	0.3303**	0.5004	1.5151	1.1623***	0.3516
	(0.1591)	(0.4030)	(1.3419)	(0.2726)	(0.7609)
Graduate cohort FE	✓	✓	✓	✓	✓
Subject FE	✓	✓	✓		
University FE	✓	✓	✓		
Department (univ. × subject) FE				✓	✓
Outcome mean	2.0295	19.0805		2.0295	19.0805
Cells	15,268	15,268	15,268	15,268.0	15,268.0

Notes: Columns (1)–(3) reproduce the static specification of Parey and Waldinger (2011), reporting the first stage, reduced form, and IV estimate. They include cohort, subject, and university effects, with graduate weights and standard errors clustered by university. The remaining columns report ETWFE counterparts on the same cells, with standard errors clustered by department. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

D Formalizing the Composition Bias

This appendix distinguishes selection that reweights a genuine mobility response from selection that can generate an apparent response without changing individual location choices. A two-type model illustrates this distinction, the protection provided by within-country comparisons, and the role of observed cohort size. The expressions are a sketch for intuition: accounting decompositions of potential observed means, written for additive effects rather than for the proportional PPML estimates, and not exact bias formulas for the pooled ETWFE coefficients. They convey the main concerns rather than quantify them.

Reweighting and composition. Consider a fixed graduate population in department–cohort (d, c). A share $\delta_{d,c} \in (0, 1)$ are type H and the remainder type L. Let $Y^j_{d,c}(a)$ denote type j's mean foreign-employment career share under Erasmus-entry status $a \in \{0, 1\}$. For simplicity, only type H responds: $Y^H_{d,c}(1) = Y^H_{d,c}(0) + \tau^H_{d,c}$ and $Y^L_{d,c}(1) = Y^L_{d,c}(0)$. These are response types, not participation categories: type H may include graduates affected through classmates or

departmental changes. The population effect is $\tau_{d,c} = \delta_{d,c}\tau^H_{d,c}$.

Let $\delta^*_{d,c}(a)$ be the share of type H among observed graduates. Assume selection changes type proportions but not type-specific potential-outcome means. The observed outcome is

$$Y^*_{d,c}(a) = \delta^*_{d,c}(a)Y^H_{d,c}(a) + [1 - \delta^*_{d,c}(a)]Y^L_{d,c}(a). \tag{24}$$

Writing $\Delta\delta^*_{d,c} = \delta^*_{d,c}(1) - \delta^*_{d,c}(0)$, the observed change decomposes as

$$\begin{aligned}\tau^*_{d,c} &= Y^*_{d,c}(1) - Y^*_{d,c}(0) \\ &= \underbrace{\frac{\delta^*_{d,c}(0)}{\delta_{d,c}}\tau_{d,c}}_{\text{effect in the observed population}} + \underbrace{\frac{\Delta\delta^*_{d,c}}{\delta_{d,c}}\tau_{d,c}}_{\text{B1: treatment-induced reweighting}} + \underbrace{\Delta\delta^*_{d,c}\left[Y^H_{d,c}(0) - Y^L_{d,c}(0)\right]}_{\text{B2: composition effect}}.\end{aligned} \tag{25}$$

The first term is not a bias. Since $\tau_{d,c} = \delta_{d,c}\tau^H_{d,c}$, it equals $\delta^*_{d,c}(0)\tau^H_{d,c}$, the average effect among the graduates LinkedIn observes given the fixed representation of responsive types among them. It differs from the population effect only through that representation, which does not depend on treatment, so the gap is a matter of whom the estimates describe rather than of internal validity. The two bias terms are B1 and B2. B1 arises only if the link changes the representation of responsive graduates, and it vanishes when $\tau^H_{d,c} = 0$. If Erasmus changes representation only through the moves it induces, $\Delta\delta^*_{d,c}$ is at most the share of induced movers among observed graduates, so B1 is small relative to the population effect. B2 can remain positive without a behavioral response: if Erasmus increases the representation of graduates who would have been more mobile anyway, both $\Delta\delta^*_{d,c}$ and the untreated outcome gap are positive.

Two causal stories matter. If Erasmus changes representation only through the mobility it induces, the sharp null of no individual mobility response also implies $\Delta\delta^*_{d,c} = 0$: B1 and B2 vanish. Selection through this pathway can distort a genuine effect but cannot create one under that null. Alternatively, Erasmus could encourage LinkedIn representation among internationally oriented graduates without changing their locations, allowing B2 to generate an apparent effect. These stories do not map one-to-one to B1 and B2, since selection following induced mobility can affect both terms. The distinction concerns whether representation changes under the sharp null, not merely whether the average effect is zero.

What within-country comparisons absorb. The same decomposition applies to a destination share by replacing $Y^j_{d,c}(a)$ with $\pi^j_{d,l,c}(a)$ in equation (25). Here a denotes the status of the link under consideration, holding other exposures fixed. Expected observed employment in l is $N^*_{d,c}(a)\pi^*_{d,l,c}(a)$, where N^* is the expected observed graduate count. Department $\times$ cohort effects absorb changes in this count, but not changes in destination composition.

For country $K = K(l)$, write $\pi^j_{d,l,c}(0) = M^j_{d,K,c}(0)q^j_{d,l|K,c}(0)$, where M^j is the probability of

working in K and q^j the conditional regional share, defined for positive country probabilities. Suppose types differ in country orientation but share the same conditional regional distribution:

$$q^H_{d,l|K,c}(0) = q^L_{d,l|K,c}(0) = q_{d,l|K,c}. \tag{26}$$

Under the no-response null, observed destination shares then factorize as

$$\begin{aligned} \pi^*_{d,l,c}(a) &= q_{d,l|K,c} M^*_{d,K,c}(a), \\ M^*_{d,K,c}(a) &= \delta^*_{d,c}(a) M^H_{d,K,c}(0) + [1 - \delta^*_{d,c}(a)] M^L_{d,K,c}(0). \end{aligned} \tag{27}$$

Consequently,

$$\ln \frac{\pi^*_{d,l,c}(1)}{\pi^*_{d,l,c}(0)} = \ln \frac{M^*_{d,K,c}(1)}{M^*_{d,K,c}(0)}, \tag{28}$$

which is common to all regions within country K.

Department $\times$ destination-country $\times$ cohort effects absorb this factor exactly. Greater representation of graduates oriented toward Spain cannot therefore generate a Madrid-specific response if their conditional regional distribution is unchanged. Selection can still contaminate the corridor estimate when types have different regional preferences and changes in their representation favor the newly linked region at the time of opening. The richer specification thus restricts selection explanations; it does not eliminate region-specific selection or recover population effect weights automatically.