Switching to English to Attract Global Talent

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October 15, 2025

Abstract

When organizations' demand for talent exceeds domestic supply, they can recruit from abroad, but language barriers impede matching. Switching operations to the *lingua franca* can, in principle, relax this constraint. We show for a model of assortative matching that the abolition of a language constraint increases the average ability of new hires. But an increase in capacity lowers it. We test these predictions in a laboratory of European universities' economics and business academe. Leveraging a staggered difference-in-differences design, we show that when universities introduce English-taught degree programs, the number of new hires and their average ability—measured by top-quartile publications—rises substantially. After the switch European universities increase their size to that of the US control group. These gains must be weighed against potential costs related to cultural preservation and native-language labor supply.

Keywords: Labor Mobility; Research Performance; Matching; Language (*JEL: I23, J6*1)

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We started teaching in English... when it was illegal. — Jean Tirole[1]

1 Introduction

How to attract more and better workers in the global "war for talent"?[2] One obvious answer is to increase wage. Another is to lower barriers to mobility. With falling birth rates, that increasingly means lowering barriers for international talent for whom certain barriers like work permits, culture and language are specific. In this paper we analyze the effect of a university's switch to English on the quantity and ability of its new hires.

Academia illustrates the organizational challenge and response. When Jean Tirole, the 2014 economics Nobel prize winner, and Jean-Jacques Laffont broke French law by introducing English as the language of instruction in 1995, the Toulouse School of Economics (TSE) was considered 'minor league' (Kuper, 2015). Today, TSE is world-renowned. Switching to English attracted top students from abroad, and globally leading faculty followed. Research publications in international journals soared. Since then, many universities in non-English speaking countries have joined TSE in switching their instruction to English.

We use European universities as a laboratory for language switches, as academia allows us to easily quantify the phenomenon compared to firms. First, universities might introduce English more abruptly compared to firms. Second, it is easy to observe individual academics' ability and moves. Moreover, because of the specificity of the labor required, some European universities probably switched to English as early as, if not earlier than any business firm.

We surveyed some of the world's earliest switchers in Europe, to collect an original set of dates when they first introduced a Business or Economics degree program completely in English. And we collect individuals' publication records from Scopus to define proxies for productivity.

[1] (Kuper, 2015)

[2] The phrase was coined by Chambers et al. (1998). Already at the time they gave this example: "One US-based accounting firm hired a third of one year's graduates from a top Indian college."

Our empirical analysis is informed by a theoretical model of assortative matching. We add a "skill", here language, requirement to a standard model of many-to-one matching. For foreigners, that skill constraint is binding and they cannot possibly work for a university that requires the skill. The model predicts that an abolition of the skill requirement increases hires' average ability at the focal university. An increase in capacity lowers it. This model makes no prediction for the effect of the switch on the number of new hires. But in the presence of economies of scale in teaching and/or research we might expect universities to grow if the language constraint on hiring was binding.

Figure 1 provides descriptive evidence for several switching universities in our sample. The horizontal axis has time. The vertical axis has research productivity of newly-hired staff. The vertical lines give the years the selected universities introduced their first English business or economics degree program: Tilburg University in 1995, Wageningen in 2002, TU Berlin in 2006, and University of Twente in 2013. In each case, the research productivity of its new hires visibly increases in the following years. There is also evidence it increases relative to US universities, our control group.

We confirm the patterns in Figure 1 using regression difference-in-differences models. To model the cohort and event-time heterogeneity for our staggered treatment design, we use the extended two-way fixed effects (ETWFE) estimator. Even though universities decide on the policy change, we assume that after controlling for different unobserved effects and several covariants, this quasi-treatment is as good as random.

Our results show that the switch substantially increases the total productivity of a university's new hires. There is a large increase in the number of hires and of their average ability. The latter effect is robust to controlling for the number of new hires. Empirically, whereas the quantity effect is stronger for foreign hires, the quality effect is stronger for domestic hires. This implies that the strategic switch to the *lingua franca* reduces language barriers to talent mobility, and potentially increases universities' recruiting and research power.

Our results are topical and contribute to an ongoing debate around globalization of work and in particular the brain drain of European scientists to the US (Docquier & Rapoport, 2012;

Grogger & Hanson, 2015; Hunter et al., 2009), as well as its potential reversal. Recently, the European Commission started to take action under the slogan "Choose Europe for Science". But despite these European efforts, in more and more places, the use of English in academia gets caught up in the push for deglobalisation. Interestingly, some of the European first movers like Belgium, Scandinavia, and the Netherlands have recently started to push back against the use of English at universities. Policymakers and university officials, concerned with preserving national culture, supplying the native-speaking labor market, and keeping house prices down have imposed quotas on English language programs (Guimarães & Kremer, 2020; Nordic Council, 2007) and requirements to learn the local language on new hires. The Belgian region of Flanders introduced a law explicitly requiring all faculty to learn Dutch.[3] Our results show the benefits of switching university instruction to the *lingua franca*. These must be weighted against the costs. Besides those macro costs listed above language diversity can also lower workplace productivity (Dale-Olsen & Finseraas, 2020).

We contribute to a literature that analyses the effects of pairing constraints in labor markets on productivity. Economists believe that productivity is higher if the fit between workers, places, jobs, firms, or other workers is "good". Good matches can be achieved through a well-designed allocation mechanism (Roth & Sotomayor, 1992) and might require learning (Jovanovic, 1979). Thanks to Latin as the *lingua franca*, medieval Europe benefited from an integrated academic market with assortative matching between scholars and universities (de la Croix et al., 2024). Assortative matching is partly driven by spillovers in the production of research output (Borjas & Doran, 2015; Bosquet et al., 2022; Oettl, 2012; Waldinger, 2012). There is also evidence that scientists are very mobile internationally and selection is probably positive (Borjas & Doran, 2012; Franzoni et al., 2014; Gaulé & Piacentini, 2013; Stephan & Levin, 2001; Van Noorden, 2012).

Various constraints might prevent optimal pairing. Constraints might be due to discrimination (Hsieh et al., 2019), information frictions (Ferris & McKee, 2005), or immigration regulations (Kerr & Lincoln, 2010). We focus on language, in particular, not using English, as a constraint.[4] Several papers recognize the importance of language for optimal pairing

[3] https://etaamb.openjustice.be/nl/decreet-van-13-juli-2012_n2012036158.html, last accessed 15.10.2025.

[4] Alternatively, immigrants could learn the local language. In fact, local language skills improve the productivity of immigrants (Dustmann & Van Soest, 2002). But the opportunity cost of learning the local language might be too

(Andersson & Ehlers, 2020; Maaz & Papanastasiou, 2020), but to the best of our knowledge our is the first empirical test of the relevant theoretical predictions. Adserà and Pytliková (2015) show that language distance between home and host countries is an important driver for migration. But this effect disappears when the host country is English-speaking. Others have confirmed that language is not a barrier for English-speaking countries (Borjas & Doran, 2012; Moser et al., 2014). Language is not only a barrier to physical movement, it is also a barrier to knowledge flows. Cao et al. (2024) show that a university's research ranking decreases in the distance between its local language and English. Guillouët et al. (2024) show that stronger English proficiency improves knowledge flow within organizations, especially between foreign and domestic managers. Nocito (2021) shows that having had English language courses at university increases the probability to emigrate. Whereas the literature largely takes language as a given, we look at language as an organizational decision variable. For organizations, a switch to English, like an increase in wages, is an important measure to attract staff, especially foreign. Many business scholars recognize this and advocate for a switch to English for business firms (Neeley, 2012).

There is a (largely qualitative) literature that points to the importance of language, and especially English for organizational performance. Borghans and Cörvers (2010) and Van Hoorde (2015) document the increasing use of English in European academia, in particular for teaching, doctoral dissertations, and journal publications. Drèze and Estevan (2007) argue that European countries should adopt English throughout economics to compete globally.

We also contribute to the literature on the optimal university size. Although the switch to English itself does not change the number of new hires, it does loosen labor supply constraints, which might have stopped a university from benefiting from economics of scale and agglomeration. Prior evidence suggests that both at the department and university level economies of scale in respect to teaching and research outputs exist up to some point (Cohn & Cooper, 2004; Daraio et al., 2015; Dundar & Lewis, 1995; Von Tunzelmann et al., 2003). We do not analyze a university's optimal size, but show that when it is free to change it, it does so. Besides economies of scale, larger departments might benefit more from positive externalities

high for mobile knowledge workers. This is similar to the argument that political as opposed to economic refugees are more likely to invest in host-country specific human capital (Borjas, 1982).

among researchers (Bosetti et al., 2015; Bosquet & Combes, 2013; Hunt & Gauthier-Loiselle, 2010).

The next section of this paper explores theoretical considerations, section 3 gives the identification strategy. Section 4 describes the data collection process and the variables. Section 5 discusses the results, and the final section concludes.

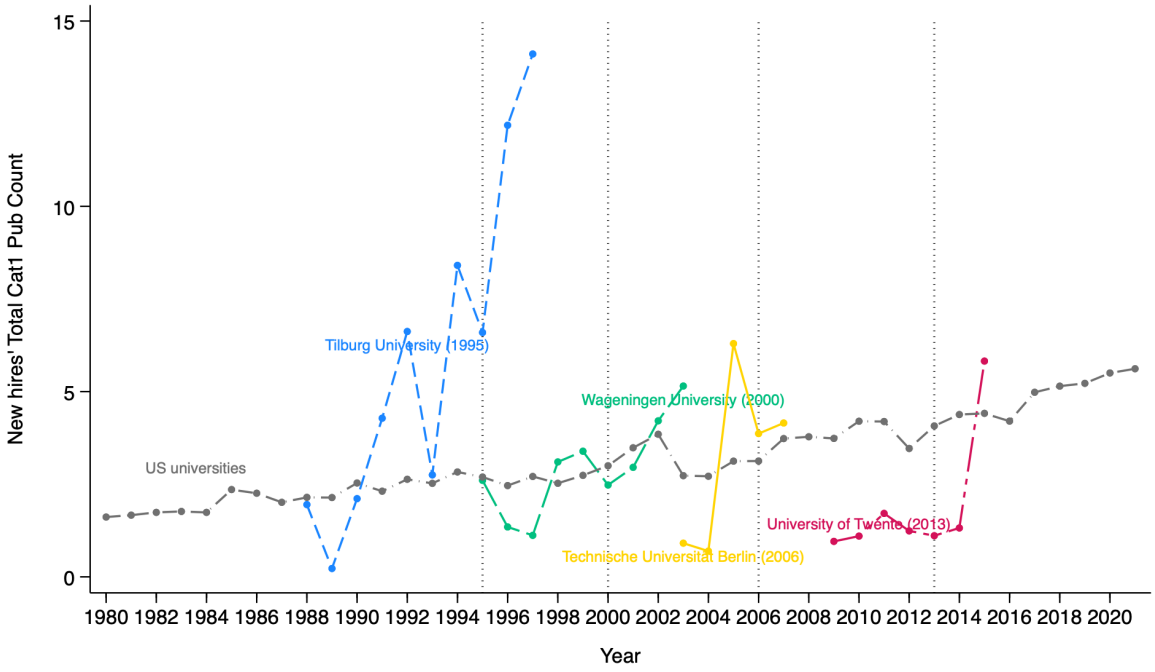


Figure 1: Switch to English Drives research productivity

Note: The graph provides descriptive evidence from a subset of switching universities compared to US universities listed by Amir and Knauff (2008). Research productivity of newly-hired staff by year is measured by the count of their publications in top-ranking journals. Dates of introducing first English degree program are given in parentheses and denoted by vertical dotted lines.

2 Theory

Here we introduce a theoretical microeconomic framework for our empirical analysis. We reduce the global academic labor market to a many-to-one matching market in which academics universally rank universities the same way and vice-versa. However, because the default teaching language is the local one, the simple assortative solution is constrained—regardless of how much a university would like to employ candidate x, she needs to be able to teach. A non-Anglosphere university, can though, allow teaching in English. This has two effects: (1) It enlarges the potential pool of acceptable academics to hire from. (2) It also enlarges the potential pool of international students who can attend. We assume this larger pool leads to greater demand for academics. We model this as increase in the university's capacity constraint (on number of academics employed). We derive our empirical hypotheses and justification for our untreated control group from the comparative statics on these two effects.

2.1 Model

Let $E = \{1, \ldots, M\}$ denote the set of employers ("universities"), ordered by prestige so that 1 is best. Employer e has capacity $q_e \in \mathbb{N}$. Define prefix capacity $Q_e \equiv \sum_{j=1}^{e} q_j$ as the total capacity of the top e employers. Let $W = \{1, \ldots, N\}$ denote the set of workers ("academics"), ordered by quality so that 1 is best. For each employer e, $A_e \subseteq W$ is the set of workers eligible (i.e., satisfy the skill requirement) at e. Define the eligible-pool prefix as $P_e \equiv \bigcup_{j=1}^{e} A_j \subseteq W$ and $M_e \equiv \min\{Q_e, |P_e|\}$.

Employers have responsive preferences over acceptable workers: given a set $X \subseteq W$, the choice function is $C[X] =$ the top q_e workers in $X \cap A_e$ according to a strict common order $\succ_W$: $1 \succ 2 \succ \cdots \succ N$ over workers. Workers share a strict common order $\succ_E$: $1 \succ 2 \succ \cdots \succ M$ over employers.[5]. We refer to this assumption as *common rankings* (CR).

[5]We abstract from any worker complementarities.

A many-to-one matching μ satisfies: (1) $\mu[e] \subseteq W, |\mu[e]| \leq q_e$ for each employer e. (2) $\mu[w] \in E \cup \{\varnothing\}$ for each worker w. (3, Two-way consistency) $w \in \mu[e] \iff \mu[w] = e$. (4, IR) $\mu[w] = e \implies w \in A_e$. We define the hires prefix

$$H_e[\mu] \equiv \begin{cases} \varnothing & \text{for} \quad e = 0 \\ \bigcup_{j=1}^{e} \mu[j] \subseteq W & \text{for} \quad e \in W \end{cases}.$$

μ is stable if it is IR and there is no blocking pair $\langle e, w \rangle$ with $w \in A_e$ such that w prefers e to $\mu[w]$ and either $|\mu[e]| < q_e$ or e prefers w to some $w' \in \mu[e]$.

2.2 Stability and Uniqueness

Gale and Shapley (1962) showed that a stable matching exists by *deferred acceptance* in many-to-one markets with responsive preferences and acceptability constraints, even without CR. Under CR, we construct the stable matching by a simple greedy procedure and show it is unique.[6]

Definition 1 (Greedy) *Process employers in order $e = 1, 2, \ldots, M$; at step e, assign to e the best q_e workers in A_e among those not previously assigned. Stop when all employers are processed or no eligible workers remain. Call the outcome μ^G.*

It is useful to define the residual pool of workers available to e after $e-1$ Greedy steps as $R_e[\mu] \equiv A_e \backslash H_{e-1}[\mu]$, and $T_k[S]$ as the $k \geq 0$ smallest elements in $S \subseteq W$ (if $|S| < k$, then $T_k[S] = S$). The Greedy solution can then be represented recursively:

$$G[e] \equiv \begin{cases} T_{q_1}[A_1] & \text{for} \quad e = 1 \\ T_{q_e}[R_e[G]] & \text{for} \quad e > 1 \end{cases}$$

[6] We are almost certainly not the first to establish stability and uniqueness of Greedy under CR, but we need these for our comparative statics.

Proposition 2 (Stability) *Greedy produces a stable assignment μ^G under strict CR.*

Proof. Fix e. By construction $G[e] = T_{q_e}[R_e[G]]$. (1) If $|R_e[G]| < q_e$, then $G[e] = R_e[G]$ and there is no acceptable worker left in the residual. Thus, no $w \in A_e$ such that $w \notin H_{e-1}[G]$ exists to block. (2) f $|R_e[G]| \geq q_e$, then every $w \in R_e[G] \setminus G[e]$ is strictly worse (in the employer order CR) than the worst member of $G[e]$. By responsive choice, e does not prefer any such w to its current worst, so no block arises in $R_e[G] \setminus G[e]$. Any $w \notin R_e[G]$ is either unacceptable to e or already hired by some $j < e$; by workers' CR such a w does not prefer e to j. Hence, there is no blocking pair. Since there are no blocking pairs, Greedy is stable. ■

Proposition 3 (Uniqueness) *Every stable matching under strict CR equals the Greedy assignment μ^G; hence the stable matching is unique.*

Proof. Induction on e Let μ be any stable matching.

Base step $e = 1$.Suppose $\mu[1] \neq G[1]$. Pick $w \in G[1] \setminus \mu[1]$. (1) If $|\mu[1]| < q_1$, then $\langle 1, w \rangle$ blocks since $w \in A_1$: employer 1 prefers adding w (responsive), and worker 1 prefers employer 1 to any other option (workers' CR). (2) If $|\mu[1]| = q_1$, let x be the worst in $\mu[1]$. Since $w \in T_{q_1}[A_1]$, we have $w \succ_W x$ (employer's CR). By responsiveness, employer 1 prefers swapping x for w; worker w prefers 1: $\langle 1, w \rangle$ is blocking. Contradiction. So, $\mu[1] = G[1]$.

Inductive step. Assume $\mu[j] = G[j]$ for all $j < e$. Then $H_{e-1}[\mu] = H_{e-1}[G]$. So, $R_e[\mu] = R_e[G]$. Suppose $\mu[e] \neq G[1] = T_{q_e}[R_e[G]]$. Repeat the base step inside the residual: pick $w \in T_{q_e}[R_e[G]] \setminus \mu[e]$. (1) If $|\mu[e]| < q_e$, then $\langle e, w \rangle$ blocks (responsive + workers' CR). (2) If $|\mu[e]| = q_e$, let x be the worst in $\mu[e]$; $w \succ_W x$ (employer's CR), so $\langle e, w \rangle$ blocks (responsive + workers' CR). Contradiction. So, $\mu[e] = G[e]$. ■

2.3 Comparative Statics

Fix a focal employer f. Define $\bar{W}[S] \equiv 1/|S| \sum_{w \in S} w$ as the average worker index in non-empty $S \subseteq W$, where smaller is better. We define the downstream aggregate average worker index as $\bar{W}\left[\bigcup_{e>f} \mu[e]\right]$. Let μ^G and $\hat{\mu}^G$ be the unique, stable matching before and after any changes at f, respectively. We make the following simplifying *infinite reserve assumption* (IFA) for ease of presentation and because it approximately holds in our empirical setting: for every employer e there are infinitely many acceptable (potentially low quality) unemployed workers, with the consequence that in any equilibrium employers are fully staffed.

Lemma 4 (No trickle-up) *Changing A_f or q_f leaves all employers ranked above f unchanged. Formally, for all $e < f$, $\hat{\mu}^G[e] = \mu^G[e]$.*

Proof. Run Greedy after the change. Since only f's primitives changed, $R_e\left[\hat{\mu}^G\right] = R_e\left[\mu^G\right]$ for all steps $e \in \{1, ..., f-1\}$. So, Greedy makes the same choices. ■

Proposition 5 (Relax Skill Requirement) *Enlarge A_f to $\hat{A}_f \supset A_f$.*

1. *(Upstream) Upstream composition does not change.*
2. *(Focal Employer) If the top q_f elements in f's residual pool change, then its cutoff and the average worker quality in f strictly improves; otherwise composition does not change (i.e. f does not hire).*
3. *(Downstream) (a) If f makes no new hires, then the downstream composition does not change. (b) If f hires only from downstream employers, then the downstream aggregate average strictly worsens. (c) If f hires any unemployed, the direction of the change in the downstream aggregate average is ambiguous.*

Proof. Part 1. Follows from Lemma (No trickle-up).

Part 2. Since $R_f\left[\hat{\mu}^G\right] \supseteq R_f\left[\mu^G\right]$, each order statistic of $T_f\left[\hat{\mu}^G\right]$ is weakly better than $T_f\left[\mu^G\right]$, with some strictly better if $T_f\left[\hat{\mu}^G\right] \neq T_f\left[\mu^G\right]$.

Part 3. (a) Trivial.

(b) For "poaches only," handle each poach separately. Starting from the first losing employer, greedy replacement proceeds downward: at each affected employer the best acceptable available worker is either (i) employed at a lower employer, or (ii) a worker released "from above," or (iii) unemployed. In all cases the chosen replacement is strictly worse than the worker just lost (common strict order and prior optimality). The chain terminates when a replacement of type (ii) or (iii) is used. Thus, for each poach, the downstream multiset replaces one employed worker by a strictly worse worker. Summing over poaches yields a strict increase in the downstream average index (a deterioration).

(c) If f hires any unemployed, f releases incumbents to maintain headcount; lower employers may adopt those releases and replace worse incumbents, reducing the downstream average. These "better-for-worse" adoptions can offset the deterioration from poach-chains, so the net sign is ambiguous. ■

Proposition 6 (Increase Capacity) *Increase q_f to $q_f + 1$.*

1. *(Upstream) Upstream composition does not change.*
2. *(Focal Employer) f adds exactly the next-best acceptable worker from $R_f\left[\mu^G\right]$; its average strictly worsens. Formally, $\hat{\mu}^G[f] = \mu^G[f] \cup w$, and*

$$\bar{W}\left[\hat{\mu}^G[e]\right] = \frac{q_f \bar{W}\left[\mu^G[e]\right] + w}{q_f + 1} > \bar{W}\left[\mu^G[e]\right],$$

   where w is the $(q_f + 1)$-th smallest element of $R_f\left[\mu^G\right]$.
3. *(Downstream) If w was poached (from $e > f$), the downstream aggregate average worsens; otherwise (w was unemployed), the downstream composition is unchanged.*

Proof. Part 1. Follows from Lemma (No trickle-up).

Part 2. Greedy adds the $(q_f + 1)$-th best from the same $R_f\left[\mu^G\right]$; strict worsening follows because w exceeds the old worst, which was (q_f)-th best from $R_f\left[\mu^G\right]$.

Part 3. (i) A poach initiates one downward replacement chain. At each link the chosen replacement is strictly worse than the worker lost by the same reasoning as in Proposition (Relax Skill Constraint).3.b, and the chain terminates when the replacement is either an unemployed acceptable worker or a worker released from above; in either case the terminal worker is strictly worse than the worker lost at that step. Hence the downstream multiset replaces exactly one employed worker by a strictly worse worker, strictly increasing the downstream average. (ii) If w was unemployed, nobody below f loses anyone; so, composition for $e > f$ unchanged. ■

2.4 Empirical Consequences

The first implication of the model is that the intuitive control group, namely universities that do not switch their language of instruction, is a poor one. This is because, part 3 of both Propositions 5 and 6, show that not only are focal switching universities treated but so are universities ranked below them, though the predicted impacts of the treatment. At the same time, part 1 of both Propositions 5 and 6 reveal an appropriate control, namely the group of universities ranked clearly above the set of switching universities—their matches remain untreated.

The model next exposes two coincident opposing forces: when a university switches, the skill constraint on faculty and the university's capacity constraint *simultaneously* relax. The direct effect of the latter is intuitive and unambiguous–treated universities hire more faculty:

Hypothesis 1: *Switching to English at university X increases the number of new hires at X.*

We are also interested in the quality of those new hires. Here these forces interact subtlely. Proposition 5, part 2, suggests that average hire quality goes up because the hiring pool improves, while Proposition 6, part 2, suggests that it goes down because one needs to dig deeper into it, at the focal university when it switches. In order to capture the former effect, we need to empirically control for the latter effect:

Hypothesis 2: *Switching to English at X increases the average abitlity of new hires at X, when controlling for the time-varying number of new hires.*

Finally, parts 3 of both Propositions 5 and 6, suggest that we should observe a decrease in average quality of faculty at all (not necessarily each) universities below the switching university:

Hypothesis 3: *Switching to English at X decreases the average ability of new hires across all universities ranked below X.*[7]

It should also be noted that the above Hypotheses 2 and 3 describe the average effect of a change at the university level. They do not imply that the average quality of any subgroup within a university will move in the same direction. For example, suppose a group of faculty at University X has attribute Y, and the remainder do not, and (by coincidence, at University X) faculty with attribute Y have higher average ability than those who do not. When University X relaxes its skill constraint, it happens to attract a new faculty member with attribute Y who is less talented than the existing faculty with attribute Y but displaces one without. Then, notice, at University X, (1) the overall average ability increases, (2) the average ability of faculty with attribute Y decreases, and (3) the average ability of faculty without increases.

[7] In this preliminary version of the paper we do not yet test Hypothesis 3.

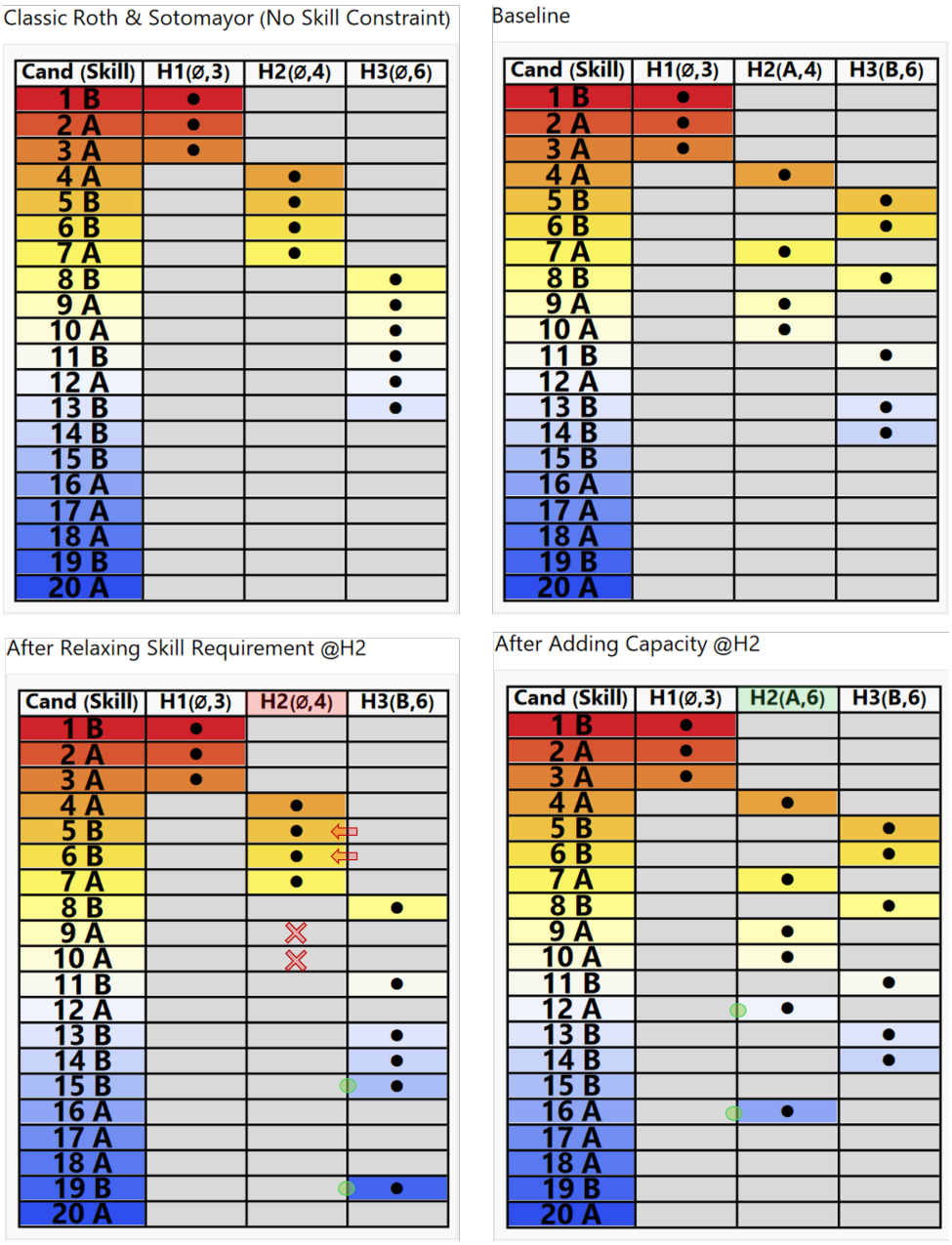


Figure 2: The four panels depict the stable matching of job candidates (listed by decreasing common desirability in rows) to employers (listed by decreasing common desirability in columns). Each of the 20 candidates speaks the *lingua franca* and an additional language, either 'A' or 'B'. Each employer (H1, H2, or H3) requires a specific language (A or B) or has no language requirement (denoted $\emptyset$), i.e., operates in the *lingua franca*, and has a fixed employee capacity (in header). The upper left panel depicts the unique stable matching equilibrium in a setting where no employer has a language requirement. The upper right panel introduces language constraints at H2 and H3. The lower left panel illustrates the effect of H2 relaxing its language constraint. The lower right panel illustrates the effect of H2, instead, relaxing its capacity constraint.

Example 7 *Figure 2 illustrates the effects on average quality of worldwide universities following one of them first offering a degree taught in the* lingua franca*. The four panels depict the stable matching of job candidates (listed by decreasing common desirability in rows) to employers (listed by decreasing common desirability in columns). Each of the 20 candidates speaks the* lingua franca *and an additional language, either 'A' or 'B'. Each employer (H1, H2, or H3) requires a specific language (A or B) or has no language requirement ∅, i.e., operates in the* lingua franca*, and has a fixed employee capacity. These are listed in the header. E.g. in all panels, H1 has no language requirement and a capacity of three. The upper left panel depicts the unique stable matching equilibrium in a setting where no employer has a language requirement. This is the classic many-to-one matching scenario described by Roth and Sotomayor (1992)–the top candidates go to the top employers, until the three slots are filled and then begin filling the next most desirable slots and so on. The upper right panel introduces language constraints at H2 and H3. This is representative of the pre-treatment state of our sample of treated universities. When a European university first offers a degree program taught in English, two simultaneous changes are plausible: (1) language constraints on faculty are dropped, and (2) due to increased student demand, the faculty capacity of the university increases. The lower left panel illustrates the effect of (1): When H2 relaxes its language constraint, nothing changes at top university H1, but the best two candidates at H3 (i.e., 5 and 6) move to H2, and the worst two candidates at H2 (i.e., 9 and 10) lose their jobs. Notice that average ability in H2 improves, but that the new arrivals could have, in principle, arrived anywhere in the distribution above the q_e-th ranked incumbent worker (here, 9)—this is why it is impossible to make predictions on whether any subgroup, like domestic or foreign, improve or deteriorate on average. The lower right panel illustrates the effect of (2): When H2 instead relaxes its capacity constraint, nothing changes at top university H1, but H2 picks up two less desirable unemployed A speakers (i.e., 12 and 16). These might also have come from a less desirable university.*

3 Identification

We want to identify the effect of a university's introduction of English as a teaching language (specifically, the first business or economics degree program taught entirely in English), on the research productivity of its new hires, as well as on the internationality of its new hires. But the decision to switch to English is unlikely to be random. For instance, a switch could be more likely if the university's native language is closer to English (Cao et al., 2024). Better-managed or more research-intensive universities could be more likely to switch. Universities in countries that are more attractive to foreign students or staff might be more likely to switch. And universities that recently struggled to attract domestic students or faculty could be more likely to switch.

To nevertheless identify a causal effect we use a difference-in-differences (DiD) model that compares the difference between pre- and post-treatment outcomes for the switching universities to that of a control group. Even though actual assignment to the treatment is not random, the model allows for the identification of the effect conditional on unobserved time-invariant effects and observed control variables; identification is as-good-as-random.

We choose a model suitable for our design (data generating process). In particular, different universities are treated, i.e., switch to English, at different dates. The model allows the treatment effects to be different for different treatment cohorts and post-treatment years. Identification requires two assumptions: treated units cannot change the outcome prior to treatment (no anticipation) and had treated units not been treated their outcomes would have been the same as those of the control group (common trends). The latter assumption might be more plausible for some control groups than for others. However, different trends between groups can be modeled.

The treatment group includes Dutch, Belgian, Danish, German, and Spanish universities. 30 universities had switched to English by the end of the sample. We use the list of top US economics departments by Amir and Knauff (2008) as our control group and also include not-yet treated universities. Our theoretical model shows that a switch at the focal university

also changes the ability of new hires at all lower ranked universities but not at higher ranked universities. Our control group outranks almost all of our treated universities. The few exemptions include, for instance, Toulouse. For the event-study design, used to test the common trends assumption, we exclude not-yet treated departments from the control group as this strengthens the test.

There are other potential control groups, for instance European universities from the same countries as the treated universities. However, it seems possible that there could be spillover effects, due to, for instance, agglomeration or reputation, to these universities. Such spillovers would cause the trends of the control and counterfactual to be different. Also, for European universities (including the UK) due to geographic proximity, hiring might be zero sum. That is, an Italian academic instead of moving to a UK university might move to one of the switching continental universities. Spillovers and crowding out seem less likely for top US universities. And globally, due to the large supply of doctoral graduates, it seems unlikely that hiring is zero sum except for the most experienced scientists (Kwon, 2025). Figure 3 plots the number of annual US PhD graduates in Business and Economics. It is unlikely that supply decreased in other countries, and so global supply of potential academics has increased substantially over our sample period.

However, publishing in the top journals might be zero-sum and crowding out might violate the common trends assumption. Suppose that after the switch treated universities take some of the journal space that would otherwise have gone to the control group. The control group might do worse than the true counterfactual and we overestimate the effect. However, Figure 1 suggests that treatment does not seem to cause a US publication counts (for new hires) to deviate from trend. Our model identifies the difference-in-differences at the cohort level, which makes it less likely that any specific unobserved factor correlates with our treatment for all cohorts.

We conduct a placebo test by randomising the treatment year at the university level. This test provides assurance that any identified treatment effects are not spurious. Specifically, for each treated university we draw a random treatment date and repeat this five hundred times.[8]

[8] See Appendix B for details of randomisation.

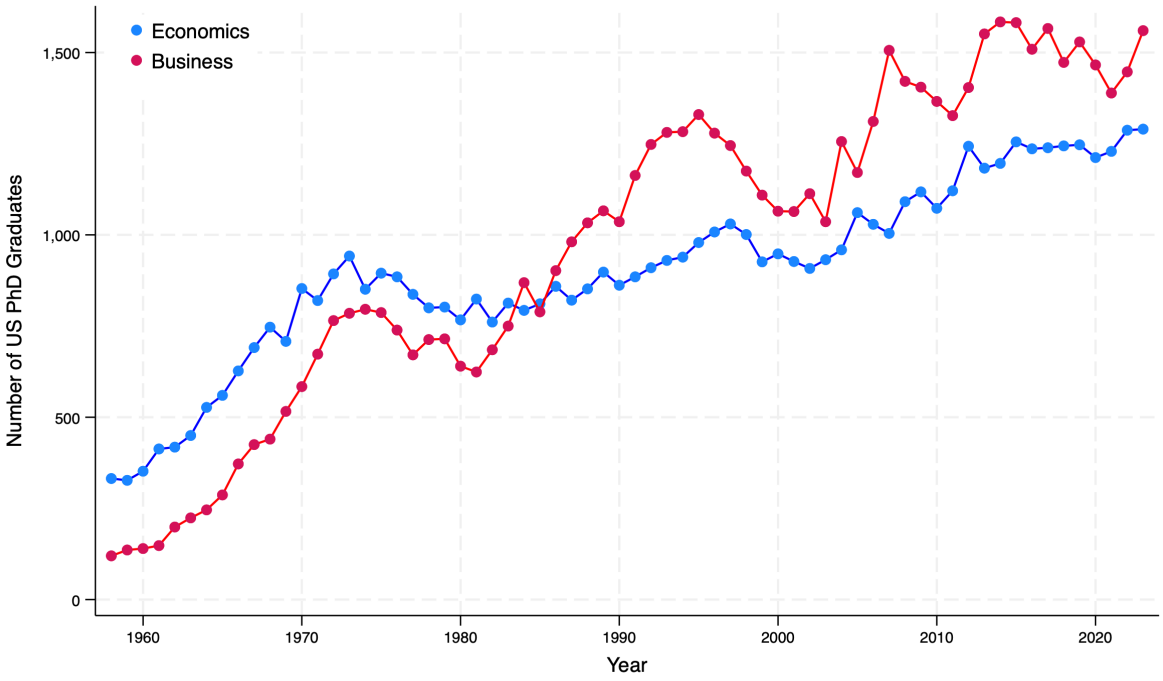


Figure 3: Number of Business and Economics PhD graduates in the US

Note: The graph plots the number of annual US PhD graduates in Business and Economics (National Center for Science and Engineering Statistics (NCSES), 2024).

The next section gives details of the empirical model which the casual reader might skip.

3.1 Model details

Our model is the extended two-way-fixed-effects model proposed by Wooldridge (2021). It is well known that with a staggered design the model needs to avoid "bad" comparisons, e.g., comparing treated units to already-treated ones. The model does this by essentially identifying two-by-two difference-in-differences for each cohort and each post-treatment period. An important advantage of this model is that it can be estimated by Ordinary Least Squares (OLS).[9] Using the notation of Wooldridge (2021) the model identifies the average treatment effects on the treated (ATT) as follows:

$$ATT_t(r) = \mathbb{E}\{Y_t(r) - Y_t(\infty)|D_r = 1, r = q, ..., T\}. \tag{1}$$

where r indicates treatment cohorts which are identified by their treatment year where q is the first possible treatment year and T the last: $r \in \{q, q+1, ..., T\}$. Never treated units also form a cohort and each university belongs to one cohort only. Figure 4 below illustrates the design. In 2000 Groningen and Wageningen switched. They form the 2000 cohort. This cohort has treatment effects for each year after 2000. $Y_t(r)$ is the potential outcome in year t for cohort first treated in year $t = r$. $Y(\infty)$ in the potential outcome for the never-treated cohort. D_r is the treatment indicator. Identification of the treatment effects requires two assumptions. First: no anticipation. As the potential outcomes depend on future treatment, units must not change their behaviour in a way that changes outcomes in anticipation. We assume $ATT = 0$ for $t < r$. Second: common (or parallel) trends. That is, the differences in potential outcomes for the treatment and control groups are the same. Note that there are as many treatment effects as there are cohorts and post-treatment periods. The regression model that identifies these

[9] We assume a liner probability model where relevant.

treatment effects is as follows:

$$Y_{it}^{\omega} = \alpha + \sum_{r=q}^{T} \beta_r D_r + \sum_{s=2}^{T} \gamma_t f s_t + \sum_{r=q}^{T} \sum_{s=r}^{T} \eta_r \left(D_r \times f s_t\right) + \sum_{r=q}^{T} \delta_{r,t} \left(D_r \times t\right) \tag{2}$$

where i identifies universities. D_r are cohort fixed effects. $f s_t, t = 2, ..., T$ are year fixed effects with $f s_t = 1$ if $s = t$ and $f s_t = 0$ if $s \neq t$. For identification, we drop the indicator for the first period. $D_r \times f s_t$ are the treatment indicators. There are separate treatment effects η for each cohort and post-treatment period.[10] As a robustness test we also include cohort-specific time trends $D_r \times t$. These condition treatment, but also absorb any trends in the treatment effects.

Treatment effects can be conditioned further on observables. We include a set of observed university-level X_{it} and country-level X_{ct} control variables (not shown in the equation above). The university-level controls are university size (number of faculty) and the average academic age of new hires. Age is defined from the date of first publication. As these control variables might be affected by the treatment, we set them equal to their pre-treatment means post treatment (Wooldridge, 2021) unless we want to specifically control for one of them to isolate a particular mechanism.

We also include five country-level controls: R&D expenditure in higher education as a proxy for research funding, average wage as a proxy for academic wages,[11] GDP per capita, and exchange rate – national currency per US dollar –, and the population aged 20-24 as a proxy for (domestic) demand. We let these vary with time throughout our sample as the behaviour of individual universities would hardly affect them.

As a robustness check we also add cohort-specific trends to one specification.

We estimate equation 2 for different dependent variables and sub-samples as indicated by ω. The baseline outcome Y_{it} is the total productivity, comprising quantity and quality, of all new

[10] For estimation we use the user-written Stata command jwdid.

[11] There might be more accurate measures of academic wages today but these data do not go back to the beginning of our sample.

hires. To better understand the mechanisms, we use four components of total productivity as outcomes:[12] the number of university i's newly-hired foreign or domestic staff and the average ability of foreign or domestic staff. These different outcome variables are defined in the next section.

Treatment effects can be aggregated in different ways, also across regression models, e.g. the ratio of new foreign to domestic hires. The delta-method (see Appendix C) allows for aggregation as well as inference for these aggregates.

4 Data & Variables

Our analysis relies on two data sources: (1) bibliographic data from SCOPUS[13], and (2) our own survey of continental European universities for the dates on which they first switched to English.

We surveyed 49 research-intensive universities in the Netherlands, Belgium, Denmark, Germany, and Spain, on their switching dates. We also include the Toulouse School of Economics (TSE), as its switching date is publicly known (Kuper, 2015). Except for Spain, we surveyed the population of research-intensive universities.[14] We focus on research-intensive universities because the outcome of interest is research performance. The survey question was: 'When did the first Business or Economics program switch to English?'[15] In reality, the introduction of English was certainly gradual at our switching universities but it seems impossible to record the entire sequence of switching dates. We surveyed 'the first' switching program because: (1) most universities do not seem to have kept official records on teaching

[12] Instead of estimating the total effect the product rule allows the correct aggregation of the four separate effects.

[13] The SCOPUS database is a commercial Elsevier product: https://www.scopus.com/home.uri

[14] According to the Universities of The Netherlands (UNL), there are 14 research-intensive Dutch universities. Other countries have no official list of research-intensive universities and we survey those in the top-1000 of the Academic Ranking of World Universities (ARWU).

[15] We identify the first switching program as, for instance, Erasmus University Rotterdam had its Bachelor's International Business Administration switch to English in 2001 and its Bachelor's Business and Economics in 2007. We take 2001 as its treatment year.

language[16] and the first might be better remembered, and (2) this should be sufficient for the mechanism. Reasonably common pre-trends suggest there was little prior use of English-language teaching. Any increase in the intensity in English-teaching is captured by the post-treatment heterogeneity in our model. Also, we surveyed at the program level, while the department might have introduced English-taught courses before the program switching to English. Again, this would lead us to underestimate the effect. We focus on business and economics, because it allowed us to rely on our personal networks, which yielded a very high response rate — about 71 per cent of the contacted universities. It might be possible though that relying on personal contacts increases the probability of a response for universities that benefited more from a switch and thus leads to an upward bias of the effect.

The timeline below (and Table A1) shows switches for 30 universities — 9 Dutch, 5 Belgian, 1 Danish, 10 German, 1 French, and 4 Spanish — between 1985 and 2021. Maastricht University had its first Business bachelor's switching in 1993. University of Amsterdam followed in 1997. Belgian law only permits an official English program if the same program is also offered in Flemish (in Flanders) or French (in Wallonia). Hence, among our sample of Belgian universities, only Katholieke Universiteit Leuven (KU Leuven) has a bachelor's program completely in English.[17] Université de Namur's (UNamur) in 2001 launched its research master's, the first English master's program in Belgium. All of German universities in our sample switched after 2005.

[16]We could not find administrative data (at universities or other organizations) for the dates when European universities switched teaching to English.

[17]Université Libre de Bruxelles provides some fraction of the courses in English. HEC Liège provides the opportunity to apply for a special bachelor's final year full in English. Université catholique de Louvain (UCLouvain) has the bachelor's program in French for the first two years and in English for the last year.

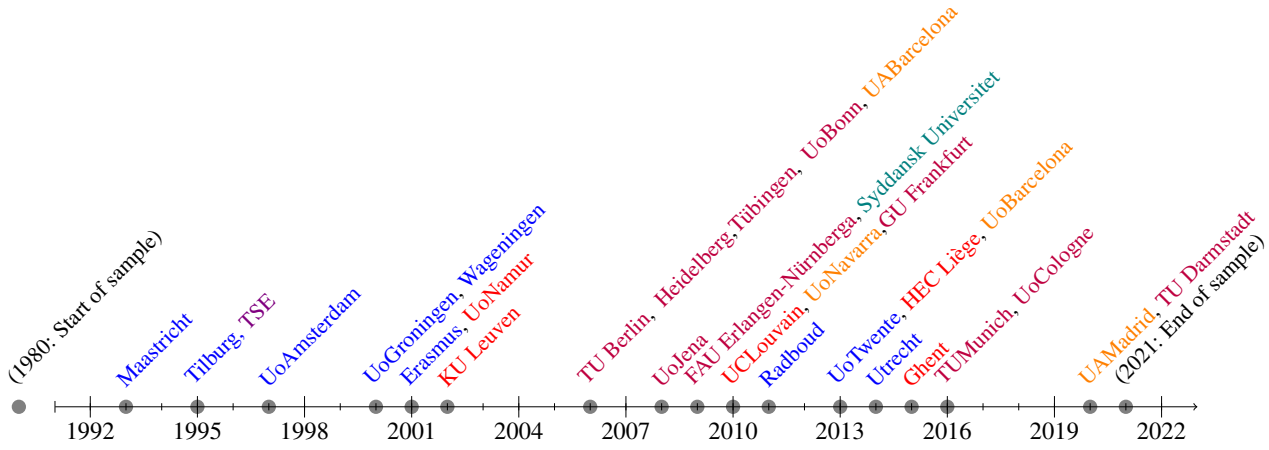


Figure 4: Treatment timeline

We use publication data from Scopus, a commercial, source-neutral, abstract and citation database, to construct our proxies for research productivity.

To measure the total productivity of new hires at the university level, we proceed in two steps. First, we define the average annual research productivity of individual h over the period $\left[\underline{t}, \bar{t}\right]$ as follows:

$$\theta_{h,\underline{t}}^{\kappa,\bar{t}} = \frac{1}{\bar{t} - \underline{t} + 1} \sum_{\tau=\underline{t}}^{\bar{t}} \sum_{k \in P_{h,\tau}} 1\left\{k \in Cat1\right\} + \kappa \times 1\left\{k \in Cat2\right\} \tag{3}$$

where $1\{\bullet\}$ denotes the indicator function, $\kappa \in [0,1]$ is a weighting factor on Category 2 publications (Category 1 publications always receive full weight and Categories 3 and 4 zero), and $P_{h,\tau}$ is the set of all publications by individual h in year τ. $\underline{t}$ is the first year of publication and $\bar{t}$ is either two years after joining a university or the year of the last publication. Both measures are imperfect. They are proxies. The career length measure is more likely to capture ability for individuals where publications only reveal it late, which might be true for many social science scholars (Jones, 2010; Weinberg & Galenson, 2019). However, the career length measure also captures any effects of working at a switching university as opposed to being hired. We sometimes refer to this as the "treatment" effect. As we do not observe individuals before they publish, we omit very junior hires. Our two proxies might also be affected by the truncation of our sample when publication quality is non-stationary, e.g., the well-documented Matthew effect. The new hires of early switchers have a shorter observed publication history and therefore might be in a later career stage with a higher yearly publication average. On the other hand, the late switchers' new hires have shorter ex-post periods and therefore might be more

likely to be in an early career stage potentially biasing ability downwards. We can use the time-series heterogeneity of the treatment effects as a diagnostic for the potential importance of these biasses. The publication categorisation is from Comité National de la Recherche Scientifique (2020).[18] Note that strictly speaking it is not the university, but (virtual) department level. Publications in the relevant economics and business journals define our virtual department. We use "university" and "department" interchangeably.

Second, we aggregate this productivity $\theta_{h,\underline{t}}^{\kappa,\bar{t}}$ over various subsets of university i's new hires. We count all new hires including those identified by publication lower ranked than Cat2, i.e., the number of new hires does not depend on the ability definition. Let $\Omega \equiv \{AllNewHires, DomesticNewHires, ForeignNewHires\}$ denote the set of non-exclusive, focal demographic groups for our analyses. Then the *total* productivity of university $i's$ new hires in year t by group $\omega \in \Omega$ is given by:

$$\Theta_{it}^{\omega} \equiv \sum_{h \in H_{it}^{\omega}} \theta_{h,\underline{t}}^{\kappa,\bar{t}} = \bar{\Theta}_{it}^{\omega} N_{it} \,, \tag{4}$$

where H_{it}^{ω} denotes the set of university i's researchers from demographic ω in year t. By university we mean a virtual economics and business department or academe. We use university and department interchangeably. The *average* productivity of individuals in the focal group is

$$\bar{\Theta}_{it}^{\omega} \equiv \frac{1}{|H_{it}^{\omega}|} \sum_{h \in H_{it}^{\omega}} \theta_{h,\underline{t}}^{\kappa,\bar{t}} \,, \tag{5}$$

and the *number of individuals* in the focal group is

$$N_{it}^{\omega} \equiv |H_{it}^{\omega}| \ . \tag{6}$$

These metrics are the dependent variables $Y_{it} \in \left\{\Theta_{it}^{\omega}, \bar{\Theta}_{it}^{\omega}, N_{it}^{\omega}\right\}$ in our estimates of the effect of switching to English. Note that we assume perfect substitutability when aggregating individual abilities. Even though academic ability might be multi-dimensional top publications are a salient proxy. Certainly identification would be threatened if recruitment criteria changed with the switch to English, e.g. if top publications became more important we would overestimate

[18] We assume Category 1 journals are top journals.

the true effect.

We identify international staff by family name's nationality. We use the information from Forebears (2022), which is a genealogy portal providing the national population with a particular surname. We proxy nationality as the nation with highest population share for a specific surname. We then identify staff as international if their surname's nationality is not that of the focual country.[19] Where last name is missing in SCOPUS we use a change in affiliation. Note that this definition is not entirely consistent with our theoretical definition, which relies on language skill only. In the theory a foreigner is somebody that does not speak the local language but of course there might be academics with Italian names in France that do speak French.

We include all journal articles in Business and Economics published by a university's faculty, querying by university and subject area.[20] We trace an author's mobility across universities by publication year and author affiliation information, interpreting a change in primary affiliation as a job move. We collect the publication data for the surveyed Dutch, Belgian, Danish, German, and Spanish universities, and 33 universities in the United States listed by Amir and Knauff (2008).

Table 1 gives the variable descriptions and Table 2 gives the summary statistics for treated and control universities. The first four rows give our different ability proxies. Average ability is higher for our control, i.e., US universities. Control universities are larger measured by number of staff. We identify staff via publication affiliations, i.e., we treat an author as an institution's staff from the date of their first publication through the date of their last publication listing in that institution.

[19] This proxy might cause an overestimation of international staff impact.

[20] For each document listed, the Scopus (*www.scopus.com*) extract provides the document title, ISSN number, citation counts, affiliation name and ID, published year, author name and ID (up to ten), subject area, document type (article, conference paper, review, etc.), source title, source type (journal, book, etc.), and publication stage (final or article in press). Business and Economics subject areas are denoted 'Business, Management and Accounting' and 'Economics, Econometrics and Finance' in Scopus respectively. We include publications by both incumbent and previous staff. This should not bias the estimator as long as the staff is considered as 'new' in that particular year.

Table 1: Variable Descriptions

Variable	Description	Source
Top Pubs over Career	The sum of university i's year t hires' career productivity (i.e., annual average publications in CNRS Category 1 journals)	*www.scopus.com*
Top Pubs at Hire	The sum of university i's year t hires' productivity (i.e., annual average publications in CNRS Category 1 journals) up to 2 years after joining	*www.scopus.com*
Good Pubs over Career	The sum of university i's year t hires' career productivity (i.e., annual average publications in CNRS Category 1 and 0.5 * Category 2 journals)	*www.scopus.com*
Good Pubs at Hire	The sum of university i's year t hires' productivity (i.e., annual average publications in CNRS Category 1 and 0.5 * Category 2 journals) up to 2 years after joining	*www.scopus.com*
Number of staff	The number of university i's staff in year t	*www.scopus.com*
Population aged 20-24	Population aged between 20 and 24, as a proxy for university's students size	*data.worldbank.org*
Wage	Average annual wages per employee in full-time equivalent unit in the total economy, converted in US Dollar Purchasing Power Parity (USD PPPs)	*data-explorer.oecd.org*
GDP per capita	Gross domestic product divided by population at NUTS3 (Nomenclature of territorial units for statistics) level	*ec.europa.eu/eurostat, data.worldbank.org*
Higher education (HE) research funding	Gross domestic expenditure on R&D (GERD) at national level higher education	*data-explorer.oecd.org, www.aaas.org*
Exchange rate	National currency per US dollar	*ec.europa.eu/eurostat, data.worldbank.org*

Table 2: Summary Statistics

	Treated					*Control*				
Variable	***N***	***Mean***	***SD***	***Min***	***Max***	***N***	***Mean***	***SD***	***Min***	***Max***
Top Pubs over Career	1110	3.04	3.72	0.00	28.67	1221	6.91	5.25	0.00	30.60
Top Pubs at Hire	1110	2.30	2.94	0.00	19.94	1221	3.05	2.97	0.00	21.37
Good Pubs over Career	1110	5.05	5.78	0.00	37.70	1221	8.61	6.18	0.00	35.82
Good Pubs at Hire	1110	3.81	4.51	0.00	28.80	1221	3.84	3.48	0.00	23.63
Number of staff	1110	85.48	86.60	1.00	507.00	1221	126.72	79.63	10.00	357.00
Population aged 20-24 (thousands)	1110	2701.84	1986.59	2919.36	6763.00	1221	20653.24	1450.55	18210.18	22829.43
Wage (USD PPPs)	1110	64211.99	9401.91	45417.77	80537.96	1221	65589.20	9014.72	51677.11	84211.21
GDP per capita (thousand euro)	1110	33.95	15.52	8.10	79.68	1221	37.61	13.65	16.78	65.37
HE research funding (billion euro)	1110	5.01	4.85	0.23	20.66	1221	9.98	4.64	4.79	17.68
Exchange rate	1110	1.05	1.04	0.61	10.60	1221	1.00	0.00	1.00	1.00

Wage data starts from 1990. We use GDP per capita to predict wage between 1985 and 1989. We check the wage trend for each country before prediction to ensure that there is no abnormal data structure.

5 Results

First, we depict our regression results in event study graphs. Figure 5 plots cohort and event-time aggregated treatment effects relative to the period just before treatment. The outcome is new hires' total productivity measured using top publications over full-career length. To identify pre-treatment effects, this model uses only never-treated observations as the control group. Also, we do not estimate treatment effects for our entire time series. This model adds university size as the only covariate. The latter is made time-invariant post-treatment.

At treatment, there is a clear level increase in new hires' total productivity. The effects increase in event-time and peak roughly two decades after the initial treatment. The increase could be due to universities expanding English language teaching or the effect becoming stronger, through, for instance, increasing reputation. Pre-treatment trends are very similar, which we take as evidence that the common trends assumption holds.

How much of the total effect is due to a change in numbers and how much is due to a change in average ability? Figure 6 shows the same treatment effects for the number of new hires (international and domestic). The pattern is very similar to that of the overall effect suggesting that it is largely driven by changes in the number of new hires. It seems that the switch to English drives the expansion of these treated universities. There is a slight upward trend in the number of new hires before treatment. It is possible that the switch to English was part of a wider expansion and internationalization strategy. In some of our model we include cohort-trends to further condition treatment.

What was the expansion of our treated European universities relative to US universities? Figure 7 plots the time series for the average size of control and treated universities. We see that at the start of our sample European economics and business departments are much smaller but converge in size with US departments at the end of our sample. It seems that the switch to English was an important driver for this expansion. Assuming that US universities have been operating at an optimal scale, European universities expanded towards that scale.

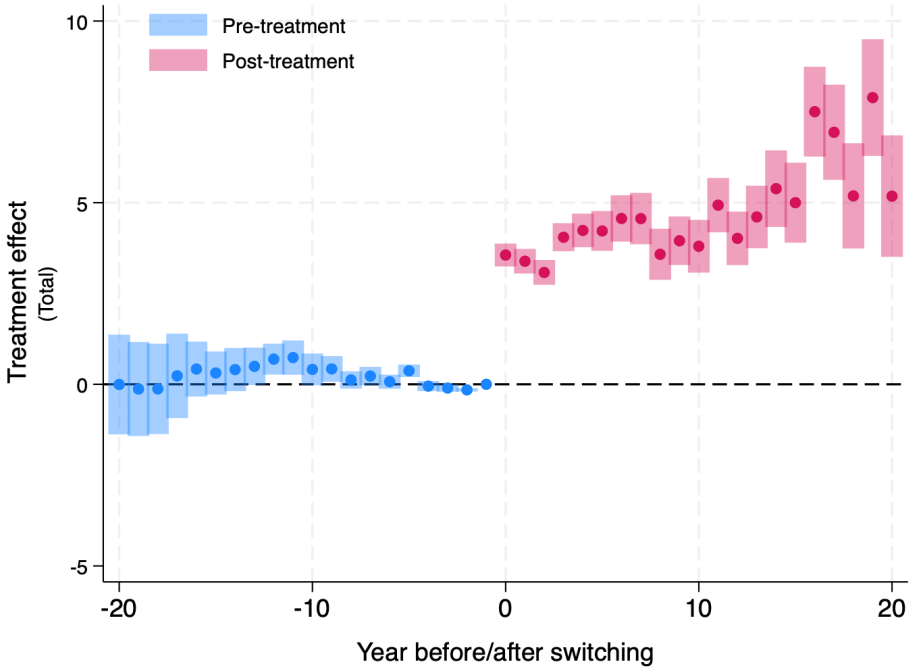


Figure 5: Yearly Treatment Effects of Switch on New Hires' Research Productivity

Note: The graph plots the yearly treatment effects of switching to English on research productivity of new staff, using the event study in the ETWFE estimator. Research productivity of new staff increases since switching to English.

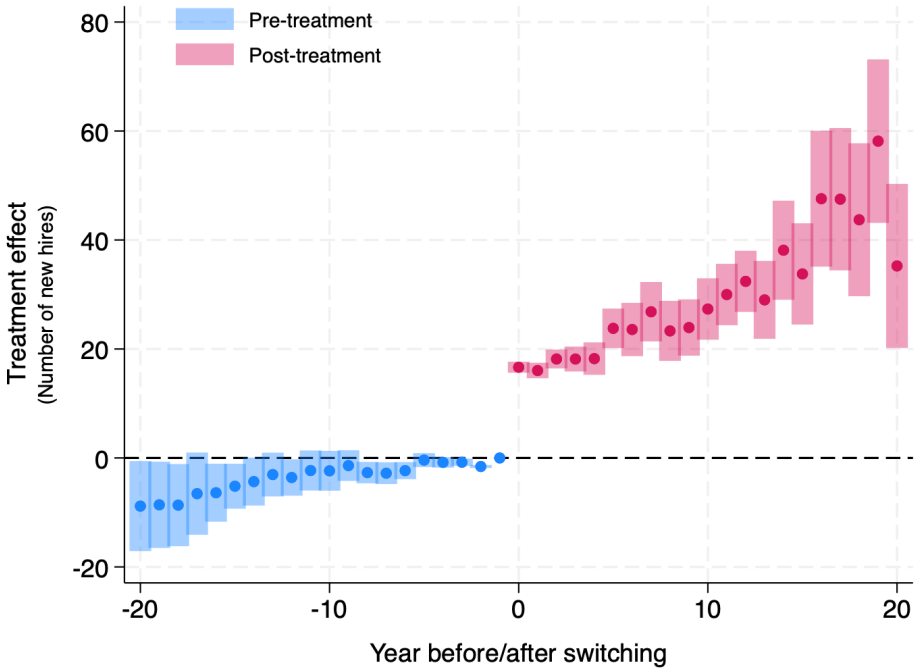


Figure 6: Yearly Treatment Effects of Switch on New Hires' Quantity

Note: The graph plots the yearly treatment effects of switching to English on the number of new hires, using the event study in the ETWFE estimator. The number of new hires increases since switching to English.

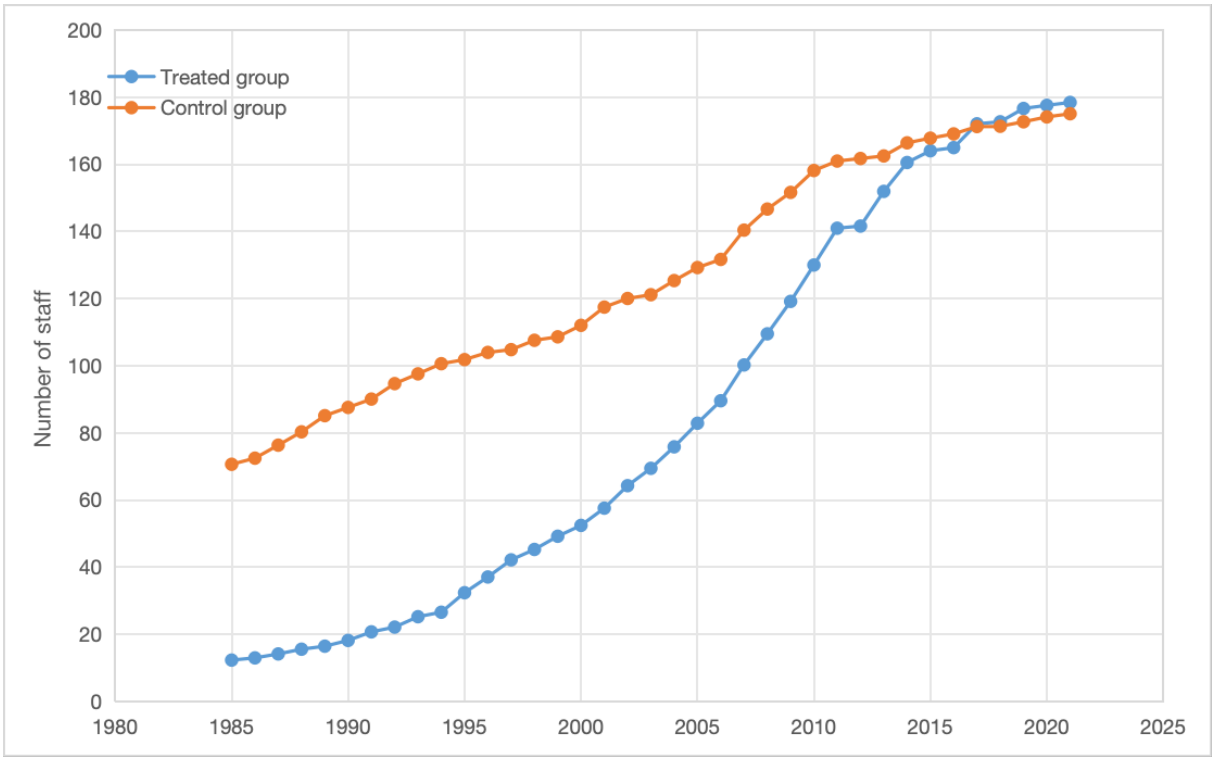


Figure 7: Average department size (number of staff) over time

Note: The graph plots average department size proxied by number of staff for treated and control group over time.

Figure 8 gives the event study for the new hires' average ability. At impact there is no effect but after about three years the effects are mostly larger than zero. Throughout the effects are volatile. The quality of new hires might be lumpy due to, for instance, the need to recruit for specific teaching needs. Again, there is some evidence for a pre-treatment increase in the quality of new hires.

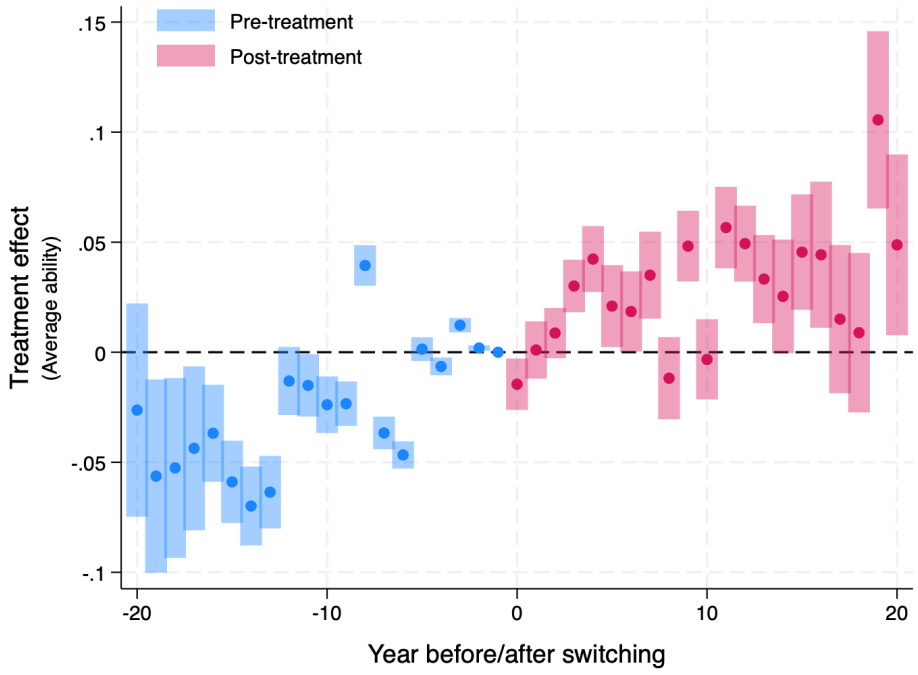


Figure 8: Yearly Treatment Effects of Switch on New Hires' Quality

Note: The graph plots the yearly treatment effects of switching to English on average ability of new hires, using the event study in the ETWFE estimator.

Next, we show more detailed regression results. Our most important theoretical prediction is that after the switch to English average ability of new hires, conditional on their number, increases. The results are in Table 3. Coefficient estimates are averages over all cohort and year-specific treatment effects. All models control for department size, hires' average academic age, and number of new hires as well as several country-level factors including average wage to control for monetary drivers of migration (Borjas, 1989). Whereas Column 1 is for all new hires, columns 2 and 3 are for the sub-samples of foreign and domestic new hires, respectively. As predicted, the switch to English increases the average ability of new hires. It also does so for foreign and domestic hires, separately.

But there are no theoretical predictions for the averages of these sub-samples. Note that our theory assumes that the language constraint is binding, but in reality universities hired foreigners before the switch. This could be because foreigners learned the domestic language or where hired for research only.

The effects are economically and statistically significant. For the entire sample, average ability of new hires increases by about one third.

The event study graphs above show that pre-trends are not necessarily common. In Table 4 we also add cohort-specific trends to condition treatment effects further. Column 1 shows that the effect is still positive and larger. The switch now increases average ability by more than 40 per cent. Note that the sign for foreign new hires is now negative, but again we have no prediction for this sub-sample. Empirically, it could be that after the switch universities hire foreigners primarily for their English as opposed to research skills. Before the switch foreigners might have been hired exclusively for their research skills and possibly been exempt from teaching. The average ability of domestic hires increases with the switch.

Table 3: Switching to English Drives New Hires' Productivity
(Top Pubs over Career)

	(1) *All*	(2) *Foreign*	(3) *Domestic*
Dependent Variable	Average ability	Average ability	Average ability
Switch to English (==1)	0.037*	0.043*	0.073***
	(0.018)	(0.018)	(0.021)
Department size (time-invariant)	Yes	Yes	Yes
Academic age (time-invariant)	Yes	Yes	Yes
New hires number (time-varying)	Yes	Yes	Yes
Country-level factors	Yes	Yes	Yes
Cohort trend	No	No	No
CVs#cohort dummy	No	No	No
CVs#time dummy	No	No	No
Cohort FEs	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes
Pre-treatment DV treated	0.132	0.110	0.118
Observations	2322	2322	2322

* p <0.05, ** p <0.01, *** p <0.001, standard errors clustered at the cohort level. Panel is 1985 to 2021. There are 0.2% missing values due to no observed publications in some years, which we replace with 0.

Dependent variables from column (1) to (3) are all new hires' average publications in Cat1 journals over the career length, the average of foreign new hires' publications in Cat1 journals over the career length, and the average of domestic new hires' publications in Cat1 journals over the career length. We control for the number of new hires to isolate the ability effect conditional on the number of new hires. We control for the number of staff as a proxy for the university's department size, and the average academic age of incumbent staff, making them time-invariant after the treatment by taking the pre-treatment average. We control for country-level macro factors, i.e., population aged 20-24, average wage, higher education research funding, and GDP per capita. Mean of dependent variable only includes samples in the treated countries. Control group consists of listed US universities by Amir and Knauff (2008).

Table 4: Switching to English Drives New Hires' Productivity
(Top Pubs over Career)

	(1) *All*	(2) *Foreign*	(3) *Domestic*
Dependent Variable	Average ability	Average ability	Average ability
Switch to English (==1)	0.057**	-0.058**	0.068**
	(0.021)	(0.019)	(0.022)
Department size (time-invariant)	Yes	Yes	Yes
Academic age (time-invariant)	Yes	Yes	Yes
New hires number (time-varying)	Yes	Yes	Yes
Country-level factors	Yes	Yes	Yes
Cohort trend	Yes	Yes	Yes
CVs#cohort dummy	No	No	No
CVs#time dummy	No	No	No
Cohort FEs	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes
Pre-treatment DV treated	0.132	0.110	0.118
Observations	2322	2322	2322

* p <0.05, ** p <0.01, *** p <0.001, standard errors clustered at the cohort level. Panel is 1985 to 2021. There are 0.2% missing values due to no observed publications in some years, which we replace with 0.

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We now turn to a more detailed comparison of the number and average ability of new hires. Table 5 gives the effects for total productivity (column 1) as well as numbers (columns 2 and 4) and average abilities (columns 3 and 5) as discussed in section 4. We no longer control for the number of new hires, but we control for university size throughout. We also provide separate results for the sub-samples of domestic and foreign hires. We use our preferred ability measure, where individual productivity is measured using top publications for full career length.

In almost all columns the effects are economically and statistically significant. Switching to English increase new hires' productivity by about 280 per cent compared to the pre-treatment mean. But what drives the effect? Columns 2 and 3 give the effect for the number and average ability of foreigners. The number of foreign new hires quadruples and the average ability increases by about 20 per cent.

Columns 3 and 4 provide the same breakdown for domestic new hires. Whereas numbers triple, average ability increases by about 30 per cent. Pre-treatment the proportion of new foreign hires is about 0.5 for the treated universities. We can use the results in Appendix C to calculate that the switch to English increases this by about 0.06 with standard error of about 0.01. That is, the switch to English increases the proportion of foreign new hires substantially.

Measuring ability for the career length potentially captures an "incentive" effect, too. That is, the effect of working now at an English-language teaching university (assuming new hires stay for ever). Table 6 shows results for the same regressions, but ability is measured up to hiring instead of (observed) career end. Note that the results for the number of new hires (columns 2 and 4) are unchanged because the different ability measure does not change the count of new hires. We print them only for comparison. The coefficient estimates for average ability are smaller but the changes relative to pre-treatment means are virtually the same. There is no evidence for any incentive effects.

So far we restricted our measure of ability to CNRS Category 1 ("Top Pubs") publications. But that might be too narrow and not provide sufficient statistical power. We now use an ability measure that adds Category 2 publications with a 0.5 weight. We refer to "Good Pubs". Table 7

has the regression results for an ability measure based on Good publications over career length. Column 1 shows that the increase in total productivity is about 270 per cent, slightly lower than for Top publications. The effects for the number of new hires (columns 2 and 4) is again unchanged. The effects on average ability for foreign and domestic hires change little: 23 and 31 per cent, respectively. Statistical significance is somewhat higher.

Finally, Table 8 uses Good publication up to hire as ability proxy. Column 1 shows that the total productivity increase is about 230 per cent. For foreign and domestic hires, average ability increases by just above 50 per cent. Both effects are statistically significant at 5 per cent. Again, there is little difference with the effects for the career-length ability proxy. If anything, there is a small positive incentive effect.

As discussed above, the switch to English was probably part of a wider internationalization program at European universities. To make sure that our treatment dates (and estimated effects) are not spurious, we perform a randomization test for the regressions in column 1 of Tables 3 and 5. For our estimated treatment effects we can easily reject the null of a random, i.e., zero, effect. The details are in Appendix B.

Table 5: Switching to English Drives New Hires' Productivity
(Top Pubs over Career)

	(1)	(2)	(3)	(4)	(5)
	Total	*Foreign*		*Domestic*	
Dependent Variable		Number	Average ability	Number	Average ability
Switch to English (==1)	4.433***	16.033***	0.022	11.293***	0.033**
	(0.280)	(1.699)	(0.014)	(1.405)	(0.012)
Department size (time-invariant)	Yes	Yes	Yes	Yes	Yes
Academic age (time-invariant)	Yes	Yes	Yes	Yes	Yes
Country-level factors	Yes	Yes	Yes	Yes	Yes
CVs#cohort dummy	No	No	No	No	No
CVs#time dummy	No	No	No	No	No
Cohort FEs	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes
Pre-treatment DV treated	1.556	5.888	0.110	5.766	0.118
Observations	2322	2322	2322	2322	2322

* p <0.05, ** p <0.01, *** p <0.001, standard errors clustered at the cohort level. Panel is 1985 to 2021. There are 0.2% missing values due to no observed publications from Scopus in some years, which we replace with 0.

Dependent variables from column (1) to (5) are all new hires' publications in Cat1 journals over the career length, the number of foreign new hires, the average of foreign new hires' publications in Cat1 journals over the career length, the number of domestic new hires, and the average of domestic new hires' publications in Cat1 journals over the career length. We control for the number of staff as a proxy for the university's department size, and the average academic age of incumbent staff, making them time-invariant after the treatment by taking the pre-treatment average. We control for country-level macro factors, i.e., population aged 20-24, average wage, higher education research funding, and GDP per capita. Mean of dependent variable only includes samples in the treated countries. Control group consists of listed US universities by Amir and Knauff (2008).

Table 6: Switching to English Drives New Hires' Productivity

(Top Pubs at Hire)

	(1)	(2)	(3)	(4)	(5)
	Total	*Foreign*		*Domestic*	
Dependent Variable		Number	Average ability	Number	Average ability
Switch to English (==1)	2.116***	16.033***	0.021*	11.293***	0.025**
	(0.192)	(1.699)	(0.010)	(1.405)	(0.009)
Department size (time-invariant)	Yes	Yes	Yes	Yes	Yes
Academic age (time-invariant)	Yes	Yes	Yes	Yes	Yes
Country-level factors	Yes	Yes	Yes	Yes	Yes
CVs#cohort dummy	No	No	No	No	No
CVs#time dummy	No	No	No	No	No
Cohort FEs	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes
Pre-treatment DV treated	1.220	5.888	0.089	5.766	0.087
Observations	2322	2322	2322	2322	2322

* p <0.05, ** p <0.01, *** p <0.001, standard errors clustered at the cohort level. Panel is 1985 to 2021. There are 0.2% missing values due to no observed publications from Scopus in some years, which we replace with 0.

Dependent variables from column (1) to (5) are all new hires' publications in Cat1 journals up to two years joining the university, the number of foreign new hires, the average of foreign new hires' publications in Cat1 journals up to two years joining the university, the number of domestic new hires, and the average of domestic new hires' publications in Cat1 journals up to two years joining the university. We control for the number of staff as a proxy for the university's department size, and the average academic age of incumbent staff, making them time-invariant after the treatment by taking the pre-treatment average. We control for country-level macro factors, i.e., population aged 20-24, average wage, GDP per capita, higher education research funding, and exchange rate. Mean of dependent variable only includes samples in the treated countries. Control group consists of listed US universities by Amir and Knauff (2008).

Table 7: Switching to English Drives New Hires' Productivity
(Good Pubs over Career)

	(1)	(2)	(3)	(4)	(5)
	Total	*Foreign*		*Domestic*	
Dependent Variable		Number	Average ability	Number	Average ability
Switch to English (==1)	7.018***	16.033***	0.040*	11.293***	0.059***
	(0.430)	(1.699)	(0.018)	(1.405)	(0.016)
Department size (time-invariant)	Yes	Yes	Yes	Yes	Yes
Academic age (time-invariant)	Yes	Yes	Yes	Yes	Yes
Country-level factors	Yes	Yes	Yes	Yes	Yes
CVs#cohort dummy	No	No	No	No	No
CVs#time dummy	No	No	No	No	No
Cohort FEs	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes
Pre-treatment DV treated	2.592	5.888	0.177	5.766	0.188
Observations	2322	2322	2322	2322	2322

* p <0.05, ** p <0.01, *** p <0.001, standard errors clustered at the cohort level. Panel is 1985 to 2021. There are 0.2% missing values due to no observed publications from Scopus in some years, which we replace with 0.

Dependent variables from column (1) to (5) are all new hires' publications in Cat1&0.5*Cat2 journals over the career length, the number of foreign new hires, the average of foreign new hires' publications in Cat1&0.5*Cat2 journals over the career length, the number of domestic new hires, and the average of domestic new hires' publications in Cat1&0.5*Cat2 journals over the career length. We control for the number of staff as a proxy for the university's department size, and the average academic age of incumbent staff, making them time-invariant after the treatment by taking the pre-treatment average. We control for country-level macro factors, i.e., population aged 20-24, average wage, GDP per capita, higher education research funding, and exchange rate. Mean of dependent variable only includes samples in the treated countries. Control group consists of listed US universities by Amir and Knauff (2008).

Table 8: Switching to English Drives New Hires' Productivity

(Good Pubs at Hire)

	(1)	(2)	(3)	(4)	(5)
	Total	*Foreign*		*Domestic*	
Dependent Variable		Number	Average ability	Number	Average ability
Switch to English (==1)	3.709***	16.033***	0.037**	11.293***	0.051***
	(0.375)	(1.699)	(0.012)	(1.405)	(0.011)
Department size (time-invariant)	Yes	Yes	Yes	Yes	Yes
Academic age (time-invariant)	Yes	Yes	Yes	Yes	Yes
Country-level factors	Yes	Yes	Yes	Yes	Yes
CVs#cohort dummy	No	No	No	No	No
CVs#time dummy	No	No	No	No	No
Cohort FEs	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes
Pre-treatment DV treated	2.019	5.888	0.140	5.766	0.140
Observations	2322	2322	2322	2322	2322

* p <0.05, ** p <0.01, *** p <0.001, standard errors clustered at the cohort level. Panel is 1985 to 2021. There are 0.2% missing values due to no observed publications from Scopus in some years, which we replace with 0.

Dependent variables from column (1) to (5) are all new hires' publications in Cat1&0.5*Cat2 journals up to two years joining the university, the number of foreign new hires, the average of foreign new hires' publications in Cat1&0.5*Cat2 journals up to two years joining the university, the number of domestic new hires, and the average of domestic new hires' publications in Cat1&0.5*Cat2 journals up to two years joining the university. We control for the number of staff as a proxy for the university's department size, and the average academic age of incumbent staff, making them time-invariant after the treatment by taking the pre-treatment average. We control for country-level macro factors, i.e., population aged 20-24, average wage, GDP per capita, higher education research funding, and exchange rate. Mean of dependent variable only includes samples in the treated countries. Control group consists of listed US universities by Amir and Knauff (2008).

6 Conclusion

We show that switching to English increases the number of new hires and their average ability at European universities, consistent with predictions from a theory of assortative matching. European universities expand their size to that of US peers and do so by hiring substantially more foreigners.

Our evidence suggests that this gain is due to reducing language barriers to academic research faculty's labour mobility. Attracting such global talent may accelerate knowledge diffusion and spur innovation. Policy makers in non-English speaking countries should weigh this gain against other priorities.

Although the choice of corporate language for a business firm need not be as discrete as switching the official language of a degree program, the same forces are likely at work. As businesses strategically choose to operate more in the *lingua franca*, the labour pool from which they draw becomes more globalised. Hence, we expect industries most constrained by local human capital supply to switch to English first.

In this work, we have considered only the partial impact on the switching university. If the mechanism is indeed mobility, then when a university switches there must also be competitive effects on universities in English-speaking countries that benefited from decades of intellectual cream-skimming, on universities in non-English countries that have previously made the switch and those who have chosen not to. We leave this for future research.

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Appendix

A English instruction list

Table A1: Timing of English Instruction

University	Country	Year	Source
Maastricht University	Netherlands	1993	Private communication
Tilburg University	Netherlands	1995	Private communication
University of Amsterdam	Netherlands	1997	Private communication
University of Groningen	Netherlands	2000	Private communication
Wageningen University Research	Netherlands	2000	Private communication
Erasmus University Rotterdam	Netherlands	2001	Private communication
Radboud University Nijmegen	Netherlands	2011	Private communication
University of Twente	Netherlands	2013	Private communication
Utrecht University	Netherlands	2014	Private communication
UNamur	Belgium	2001	Private communication
KU Leuven	Belgium	2002	Private communication
UCLouvain	Belgium	2010	Private communication
HEC Liège	Belgium	2013	Private communication
Ghent University	Belgium	2015	Private communication
Syddansk Universitet	Denmark	2009	Private communication
Technische Universität Berlin	Germany	2006	Private communication
Universität Heidelberg	Germany	2006	Private communication
Eberhard Karls Universität Tübingen	Germany	2006	Private communication
Universität Bonn	Germany	2006	Private communication
Friedrich-Schiller-Universität Jena	Germany	2008	Private communication
Friedrich-Alexander-Universität Erlangen-Nürnberg	Germany	2009	Private communication
Goethe-Universität Frankfurt am Main	Germany	2010	Private communication
Technical University of Munich	Germany	2016	Private communication
University of Cologne	Germany	2016	Private communication
Technische Universität Darmstadt	Germany	2020	Private communication
Universitat Autònoma de Barcelona	Spain	2006	Private communication
Universidad de Navarra	Spain	2010	Private communication
Universitat de Barcelona	Spain	2013	Private communication
Universidad Autónoma de Madrid	Spain	2020	Private communication

B Placebo Test

To assess whether our ETWFE results could be driven by spurious timing or serial correlation, we implement a placebo test based on randomizing the adoption timing among treated units. Specifically, for each university that was ever treated, we draw a placebo cohort year uniformly at random from its own observed support $[y_{\min,i}+1,\ y_{\max,i}]$, ensuring at least one pre-treatment period. We then construct a placebo path $D_{it}^{\mathrm{pl}} \ = \ 1\{\, t \geq g_i^{\mathrm{pl}} \,\}$ and re-estimate the model. We repeat the procedure 500 times and, from each run, record the overall ATT via *estat simple*. We plot a 50-bin histogram (y-axis in percent) with a normal fit and mark the baseline estimate (or a known true value) on the x-axis for comparison.

We randomise the treatment year as follows.

1. Support and placebo timing. For each treated university i,compute its observed time support endpoints $(y_{\min,i}, y_{\max,i})$, and draw a placebo cohort (first-treatment) year uniformly from that support (ensuring at least one pre-treatment period):

$$g_i^{\mathrm{pl}} \sim \mathrm{Unif}\{\, y_{\min,i}+1, \ldots, y_{\max,i} \,\}.$$

Construct the placebo treatment path as the indicator

$$D_{it}^{\mathrm{pl}} \ = \ \mathbb{1}\{\, t \geq g_i^{\mathrm{pl}} \,\}.$$

2. Re-estimate the model, recording the overall ATT via *estat simple*.

3. Repeat 500 times. Plot the empirical distribution (50 bins, percent y-axis) with a normal fit. Optionally mark the baseline estimate/true value on the x-axis and report its percentile within the placebo distribution.

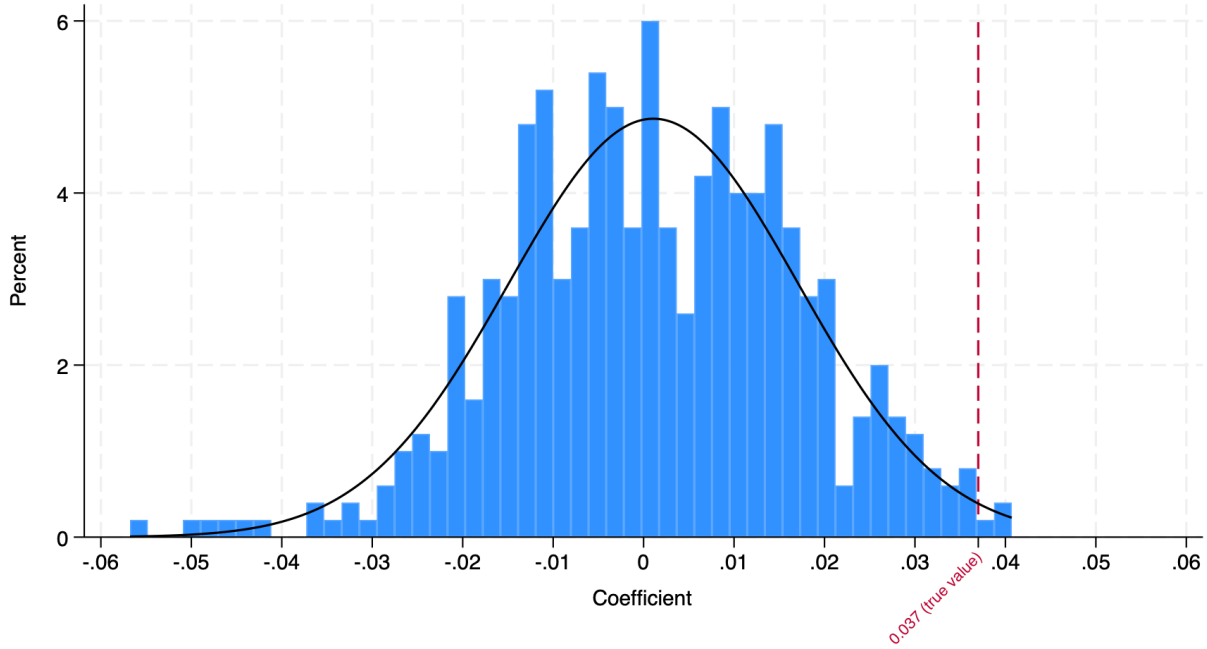


Figure B1: The graph plots the empirical distibution (50 bins) with a normal fit, using the model consistent with column 1 in Table 3.

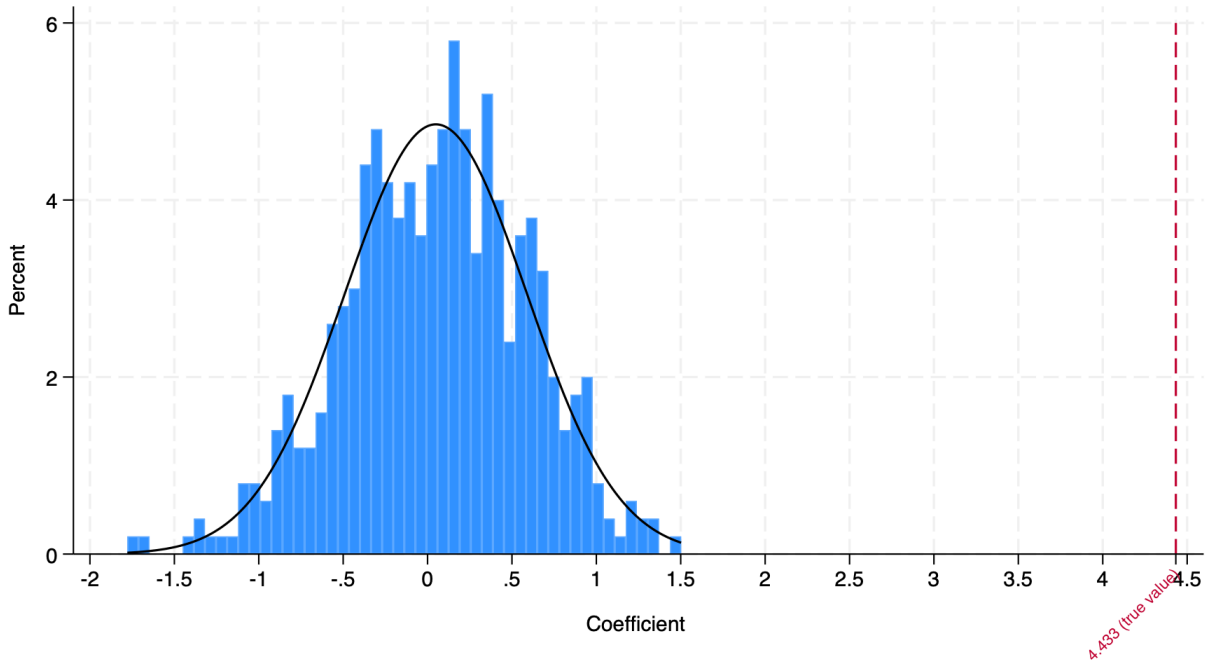


Figure B2: The graph plots the empirical distibution (50 bins) with a normal fit, using the model consistent with column 1 in Table 5.

C Computing the Effect on the Proportion of Foreign New Hires

The Delta-Method Theorem: Suppose a vector of consistent estimators $\boldsymbol{B}$ which converges in probability to the vector of its true values $\boldsymbol{\beta}$ such that

$$\boldsymbol{B} \xrightarrow{D} N\left[\boldsymbol{\beta}, \frac{1}{n}\Sigma\right] \tag{1}$$

where Σ is a covariance matrix, and a continuously differentiable function $g[\boldsymbol{\beta}]$. Then rearranging (1) we obtain

$$\sqrt{n}\left(\boldsymbol{B}-\boldsymbol{\beta}\right) \xrightarrow{D} N\left[0, \Sigma\right]$$

and the first order delta-method implies

$$\sqrt{n}\left(g\left[\boldsymbol{B}\right]-g\left[\boldsymbol{\beta}\right]\right) \xrightarrow{D} N\left[0, \nabla g\left[\boldsymbol{\beta}\right]^{T}\Sigma\nabla g\left[\boldsymbol{\beta}\right]\right] .$$

(Greene, 2012)

Application: Let $\boldsymbol{B}^{T} \equiv \left\langle \hat{\beta}_F, \hat{\beta}_D \right\rangle^{T}$ be the vector of estimators of the treatment's effect on the number of foreign and domestic new hires respectively, converging in probability to their true values $\boldsymbol{\beta}^{T} \equiv \left\langle \beta_F, \beta_D \right\rangle^{T}$. The estimators are independent of each other but have standard errors $\boldsymbol{SE} \equiv \left\langle SE\left[\hat{\beta}_F\right], SE\left[\hat{\beta}_D\right] \right\rangle^{T} = \left\langle s_F/\sqrt{n}, s_D/\sqrt{n} \right\rangle^{T}$. The true change in the proportion of foreign new hires is given by

$$g\left[\boldsymbol{\beta}\right] \equiv \frac{N_F+\beta_F}{N_D+\beta_D+N_F+\beta_F} - \frac{N_F}{N_D+N_F}$$

We would like to approximate the sampling distribution of the estimator of the proportion of foreign new hires $g\left[\boldsymbol{B}\right]$.

The gradient of g at the true values is

$$\nabla g\left[\boldsymbol{\beta}\right] = \begin{bmatrix} \frac{\partial}{\partial\beta_F}\left[\frac{N_F+\beta_F}{N_D+\beta_D+N_F+\beta_F} - \frac{N_F}{N_D+N_F}\right] \\ \frac{\partial}{\partial\beta_D}\left[\frac{N_F+\beta_F}{N_D+\beta_D+N_F+\beta_F} - \frac{N_F}{N_D+N_F}\right] \end{bmatrix} = \frac{\begin{bmatrix} N_D+\beta_D \\ -\left(N_F+\beta_F\right) \end{bmatrix}}{\left(N_D+\beta_D+N_F+\beta_F\right)^2}$$

From the Theorem, we know the asymptotic distribution of the function of the estimators

$$g\left[\boldsymbol{B}\right] \xrightarrow{D} N\left[g\left[\boldsymbol{\beta}\right], \frac{1}{\left(N_D+N_F\right)^4}\begin{bmatrix} N_D & -N_F \end{bmatrix}\begin{bmatrix} \frac{s_F^2}{n} & 0 \\ 0 & \frac{s_D^2}{n} \end{bmatrix}\begin{bmatrix} N_D \\ -N_F \end{bmatrix}\right]$$

Simplifying,

$$g\left[\boldsymbol{B}\right] \xrightarrow{D} N\left[\frac{N_F+\hat{\beta}_F}{N_D+\hat{\beta}_D+N_F+\hat{\beta}_F} - \frac{N_F}{N_D+N_F}, \frac{\left(N_D+\hat{\beta}_D\right)^2\frac{s_F^2}{n} + \left(N_F+\hat{\beta}_F\right)^2\frac{s_D^2}{n}}{\left(N_D+\hat{\beta}_D+N_F+\hat{\beta}_F\right)^4}\right]$$

Hence, the estimator of the change in proportion of foreign new hires due to switching to English is asymptotically normal with mean

$$\frac{N_F + \hat{\beta}_F}{N_D + \hat{\beta}_D + N_F + \hat{\beta}_F} - \frac{N_F}{N_D + N_F} \tag{2}$$

and standard error

$$\sqrt{\frac{\left(N_D + \hat{\beta}_D\right)^2 SE\left[\hat{\beta}_F\right]^2 + \left(N_F + \hat{\beta}_F\right)^2 SE\left[\hat{\beta}_D\right]^2}{\left(N_D + \hat{\beta}_D + N_F + \hat{\beta}_F\right)^4}} \tag{3}$$