

# The Brexit Tuition Shock and the Price Elasticity of International Students: Evidence from a Natural Experiment

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## Article

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# **The Brexit Tuition Shock and the Price Elasticity of International Students: Evidence from a Natural Experiment**

## **ABSTRACT**

This paper estimates the price elasticity of EU demand for UK higher education using the post-2021 Brexit-induced loss of Home Fee status and student-loan access as an externally imposed tuition shock. A weighted difference-in-differences design produces country-specific post-reform effects that synthetic difference-in-differences estimates closely reproduce. The reform reduced EU applicant numbers by approximately 67% on average, with substantial and persistent variation across origins. When the reform is treated as a common nominal price change, the implied elasticities exceed conventional estimates and surpass 1.5 in absolute magnitude for several countries, with applicants from lower-income origins appearing more responsive. However, the pre-Brexit fee was financed through an income-contingent loan whose present value varies with origin-specific earnings, repayment thresholds, and the subjective discounting of future payments. We map the same estimated treatment effects across country-specific effective price changes and a range of subjective discount rates to reveal the dualistic nature of income-contingent financing as well as the drivers of structural inequality of access to higher education. The relationship between elasticity and GDP per capita becomes statistically insignificant at approximately a 6% subjective discount rate and reaches income neutrality at approximately a 14.9% subjective discount rate. Many implied elasticities then approach the ranges reported in the literature, although some remain above one in absolute magnitude. The results show that cross-border demand elasticity depends on baseline financing conditions and that the UK student-loan system operated as an equalizing institution for European educational

mobility. We argue that an unequal access is a function of structural conditions embedded in the receiving higher educational systems.

**JEL codes:** I22; I23; F22; J61; D12; C23

**Keywords:** Brexit; international student mobility; higher education demand; tuition fees; price elasticity; EU students; UCAS applicants; difference-in-differences; synthetic control; income-contingent loans; educational mobility; heterogeneous treatment effects.

## 1. Introduction

International higher-education demand studies struggle to isolate own-price elasticity because they generally infer demand from enrollment changes, lack sufficient exogenous price variation, or face both problems simultaneously. Many estimates of the price elasticity of college enrollment “treated the application decision as exogenous by focusing only on the influence of price on the college-going behavior of the admitted applicants” (Savoca, 1990), and so “ignored the possibility that a change in tuition may affect enrollments through its effect on the applicant pool” itself (p. 123). Standard demand models using enrollment data thus conflate the desire to attend with the university’s choice to admit. Recent extensions retain this enrollment-based approach. For example, McQuone and Simons (2025), building on Conlon et al. (2021), estimate short-run elasticities between  $-0.54$  and  $-0.81$  for non-EU students, magnitudes that remain too small to account for the post-2021 collapse observed in several EU-origin applicant series.

To address the lack of exogenous tuition variation and the limitations of enrollment as a demand proxy, we treat the number of Universities and Colleges Admissions Service (UCAS) applicants from each domicile as expressed origin-country demand before universities ration places. Applicant counts are observed before admission decisions, offers, acceptances, and enrollment. Identification comes from within-country changes over time around the externally

imposed loss of Home Fee status and loan access. Because the financial consequences varied across origin countries, the design also permits country-specific treatment effects that can be mapped to constant-elasticity demand responses under the maintained price-shock assumptions.

First, we show that applicants from lower-income EU countries have declined more relative to their richer counterparts; we then argue that this, by itself, does not mean that their preferences are inherently more elastic. Although Brexit caused a common shock in nominal prices, how countries absorbed it varied because the value of the income-contingent loan lost depended on country-specific earnings, repayment thresholds, and how individuals subjectively discounted future repayments. In countries where poverty was endemic or generational, loan payments were delayed; in richer countries, payments were made immediately because expected earnings were higher. We later show that the relationship between elasticity and GDP per capita becomes statistically insignificant at a “subjective” discount rate of about 6% and reaches income neutrality at about 14.9%, and that as subjective discount rates increase, elasticities move towards the conventional rates in the literature. Countries' responses to price changes therefore depend on the financing conditions embedded in the pre-Brexit system, illustrating that demand elasticity cannot be understood without accounting for the actual price applicants experience. Finally, we examine how fiscal drag from nominal wage convergence in the EU may have affected the income neutrality of the estimated elasticities.

## **2. Context**

### **2.1. Brexit and higher education**

Greater competition in US higher education increased tuition and institutional differentiation (Hoxby, 1997). In 2012, England subsequently pursued market-oriented reforms, although Brown (2011) questioned their implications for quality, financial sustainability, and stratification. Against the broader expansion of globalized higher education, Brexit, however, marked a major reversal of UK-EU integration. Calhoun (2017) reads the Leave vote partly as a populist reaction against London-centered globalization and cosmopolitan elites seen to have benefited disproportionately, accompanied by nostalgia for an earlier Britain and frustration with existing conditions. For higher education, the most consequential outcome was the rupture of

academic and financial ties with the EU. The 2020/21 academic year was the last in which new EU students generally qualified for Home Fee status and government-supported loans. From 2021/22 they became liable for international fees, which often doubled or tripled their costs. As a result, Brexit produced one of the largest discrete tuition shocks faced by a clearly defined group of students in twenty-first-century British higher education.

The Brexit fallout also echoed in financial and reputational concerns about recruiting EU students and staff, accessing European research funding, and UK students studying abroad (Mayhew, 2022). Even so, the UK remained the world's second most popular destination for international students in 2023 (Cuibus et al., 2025). About 730,000 international students were enrolled at UK universities in 2023/24, some 25 percent of the student population, and their fees generated 23 percent of total university income (Cuibus et al., 2025). Cuibus et al. (2025) report that EU numbers fell sharply after Brexit's 2021/2022 tuition hikes and then stabilized, so that by 2023/24 EU students were only 7 percent of newly enrolled international students. Mayhew (2022) notes that the decline in EU enrollments during 2021/22 was offset by growth among UK and non-EU students, illustrating that enrollment is an equilibrium outcome jointly determined by applications, offers, acceptances, and institutional capacity, and that its time series cannot identify demand. Brexit also disrupted established mobility patterns and institutional ties, even far before Brexit Findlay et al. (2006) note that UK students' decreasing mobility to Europe was more than compensated by rising flows to other destinations, universities' reliance on EU faculty (Locke & Marini, 2021), and EU staff's sense of legal and social security (Luthra, 2020).

## **2.2. Previous attempts to capture demand elasticities in higher education**

Causal evaluation of the 2021 reform became possible only once enough post-treatment UCAS observations accumulated. Before that, policy reports relied on forecasting. Conlon et al. (2021),

for instance, estimated error-correction models and used the resulting elasticities to simulate the response to the new regime. Once data through 2022 arrived, the literature moved from ex ante forecasting to ex post evaluation. Neville et al. (2024) document a sharp decline in successful EU applications after 2021, even net of COVID-19. Clifton-Sprigg et al. (2025) separate the components of Brexit and conclude that international tuition fees, not the referendum result or visa requirements, drove the contraction in EU student mobility. Crawford et al. (2025) provide complementary evidence to Conlon et al. (2021) on demand elasticities.

Savoca (1990) adds her estimated application elasticity ( $-0.26$ ) to the elasticity of enrollment given admission implied by Fuller et al. (1982) ( $-0.23$ ). She concludes that, "if we assume that a school's tuition charge and admission policy are set independently," the combined  $-0.49$  means "the true elasticity of demand may be more than double the estimates reported in the literature" (p. 128). In a different context, Dwenger et al. (2012) find that among German high-school graduates, living in a state with tuition fees lowers the probability of applying to a home-state medical university by 2 percentage points (against a 69% baseline), with stronger responses among higher-achieving students.

Savoca's (1990) criticism applies more broadly, however, to models that treat observed enrollment as demand without explicitly modeling the preceding application and admission stages. It is especially relevant to BLP-type models (Berry et al., 1995), building on McFadden (1974), when elasticities are derived from enrollment shares across differentiated alternatives. Although such models are micro-founded and can accommodate preference heterogeneity, fully representing the student demand curve becomes increasingly demanding once applicants face different choice sets, prices, admission probabilities, and financial conditions. This becomes even more difficult when students from different countries face distinct circumstances at the time of application.

Enrollment-based elasticities also face tuition endogeneity. Defined as  $\varepsilon = E/P$  -- a percentage change in enrollment divided by a percentage change in price (Leslie & Brinkman, 1987), a causal interpretation would require tuition to be independent of other demand determinants. Competition can jointly change tuition, quality, differentiation, and sorting, violating this condition (Hoxby, 1997). The price denominator also matters. Campbell and Siegel (1967) report that "the income and price elasticities of demand (...) are +1.20 and  $-0.44$ " (p. 492) for the period 1927–1963. Leslie and Brinkman (1987) observe that "the tuition increase was 148 percent of total direct cost increase between 1927 and 1963," and conclude that "tuition elasticity of  $-0.44$  equates to a total cost elasticity of  $-0.65$ " (p. 185). The same enrollment response thus yields different elasticities depending on whether tuition or total cost is used as the denominator.

Methodologically, Conlon et al. (2021), seeking a specific EU elasticity for the UK, acknowledge that tuition fees are among the variables that are the same across countries, then estimate elasticities from a country panel with no within-year cross-country price variation, so they identify the tuition coefficient only through the common time series. Conlon et al. (2021) nonetheless claim the cointegrating coefficient measures the causal effect over future periods (p. 19) and find a statistically significant long-run elasticity implying that a 10% fee increase for EU full-time first-year undergraduates lowers demand by 3.5% (an elasticity of 0.35, which we return to later). Clifton-Sprigg et al. (2025) divided the ATT implied post-2021 change in applications by a 140% price change to obtain about 0.40. In the Appendix, we show why this understates the true slope of an isoelastic demand curve when the price shock is large.

Our closest predecessor is Sá (2014, 2017, 2019), who uses UCAS applications and difference-in-differences to study the 2012 tuition reform. Reported elasticity magnitudes range from 0.14 to 0.26 in the working paper to a reform-specific 0.110 and a full-period 0.356 in later versions across different working papers. These differences reflect the elasticity formula, fee measure, post-treatment window, sample, and specification, illustrating how sensitive

comparisons are to their construction. Overall, the literature has remained broadly consistent in finding that price elasticities in higher education typically range in absolute magnitude from approximately 0.10 to 0.70-0.80 (Savoca, 1990; Sá, 2014; Crawford et al., 2025; Conlon et al., 2021; McQuone & Simons, 2025) depending on the choice of denominator, fee assumptions, type of price variation, and baseline assumptions. Although most of those papers failed to account for the implicit effects of the income-contingent loans supporting those numbers, we will reconcile our estimates regardless. We retain this range as a later benchmark for the subjective present-value calibrations that follow in the methodology section.

### **2.3. Watch the denominator**

The major problem in the literature, as we understand it so far, is that nominal tuition changes are confused with the change students actually experience when computing elasticity. Sá (2019) on the tuition reforms from 2012 and Clifton-Sprigg et al. (2025) on the actual Brexit shock, and most of the literature, compute price shifts as if students jump from one tuition to another without discounting the fees. This understates the denominator in the elasticity equation (change in demand/change in prices) because students exposed to the pre-Brexit loan regime (or pre 2012 prices) can subjectively discount the loan's present value and experience the old price as much lower than the posted tuition.

To demonstrate similar mechanisms, drawing on 927 estimates from 56 studies, Matousek et al. (2022) report a publication-bias-corrected mean annual discount rate of 33%. Their results also indicate that discounting differs across domains, with individuals displaying greater impatience over health outcomes than over money, while negative framing is associated with greater patience. Li et al. (2022) study hypothetical credit-repayment choices among 2,100 UK adults. Mean annual discount rates are 6–7%, partly because more than half chose immediate repayment and were assigned zero rates. Among participants delaying repayment, estimates are

approximately 9–15%, with some choices implying rates of at least 35%. These heterogeneous estimates motivate similar calibration ranges in the present paper's methodology. Furthermore, evidence from consequential financial decisions points in a similar direction. Warner and Pleeter (2001) examine more than 65,000 military personnel who chose between an immediate lump-sum payment and a future annuity. The break-even discount rates exceeded 17%; most participants selected the lump sum. We therefore calculate implied elasticities across subjective discount rates and assess their consistency with the literature benchmark.

### 3. Data

We use UCAS (2025) applicant counts by domicile for 2016–2025, observing expressed demand before admissions decisions. Country-level totals count each applicant once, avoiding the multiple university choices recorded in application-level data, unlike the approach in Sá (2014, 2017, 2019). We aggregate across sex and gender categories and retain the origin-country-year as the unit of analysis. We leave gender-, age-, and subject-specific responses for future work because disaggregation produces smaller cells and raises concerns about the implicit OLS weighting in unbalanced panels. UCAS (2024) covers 2006–2024; UCAS (2025) begins in 2016. We use the editions separately because definitions may differ. Administrative records can contain changing measures and structured reporting errors (Connelly et al., 2016; Hand, 2018), which remain relevant even with broad coverage. HESA enrollment data cannot replace applicant counts for our expressed-demand outcome because it only reports enrollment numbers.

The unit of observation is the origin-country-year in a panel of both EU countries (treated) and non-EU countries used to build the pseudo-EU (control). The outcome is the log number of applicants from country  $i$  in year  $t$ . To study heterogeneity in the response, we merge

the UCAS data with World Bank macroeconomic indicators, including GDP per capita and labor-market variables. Table 1 reports descriptive statistics.

**Table 1.** Descriptive statistics of the core variable in the analysis for both the treated and control groups. Statistics are computed on the 468 country-year World Bank estimation sample (52 countries over 2016 to 2024). Education spending as a share of GDP is available for 321 of these observations and does not restrict the sample.

| Variable                                             | n    | mean     | sd        | median  | min    | max       |
|------------------------------------------------------|------|----------|-----------|---------|--------|-----------|
| <b>Donors matching sample (applicants &gt; 100)*</b> |      |          |           |         |        |           |
| Applicants                                           | 546  | 1,519.61 | 2,359.97  | 645     | 100    | 26655     |
| <b>Baseline DiD sample**</b>                         |      |          |           |         |        |           |
| Applicants                                           | 520  | 1765.11  | 3903.00   | 667.5   | 30.0   | 36385.00  |
| <b>Synthetic DiD sample**</b>                        |      |          |           |         |        |           |
| Applicants                                           | 1200 | 1095.35  | 2864.01   | 330.0   | 25.00  | 36385.0   |
| <b>World Bank (WDI) regression sample**</b>          |      |          |           |         |        |           |
| Applicants                                           | 468  | 1767.95  | 3745.32   | 715.00  | 35.00  | 33660.00  |
| log_app                                              | 468  | 6.63     | 1.23      | 6.57    | 3.56   | 10.42     |
| EU                                                   | 468  | 0.50     | 0.50      | 0.50    | 0.00   | 1.00      |
| Post                                                 | 468  | 0.44     | 0.50      | 0.00    | 0.00   | 1.00      |
| did                                                  | 468  | 0.22     | 0.42      | 0.00    | 0.00   | 1.00      |
| event_time                                           | 468  | 0.00     | 2.58      | 0.00    | -4.00  | 4.00      |
| regression_weight                                    | 468  | 1.00     | 0.77      | 1.00    | 0.00   | 4.18      |
| GDPpc                                                | 468  | 46395    | 26718     | 43080   | 2543   | 136772    |
| Pop15_64                                             | 468  | 42599339 | 138073628 | 6552302 | 305378 | 988168271 |
| YouthUnempl                                          | 468  | 15.32    | 8.43      | 13.56   | 0.30   | 47.22     |
| EduSpendGDP                                          | 321  | 4.59     | 1.43      | 4.59    | 0.38   | 8.74      |
| c_log_GDPpc                                          | 468  | 0.00     | 0.78      | 0.15    | -2.68  | 1.30      |
| c_log_Pop15_64                                       | 468  | 0.00     | 1.73      | -0.22   | -3.28  | 4.80      |
| c_YouthUnempl                                        | 468  | 0.00     | 8.43      | -1.75   | -15.02 | 31.91     |

*Note:* “\*” denotes the 2024 UCAS data edition, containing a panel spanning 2006 to 2024 (utilized period 2014 to 2020); “\*\*” denotes the 2025 UCAS data edition, containing a panel spanning 2016 to 2025 (utilized period 2016 to 2025)

Country-specific average wages ( $w_j$ ) are taken from Eurostat’s Average full-time adjusted salary per employee indicator (nama\_10\_fte) for 2024 (Eurostat, 2025) and for 2021 in the robustness extension. The indicator reports gross annual wages and salaries in current euros and applies a harmonized full-time adjustment based on the ratio of usual weekly hours worked by full-time employees to those worked by all employees in the EU Labor Force Survey. It is available for 25

of the 26 treated Member States. The Netherlands is omitted from the wage-based calculations because Eurostat does not publish `nama_10_fte` for it and instead refers users to a national register-based measure constructed using a different methodology.

Macroeconomic controls come from the World Bank’s World Development Indicators via the WDI R package (Arel-Bundock, 2022): GDP per capita in PPP constant international dollars (NY.GDP.PCAP.PP.KD), working-age population 15-64 (SP.POP.1564.TO), and youth unemployment 15-24 (SL.UEM.1524.ZS). We harmonized country codes to ISO3 with the `countrycode` package (Arel-Bundock et al., 2018) and merged them with the UCAS panel on a country-year basis. World Bank data run through 2024 (World Bank, 2025). Lebanon was missing GDP per capita and youth unemployment for 2024, so we filled both with the average of the previous two years to keep the panel balanced.

## **4. Empirical strategy**

### **4.1. Constructing the counterfactual European Union**

The control group is built in two stages, with the full derivation in Online Appendix I. First, each origin country’s pre-treatment path is converted into a standardized log-applicant trajectory for 2014 to 2020. We start in 2014 because Croatia joined the EU (becoming treated) in 2013, so this window avoids contaminating the pre-treatment trajectories, for the donor pool we keep only countries with at least 100 applicants per year. For every treated EU country, the three closest non-EU donors are selected by Euclidean distance between standardized pre-treatment paths, so donor choice depends on trajectory shape, not market size.

Second, synthetic-control weights are estimated separately for each treated country over the pre-treatment period, then collapsed into a single global donor-weight vector for the saturated DiD regressions. Treated observations receive unit weight, while donor weights are scaled so

aggregate control mass equals aggregate treated mass. This keeps the outcome in logged levels while using the donor pool only to build a comparable weighted counterfactual. Table 2 lists the counterfactual European Union and its regression weights.

**Table 2.** Donor countries used to construct the synthetic counterfactual European Union, with global and regression weights.

| Donor country                | Global weight | Global weight $\times$ 26 |
|------------------------------|---------------|---------------------------|
| Korea, Republic of           | 0.16          | 4.18                      |
| Trinidad and Tobago          | 0.12          | 3.08                      |
| Russian Federation           | 0.11          | 2.80                      |
| Malaysia                     | 0.10          | 2.53                      |
| Mauritius                    | 0.08          | 2.07                      |
| Egypt                        | 0.07          | 1.85                      |
| New Zealand                  | 0.04          | 1.11                      |
| United States of America     | 0.04          | 1.10                      |
| Qatar                        | 0.04          | 1.00                      |
| Israel                       | 0.04          | 1.00                      |
| Zimbabwe                     | 0.04          | 0.96                      |
| United Arab Emirates         | 0.04          | 0.92                      |
| Indonesia                    | 0.03          | 0.80                      |
| Mexico                       | 0.02          | 0.62                      |
| Australia                    | 0.02          | 0.44                      |
| Tanzania, United Republic of | 0.02          | 0.43                      |
| China                        | 0.01          | 0.33                      |
| Norway                       | 0.01          | 0.33                      |
| Lebanon                      | 0.01          | 0.27                      |
| Ireland                      | 0.01          | 0.20                      |
| Switzerland                  | <0.00         | <0.01                     |
| Turkey                       | <0.00         | <0.01                     |
| Botswana                     | <0.00         | <0.01                     |
| Iran, Islamic Republic of    | <0.00         | <0.01                     |
| Uganda                       | <0.00         | <0.01                     |
| Brunei Darussalam            | <0.00         | <0.01                     |

**Note:** Estimates based only on donor countries with recorded number of applicants of at least 100.

An alternative is to estimate each treated country separately using the synthetic difference-in-differences estimator of Arkhangelsky et al. (2021). We use a common weighting scheme instead, because the country-specific coefficients later enter a second-stage regression on income and on  $k_j$ , and per-country donor selection would define each coefficient against a different

counterfactual. That difference would not be random, since donor weights are chosen on pre-treatment fit, pre-treatment applicant counts are income-related, and wealthier EU countries would draw on wealthier donors, so the induced error in the second-stage dependent variable would correlate with the regressor of interest.

## **4.2. Identification strategy and parallel trends**

The closest design to ours, Sá (2014, 2017, 2019) exploits within-country, across-time variation in fees with binary treatment and 2 country clusters. We instead exploit cross-country, across-time variation, where the shock  $k_j$  itself differs across countries because the present value of the foregone loan differs with the average wage. The identifying variation thus has two dimensions at once, whether you are treated (EU vs non-EU) and how intensely (which EU country you are from) you are treated. This corrects the mentioned limitation in Sá (2014, 2017, 2019) and extends the analysis to international student higher education demand.

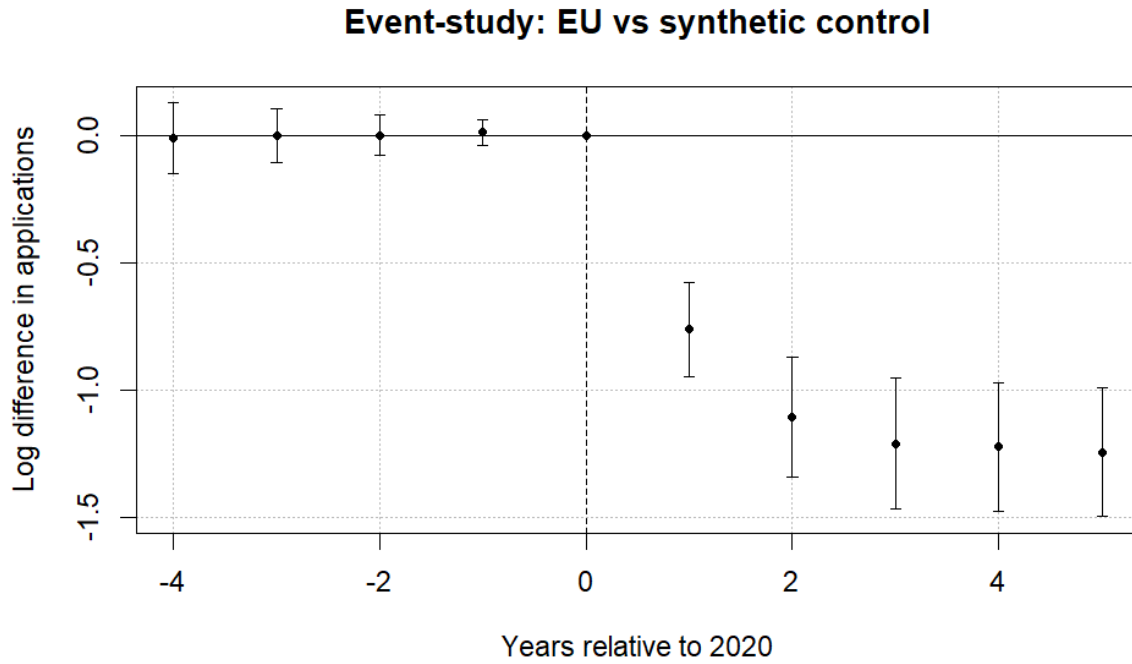
The baseline regression period used in the estimation panel starts in 2016 and therefore omits Bulgaria and Romania's accession in 2007, Croatia's entry in 2013, the cessation of the transitional labor-market restrictions in 2014, and the increase in the fee cap in England in 2012/13. However, this does not mean that all earlier events are excluded from the pre-treatment period; the 2016 referendum and the £250 rise in tuition fees in 2017 are still included. We thus examine whether these events caused differential EU-control movements using the event-study leads and the leads-only specification. The donor-selection step described in section 4.1 makes use of the longer 2014–2020 window in order to obtain a more robust donor-matching sample.

Following these design choices, we estimate a weighted dynamic difference-in-differences specification. This traces the evolution of applicant counts around the Brexit tuition reform:

$$\log A_{it} = \alpha_i + \gamma_t + \sum_{\ell \neq 0} \delta_\ell (EU_i \times 1\{t - 2020 = \ell\}) + \varepsilon_{it} \quad (1)$$

The coefficients  $\delta_\ell$  measure year-by-year deviations in log applicant counts for EU-origin countries (except Ireland) relative to the omitted pre-reform year ( $\ell = 0$ , i.e. 2020), again estimated with the weights from the previous subsection.

**Figure 1.** Event-study estimates comparing EU applicants to the synthetic control group.

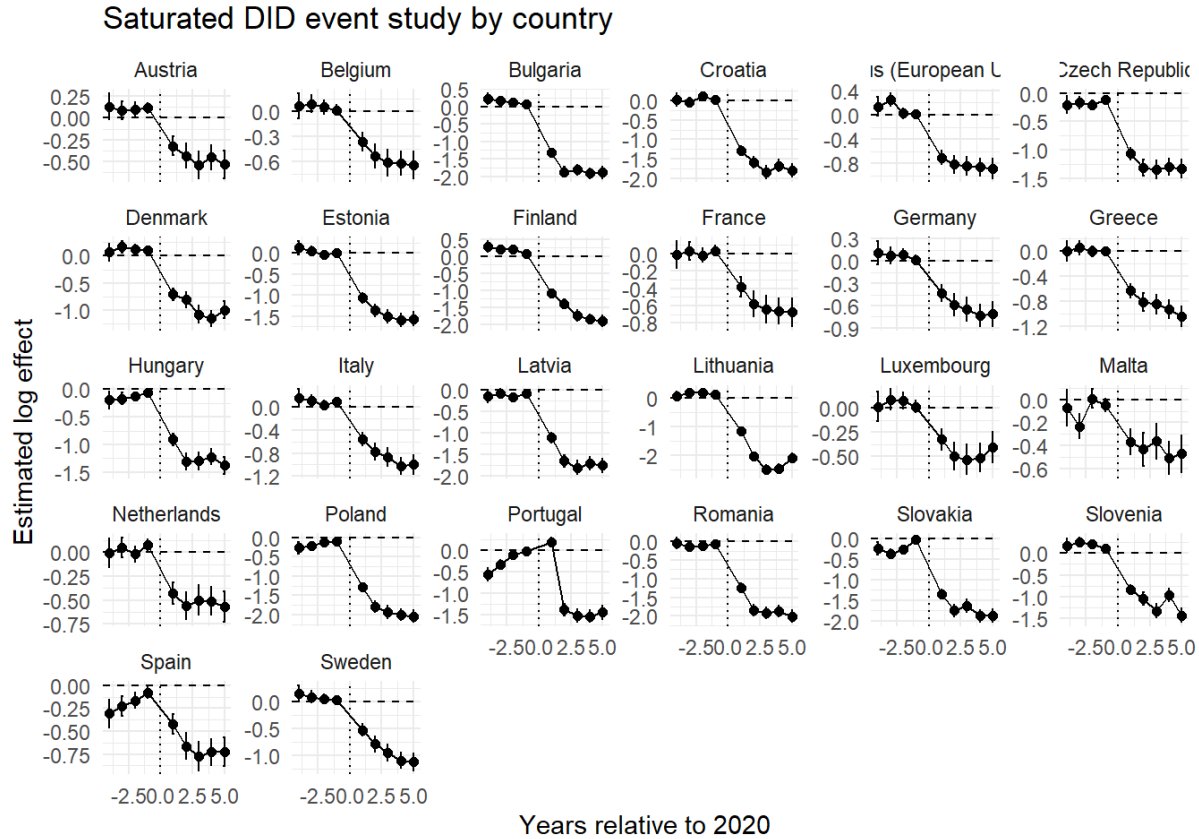


**Note:** The figure plots coefficients from a difference-in-differences event-study comparing EU applicants to a synthetic control group of non-EU countries. The horizontal axis shows years relative to 2020, the last pre-treatment year. Points represent estimated differences in log applicant counts and bars denote 95% confidence intervals clustered at the country level. The dashed vertical line marks the final pre-treatment year. Pre-treatment coefficients are small and close to zero, while a sharp decline in EU applicant numbers emerges beginning in 2021 after the tuition reform took effect. A joint Wald test cannot reject the null that the pre-treatment coefficients are jointly zero ( $p = 0.96$ ), providing no strong evidence of differential pre-trends.

Figure 1 reports the event-study coefficients from equation (1). The pre-treatment leads are small and a joint Wald test fails to reject that they are jointly zero ( $p = .96$ ), providing no evidence of differential pre-trends in this short window. The post-treatment decline persists through 2024 and 2025, which is inconsistent with a purely transitory pandemic explanation.

A leads-only specification likewise finds no evidence of anticipatory adjustment before the reform. The early leads show no systematic pattern and are jointly statistically indistinguishable from zero ( $p = .96$ ). Allowing country-specific dynamic effects yields similar patterns, with post-treatment coefficients consistently negative across EU countries and pre-treatment estimates near zero (Figure 2). The one exception is Portugal, whose applicant count did not immediately fall in 2021, which traces to a UCAS reporting issue where some Recorded Prior Acceptances were mistakenly submitted as Portugal-domiciled in the 2021 cycle, so the true count is lower than reported (UCAS, 2022). Figure 2 also provides an important correction to Clifton-Sprigg et al. (2025), as they used UCAS data capped at 2022, when applicants were still adjusting to the new regime. The stable level of the collapse is revealed after 2022 and is much larger than the original estimates.

301

302 **Figure 2.** Saturated DiD event-study estimates by country.

303

304 *Note:* Each panel reports country-specific dynamic treatment effects from the saturated  
 305 difference-in-differences specification comparing EU countries with the pooled control group.  
 306 Coefficients represent differences in log applicant counts relative to the baseline year (2020).

307

### 308 4.3. Baseline elasticity estimates and heterogeneous price shocks

309 Let  $\mathcal{C}$  denote the set of origin countries, partitioned into the treated EU markets  $\Omega \subset \mathcal{C}$  (with  
 310 Ireland removed), with  $J = |\Omega|$ , and the candidate donor pool  $\mathcal{D} = \mathcal{C} \setminus \Omega$  of non-EU markets. The  
 311 counterfactual European Union gives a synthetic untreated group, letting us estimate the  
 312 weighted saturated two-way DiD model:

$$\log(A_{it}) = \alpha_i + \gamma_t + \sum_{j \in \Omega} \beta_j (\mathbf{1}\{i = j\} \times Post_t) + \rho' \mathbf{X} + \varepsilon_{it} \quad (2)$$

where  $A_{it}$  denotes the applicant count from origin country  $i \in \mathcal{C}$  in year  $t \in [2016, 2025]$ ,  $\mathbf{1}\{i = j\}$  indicates EU member states, and  $Post_t$  marks the new tuition regime in 2021. Importantly, as long as the two groups remain under different but unchanged tuition fee regimes, their relative applicant-count trajectories should remain stable. When the treated group moves from the UK home-fee regime into the international fee regime already faced by the control group,  $\beta_j$  will capture the resulting change in the treated country-specific applicant-count trajectory relative to the control group. This relative change identifies the reduction in applicant numbers when the treated group ceased to benefit from UK home-fee status.

To compare the initial and later post-reform periods, we split the post-treatment window into 2021–2023 and 2024–2025. The specification becomes:

$$\log(A_{it}) = \alpha_i + \gamma_t + \sum_{j \in \Omega} (\beta_j^I (\mathbf{1}\{i = j\} \times I_t) + \sum_{j \in \Omega} \beta_j^L (\mathbf{1}\{i = j\} \times L_t) + \rho' \mathbf{X} + \varepsilon_{it} \quad (3)$$

Here  $\beta_j^I$  is the country-specific log effect for 2021–2023 and  $\beta_j^L$  is the corresponding effect for 2024–2025. Even though other time-varying shocks may differ across the two windows, the difference

$$\Delta_j = \beta_j^L - \beta_j^I$$

can be interpreted as the change in the residual application collapse after the early contemporaneous shocks had subsided, revealing whether the post-reform decline weakened, persisted, or intensified beyond the initial adjustment period (more information in the appendix).

Under a constant-elasticity demand curve, each country-specific log effect maps into a signed price elasticity according to (see the appendix):

$$\widehat{\eta}_j = \frac{\hat{\beta}_j}{k_j(\delta)}, \quad \overline{\eta}_j(\delta) = \frac{1}{\ln P_1 - \ln P_0} \int_{\ln P_0}^{\ln P_1} \eta_j(P) d \ln P$$

Here,  $\hat{\beta}_j$  is the estimated country-specific log demand effect from the model in equation (2), which contains all additional controls, and  $k_j(\delta)$  is the corresponding log price change.

335 Under the maintained price-shock assumptions, their ratio  $(\widehat{\eta}_j)$  estimates the average price  
 336 elasticity over the corresponding log-price interval. The second expression shows that the  
 337 population estimand  $(\overline{\eta}_j(\delta))$  is the average of the price elasticities  $(\eta_j(P))$  along that interval.  
 338 No constancy assumption is required. If the point elasticity becomes more negative as price rises,  
 339 the elasticities from the higher-price portion of the demand curve enter the interval average  
 340 through the integral. If elasticity is constant, the average reduces to the familiar expression  
 341  $(\widehat{\eta}_j = \frac{\widehat{\beta}_j}{k_j(\delta)})$ .

342 To elaborate on this point further, we first retain the conventional nominal benchmark, in  
 343 which the annual price rises from £9,250 to £22,200 and the common log change is 0.875 (as in  
 344 Clifton-Sprigg et al. (2025)). The loan-adjusted specification then allows the denominator to  
 345 vary by origin and by the “subjective” discount rate  $\delta$ . Both figures are annual prices, and the  
 346 initial balance is one year of pre-Brexit tuition,  $L = £9,250$ . The interest rate applied to the  
 347 outstanding balance is

$$348 \quad i_{jt} = \overline{RPI} + 0.03 \times \begin{cases} 0, & Y_{jt} \leq \tau_j^L \\ \frac{Y_{jt} - \tau_j^L}{\tau_j^U - \tau_j^L}, & \tau_j^L < Y_{jt} < \tau_j^U \\ 1, & Y_{jt} > \tau_j^U \end{cases}$$

349 Under Plan 2, borrowers repay 9% of annual earnings above the country-specific lower overseas  
 350 threshold. The upper threshold determines the income-related interest loading, which rises  
 351 linearly from RPI to RPI plus 3 percentage points (UK Government, n.d.-a, n.d.-c; Student Loans  
 352 Company, 2021). For the baseline  $\overline{RPI}$  we use the weighted average of the RPI values from  
 353 April 2025 (Student Loans Company, 2024) to March 2026 (Student Loans Company, 2025) (see  
 354 appendix). We fix the 2025/26 table’s (Student Loans Company, 2026) lower  $(\tau_j^L)$  and upper  
 355  $(\tau_j^U)$  thresholds throughout the simulation, hold  $\overline{RPI}$  at 3.66%,  $Y_{jt}$  is assumed to have nominal

wage growth of  $g = 2\%$  per year, and the loan is written off after 30 years (UK Government, n.d.-b), the 2024 wage value from Eurostat is extrapolated for 2025 with a growth of 2% ( $Y_{it} = w_j^{GBP}(1 + g)^{t-1}$ ). With these assumptions, the initial balance and interest recursions are

$$B_{j0} = L, \quad \tilde{B}_{jt} = B_{j,t-1}(1 + i_{jt})$$

The annual payment equals 9% of earnings above the lower threshold, capped at the balance after interest. The interest recursion begins in the first simulated post-graduation year, even if the borrower earns below the threshold and makes no repayment. The ending balance then becomes the opening balance for the following year.

$$p_{jt} = \min\{0.09 \cdot \max[0, w_j^{GBP}(1 + g)^{t-1} - \tau_j^L], \tilde{B}_{jt}\}, \quad B_{jt} = \tilde{B}_{jt} - p_{jt}$$

Discounting the resulting contractual payment stream at the annual subjective discount rate ( $\delta$ ) gives:

$$PV_{\text{subjective},j}(\delta) = \sum_{t=1}^{30} \frac{p_{jt}}{(1 + \delta)^t}$$

The country-specific experienced price shock is the log difference between the average post-Brexit international fee and the subjective present value of the former loan.

$$k_j(\delta) = \log(22\,200) - \log(PV_{\text{subjective},j}(\delta))$$

Hence, the pre-Brexit price is the subjective present value of repayments generated by the origin-country wage path. We use the 2024 average annual salary from Eurostat because a student entering in 2021 completes a standard three-year degree in 2024, then convert euro wages into pounds using the 2025/26 rate of 0.85 (posted by the SLC itself). The post-Brexit fee is treated as payable during study once UK government loan access disappears.

The subjective discount rate is a calibration parameter and cannot be independently identified because post-hoc data are unavailable. To that end, we examine annual rates from 3% to 35% and compare the implied elasticities with prior estimates. The range draws on

heterogeneous evidence from hypothetical repayment choices, experimental studies, and consequential financial decisions (Li et al., 2022; Matousek et al., 2022; Warner & Pleeter, 2001). This financing mechanism also aligns with Solís (2017), who shows that credit access affects enrollment.

Furthermore, we examine whether the loan adjustment changes the association between the country-specific elasticities and national income. At each subjective discount rate, we estimate

$$\widehat{\eta}_j = \frac{\widehat{\beta}_j}{k_j(\delta)} = \alpha + \gamma_\delta \log(GDPpc_j) + X'\rho + u \quad (4)$$

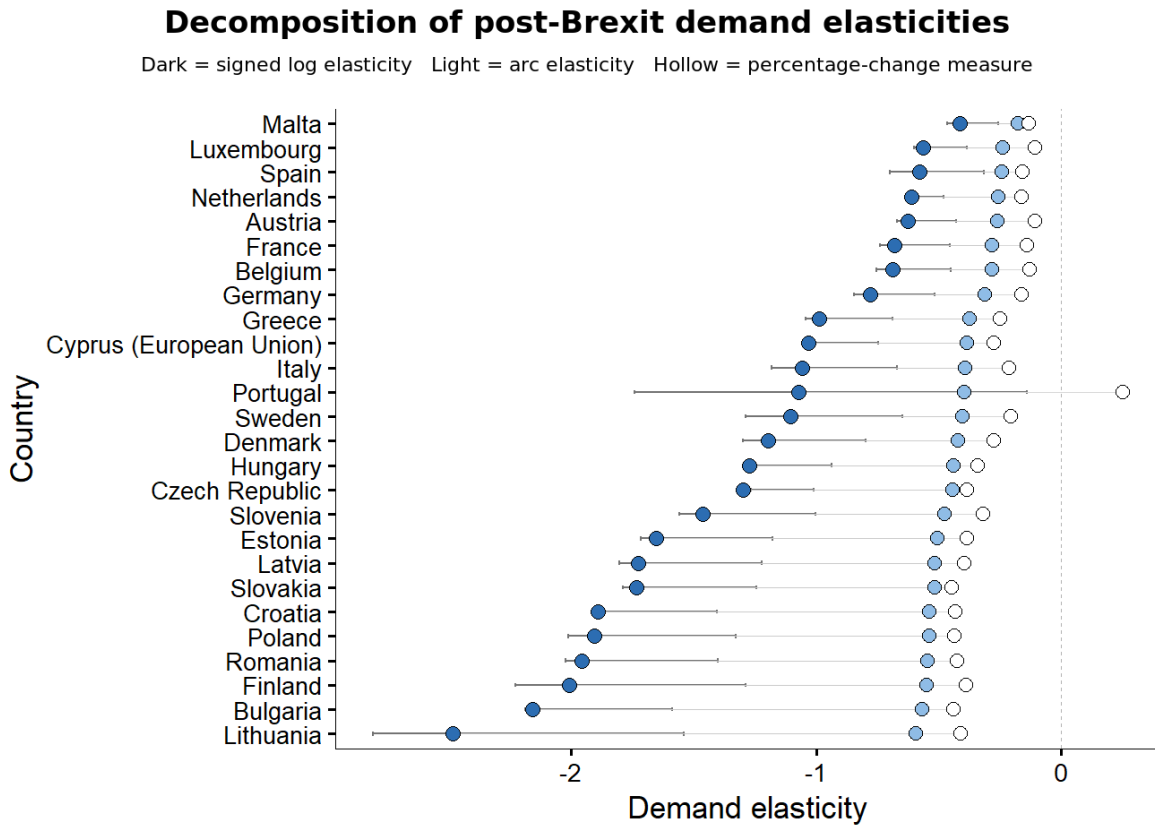
We report the elasticities at income neutrality, which is defined as the subjective discount rate  $\delta^* = \arg \min_{\delta \in D} |\widehat{\gamma}|$  over the calibration grid  $D$ . This is the diagnostic point at which the loan-adjusted elasticities no longer vary systematically with national income. It is also informative because it captures the redistributive duality of income-contingent financing. This duality is, on the one hand, revealed by the fact that expected wages relative to the country-specific lower thresholds determine whether and when repayments begin. They compress cross-country income differences through threshold protection. On the other hand, the timing of the resulting payment stream determines how strongly subjective discounting reduces its present value, providing a second form of compression through delayed repayment. An income gradient in  $\eta_j(\delta)$  may reflect an incomplete adjustment for treatment intensity rather than systematic preference heterogeneity. Income neutrality is not sufficient to identify a uniquely correct discount rate. However, comparing neutral rates across alternative wage-threshold calibrations reveals which side of this duality dominates the loan income-equalizing nature.

## 5. Results

### 5.1. Estimated baseline elasticities

Figure 3 reports country-specific elasticities from the difference-in-differences estimates without any time-varying controls. The figure translates the treatment effects into implied demand responses to the Brexit shock. The source regression tables are provided in the appendix.

**Figure 3.** *Decomposition of the post-Brexit demand elasticities by treated country.*



**Note:** Signed structural log elasticities divide each country-specific effect  $\beta_j$  by the common log price change of 0.875. Arc elasticities use  $[\exp(\beta_j) - 1]/1.4$ , the mechanical percentage-change measure uses the observed change in applications divided by 1.4. R uses natural logarithms. The saturated estimates use Driscoll–Kraay standard errors with one lag. See the online appendix for detailed regressions.

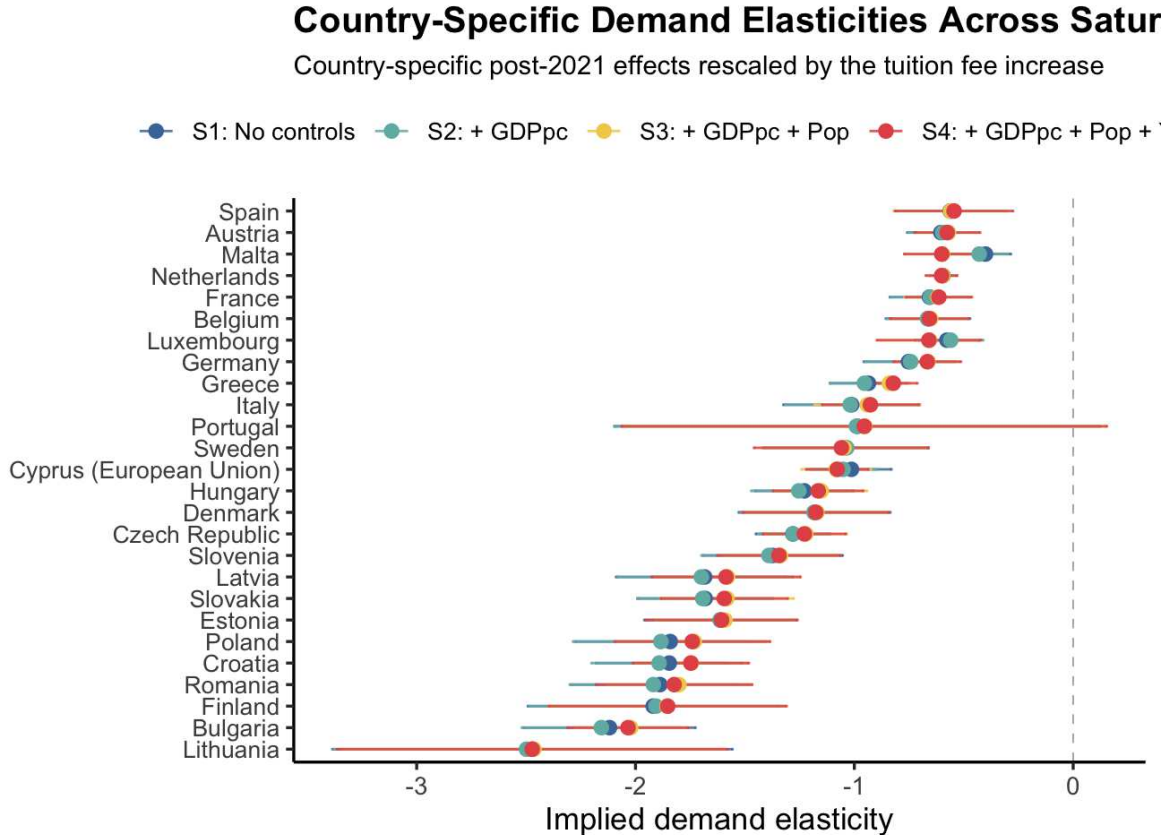
The estimates show a large and widespread post-reform contraction. Country-specific effects correspond to applicant reductions ranging from about 30% for Malta to about 89% for Lithuania and 85% for Bulgaria, relative to the control group. The average effect implies a decline of approximately 67%. The implied signed elasticities are negative in every treated country, although their magnitudes vary substantially. Under the common price-shock assumption, lower-income origins such as Bulgaria, Romania, Lithuania, Croatia, and Latvia

appear more responsive than Malta, Spain, Luxembourg, the Netherlands, and Austria, with many estimated elasticities falling between -2.5 and -1. Under the nominal benchmark, every EU country moves across the same price interval from £9,250 to £22,200. Consequently, a common tendency for demand to become more elastic as prices rise cannot mechanically generate this cross-country gradient. Such curvature would affect every treated country over the same price interval and therefore cannot explain the systematically larger application declines observed among lower-income origins. The country-specific effects are also positively associated with log GDP per capita, indicating smaller declines among richer origins. Correlations are approximately 0.51–0.56 across fixed-effects specifications and about 0.58 with the Netherlands included (Figure 5, Panel A). The association remains after adding observed macroeconomic controls, motivating an examination of heterogeneous effective price changes.

Adding macroeconomic controls leaves country rankings and effect magnitudes broadly stable (Figure 4). This persistence motivates examining the denominator, since a common

nominal tuition increase can imply different effective price shocks when the value of the lost loan varies across origins.

**Figure 4.** Saturated DiD specifications with macroeconomic controls added. Namely GDP per capita (PPP, constant prices), population and the percentage of youth unemployment as separate models.



**Note:** All saturated models use the demeaned version of the free covariates – namely, the GDP per capita, the population and the youth unemployment. Used are Driscoll-Kraay ( $L=1$ ) standard errors consistently and across all the presented models. See the online appendix for detailed regressions.

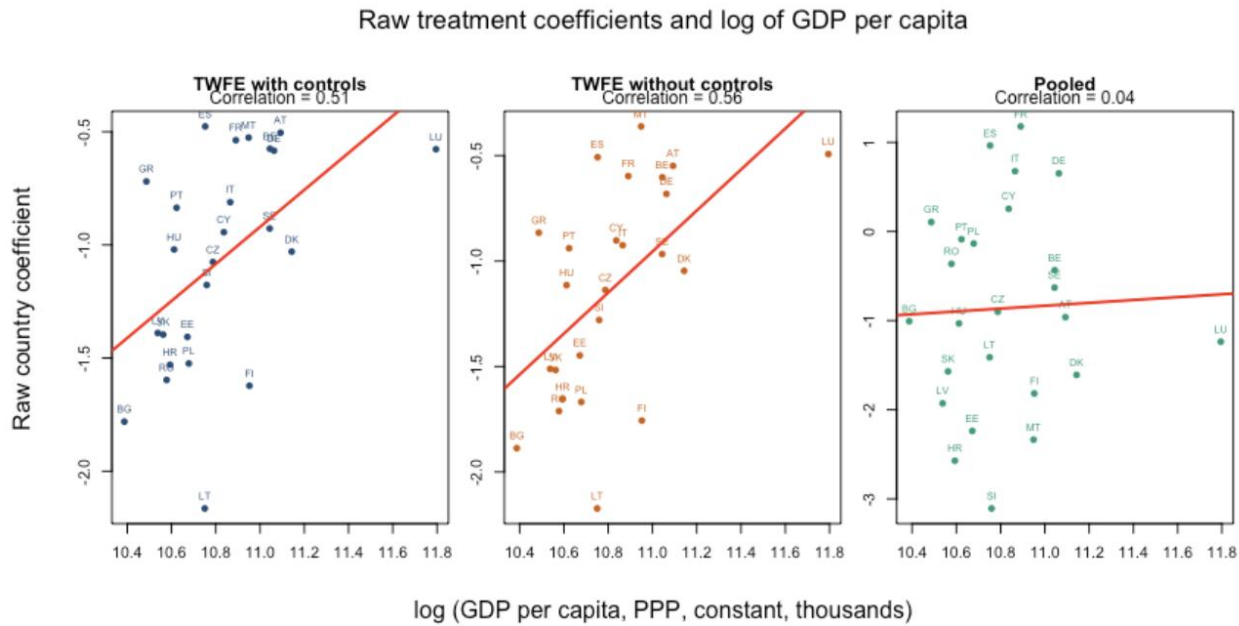
## 5.2. Loan-corrected elasticities and income neutrality

We rescale the controlled DiD effects from Figure 4, Model S4, using origin-specific loan present values. Lower expected earnings can postpone or reduce repayments, lowering the experienced pre-Brexit price and increasing the effective price shock. Dividing larger application

449 declines by these larger denominators reduces the income gradient and cross-country dispersion.  
450 Panel B shows the combined effects of threshold protection and subjective discounting.  
451 Panel B replaces the common nominal price change with the country-specific effective  
452 change  $k_j(\delta)$  and plots the resulting loan-adjusted elasticity against GDP per capita at three  
453 subjective discount rates. The middle panel marks the discount rate of 14.9 percent, at which the  
454 estimated income coefficient  $\gamma_\delta$  is exactly zero (zero up to the decimal point of precision). The  
455 gradient becomes statistically insignificant earlier, at 6.1 percent, so the plausible range of  
456 student discount rates is consistent with no systematic link between national income and price  
457 responsiveness.

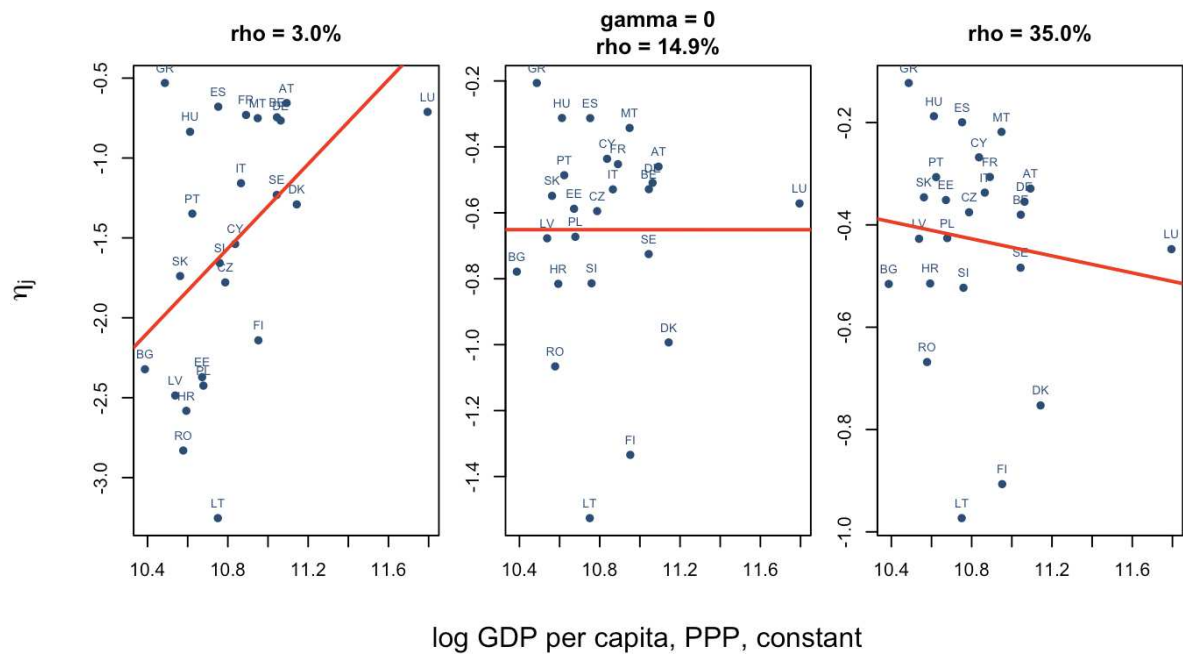
**Figure 5.** The cross-country income gradient in demand responsiveness, before and after loan adjustment.

*Panel A. Raw treatment effects against GDP per capita*



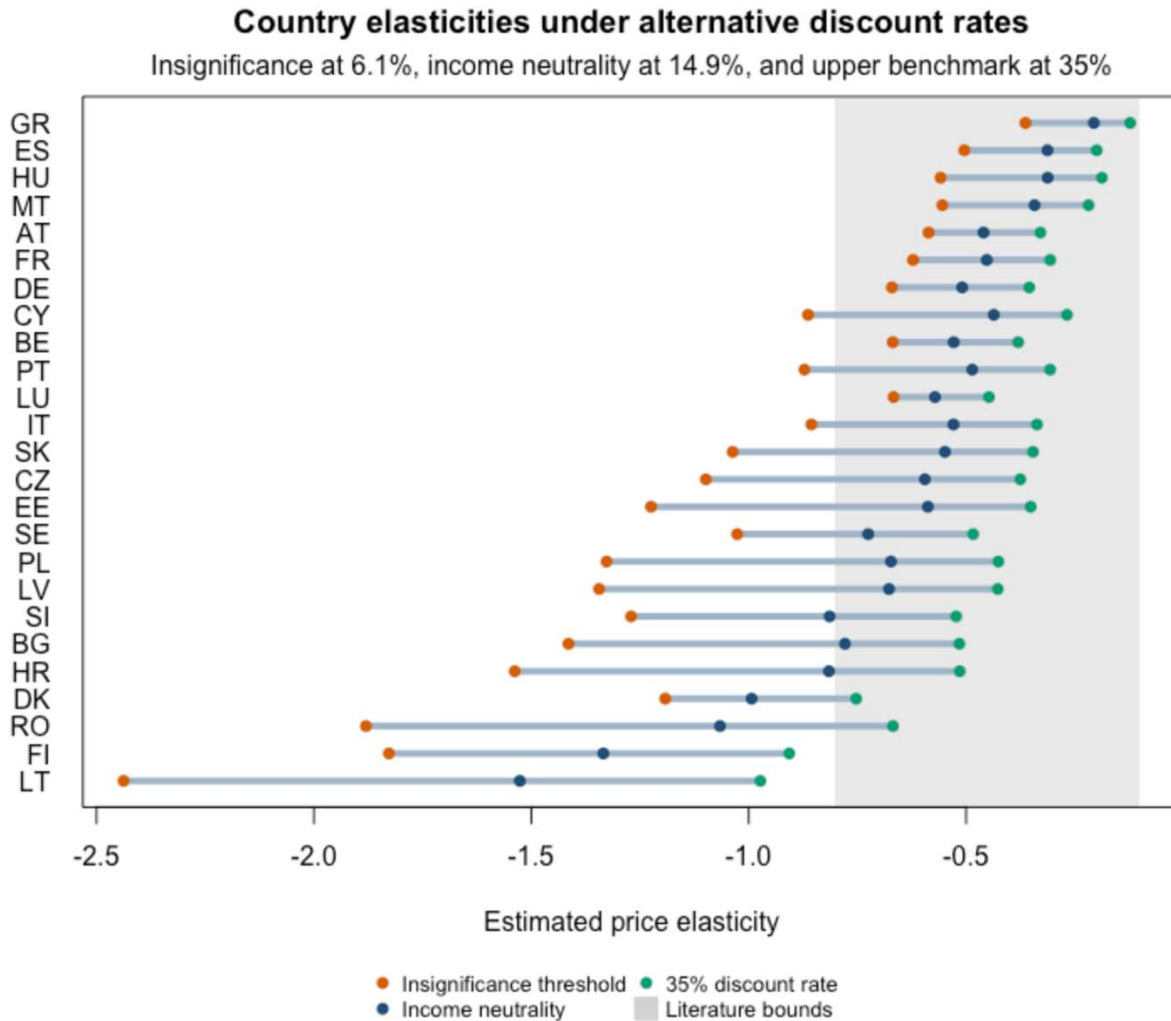
*Panel B. Loan-adjusted elasticities against GDP per capita at three discount rates*

Country elasticities at 3%, income neutrality, and 35%



464

465 *Panel C. Country-specific loan-adjusted elasticities under alternative discount rates*



466 *Note:* Panel A plots the raw country-specific treatment effect against log of GDP per capita  
 467 (PPP, constant for 2022), when  $\hat{\beta}_j$  is estimated by pooled and with country and year fixed  
 468 effects. Panel B plots the loan-adjusted elasticity  $\frac{\hat{\beta}_j}{k_j(\delta)}$  against the log of GDP per capita at  
 469 subjective discount rates of 6.1, 14.9 and 35 percent, where 14.9 percent is the rate at which the  
 470 income coefficient  $\gamma_\delta$  equals zero. Panel C reports the country-specific loan-adjusted elasticities  
 471 under the insignificance threshold (6.1 percent), income neutrality (14.9 percent) and the upper  
 472 benchmark (35 percent), with the range of prior literature estimates shaded. Details in the online  
 473 appendix.  
 474

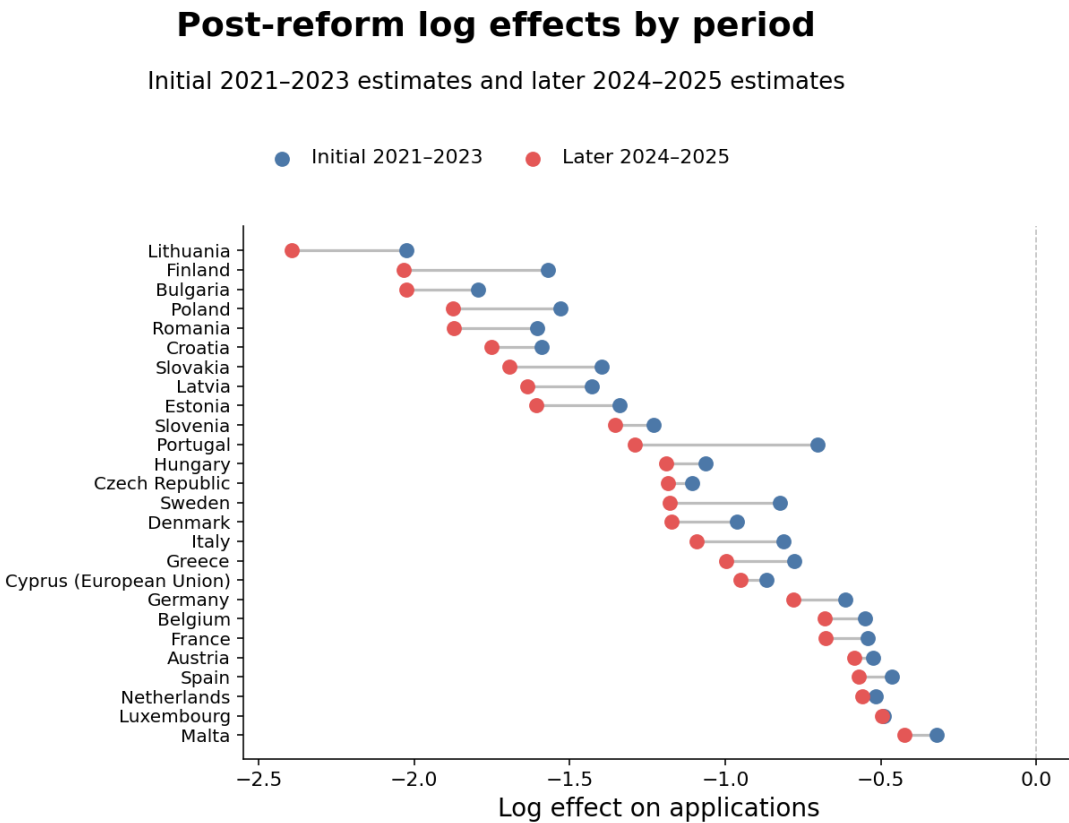
475 Finally, panel C shows the same adjustment at the country level. Under the income-  
 476 neutral calibration and the higher benchmark rate, the loan-adjusted elasticities for most origins  
 477 fall inside the range reported in the earlier literature (shaded in the panel). This means that the  
 478 reconciliation does not depend on implausible elasticities. It only requires valuing the pre-Brexit

loan from each origin's perspective. Under the baseline calibration, 23 of 25 countries begin above their lower repayment thresholds, limiting initial protection and helping explain the relatively high income-neutral discount rate. Section 6.3 compares this setting with 2021 wages, thresholds, and exchange rates to examine how wage-threshold convergence changes repayment exposure.

### 5.3. Descriptive initial and later post-reform effects

Figure 6 compares country-specific log effects in the initial 2021–2023 window with those in 2024–2025. The differences between the dots describe how the estimated post-reform association changed across the two periods (detailed tables in the appendix).

**Figure 6.** Initial and later post-reform log effects on the same measurement axis.



**Note:** The figure reports country-specific log effects, not elasticities. The initial period is 2021–2023 and the later period is 2024–2025. Both sets of estimates use Driscoll–Kraay standard

errors with one lag. The period split is descriptive and does not separately identify a COVID effect.

For nearly all EU-origin countries, the 2024–2025 estimates remain strongly negative and are often more negative than the 2021–2023 estimates, with  $\Delta_j < 0$ . This pattern suggests that the initial reduction was sustained or deepened (in contrast to the 2012 recovery in Sá (2014, 2017, 2019). It appears that successive applicant cohorts adjusted to the higher price, updated their expectations about studying in the United Kingdom, and redirected applications toward alternative destinations. This persistence contradicts fully transitory disruption such as COVID or other contemporaneous changes that occurred immediately after the pandemic's onset in 2020.

The initial estimates range from  $-0.321$  for Malta to  $-2.026$  for Lithuania; the corresponding 2024–2025 estimates are  $-0.423$  and  $-2.394$ . Excluding Portugal, whose 2021 count has a documented reporting issue, the later coefficients are generally larger in absolute value. These patterns support persistence at the country level, again contradicting a COVID-driven or mobility-attraction story, implying that UK higher education grew steadily less popular among EU students until the system reached a stable equilibrium.

#### 5.4. The ATT and wealth heterogeneity

**Table 3.** Average treatment effect and macroeconomic interactions with country and year fixed effects and Driscoll–Kraay standard errors ( $L = 1$ )

| Variable                  | M1                   | M2                   | M4                  | M5                  | M6                   |
|---------------------------|----------------------|----------------------|---------------------|---------------------|----------------------|
| did                       | -1.109***<br>(0.097) | -1.049***<br>(0.109) | -0.980***<br>(0.1)  | -0.960***<br>(0.09) | -1.210***<br>(0.118) |
| c_log_GDPpc               |                      | -0.727*<br>(0.254)   | -0.459<br>(0.257)   | -0.698*<br>(0.28)   | -0.004<br>(0.294)    |
| c_log_Pop15_64            |                      |                      | 3.075***<br>(0.456) | 3.212***<br>(0.515) | 2.629***<br>(0.458)  |
| c_YouthUnempl             |                      |                      |                     | -0.014*<br>(0.004)  | -0.008+<br>(0.004)   |
| 9did $\times$ c_log_GDPpc |                      |                      |                     |                     | 0.808***             |

|                                |                      |            |             |               |                     |
|--------------------------------|----------------------|------------|-------------|---------------|---------------------|
|                                |                      |            |             |               | (0.135)             |
| did ×<br>c_log_Pop15_64        |                      |            |             |               | 0.100***<br>(0.013) |
| did ×<br>c_YouthUnempl         |                      |            |             |               | 0.011<br>(0.006)    |
| Num. Obs.                      | 520                  | 468        | 468         | 468           | 468                 |
| R <sup>2</sup>                 | 0.962                | 0.963      | 0.968       | 0.969         | 0.974               |
| Adjusted R <sup>2</sup>        | 0.957                | 0.958      | 0.964       | 0.964         | 0.97                |
| Within R <sup>2</sup>          | 0.585                | 0.586      | 0.644       | 0.653         | 0.708               |
| Adjusted within R <sup>2</sup> | 0.584                | 0.584      | 0.641       | 0.649         | 0.703               |
| AIC                            | 167.5                | 124.4      | 53.4        | 57.3          | -18.4               |
| BIC                            | 431.3                | 381.6      | 314.8       | 322.8         | 259.5               |
| RMSE                           | 0.25                 | 0.24       | 0.22        | 0.22          | 0.21                |
| Standard errors                | Driscoll-Kraay (L=1) |            |             |               |                     |
| Country fixed effects          | X                    | X          | X           | X             | X                   |
| Year fixed effects             | X                    | X          | X           | X             | X                   |
| Significance                   | =+ p < 0.1           | * p < 0.05 | ** p < 0.01 | *** p < 0.001 |                     |

*Note: Models I–V use two-way fixed effects. Model I uses 520 observations for 52 countries in 2016–2025. Models II–V use 468 observations in 2016–2024 because the World Bank controls are capped at 2024. GDP per capita is NY.GDP.PCAP.PP.KD, purchasing-power-parity GDP per capita in constant 2021 international dollars. Logged GDP and population are demeaned over the estimation sample before interactions are formed. All models use Driscoll–Kraay standard errors with one lag. Significance codes are +  $p < .10$ , \*  $p < .05$ , \*\*  $p < .01$ , and \*\*\*  $p < .001$*

Following an extensive pooled DiD specification (table 3), the uncontrolled ATT is  $-1.109$ , equivalent to  $\exp(-1.109) - 1 = -67.0\%$ . Interestingly, dividing 67% by 140% produces an elasticity of about 0.48, which is very similar to Clifton-Sprigg et al. (2025) as well as Sá (2019). When demand is isoelastic, the actual shift in the denominator cannot be a simple percentage change as we explained in the appendix section.

To reconcile the observed ATT with estimates in the literature, consider Conlon et al.’s (2021) long-run estimate of  $-0.354$ . Applied uniformly to the 140% gross fee increase, this estimate implies a price-induced log quantity change of  $-0.354 \times \ln(2.4) = -0.310$ , equivalent to  $\exp(-0.310) - 1 = -26.7\%$ . The remaining  $-0.799$  log points would consequently be attributed to

non-price channels. If price sensitivity increases in absolute magnitude as tuition rises, the earlier estimate may characterize a lower-price or narrower segment of the demand curve and cannot be extrapolated mechanically across the much larger Brexit shock. On a demand curve that is linear in levels, for example, point elasticity becomes increasingly negative as price rises and quantity falls. In that case, the price channel would account for more than 0.310 log points of the observed decline, while the residual attributed to non-price channels would correspondingly shrink.

Since under linear attribution applicants may become more price-sensitive as tuition rises<sup>1</sup>, it follows that the same percentage increase in fees would produce a larger percentage decline in applicants at higher fee levels. Price alone could then generate the observed contraction with a smaller fee increase than the 2,194% implied by assuming that elasticity remains fixed at  $-0.354$ . The experience of the 2012 reform, however, provides little evidence that higher posted tuition by itself moved demand into a markedly more elastic region. Another possible criticism is that the collapse in applicants was about more than price, with the ATT aggregating multiple collective forces that caused the collapse in applicants. Visa requirements, relocation costs, uncertainty surrounding the change in Home-fee status, and other frictions may all have contributed. Accepting this criticism, however, would mean accepting that these non-price forces, or another persistent and unobserved exogenous force distinct from COVID-19 and other temporary post-2021 shocks, were responsible for at least 70% of the estimated ATT.

To illustrate the previous point, the present value of expected loan repayments for Bulgaria, for example, was £10,310.39 at a 3% discount rate, implying only a 77% log increase in the effective tuition price after 2021. In 2007, when the country joined the EU, the total number of Bulgarian applicants to the UK system increased by 211% (from 180 in 2006 to 560 in 2007). Using reported international and home fees gives an implied log elasticity near  $-1$  (Reddin, 2006). By contrast, the 2012 reform preserved loan access and produced a modest, temporary decline in applications (UCAS, 2014; Murphy et al., 2019). The year 2012 was, in fact, the only time applicants declined for Bulgaria after its 2007 accession, and that decline was most plausibly

---

<sup>1</sup> In a demand curve that is linear in levels and given by  $Q(P) = a - bP$ , elasticity is  $\eta(P) = \frac{-bP}{a-bP}$ , as  $P \rightarrow \left(\frac{a}{b}\right)^+$   $\eta(P) \rightarrow -\infty$ , when  $P \rightarrow 0$ ,  $\eta(P) \rightarrow 0$ . Therefore, it is possible that if the elasticity is estimated on an inelastic portion of a hypothetical demand curve the actual reported number to be smaller.

linked to the UK nominal tuition shock in 2012, when the fee cap rose from about £3,000 to £9,000. For the same tuition hikes of more than 150%, UCAS estimated that young people in general were only about 5% less likely to apply than expected.<sup>2</sup> Application rates returned to their long-term trend in 2013 and 2014, while entry rates continued to increase (UCAS, 2014; Murphy et al., 2019).

Table 3 also relates the post-reform effect to country-level economic characteristics. The ATT remains large and negative as the observed controls are added. In the interacted specification, the GDP-per-capita and working-age-population interactions are positive and precisely estimated, while the youth-unemployment interaction is not statistically distinguishable from zero.

## 6. Robustness

### 6.1. Control-group robustness

We assess sensitivity to control-group choice using several alternative donor pools and an Ireland-only comparison. Prior work motivates these checks, but it does not eliminate the need to evaluate them in the present sample (Clifton-Sprigg et al., 2025). To summarize, we have established that:

1. The event-study estimates provide no evidence of differential pre-trends in the available window.
2. The leads-only specification provides no evidence of anticipatory adjustment.

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<sup>2</sup> The IFS performed a lifecycle simulation of the pre- and post-2012 student-finance systems for young, full-time, English-domiciled students. It estimated average total loan repayments in real net-present-value terms of £15,075 under the old system and £22,843 under the new system, both in discounted 2014 terms (Crawford & Jin, 2014, p. 24). The modeled repayment burden therefore increased by 51.5%, compared with the 166.7% increase in the annual fee cap from £3,375 to £9,000. Combining the former increase with UCAS's estimate that young application rates were approximately 5% below their expected trajectory yields an implied log elasticity of  $\ln(0.95)/\ln\left(\frac{22843}{15075}\right) = -0.123$ . For comparison, using the change in the nominal fee cap itself gives  $\ln(0.95)/\ln\left(\frac{9000}{3375}\right) \approx -0.05$

3. The Croatian accession design in Clifton-Sprigg et al. (2025) provides related quasi-experimental evidence from an earlier policy change.

We compare the baseline with four alternatives: very poor countries (14 controls), very rich countries (12 controls), OECD countries (14 controls), and Ireland alone. The three multi-country groups are detailed in Table 4.

**Table 4.** Sample compositions for the alternative control groups

|                           | <i>Poor</i>                                                                                                                                            | <i>Rich</i>                                                                                                                                   | <i>OECD</i>                                                                                                                                                |
|---------------------------|--------------------------------------------------------------------------------------------------------------------------------------------------------|-----------------------------------------------------------------------------------------------------------------------------------------------|------------------------------------------------------------------------------------------------------------------------------------------------------------|
| Control-group composition | Pakistan, Nepal, Bangladesh, Ethiopia, Uganda, Tanzania, United Republic of, Rwanda, Mozambique, Malawi, Sierra Leone, Ghana, Nigeria, Kenya, Zimbabwe | United States of America, Canada, Australia, Norway, Switzerland, Singapore, Qatar, United Arab Emirates, Kuwait, Saudi Arabia, Israel, Japan | Australia, Canada, Chile, Colombia, Iceland, Israel, Japan, Korea, Republic of, Mexico, New Zealand, Norway, Switzerland, Turkey, United States of America |
| <i>EU group</i>           | 26                                                                                                                                                     |                                                                                                                                               |                                                                                                                                                            |
| <i>Observations</i>       | 400                                                                                                                                                    | 380                                                                                                                                           | 400                                                                                                                                                        |

A further concern is that the Graduate Route, introduced in July 2021, may have raised the appeal of UK study independently of tuition by letting international graduates stay up to two years after finishing (Home Office, 2021), therefore treating the control group and biasing the effect downwards. This is mitigated in two ways. First, because the Route reached treated and control countries alike after 2021, any common increase in appeal is absorbed by year fixed effects and cannot identify the DiD coefficient. Second, the Ireland-only specification is also introduced specifically for that purpose, since Irish citizens kept Common Travel Area rights and did not rely on the Route as other international students did, which limits control-group contamination.

Because this design does not build the control group by constrained optimization, we weight the counterfactual as:

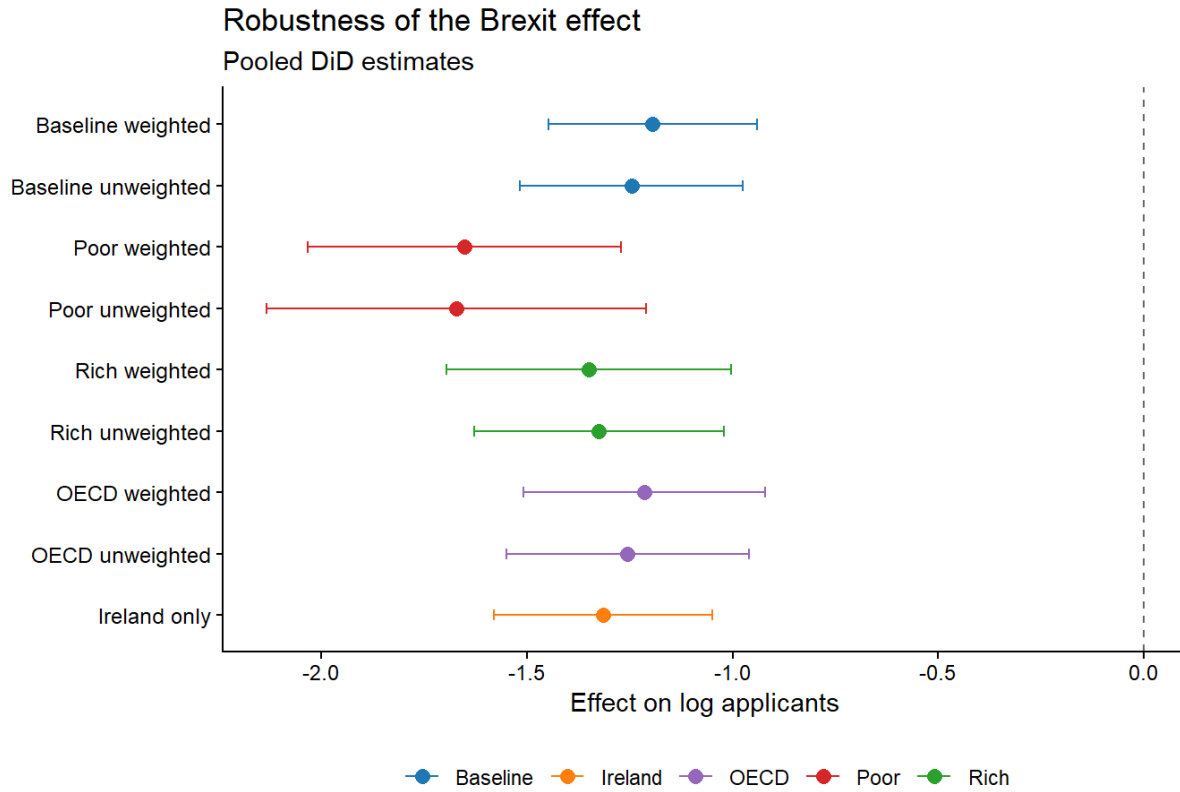
594

$$w_j = \frac{\frac{1}{d_j + 0.001}}{\frac{1}{J} \sum_{k=1}^J \frac{1}{d_k + 0.001}}$$

595 where  $d_j = \sqrt{\frac{1}{T} \sum_{t < 2021} (\log(A_{jt}) - \overline{\log(A_t^{EU})})^2}$ . The results are presented in Figures 7 and 8 for  
 596 the ATT and the country-specific responses, respectively.

597

598 **Figure 7.** Robustness of the Brexit effect across alternative control-group definitions (pooled  
 599 DiD estimates).

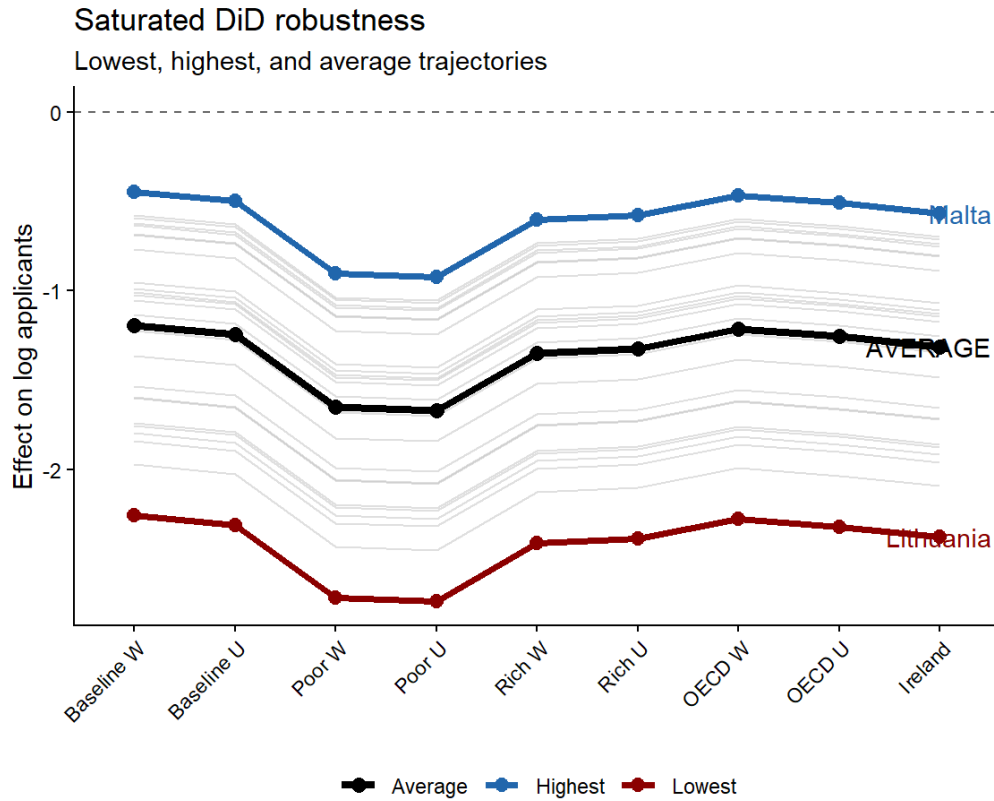


600

Notes. Points are  $EU \times Post$  coefficients. Lines are 95% confidence intervals.

601 **Note:** Points are  $EU \times Post$  coefficients from the pooled difference-in-differences specification;  
 602 horizontal bars denote 95% confidence intervals. Baseline is the synthetic donor pool used in the  
 603 main specification; Poor, Rich, and OECD are the alternative control groups described in Table  
 604 4, each shown weighted and unweighted; Ireland is a single-country comparison.

**Figure 8.** Saturated difference-in-differences robustness across control-group specifications.



**Note:** Grey lines show country-specific dynamic treatment trajectories under each alternative control-group specification; the black line marks the cross-specification average. Malta (blue) and Lithuania (red) mark the upper and lower bounds of the estimated effects.

Figures 7 and 8 show that the estimated  $EU \times Post$  coefficient remains large and negative across the baseline, OECD, high-income, low-income, and Ireland comparisons. Figure 8 demonstrates that the poor-country control group generates the most negative estimates. This is informative because it shows that the decline is not a generic post-pandemic fall in applicant numbers from low-income origins. If that were the mechanism, low-income non-EU countries should absorb much of the EU decline and push the coefficient toward zero. The Ireland comparison remains close to the other estimates, reducing concern that Graduate Route exposure explains the result. Country rankings also remain stable across donor groups, and weighting changes estimates only modestly. A separate full-donor-pool synthetic DiD using Arkhangelsky

et al. (2021) gives an ATT of  $-1.26$  with a jackknife-based 95% confidence interval of  $[-1.464, -1.053]$ <sup>3</sup>, supporting the conclusion that the large decline is insensitive to the baseline weighting scheme.

## 6.2. Rambachan–Roth parallel-trends checks and synthetic difference-in-differences comparison

We perform two additional robustness checks. The first re-examines the parallel-trends assumption behind the aggregated event study in the identification section using the HonestDiD implementation of the sensitivity analysis of Rambachan and Roth (2023), which replaces the binary test of whether pre-trends are flat with a measure of how large a post-treatment violation of parallel trends would have to be before the estimated effect loses its significance. Because the pre-treatment leads are jointly indistinguishable from zero (a joint Wald test gives  $p = 0.96$ ) and are about two orders of magnitude smaller than the post-treatment coefficients, the loan-driven collapse in EU applicant numbers survives the relative-magnitudes sensitivity analysis across the full range of violations we examine. The final breakdown value exceeds two, so the result is generally not an artifact of an assumed pre-trend.

The second check re-estimates 26 separate synthetic difference-in-differences regressions, one per treated country, so the Arkhangelsky et al. (2021) estimator collapses to  $N_{tr} = 1$ :

$$\hat{\tau}_j^{sdid} = \left( \bar{Y}_{j,post} - \sum_{t \leq T_0} \lambda_t^j Y_{jt} \right) - \sum_{i \in D} \widehat{\omega}_i^j \left( \bar{Y}_{i,post} - \sum_{t \leq T_0} \lambda_t^j Y_{it} \right)$$

Table 5 compares these estimates with the saturated weighted DiD coefficients. After consistently removing aggregates, territories, dependencies, and UK home nations from the

---

<sup>3</sup> The estimation was based only on countries having more than 25 country-year observations across the entire panel. Including countries with country-year observations of less than 25 changes the estimate by about 0.03. Therefore,  $N1=26$ ,  $N0=94$ , effective observations were 1200 in total after filtration. The panel was fully balanced.

donor pool, the two sets correlate at 0.96 and have a mean absolute difference of 0.15 log points, with the synthetic DiD estimates larger than the weighted TWFE ones. Portugal and Slovakia have the largest discrepancy at about 0.50 log points but that is expected given Portugal's documented 2021 reporting issue and Slovakia's large baseline effect of about -1.52 log points. The ranking is similar across designs, although several level differences remain economically meaningful. All effects are statistically significant under a placebo-based variance-covariance matrix (jackknife was not possible because we used only one treated entity). As expected, the correlation with log GDP per capita under synthetic DiD is stronger than under TWFE: the log-GDP correlation rises from 0.585 under TWFE to 0.661 under synthetic DiD (including the Netherlands).

**Table 5.** Per-country saturated DiD (TWFE) and synthetic difference-in-differences estimates

| Country                 | Saturated TWFE | Synthetic DiD | Absolute difference |
|-------------------------|----------------|---------------|---------------------|
| Lithuania               | -2.17          | -2.22         | 0.05                |
| Bulgaria                | -1.89          | -1.88         | 0.01                |
| Finland                 | -1.76          | -1.71         | 0.04                |
| Romania                 | -1.71          | -1.91         | 0.20                |
| Poland                  | -1.67          | -1.99         | 0.32                |
| Croatia                 | -1.66          | -1.79         | 0.13                |
| Slovakia                | -1.52          | -1.93         | 0.42                |
| Latvia                  | -1.51          | -1.73         | 0.22                |
| Estonia                 | -1.45          | -1.52         | 0.08                |
| Slovenia                | -1.28          | -1.25         | 0.03                |
| Czech Republic          | -1.14          | -1.41         | 0.28                |
| Hungary                 | -1.11          | -1.39         | 0.28                |
| Denmark                 | -1.05          | -1.10         | 0.06                |
| Sweden                  | -0.97          | -1.01         | 0.05                |
| Portugal                | -0.94          | -1.43         | 0.49                |
| Italy                   | -0.93          | -0.97         | 0.05                |
| Cyprus (European Union) | -0.90          | -0.87         | 0.03                |

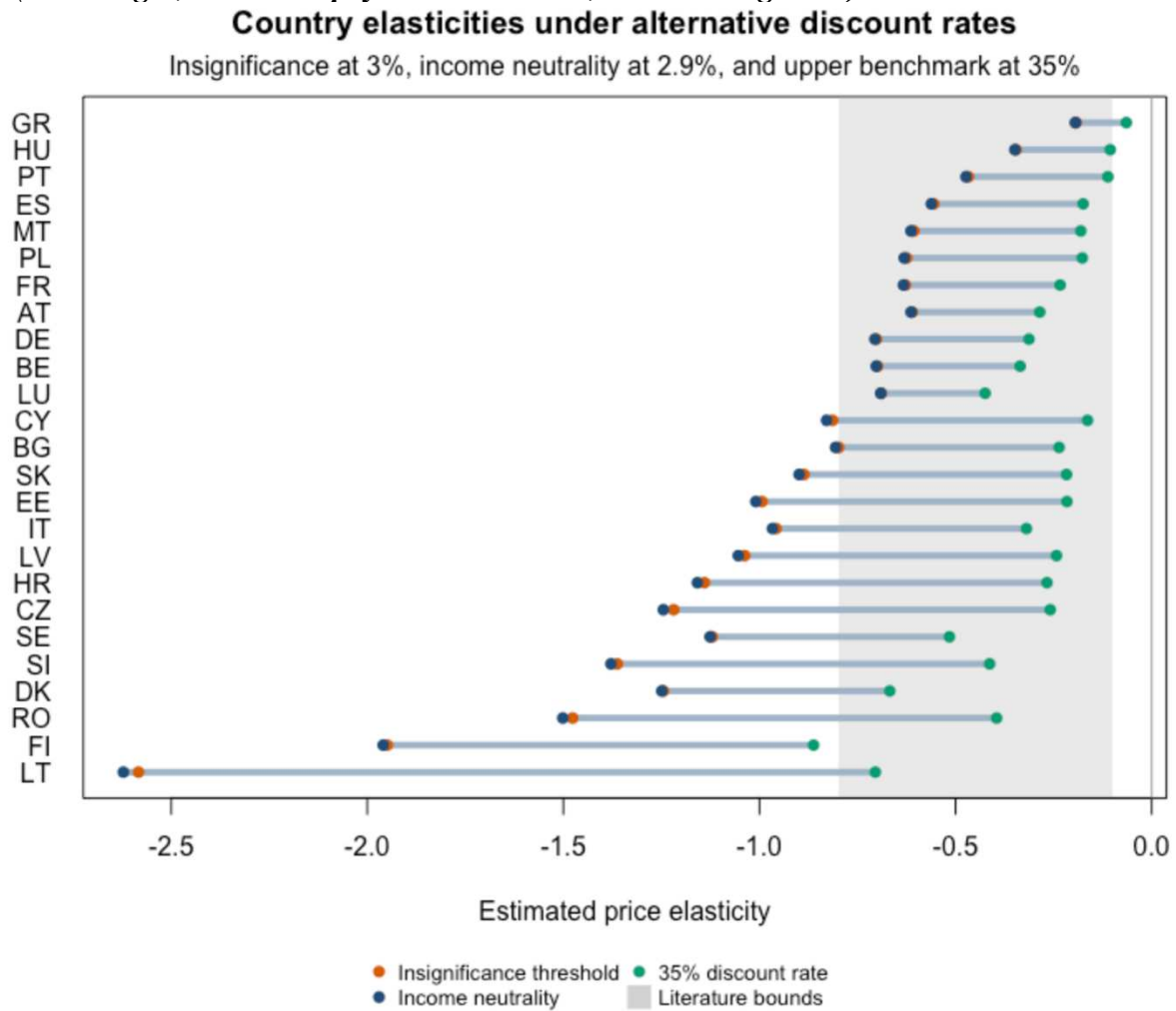
|             |       |                    |      |
|-------------|-------|--------------------|------|
| Greece      | -0.87 | -0.98              | 0.11 |
| Germany     | -0.68 | -0.74              | 0.06 |
| Belgium     | -0.60 | -0.67              | 0.07 |
| France      | -0.60 | -0.73              | 0.13 |
| Austria     | -0.55 | -0.62              | 0.07 |
| Netherlands | -0.54 | -0.67              | 0.13 |
| Spain       | -0.51 | -0.86              | 0.35 |
| Luxembourg  | -0.49 | -0.58              | 0.09 |
| Malta       | -0.36 | -0.60              | 0.23 |
| N           | 520   | 950 per regression |      |

*Note: Coefficients are on the log-applicant scale. Saturated TWFE effects are the country-specific EU  $\times$  Post coefficients from the weighted model with 520 country-year observations. Synthetic DiD effects are 26 single-treated-unit estimates against 94 eligible sovereign-country controls (applicants more than or equal to 25). The correlation is 0.96 and the mean absolute difference is 0.15 log points.*

### 6.3. Wage-threshold convergence and repayment protection

As a timing robustness check, we reconstruct the economic information set prevailing when prospective students applied by combining 2021 wages with the 2021/22 country-specific repayment thresholds and exchange rate (Student Loans Company, n.d.). The new weighted average RPI is 1.96% (see appendix). Under this calibration, the estimated relationship between national income and the country elasticities is already statistically insignificant at a subjective discount rate of 3 percent. Exact income neutrality occurs at approximately 2.9 percent. However, the elasticities reach the literature bounds at similar subjective discount rates.

**Figure 9. Country-specific loan-adjusted elasticities under the reconstructed 2021 information set (2021 wages, 2021/22 repayment thresholds, and exchange rate).**



The reason why the income neutrality occurs much earlier in Figure 9, can be summarized by the ratio between the country-specific wage and the applicable repayment threshold,  $\left(R_{jt} = \frac{w_{jt}}{\tau_{jt}^L}\right)$  during and after 2021. When  $(R_{jt} < 1)$ , no immediate repayment is required. When the ratio exceeds one, repayment begins and the present value of the pre-Brexit loan increases. In 2021, wages in 10 of 25 countries were initially below their repayment thresholds. Deferred repayment lowered loan present values and enlarged effective post-Brexit price increases, particularly for lower-income origins. These larger denominators moderated their implied elasticities and removed the income gradient at a lower subjective discount rate. This

explains earlier income neutrality without implying that all country elasticities already match the literature range.

The robustness exercise shows that low-rate income neutrality does not, however, imply that the resulting elasticity levels are plausible. At the 2.9 percent neutral rate, only 13 of the 25 country estimates lie within the literature range of  $-0.80$  to  $-0.10$  (Figure 9). The aggregate income gradient is thus neutral because contrasting country patterns cancel one another, even though several individual elasticities remain exceptionally large. This reveals **the duality of the income contingent financing**. On one side the income differences between applicants are compressed due to lower repayment thresholds relative to the expected earnings (that is why the income gradient collapses immediately on Figure 9), and the other side income contingent loans compress income differences by allowing subjective discounting to compensate for the lack of immediate financing.

In 2021, the repayment thresholds performed most of the equalization by protecting graduates in lower-wage countries. Following wage-threshold convergence, in 2025 this contractual protection weakened and subjective discounting performed more of the equalizing work. Which means that the movement from 2.9 to 14.9 percent thus reflects a change in repayment exposure across countries, while the underlying treatment estimates remain unchanged.

## **7. Discussion**

### **7.1. Why now and not in 2012?**

The central puzzle is why EU applicant numbers have not recovered, as they did after the 2012 reform, even though the UK remains the second most popular destination for international students globally (Cuibus et al., 2025). The explanation lies in access to the UK higher education market and the nature of the tuition shock itself because the 2012 and 2021 tuition shocks were

not equivalent. The 2012 fee increase was accompanied by continued access to income-contingent tuition loans that covered the regulated fee. The nominal price increased, but students were not required to pay the corresponding amount upfront and could subjectively discount repayments occurring in the future; this was applied to both sides of the denominator since both the new and the old price were based on income contingent loans (plan 1 and 2). The 2021 reform simultaneously removed this financing mechanism and exposed new EU applicants to international tuition fees. This specific situation created the conditions required to better understand the structure of the European higher educational inequality.

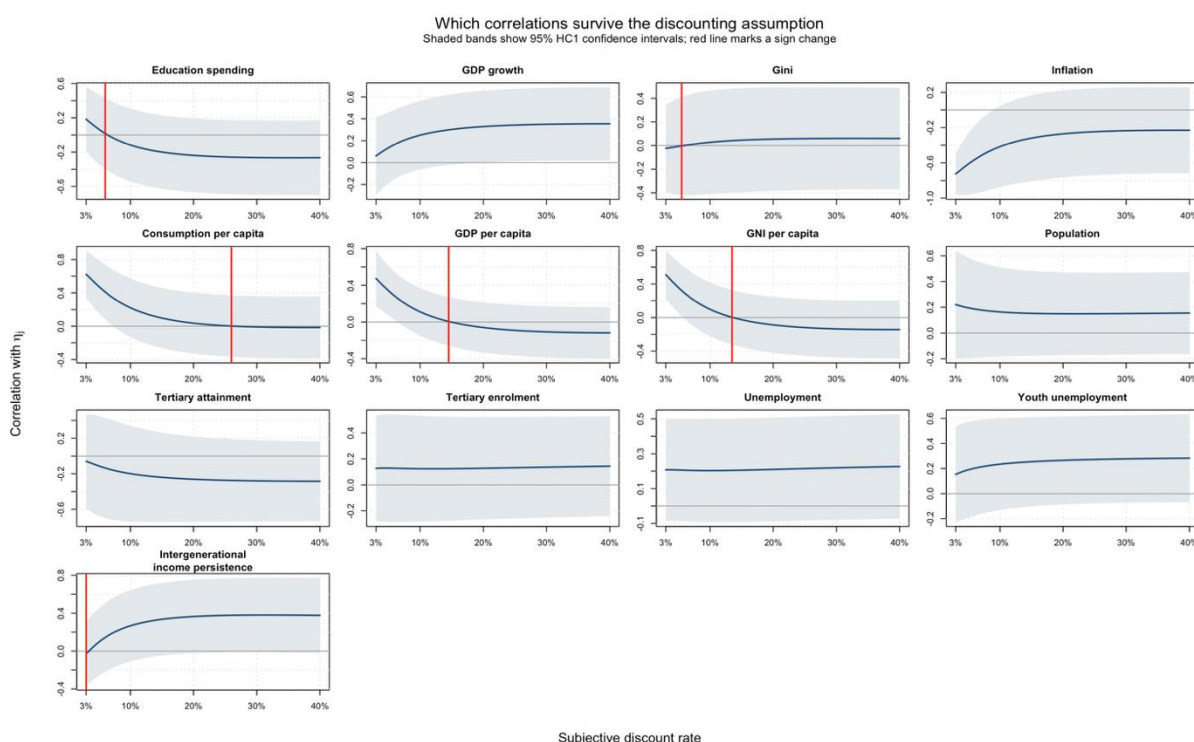
To see this, Figure 10 traces the correlations between the implied elasticities and country characteristics across alternative subjective discount rates. At low discount rates, the elasticities are positively correlated with GDP, GNI, and consumption per capita. These correlations weaken as subjective discounting increases, with the GDP and GNI gradients crossing zero near the income-neutral discount rate. Most remaining correlations retain broadly stable signs but are estimated imprecisely.

Intergenerational income persistence follows a particularly interesting pattern. Its correlation with the elasticities is slightly negative at 3%, changes sign near 4%, and then stabilizes at approximately 0.3. Since the estimated elasticities are negative, this positive correlation means that applicants from countries with stronger intergenerational income persistence appear less price sensitive under moderate and high discounting. This implies a positive selection mechanism. Applicants reaching UK universities from less mobile societies may disproportionately come from affluent families that are better able to absorb the loss of public financing. The HC1 confidence interval nevertheless includes zero at the margin, so the pattern remains suggestive.

The correlation profiles also show that the cross-country ranking of the elasticities is not invariant to the valuation of the loan. The same estimated treatment effects produce different

cross-country gradients because the experienced price change depends on starting earnings, repayment thresholds, repayment duration, and subjective discounting. The income-contingent loan compressed these differences and operated as an equalizing institution for European educational mobility. Its withdrawal made household liquidity, access to private finance and (probably) intergenerational income persistence central determinants of entry into the UK market.

**Figure 10.** Implied demand elasticity as a function of the economic value of the UK student loan.



*Note: all of the covariates were z-score normalized with the scale function in R and pushed through univariate OLS regressions across a sequence of multiple discount rates. Used are HC1 standard errors across all specifications. N is equal to 25. All data is collected from World Bank developmental indicators except the intergenerational wealth indicator that comes from “IGE\_Munoz\_VanderWeide\_June2025.dta” (Muñoz & van der Weide, 2025) pull and is converted from STATA to R programmatically.*

## 7.2. Are elasticities above one plausible?

The income-neutral panel of Figure 5 shows that the elasticities for Finland, Lithuania and Romania remain larger than one in absolute magnitude even after the income gradient is removed. These estimates are not inconsistent with a well-defined demand curve. Under the

constant-elasticity demand function ( $Q(P) = AP^\eta$ ), doubling the price changes demand by the factor ( $2^\eta$ ). An elasticity of  $-1$  therefore implies a 50 percent decline, while an elasticity of  $-2$  implies a 75 percent decline. Values above one in absolute magnitude remain compatible with positive applicant numbers.

EU applicants also possessed a wider substitution margin than domestic students. The 2012 reform increased tuition within the UK system and preserved income-contingent financing, leaving domestic applicants principally to choose among institutions facing similar rules. Following Brexit, EU applicants could instead substitute toward comparable programs in Ireland, the Netherlands or Germany. Elasticities above one consequently describe the reallocation of internationally mobile applicants across national higher education markets, with another possible reason being the lack of well-established student credit institutions across poorer EU countries capable of buffering the loss of income-contingent financing. Given the existence of similar mechanisms across the EU, we might have expected students to be more willing to continue their education in the UK.

### **7.3. Income neutrality and fiscal drag in the post-Brexit world**

The 2021 calibration illustrates how income-contingent repayment can protect graduates across unequal national labor markets. Subsequent wage-threshold convergence reduces this protection, making the distributional consequences of nominal thresholds central to the financing mechanism. This implies that the political significance of threshold protection lay in allowing an income-neutral application response without requiring equal incomes across Europe. The 2021 repayment system gave the appearance of equality. When nominal wages later rose, the progressivity of the loan decreased even though the repayment rate was not formally increased. This is similar to fiscal drag, since growth in nominal wages causes graduates to enter into repayment even if their real purchasing power has not increased in proportion. As a result, the distributional effects of a fixed loan agreement can change without a new political decision. After Brexit, the protection was completely eliminated for new EU students, resulting in a very aggressive international tuition fee. By removing the loan, the educational risk shifted from the repayment system to students and their families, revealing differences in national incomes that the earlier financing arrangement had partly absorbed.

## **8. Conclusion**

The UK reform produced a large, persistent decline in EU applications. Under a common nominal fee change, implied elasticities exceed conventional estimates and show a strong income gradient. Accounting for origin-specific loan values reduces their magnitudes and changes their ranking. In the baseline calibration, the gradient becomes statistically insignificant near 6% subjective discounting and reaches zero near 14.9%, with most elasticities then within the literature range. These results suggest that financing conditions can explain part of the apparent cross-country difference in responsiveness. Income neutrality remains a calibration diagnostic, not an independently identified discount rate or proof of common preferences. The findings highlight income-contingent finance as a potential equalizing institution for European educational mobility.

Most member states lacked comparable financial infrastructure to fund international students. In India, where studying abroad poses "a financial hurdle ... particularly for students from low- to middle-income families" (Chalil & Hoque, 2026, p. 259), banks have long offered education loans for studies abroad (Indian Banks' Association, n.d.), and students report financing overseas study "either in the form of education loan or as a financial obligation from the family" (Ahir & Joshi, 2026, p. 131). Since the EU functioned within a quite different institutional setup, in which movement of students within Europe and, prior to Brexit, access to UK student financing meant that there was no need for such private financing markets, Brexit led EU applicants to have to pay international tuition fees without having a similar financing infrastructure already existing. This resulted in severe liquidity problems, especially for applicants from less affluent member states, and likely caused some of their demand to shift to other higher-education systems.

Future research should examine student-level financing constraints, subjective discounting, and intergenerational mobility as the most substantial structural drivers of inequality. Models of international higher-education demand should account for portable student finance, substitution across destinations, and receiving-country capacity to clarify how financial access shapes educational opportunity.

## **Ethical Approval**

This article does not contain any studies with human participants performed by any of the authors

## **Informed Consent**

This article does not contain any studies with human participants performed by any of the authors.

## Data statement

The data and code underpinning this article, including raw and processed datasets, variable definitions, data transformations, and R scripts for reproducing the reported results, are publicly available at:

[https://github.com/stoychorr/The\\_Brexit\\_Tuition\\_Shock\\_and\\_the\\_Price\\_Elasticity\\_of\\_International\\_Students\\_IZA](https://github.com/stoychorr/The_Brexit_Tuition_Shock_and_the_Price_Elasticity_of_International_Students_IZA).

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