Human Capital Portability and International Student Migration

Andreu Arenas*

February 26, 2020

Abstract

This paper examines how human capital portability frictions affect the quantity and the quality of international student migration. Using administrative data on college enrolment in Spain, it evaluates a reform that lifted the requirement to take the Spanish end-of-high-school exam for foreign students from a subset of countries. The reform increased student migration from treated countries by 50%, compared to migration from control countries. Post-reform migrants have lower predicted test scores but still outperform native students. The results suggest that multilateral diploma recognitions can significantly increase international student migration without large costs in student migrant quality, and with positive effects on average student quality.

Keywords: human capital portability; student migration; immigrant selection

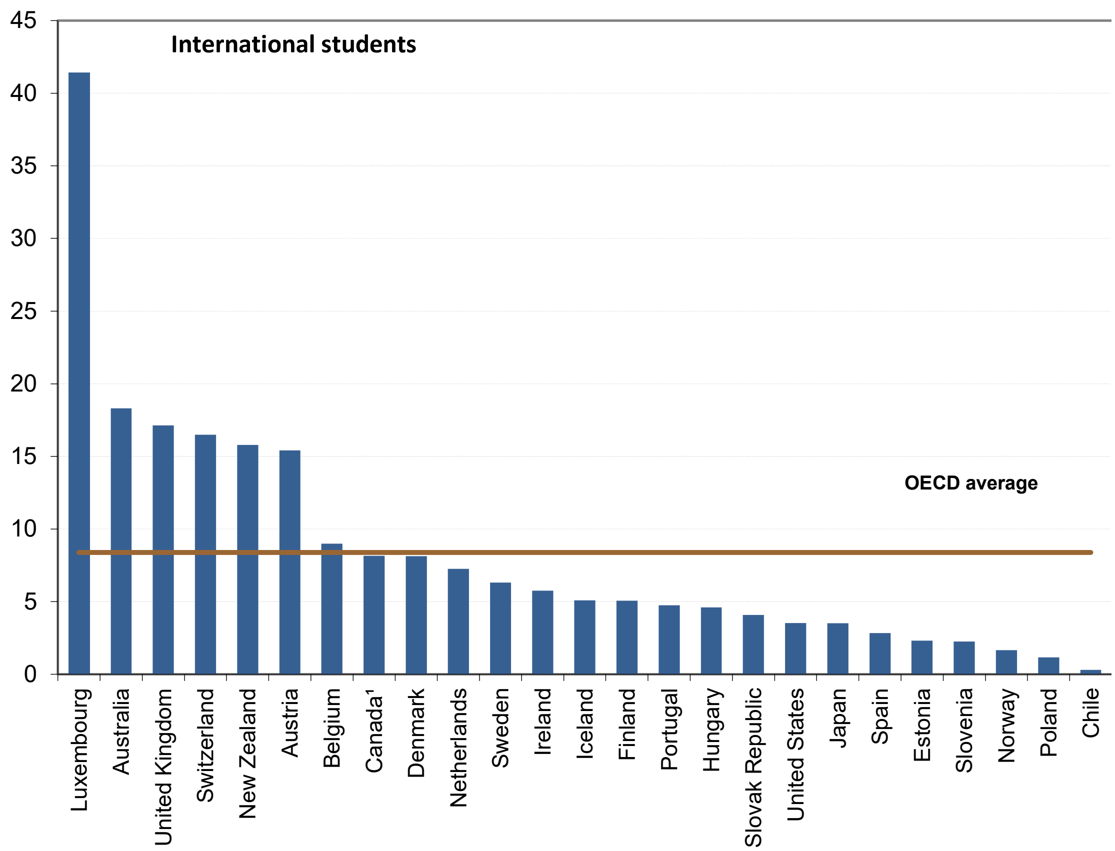
JEL codes: F22; I28; J61

*University of Barcelona and Institut d'Economia de Barcelona (IEB). andreu.arenas@ub.edu
I am grateful to the editor, two anonymous reviewers, Michel Beine, Fréderic Docquier, Matteo Gamalerio, Marcel Gérard, Albrecht Glitz, Clément Malgouyres, Marion Mercier, Albert Solé-Ollé and Judit Vall for helpful comments. I acknowledge financial support from the ERC Starting Grant 638893.

1 Introduction

Migration is very often an investment decision, driven by differences in the relative returns to skills in the labor market, or by differences in the productivity of skill investments across educational systems. The former, namely student migrations, have become increasingly relevant in the last decades. According to the OECD, the number of students enrolled outside their country of citizenship has risen from 0.8 million worldwide in 1975 to 4.5 million in 2012 (OECD, 2014). However, this increase has spread very heterogeneously across OECD countries. Figure 1 displays international student enrolment as a percentage of total tertiary enrolment across countries. While this percentage is above 15% for countries such as the UK or Austria, in other countries like Finland or Spain it remains well below 5%.

Figure 1: International students as a % of total tertiary enrolment. Source: OECD (2014)



What are the reasons behind this heterogeneity, and what frictions are quantitatively important in preventing or fostering international student migration? This is a very important question because of the role of migrants for innovation and the dynamism of the host economies. For instance, immigrants who first enter the US on a student/trainee visa or a temporary work visa have a large advantage over natives in wages, patenting, commercialising or licensing patents, and publishing (Hunt, 2011), and many of those students remain in the country after graduation and contribute to the innovations produced by American companies (Stuen *et al.*, 2012).

In many countries, the admission of international students into undergraduate higher education requires passing national admission exams. These exams aim to verify that international students are well prepared to successfully pursue college education in the host country. This might help countries to set rigorous educational standards, but it introduces a barrier. Students need to incur an additional cost to migrate to study, because the human capital acquired in the home country during high school has a lower return abroad, not automatically allowing for university enrolment. While it is easy for a student to enrol into a home-country university, if she is considering enrolment into college in different countries, she might have to take additional exams to be able to use her high school qualifications abroad, which might be too costly. This imperfect diploma recognition means that students' human capital is not perfectly portable across countries. The aim of this paper is to evaluate the effect of restricted human capital portability on the quantity and the quality of international student immigration.

To this aim, this paper evaluates the effect of a policy change in the admission rules for foreign students introduced in Spain in 2007. This reform lifted the requirement to take the Spanish end-of-high-school exam to be able to enrol into Spanish public universities, for foreign students from a subset of countries. In Spain, students are assigned to colleges through a centralised system, the Gale-Shapley mechanism (Gale and Shapley, 1962). The priority of students for being admitted into any of the public universities of the country depends solely on the students' *entrance grade*, which is a weighted average of high school grades and grades in a national end-of-high-school exam. Before the 2007 reform, taking the national exam was an inescapable requirement: any foreign student considering migrating to Spain for undergraduate education would have to take it, with the corresponding costs in terms of time, effort, and fees. In 2007, the Spanish Government lifted this requirement for students graduating from high school in several European countries and China. After the reform, foreign students from those countries would be able to enrol into Spanish universities just by satisfying the college enrolment requirements in their home country. An equivalence table would be used to translate home country grades into an *entrance grade* for those students, instead of requiring them to take the Spanish exam. Hence, the reform increased the human capital portability from treated countries to Spain.

To evaluate the effects of this reform, this paper uses administrative records on university applications and enrolment from Catalonia, a large region in Spain with some of the best universities in the country, and which is consistently ranked as one of the most attractive places for international students in Europe. The data span the 2005-2012 period, and for every student, it includes information on the country of high school studies as well as a rich array of individual covariates.

The effect of the reform on student migration is estimated by differences-in-differences, comparing the change in the number of enrolled students that migrated from treated and from control foreign countries - countries for which the requirement was not lifted. According to these estimates, the reform increased international student migration from treated countries by 50%, leading to an overall increase of almost 1pp in the average yearly share of immigrant students in Catalan universities over the post-treatment period (2007-2012). This effect is larger for the geographically and culturally close countries, for instance Southern European and Latin speaking countries.

Importantly, the nature of the data make it possible to evaluate whether this increase in student migration comes along with significant changes in students' quality, which is an important concern by policy makers. One of the consequences of the reform is that foreign students from treated countries do no longer need to take the Spanish end-of-high-school exam, meaning that post-treatment, there is no test score nor measure of ability that is comparable across students from treated and control countries. However, a rich array of individual covariates and the *entrance grades* of every student are observable for the pre-treatment period. Since the same array of individual covariates is observable in the post-treatment period, it is possible to test whether there is a change in student characteristics that were associated with good *entrance grades* in the pre-treatment period. Hence, students' predicted *entrance grades* are used as a measure of student quality.

More specifically, using pre-treatment data, a regression of university *entrance grades* on a vector of student-level covariates, mainly parental education levels and occupations, and language proficiency, is estimated. These estimates are then used to predict post-treatment test scores, which are not observable for the treated group after the treatment.[1] The results show that post-treatment migrants have observables associated with test scores that are 0.1 standard deviations (s.d.) lower. These changes are mostly driven because of differences in parental education and language proficiency, rather than by differences in parental occupation. However, although the new migrants have a significantly lower predicted ability than they would have had in the absence of the reform, they are still positively selected in comparison with native students. Pre-reform, student migrants from treated countries outperformed natives by 0.1 standard deviations (of predicted test scores); post-reform this decreases to 0.04 s.d.. This means that overall the reform increased average student quality.

Hence, the main policy implication of this paper is that cross country reciprocal diploma recognitions in college admissions can have a substantial effect on international student migration, with moderate costs in terms of student migrant quality, and with positive effects on average student quality.

One possible concern by domestic policy-makers about policies that enhance Human Capital Portability could be a distributional effect in the form of a crowding-out of native students. However, the data show that the new migrants disproportionately enrol in degrees that are at the bottom of the selectivity distribution, where capacity constraints are less binding, suggesting that crowding-out effects are likely to be small. This is consistent with Machin and Murphy (2017), that show that international undergraduate student migration does not crowd out domestic students in the UK. Interestingly, in the UK college degrees also have capacity constraints, but the volume of student migration is much larger.

[1] An entrance grade is actually observable, but it is not comparable between treated and control students because it comes from the equivalence tables and hence it might just be capturing country-specific differences in grading standards.

To the best of my knowledge, this is the first paper studying the effect of restricted human capital portability on the quantity and the quality of international student immigration. Shih (2016) shows that the openness of the US skilled labor market increases student migration by changing the expected return to studying in the US, exploiting caps in the H1-B visa.[2] Kato and Sparber (2013) study the effect of those H1-B visa caps on the quality of prospective undergraduate students from abroad. Interestingly, Kato and Sparber (2013) find that restrictive migration policy reduced the average quality of potential foreign applicants, in contrast with the results in this paper. While high-ability students might be the ones losing the most from a reduced possibility of staying and working in the US, they might also be the ones with a lower cost of taking an additional exam to migrate for education purposes, explaining the differences in the effects of these policies on student quality. Note that except for Chinese students, the reform in this paper does not induce any change in the possibility of staying in Spain, because the remaining students from treated countries are Europeans who could legally live and work in Spain both before and after the reform. Nonetheless, excluding Chinese migrants from the sample does not change the results. Hence, while restrictive visa policies reduce both the quantity and the quality of student migrants, human capital portability frictions might entail a quantity-quality trade-off, that this paper helps to quantify.

Other closely related papers have studied the effect of tuition fees on student migration, finding a negative effect of tuition fees on student migration. Beine *et al.* (2018) and Beine *et al.* (2017) show that both in Italy and in the UK, universities charging higher fees have fewer international students. In Catalonia, almost all universities are public, and tuition fees are generally low, rather homogeneous across degrees and universities, and are the same for students from any country. It is worth noting the differences between human capital portability and tuition fees as immigration costs. Human Capital Portability imposes a non-pecuniary cost on students, that requires effort and preparation, and that changes with student ability. Its main costs for the university can be undesired changes in student quality. On the other hand, tuition fees are a pecuniary cost, that in general does not

[2] Amuedo-Dorantes and Furtado (2017) also show how the careers of international students shifted towards the academic sector, that was exempt from the H-1B visa cap.

change with student ability (with the exception of scholarships, which are not the rule at the undergraduate level). Its main costs or benefits for the university are economic resources, although they could also trigger changes in student quality of uncertain sign.

Other studies of international student migration show that greater distance and being a politically unfree country significantly predict migration flows, while finding no correlation with GDP per capita in the sending countries (Bessey, 2012). Beine *et al.* (2014) show that students tend to migrate to countries with high wages, high education quality, and large shares of nationals at destination.[3]

Finally, this paper relates to a large literature that has studied the effects of Human Capital portability on labor market outcomes (Chiswick (1978), Friedberg (2000)). This paper, however, focuses on its effects on the migration decision rather than on migrants' outcomes. A crucial difference is that the effects of Human Capital Portability on migrants' employment and wages are largely due to market forces, while Human Capital Portability in access to higher education is a policy choice.

2 Institutional background

Enrolment into higher education in Spain is centralized and based on the Student Optimal Stable Mechanism (Gale and Shapley, 1962). Every student has a score that is a weighted average of high school grades and of the grades of a centralized end-of-high-school exam. This grade, also known as the *entrance grade*, determines students' priority for enrolling in *programmes* (i.e., pairs degree× university), which are the relevant unit of admission, which have capacity constraints. Over the period of analysis, tuition fees are the same for foreign and native students and are heavily subsidised, and as a result, they are very low (less than €1000 a year).

Until 2007, it was compulsory for all student migrants to take the Spanish end-of-high-school exam, which takes place in early June, three months before the start of the academic year. This requirement entailed a relevant cost in terms of travelling time and effort (be-

[3] Dreher and Poutvaara (2011) show that the stock of foreign students is an important predictor of subsequent migration to the US.Ä Belot and Hatton (2012) show that selective (point systems) immigration policies raise the share of the high skilled in total migration by about six percentage points. See also Kahanec and Králiková (2011), Van Mol (2014) or Caruso and de Wit (2015) for further cross-country evidence.

cause of economic, academic, and language proficiency reasons). Two weeks after the exam, students would receive their grades, send out their applications, and the college choice mechanism would assign them to academic programmes. In May 2007, the Spanish Government passed a resolution stating that "*Students coming from EU educational systems meeting the criteria to be admitted in their universities will have access to Spanish Universities the next academic year 2007-2008 without the need of passing the admission test (...)*".[4] The resolution also includes an agreement with Switzerland, that the year after would be extended to China, Norway, Iceland, and Liechtenstein.[5][6] Tables 11 and 12 in the Appendix B list the treated countries as well as the control countries in the sample (i.e., control countries with migrants at some point between 2005 and 2012).[7]

The reform removes the requirement of taking the end-of-high-school exam, leading to a reduction in the cost of migration. The net benefit of migrating becomes larger due to the increase in Human Capital Portability, especially for those with lower ability, for which the cost of taking an additional exam is likely to be higher. Hence, since the cost of migrating to Spain declines, one would expect an increase in student migration from the treated countries, of uncertain size. Who will be the new migrants? The answer depends on the pre-reform selection pattern and on the characteristics of the marginal student, and it is an empirical question. Consider, for instance, the case of a home country where student migrants to Spain are more likely to be at the top of the home country ability distribution. Then, the marginal student migrant enrolling in a Spanish university due to the reform will be less skilled, and the reform will reduce average migrant quality. Instead, consider the case of a home country where student migrants to Spain are more likely to be at the bottom of the home country ability distribution. Then, the marginal student migrant enrolling in a Spanish university due to the reform will be more skilled, and the reform will increase average migrant quality. A final possibility is the case of a home country where student migrants are

[4]Link to the resolution, accessed on June 2018., Media Report 1, accessed on June 2018. Media Report 2, accessed on June 2018.

[5]Source, accessed on June 2018.

[6]In June 2010, the formula to compute the *entrance grade* changed in a way that gave less points to foreign students not taking the exam, but they remained exempt and with good grades could make it to a large percentage of degrees.

[7]Andorra is excluded from the sample of countries of origin because most Andorran schools teach the Spanish curriculum.

independently distributed across its ability distribution. Belot and Ederveen (2012) show that cultural barriers do a much better job in explaining the pattern of migration flows between developed countries than traditional economic variables, which could explain such a pattern.

Whether the reform increases or reduces average student quality, however, will depend on whether those students at the margin are more or less skilled than natives. In fact, one could have an increase in student migrant quality and a decline in average student quality, an increase or a decline in both, or a decrease in student migrant quality and an increase in average student quality, depending on the differences in the ability distributions between home countries and Spain. The Appendix A illustrates these results with a simple model.

3 Data, Empirical Strategy and Results

The main data source consists of administrative records on college applications in Catalonia, a large region in Spain with some of the best universities in the country, and which is consistently ranked as one of the most attractive places for international students in Europe.[8] Data are only from Catalonia because applications are managed by regional authorities (students must submit an application form to every region where they want to apply). Between 2005 and 2012, for every first year student, applications have information on the country of high school studies -which is used use to define immigrant status-, the college programme of enrolment (i.e., the pair university $\times$ degree), and individual covariates (gender, parental education, parental occupation, language proficiency). When studying the effect of the reform on student migration, the outcome of interest are student counts, which are obtained by collapsing the data at the country of origin by year level. When studying the effect of the reform on migrants' characteristics, the data are at the student-level.

[8] According to the 2018 Times Higher Education World University Ranking, five out of the six best Spanish Universities are Catalan: Universitat Pompeu Fabra, Universitat Autònoma de Barcelona, Universitat de Barcelona, Universitat Politècnica de Catalunya, and Universitat Rovira i Virgili. All of them enrol at least 10% of international students. Source, accessed in June 2018. A number of listing and reviews websites for programs abroad list Barcelona as one of the best European destinations for international students. Source 1, accessed in June 2018. Source 2, accessed in June 2018.

3.1 Immigration flows

The effects of Human Capital Portability on International Student Migration are investigated by estimating the following regression:

$$\#Students_{ct} = \alpha_c + \delta_t + \beta\,(Treated_c \times Post_t) + \epsilon_{ct}$$

Where $\#Students_{ct}$ is the number of students migrants from country c that enrol in Catalan universities as 1st-year students in year t. The estimating equation includes country of origin fixed effects, that account for time-invariant differences in migration flows by country of origin, and year fixed effects, that account for yearly-specific factors that have the same effect on student migration from all countries. The coefficient of interest is β, and it measures a difference-in-differences, namely the difference between treated and control countries in the change in student migration after the reform. The crucial assumption for a causal interpretation of β is that migration flows from treated and control countries would have followed parallel trends in the absence of the policy change. Although this assumption is fundamentally untestable, it is useful to assess whether treatment and control countries were trending in parallel before the reform. Figure 2 displays the evolution of the number of immigrants per origin country over time, across treated and control countries of origin. Although only observe two pre-treatment periods are observable, at least in the year before the reform there were no visible differences in trends in student migration. Figure 2 indicates that before 2007, student migration from treated countries was only slightly larger than the migration from control countries. However, after the 2007 reform, there is a substantial increase in student migration from treated countries, while student migration from control countries remains rather stable.

Although pre-treatment differences in migration levels are absorbed by country fixed effects and there are no visible pre-treatment differences in trends, one might be concerned about the plausibility of the common trends assumption whenever treatment and control groups are quite different. To mitigate this concern, the baseline regression is also estimated on a sample that includes, within control countries, only the subset that is classified by the World Bank (Fantom and Serajuddin, 2016) as high or upper-middle countries, which are more similar to treatment countries in pre-treatment student migration levels and characteristics. Conditional on satisfying the parallel trends assumption, β will estimate an Average Treatment Effect on the Treated (ATT). It is the treatment effect for the subgroup of countries for which the policy changed. It is important to have in mind that this is a group of high-income countries (i.e., mostly EU countries) and that the results will be relevant for policy changes affecting similar countries, but might be less informative for policy changes affecting developing countries, for instance.

Figure 2: International Student Migration, treated vs. control countries

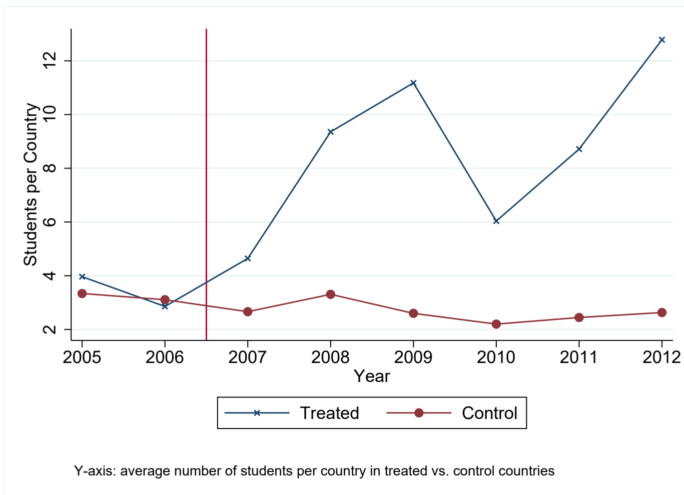


Table 13 in the Appendix B reports descriptive statistics on the number of migrants, in levels, logs, and as a % of total student enrolment. The unit of observation is a country by year. As previously suggested by figure 2, the descriptive statistics indicate that there is a substantial change in enrolment after the reform for the treatment group, while enrolment in the control group remains rather stable. Table 1 reports the main results of the effect of Human Capital Portability (HKP) on International Student Migration (ISM), with different transformations of the dependent variable: in levels (# Students), logs (1+#Students), and as a percentage of the total student population (% Students). To account for serial correlation in migration decisions within countries of origin, standard errors are clustered at the country of origin level. The point estimates indicate that the reform leads to around 6 more student migrants per treated country and year. Given the number of treated countries, this implies that the reform brought more than 1000 student migrants to Catalan universities between

2007 and 2012. The estimates of the effect of the reform on the log of the number of students per country of origin indicate that this corresponds to an average increase of more than 50% per country of origin. The point estimates on student migrants as a percentage of the total student population indicate that the reform increased it by around one percentage points. Hence, the effect of the reform in relative terms is quantitatively important. The within R^2 further indicates that the reform explains between 3% and 6% of the within-country variance in student migration over the period.

Table 1: HK Portability and ISM

	(1) #Students	(2) log(1+#Students)	(3) % Students
Treated Country × Post	5.957**	0.604***	0.0269**
	(2.620)	(0.129)	(0.0116)
Country FE	✓	✓	✓
Year FE	✓	✓	✓
Mean Dep. Var	4.188	0.854	0.0195
R^2 within	.025	.071	.026
N	744	744	744

Standard errors clustered at the country level in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 14 in the Appendix B reports estimates including a lead of the treatment indicator. Its coefficient is close to zero and not statistically significant, suggesting that in the two-year pre-treatment period there were no differential trends in student migration across the treatment and the control group, as suggested by figure 2. Table 15 in the Appendix B reports descriptive statistics for the subset of high and upper-middle-income control countries. Their pre-treatment levels of student migration are higher than for the full control group, and they remain very stable post-treatment as well. Tables 16 and 17 in the Appendix B report estimates when the sample of control countries is restricted to higher and upper-middle-income countries. Note that the mean of the dependent variables is higher now - these are countries with students with a higher propensity to migrate to Spain for higher education purposes. Quantitatively, the results on student migration are almost identical, and in spite of the smaller sample size, they are still statistically significant. Likewise, the placebo test for pre-treatment trends delivers a similar result. Tables 18-20 in the Appendix B show that these results are robust to excluding students from Bulgaria (BGR) and Romania (ROU), which joined the EU on January 2007; to excluding China, the only country for which a student migration could increase the probability of staying; and to controlling for the unemployment rate in the country of origin, given that the great recession took place during the post-treatment period.

While figure 2 displays a large increase in student migration after the reform, there is a slump in 2010. This is presumably because, in 2010, the way in which the *entrance grade* is computed was modified, making foreign students applications a little less competitive unless they would take the national exam. This was part of a nation-wide change in the way entrance grades are computed, not specific to foreign students.[9] Table 21 in the Appendix B reports results constraining the sample to the 2006-2009 period, with very similar results.

The reform increases human capital portability for all treated countries, however, the effect might be quite heterogeneous across countries. More precisely, one would expect the reform to entail a larger relative reduction in migration costs for culturally and geographically close countries, where other migration costs are lower. And because overall migration costs in these countries might have been lower from the beginning, one would expect a higher number of students at the margin. This heterogeneity is investigated in table 2, which reports estimates of heterogeneous effects by country, focusing on Latin-speaking Southern-European treated countries, namely France, Italy, and Portugal. The results show that the effect of the reform is positive and significant for the remaining countries and that the effect for these culturally and geographically close countries is much larger - although imprecisely estimated, given that these are just three countries. Table 22 in the Appendix B presents results where heterogeneous effects are estimated for the group of Latin-speaking treated countries overall, including Belgium, Luxembourg, Switzerland, and Rumania, in addition to the Southern Latin-speaking countries. The results show that the reform has a

[9]In particular, before 2010, the entrance grade was a weighted average of the national exam grades (60%) and high-school grades (40%). After 2010, it was a weighted average of high school grades (4 points), part 1 of the national exam (6 points), and part B of the national exam (4 points). With their foreign grades, foreign students would get up to 10 points, and would have the option of taking part B of the national exam (2 subjects at most), to reach the maximum of 14 points). This was optional however and they could access a large share of the programmes with reasonable good grades without the need of taking part B of the national exam.

small significant positive effect on migration from non-Latin-speaking countries and that the average size of the effect is quite larger for Latin-speaking countries overall, although this heterogeneity is imprecisely estimated.

Table 2: HK Portability and ISM - heterogeneous effects

	(1) #Students	(2) log(1+#Students)	(3) % Students
Treated Country × Post	3.095***	0.566***	0.0147***
	(0.786)	(0.127)	(0.00361)
Treated Country × Post × Southern Latin-speaking	26.71	0.353	0.115
	(18.34)	(0.506)	(0.0824)
Country FE	✓	✓	✓
Year FE	✓	✓	✓
Mean Dep. Var	4.188	0.854	0.0195
R^2 within	.093	.075	.091
N	744	744	744

Standard errors clustered at the country level in parentheses.
Southern Latin-speaking: Italy, France, and Portugal.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

While the results show an increase in student migration due to the reform, a concern regarding their interpretation as an effect of human capital portability is that this increase could be driven by a change in the universities' selectivity policy rather than by an actual change in the mobility of foreign students. This alternative explanation would also lead to an increase in student migration and a slight reduction in the quality of foreign students, without any change in applications. To deal with this concern, tables 3 and 4 report estimates of the effect of the reform on applications by foreign students, and of the effect of the reform on student migration after controlling for applications. The results show that the reform had a significant effect on applications, of a slightly larger magnitude than the effect on student migration; and that the effect on applications fully explains the effect of the reform on student migration. This is in line with the interpretation that the results are driven by a change in the mobility of foreign students affected by the reform.

Table 3: HKP and international students' applications

	(1) #Applicants	(2) log(1+#Applicants)	(3) % Applicants
Treated Country × Post	8.929***	0.679***	0.0279**
	(3.367)	(0.142)	(0.0109)
Country FE	✓	✓	✓
Year FE	✓	✓	✓
Mean Dep. Var	8.109	1.001	0.0288
R^2 within	.031	.08	.025
N	744	744	744

Standard errors clustered at the country level in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: HKP and ISM, controlling for applications

	(1) #Students	(2) log(1+#Students)	(3) % Students
Treated Country × Post	-0.239	0.000853	0.00332
	(0.492)	(0.0428)	(0.00427)
#Applicants	✓		
log(1+#Applicants)		✓	
% Applicants			✓
Country FE	✓	✓	✓
Year FE	✓	✓	✓
Mean Dep. Var	4.188	0.854	0.0195
R^2 within	.869	.8961	.8186
N	744	744	744

Standard errors clustered at the country level in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

3.2 Immigrants' characteristics

After finding a positive effect of the reform on international student migration, the next step is studying the effect of the reform on student migrants' characteristics, to answer two different questions. The first question is whether immigrants are more or less positively selected than they would have been in the absence of the reform. This question is addressed by comparing changes in student characteristics' between migrants from treated and control countries, following an empirical strategy similar to that in the previous section, using student-level data. The second question is whether immigrants are still more or less positively selected in comparison with natives, which is relevant for understanding the effect of the reform on average student quality. The answer to this second question is more descriptive because it is difficult to think of natives and migrants as reacting similarly to aggregate shocks and hence as following parallel trends.

To measure immigrant characteristics, it is useful to combine the data on individual covariates and entrance grades. Pre-treatment, entrance grades are comparable across students. Post-treatment, they are not available nor comparable across students - this is precisely one of the consequences of the reform. Hence, it is not possible to test whether the post-treatment student migrants academically outperform the pre-treatment migrants. However, it is possible to observe whether they have observables that are typically associated with higher academic performance, according to pre-treatment standards. Hence, it is possible to use predicted test scores as a measure of student quality, proceeding as follows:

As a 1st step, the *Entrance Grade* of student i is regressed on a vector of covariates X_i, constraining the sample to student migrants in the pre-treatment period:

$$Entrance\,Grade_i = \delta_0 + \delta_1' X_i + \nu_i$$

Where *Entrance Grades* are standardized by year, for the whole student population. X_i includes a treated country indicator, gender, parental education, maternal education, parental occupation, maternal occupation, and language proficiency. Each of the four parental background variables takes between seven and ten discrete values, which are included as dummy variables. Language proficiency is measured with a set of dummies for English proficiency, Spanish proficiency, and Catalan proficiency.[10]

As a 2nd step, this regression's coefficient estimates are used to compute predicted test-scores for the full sample of student migrants, including those in the post-treatment period:

$$\widehat{Entrance\,Grade}_i = \hat{\delta}_0 + \hat{\delta}_1' X_i$$

Predicted test scores are a meaningful measure of migrants' selection because national standardised test scores are a strong predictor of future education and labor market outcomes (MacLeod *et al.* (2017), de Hoyos *et al.* (2018)). Moreover, this approach is suitable because the interpretation of the effect of the reform on each of the covariates used to predict test scores is not as clear by itself, and on the other hand there is a risk of over-interpretation due to multiple testing. This is because the variables that are used to predict test scores are categorical, and they are included as dummy variables in the regression to predict test scores. For instance, parental occupation education and occupation variables take 7 and 10 possible values, respectively, and in addition, there are language proficiency variables, expressed in various dummies for each of the three languages. Estimating the effect of the reform on each of these indicator variables separately would amount to estimating more than twenty regressions. This is not convenient, for two reasons. First, because jointly interpreting the estimates of such a high number of regressions is more difficult than interpreting the estimate in one regression where the dependent variable is predicted test scores. Second, because statistical inference also becomes more complicated, due to the simultaneous testing of such a large number of non-independent hypothesis. Hence, a more parsimonious alternative is the proposed approach of estimating the effect of the reform on predicted test scores, predicted with all those covariates.

As a 3rd step, the following differences-in-differences regression is estimated:

$$\widehat{Entrance\,Grade}_{it} = \alpha + \pi Treated_{c(i)} + \tau_t + \gamma \left(Treated_{c(i)} \times Post_t \right) + \nu_{it}$$

[10] Language proficiency can take five values, 1 if the student has a good level of reading, writing, listening, and speaking, 0.75 if a good level on 3 of the 4 possible language skills, and so on. Dummies are included for every language, for every possible language-specific proficiency level (0, 0.25, 0.5, 0.75, and 1).

Where $\widehat{Entrance\,Grade}_{it}$ is the predicted test score for student migrant i enrolling in a Catalan university as a 1st-year student in year t. The estimating equation includes a dummy for being an immigrant from the treated group of countries, that accounts for time-invariant differences in student characteristics across treatment and control groups, and year fixed effects, that account for factors that had the same effect on migrants' characteristics, from all countries, in a given year. The coefficient of interest is γ, and it measures a difference-in-differences, namely the difference between treated and control countries in the change in predicted test scores after the reform.

Note that this specification is different to the previous one, where the unit of observation was the country of origin and the dependent variable was the yearly number of students. Firstly, this is because whenever there are no students in a year by country-of-origin, average quality is not defined. Secondly, this is because such a specification would give equal weight to average student quality across cells of different and changing size (i.e., country of origin by year cells), because the size of the student inflow is changing every year, and is changing differently across countries. Indeed, it is appropriate to give more weight to quality changes from countries where the size of the student inflow changed the most, which is what the student-level specification will deliver. That the reform attracts more students from countries that have a better or worst pool of prospective students is part of the treatment, and this is why this specification does not include country fixed effects.

Table 25 in the Appendix B reports the correlation between predicted test scores and actual test scores for natives, that is quite similar before and after the treatment period. It also reports a regression of post-treatment test scores on predicted test scores (estimated on pre-treatment data) for natives. The results suggest that the predicted test scores (based on the pre-treatment period) are good predictors of test scores in the post-treatment period. Likewise, table 5 displays the relationship between actual immigrant test scores in 2006 and predicted immigrant test scores, estimated on 2005 data. Both the graph and the regression coefficient suggest that the predicted test scores have good predictive power.

Table 5: Predicted test scores and post-treatment test scores, Immigrants

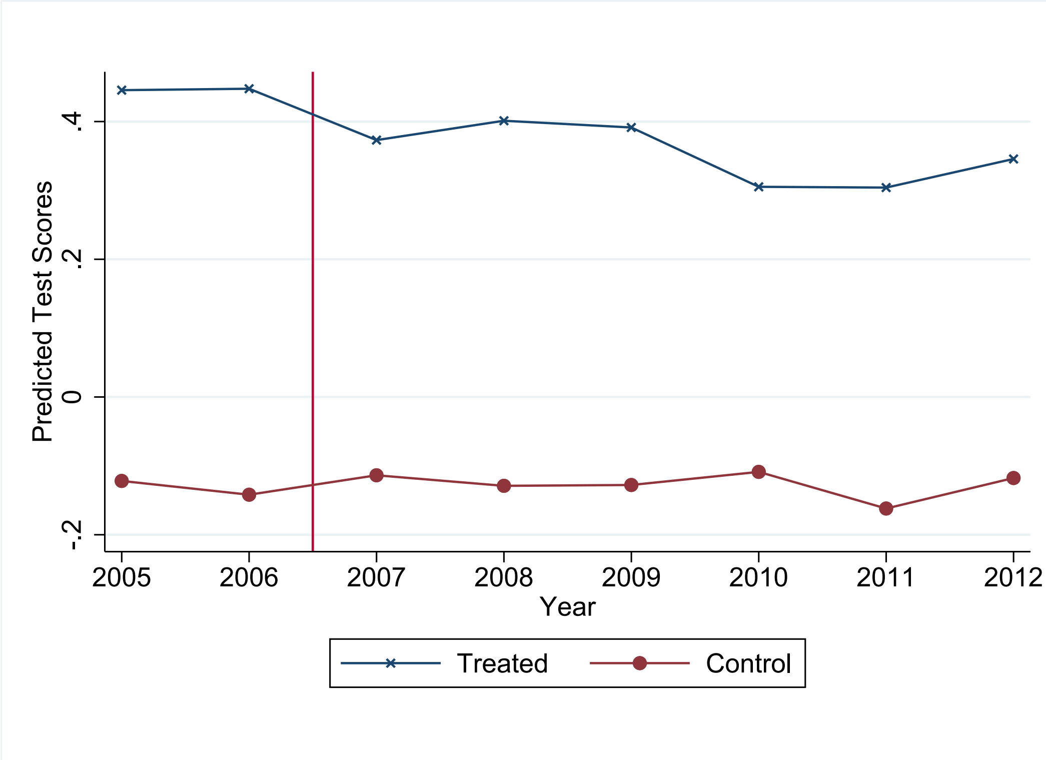
	Test Scores in 2006
Predicted Test Scores (from 2005)	0.638***
	(0.1048)
N	282

Only Immigrants.
Robust standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Again, the crucial assumption for a causal interpretation of γ is that migrant characteristics from treated and control countries would have followed parallel trends in the absence of the policy change. Figure 3 displays the evolution of the predicted test scores over time, across treated and control countries of origin. Although only observe two pre-treatment periods are observable, at least in the year before the reform there were no visible differences in trends in predicted test-scores. Figure 3 indicates that before 2007, migrants' characteristics from treated countries were significantly better in terms of predicted test scores. However, after the 2007 reform, there is some convergence, partially reducing the pre-treatment difference. Figure 8 in the Appendix B, on the other hand, shows that predicted test scores for natives slightly steadily increased throughout the period. This figure also shows that even if the predicted test scores of migrants from treated countries slightly decline after the reform, in any case, they are larger than those of natives.[11] Table 23 in the Appendix B reports

[11]Predicted test scores for natives are estimated in a separate regression, following the same procedure.

Figure 3: Predicted Test Scores, treated vs. control countries



descriptive statistics comparing Spanish and immigrant students, subsequently split into students from treated and control countries. It shows that predicted test scores are higher for immigrants than for Spanish students and that this is because of the high predicted test scores of immigrants in the treatment group. Migrants from the control group, instead, have rather low predicted test scores. Table 6 reports that there are no large pre-treatment differences in characteristics between migrants from treated and control migrants. Although individual covariates are a powerful predictor of test scores, there is an initial, unconditional difference in test scores between migrants from treated and control countries, which is also visible in the first stage, reported in table 24 in the Appendix B.[12] After the reform, however, predicted scores from the treated group decline, because they are less likely to have an educated father or mother, a white-collar father or mother, or less likely to be proficient

[12]Unconditional differences between treated and control migrants' skills might be due to geographical and historical ties. For instance, Antecol *et al.* (2003) show that Australian and Canadian immigrants have higher levels of English fluency, education, and income than U.S. immigrants, primarily because the United States receives a much larger share of immigrants from Latin America than do the other two countries.

in English, Spanish, or Catalan.[13] Language proficiency variables range from 0 to 1, where 1 means dominating 4 skills -speaking, writing, listening, reading-; 0.75 means dominating 3 skills; and so on, until zero, that means dominating none. Instead, the characteristics of migrants from the control group remain rather stable.

Table 6: Descriptive statistics, migrants, before and after 2007

Variable Name	Treated, Pre	Control, Pre	Treated, Post	Control, Post
Predicted Test Scores	0.446	-0.131	0.356	-0.127
	(0.280)	(0.294)	(0.332)	(0.326)
Educated Father	0.518	0.518	0.462	0.521
	(0.501)	(0.500)	(0.499)	(0.500)
Educated Mother	0.445	0.463	0.443	0.482
	(0.498)	(0.499)	(0.497)	(0.500)
White Collar father	0.382	0.358	0.296	0.341
	(0.487)	(0.480)	(0.457)	(0.474)
White Collar Mother	0.262	0.229	0.257	0.250
	(0.441)	(0.421)	(0.437)	(0.434)
Spanish Proficiency Score	0.911	0.973	0.670	0.952
	(0.236)	(0.138)	(0.427)	(0.188)
English Proficiency Score	0.815	0.589	0.641	0.623
	(0.345)	(0.451)	(0.445)	(0.443)
Catalan Proficiency Score	0.209	0.273	0.159	0.255
	(0.346)	(0.335)	(0.310)	(0.345)
Female	0.675	0.570	0.645	0.576
	(0.469)	(0.496)	(0.479)	(0.494)
STEM degree	0.660	0.408	0.621	0.479
	(0.475)	(0.492)	(0.485)	(0.500)
N	191	419	1476	1030
# of countries	28	65	28	65

Only immigrants.

Table 7 presents estimates of the effect of the reform on student migrants' characteristics. The estimate in the first column of the top panel indicates that the reform reduced the predicted test scores in the treatment group by almost 0.1 standard deviations, a substantial effect. This table also reports estimates of the effect of the reform on student migrants' characteristics predicted from raw test scores, which range from 5000 to 10000 (5000 being the minimum to pass), instead of standardized test scores (with zero mean and a standard deviation of one). The results suggest that both the magnitude and the interpretation

[13] Recall that when predicting test scores, parental/occupation education and occupation variables are categorical, with 7 and 10 possible values, respectively. White-collar indicators are presented in descriptive statistics for simplicity.

of the results are very similar. Table 8 presents estimates based on test scores predicted using either parental education, parental occupation, or language proficiency variables. An important point when interpreting these results (and also the baseline) is to note that the effect of the reform on predicted test scores depends on the effect on the reform on students' characteristics, weighted by the characteristics' first stage coefficients (i.e., weighted by the characteristics' predictive power on student quality). The results suggest that differences in post-treatment migrants' predicted quality are driven by parental education and language proficiency, but not by parental occupation. The F-statistics (Kleibergen-Paap Wald F-statistics) capture the relevance of each subset of predictors in the first stage, and they indicate that the similar effects when predicting test scores with parental education and language variables come from different combinations of first stage strength and changes in characteristics. Language proficiency changes relatively more than parental education, however, parental education variables are better predictors of student quality, and hence, smaller changes are more consequential. On the other hand, looking at the simplified measure of parental occupation in the descriptive statistics of table 6, it seems that the absence of effects when predicting test scores with parental occupation variables is due to its low predictive power, rather than because of a lack of a change in parental occupation variables as a result of the reform.

To shed further light on the effect of the reform on student characteristics, figure 4 displays how the density of students across the predicted test score distribution changes between treatment groups and periods. The top panel indicates that the distribution of predicted test scores in the control group remained very stable after the reform, while the distribution of predicted test scores in the treatment group shifted left in the post-reform period. Figure 9 in the Appendix B reports changes in the average yearly frequency of students at different points of the distribution of predicted test scores, between treatment groups and periods. The top panel indicates that the frequency of students after the reform slightly declined and that this decline was rather proportional across the distribution. On the other hand, the bottom panel shows that the frequency of students in the treatment group increased at almost all points of the distribution of predicted test scores, but especially left of the center of the distribution.

Table 7: HK Portability and student characteristics I

	Predicted Test Scores		P. Test Scores (non-std.)	
	(1)	(2)	(3)	(4)
Treated× Post	-0.0932**	-0.106**	-93.71**	-105.9*
	(0.0397)	(0.0523)	(40.66)	(53.65)
Treated× Post 2005		0.0221		21.72
		(0.0465)		(47.38)
Treated Country	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Mean Dep. Var	0.137	0.137	6760.7	6760.7
R^2 within	.003	.003	.003	.003
F-stat 1st Stage	776.7	776.7	828.1	828.1
N	3116	3116	3116	3116

Standard errors clustered by country of origin in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: HK Portability and student characteristics II

	Predicted Test Scores		
	(1)	(2)	(3)
Treated× Post	-0.0510**	-0.0187	-0.0624**
	(0.0213)	(0.0271)	(0.0255)
Treated Country	✓	✓	✓
Year FE	✓	✓	✓
Main Predictor	P. Education	P. Occupation	Language prof.
Mean Dep. Var	0.148	0.173	0.152
R^2 within	.002	0	.004
F-stat 1st Stage	65.77	14.34	3.768
N	3116	3116	3116

Main Predictor indicates the main variables used to predict test scores.

In all cases, gender is included as a predictor.

Standard errors clustered by country of origin in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	Predicted Test Scores (non-std.)		
	(1)	(2)	(3)
Treated× Post	-52.34**	-19.22	-61.89**
	(21.89)	(27.60)	(25.63)
Treated Country	✓	✓	✓
Year FE	✓	✓	✓
Main Predictor	P. Education	P. Occupation	Language prof.
Mean Dep. Var	6771.6	6796.8	6776.9
R^2 within	.002	0	.004
F-stat 1st Stage	65.68	13.67	3.754
N	3116	3116	3116

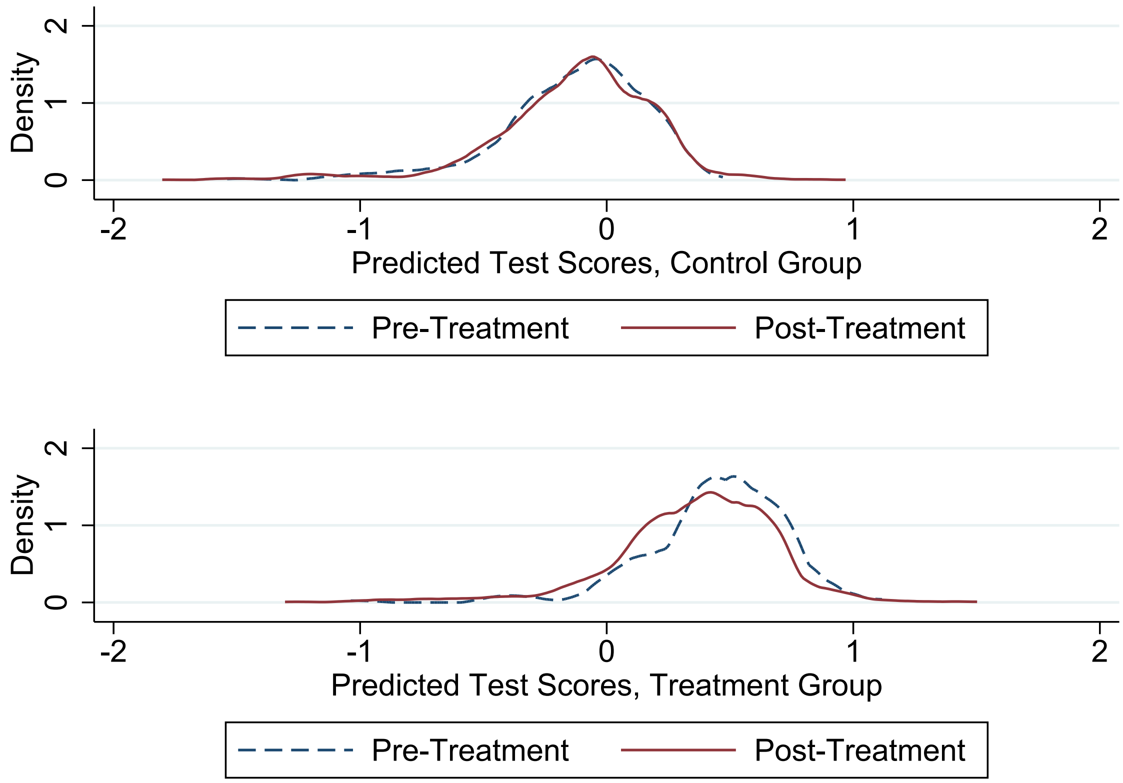
Main Predictor indicates the main variables used to predict test scores.

In all cases, gender is included as a predictor.

Standard errors clustered by country of origin in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 4: Density differences in predicted test scores



Previous research has emphasised the benefits from high-skilled migration in the US, which has been predominantly in STEM (Hunt and Gauthier-Loiselle (2010), Kerr and Lincoln (2010), Peri *et al.* (2015)). Table 27 and figures 11 and 12 in the Appendix B report estimates of the effect the reform, finding that post-reform migrants are 12pp less likely to enrol in STEM degrees than control migrants, although this is imprecisely estimated. Nonetheless, the new migrants are still more likely to enrol in STEM degrees than natives.

3.2.1 Validity concerns and robustness checks

There are two main threats to the validity of predicted test scores as a measure of student quality: overfitting and measurement error. Overfitting is a concern because OLS estimates the parameters in the first stage just to maximize the in-sample fit. Instead, Machine Learning methods, such as Lasso regression, are estimated to maximize their out-of-sample predictive power, although their estimated parameters cannot be interpreted as indicating any meaningful structure (Mullainathan and Spiess, 2017). Given the large set of covariates to predict test scores, and that we are interested in the effect of the reform on student quality, this is a suitable approach. Lasso regressions are a form of penalized regression, with a penalty for each non-zero coefficient, that overcome overfitting via cross-validation: slicing the sample into different parts, a training sample and a testing sample, and delivering estimates that maximize the predictive power of the training samples on the testing samples (Athey and Imbens, 2019).[14] To test the robustness of the measure of predicted student quality to this concern, table 9 presents estimates where student quality is predicted using Lasso regressions, where the penalty parameter is selected either using regular cross-validation, or adaptive cross-validation, an iterative procedure of cross-validation that puts larger penalty loadings on small coefficients.[15] Table 9 shows similar results to those in table 7: the new migrants are significantly worse in terms of predicted test scores.

Table 9: HK Portability and student characteristics, Lasso

	Predicted Test Scores		P. Test Scores (non-std.)	
	(1)	(2)	(3)	(4)
Treated$\times$ Post	-0.0642**	-0.0535**	-62.68**	-54.24**
	(0.0285)	(0.0266)	(27.87)	(27.27)
Treated Country	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Selection Model	Regular CV	Adaptive CV	Regular CV	Adaptive CV
Mean Dep. Var	0.135	0.148	6758.2	6771.4
R^2 within	.003	.002	.003	.002
N	3116	3116	3116	3116

Cross Validation (CV) in 10 folds.
Predicted test scores from a Lasso regression on all the baseline predictors.
Standard errors clustered by country of origin in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The second concern is measurement error, especially in the language variables, which are self-reported. All the predictors in the first stage are dummy variables, which means that measurement error will be non-classical, by definition. In a bivariate case, it is straightforward to show that $\hat{\beta} = \beta\left(E[x_i|\tilde{x}_i = 1] - E[x_i|\tilde{x}_i = 0]\right)$, where β is the parameter cor-

[14]In this case, the pre-treatment sample is sliced into ten different parts, and the resulting estimates are used to predict post-treatment student quality.

[15]More precisely, Lasso's $\hat{\beta}$ are the solution to: $\widehat{\boldsymbol{\beta}} = \arg\min_{\boldsymbol{\beta}} \left\{ \frac{1}{2n} \sum_{i=1}^{n} \left(y_i - \mathbf{x}_i \boldsymbol{\beta}' \right)^2 + \lambda \sum_{j=1}^{p} \omega_j |\beta_j| \right\}$, where $\lambda > 0$ is the Lasso penalty parameter, and ω_j are the penalty loadings.

responding to the regression without measurement error, x is the true variable and $\tilde{x}$ the mismeasured variable. Hence, with the exception of the extreme case in which mismeasurement leads to the wrong sign, it is likely that there is some attenuation bias in the first stage coefficients. In turn, this would lead to measurement error in the second-stage outcome (predicted test scores), which would bias standard errors upwards. Nonetheless, it turns out that the estimates in table 7 are still relatively precise and significant at conventional levels, which suggests that this is not a fundamental concern in this case.

3.2.2 Crowding-out

One possible concern by domestic policy-makers about policies that enhance Human Capital Portability could be the crowding-out of native students. In this case-study, the size of the student migration flows, both before and after the reform, is quite small, meaning that this is unlikely to be a large concern. However, this is important when thinking about adopting such a policy in a context where its quantitative effects, in absolute numbers, might be larger. Machin and Murphy (2017) show that international undergraduate student migration does not crowd out domestic students in the UK, which is a very important result for the welfare implications of this policy. Although the UK and the Spanish higher education markets and systems are very different, one common feature is the common fees for foreign and native systems, as well as the quotas regulating the number of students in each university and degree.

Nevertheless, one way of assessing the importance of the hypothetical crowding-out effects in this context is by looking at the selectivity of the degrees of enrolment of the new student migrants. Given that the Spanish system features capacity constraints, every academic programme features a threshold grade, which is the entrance grade of the student with the lowest threshold grade that was admitted. Hence, academic programmes can be sorted according to their threshold grades, with low threshold grades meaning that capacity constraints are less likely to be binding. Panel (a) in figure 5 displays the total number of students from treated and control countries enrolling in programmes within the top 25% of selectivity (top 25% of threshold grades). The figure shows that the reform did not increase enrolment in those programmes, meaning that it did not crowd out native students in the most demanded programmes. Panel (b) in figure 5 displays the total number of students from treated and control countries enrolling in programmes within the bottom 25% of selectivity (top 25% of threshold grades). The figure shows how the reform substantially increased migrants' enrolment in those programmes where capacity constraints are less binding (in some cases, non-binding), and hence, where student migrants are less likely to crowd out native enrolment. Note also that for the top 25% selective programs, there appears to be some crowding out effect until 2009, but from 2009 onwards the crowding out effect disappears, but not for the bottom 25%. This is presumably because in 2010, the way in which the *entrance grade* is computed was modified, making applications from foreign students who do not take the national exam slightly less competitive.[16]

To investigate more formally the possibility crowding-out effects, academic programmes are divided into percentiles according to their selectivity, and the following regression is estimated:

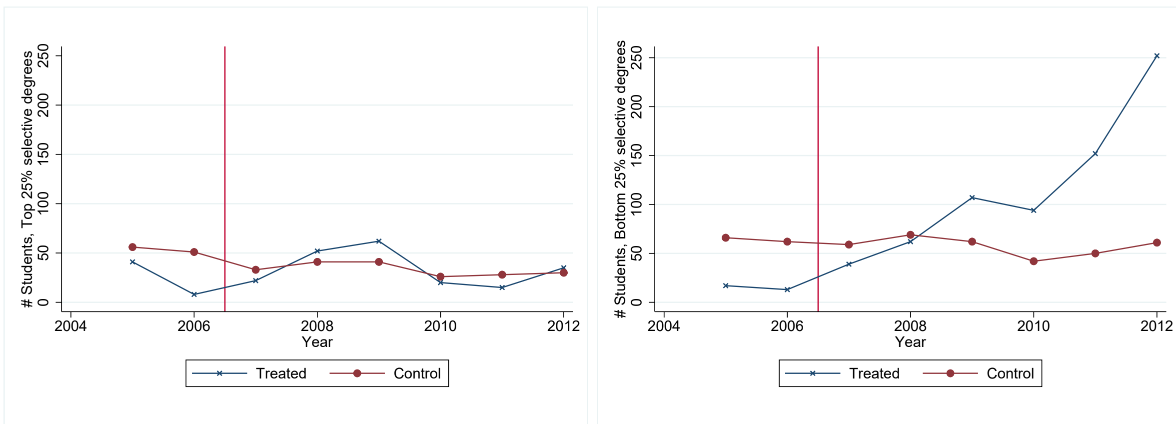
$$\%Migrant\ Students_{pt} = \alpha_p + \delta_t + \beta\,(Selectivity\ Rank_p \times Post_t) + \epsilon_{pt}$$

The percentage of migrants in the percentile p in year t is regressed on percentile fixed effects, year fixed effects, and an interaction between a post-reform dummy and the selectivity rank of the percentile, such that the most selective percentile has a selectivity rank of 1, and the least selective percentile has a selectivity rank of 100. Using percentiles and their selectivity rank is convenient because the number and the definition or content of academic programmes can change over time, and hence, longitudinal analyses are less straightforward. The first column of table 10 shows that on average, due to the reform, the percentage of foreign students declines in the most selective programmes when compared to the least selective programmes. More specifically, the percentage of student migrants in the bottom 25% increases by 1.07 percentage points, compared to the percentage of migrants in the top 25%. Columns 2 and 3 split the sample between the top and the bottom 50 percentiles, showing that the result in the first column is due to new foreign students mostly going

[16] However, results available upon request show that the magnitude of this on crowding-out in top programmes is small: the estimates on the full sample are similar to those when restricting the sample to the pre-2009 period.

to the least selective programmes within those below the median selectivity. Hence, these results suggest that the reform had no crowding-out effects in programmes with relatively high demand. Further welfare considerations would require data on college performance, the staying rates or the fiscal contribution of the new immigrants, which is left for further research.

Figure 5: Students' allocation: high vs. low capacity constraints



(a) Top 25% selective programmes

(b) Bottom 25% selective programmes

Table 10: Students' allocation: high vs. low capacity constraints

	% of Immigrant Students		
	(1)	(2)	(3)
Selectivity percentile × Post	-0.0216***	-0.0590***	0.000699
	(0.00416)	(0.0103)	(0.00959)
Selectivity Percentile FE	✓	✓	✓
Year FE	✓	✓	✓
Sample	Full	Selectivity below the median	Selectivity above the median
Mean Dep. Var	1.819	2.139	1.499
R^2 within	.033	.061	0
N	800	400	400

Selectivity percentile is 1 for the least selective programme, and 100 for the most selective.
Estimates weighted by the number of students in each percentile
Selectivity thresholds may span multiple percentiles, in which case they are randomly assigned within those percentiles. Standard errors clustered by selectivity percentile in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

4 Conclusions

International student migration is an increasing trend around the world. However, its expansion across similar countries has been very heterogeneous, especially when compared, for instance, to the expansion of student exchange programmes. While it is well understood that some of these differences are due to language barriers, economic conditions, distance, or university quality, among others, the focus of this paper is on a novel factor, namely the portability of human capital.

Understanding the effect of human capital portability on international student migration is interesting because it is fundamentally a policy question. It is moreover a very empirical question because barriers to human capital portability are likely to entail a trade-off between the quantity and the quality of student migrants. This is because human capital portability restrictions are easier to circumvent by high ability students, and are meant to be a quality filter. The contribution of this paper is to deal with this novel question by combining administrative data with a policy change that lifted the requirement of taking the Spanish end-of-high school centralized exam as a pre-requisite for college enrolment and allowed for cross-country test score recognitions.

The main result of the paper is that reducing human capital portability frictions has the potential to significantly increase international student migration without large costs in student migrant quality, and with positive effects on average student quality. In particular, the reform increased migration from the treated countries by 50%, compared to migration from control countries. The characteristics of migrants from treated countries, measured in terms of predicted test scores, decline by 0.1 standard deviations after the reform, mainly due to changes in language proficiency and parental education. Nevertheless, student migrants' predicted test scores are still higher on average than those of native students, and a similar pattern holds for STEM degrees enrolment rates, although this is not precisely estimated. The new student migrants disproportionately enrol in degrees where capacity constraints are the least binding, suggesting that there might be little crowding-out effects on native students.

References

Amuedo-Dorantes, C. and Furtado, D. (2017). Settling for Academia? H-1B Visas and the Career Choices of International Students in the United States. *Journal of Human Resources*, pp. 0816–8167r1.

Antecol, H., Cobb-Clark, D. A. and Trejo, S. J. (2003). Immigration policy and the skills of immigrants to Australia, Canada, and the United States. *Journal of Human Resources*, **38** (1), 192–218.

Athey, S. and Imbens, G. W. (2019). Machine learning methods that economists should know about. *Annual Review of Economics*, **11**.

Beine, M., Delogu, M. and Ragot, L. (2017). *Understanding the Impact of Tuition Fees in Foreign Education: the Case of the UK*. Working Papers 17-15, Center for Research in Economic Analysis - University of Luxembourg.

—, — and — (2018). The role of fees in foreign education: evidence from Italy. *Journal of Economic Geography*.

—, Noël, R. and Ragot, L. (2014). Determinants of the international mobility of students. *Economics of Education Review*, **41**, 40–54.

Belot, M. and Ederveen, S. (2012). Cultural barriers in migration between OECD countries. *Journal of Population Economics*, **25** (3), 1077–1105.

— and Hatton, T. J. (2012). Immigrant Selection in the OECD. *The Scandinavian Journal of Economics*, **114** (4), 1105–1128.

Bessey, D. (2012). International student migration to Germany. *Empirical Economics*, **42** (1), 345–361.

Caruso, R. and de Wit, H. (2015). Determinants of Mobility of Students in Europe: Empirical Evidence for the Period 1998-2009. *Journal of Studies in International Education*, **19** (3), 265–282.

Chiswick, B. R. (1978). The effect of Americanization on the earnings of foreign-born men. *Journal of Political Economy*, **86** (5), 897–921.

de Hoyos, R., Estrada, R. and Vargas, M. (2018). *Predicting Individual Wellbeing*

Through Test Scores : Evidence from a National Assessment in Mexico. Policy Research Working Paper 8459, World Bank.

Dreher, A. and Poutvaara, P. (2011). Foreign students and migration to the United States. *World Development*, **39** (8), 1294–1307.

Dustmann, C., Fadlon, I. and Weiss, Y. (2011). Return migration, human capital accumulation and the brain drain. *Journal of Development Economics*, **95** (1), 58–67.

— and Glitz, A. (2011). Migration and education. In *Handbook of the Economics of Education*, vol. 4, Elsevier, pp. 327–439.

— and Görlach, J.-S. (2016). The economics of temporary migrations. *Journal of Economic Literature*, **54** (1), 98–136.

Fantom, N. J. and Serajuddin, U. (2016). *The World Bank's classification of countries by income*. Policy Research Working Paper Series 7528, The World Bank.

Friedberg, R. M. (2000). You Can't Take It with You? Immigrant Assimilation and the Portability of Human Capital. *Journal of Labor Economics*, **18** (2), 221–251.

Gale, D. and Shapley, L. S. (1962). College admissions and the stability of marriage. *The American Mathematical Monthly*, **69** (1), 9–15.

Hunt, J. (2011). Which Immigrants Are Most Innovative and Entrepreneurial? Distinctions by Entry Visa. *Journal of Labor Economics*, **29** (3), 417–457.

— and Gauthier-Loiselle, M. (2010). How much does immigration boost innovation? *American Economic Journal: Macroeconomics*, **2** (2), 31–56.

Kahanec, M. and Králiková, R. (2011). *Pulls of International Student Mobility*. IZA Discussion Papers 6233, Institute for the Study of Labor (IZA).

Kato, T. and Sparber, C. (2013). Quotas and quality: The effect of H-1B visa restrictions on the pool of prospective undergraduate students from abroad. *Review of Economics and Statistics*, **95** (1), 109–126.

Kerr, W. R. and Lincoln, W. F. (2010). The supply side of innovation: H-1B visa reforms and US ethnic invention. *Journal of Labor Economics*, **28** (3), 473–508.

Machin, S. and Murphy, R. (2017). Paying out and crowding out? The globalization of higher education. *Journal of Economic Geography*, **17** (5), 1075–1110.

MacLeod, W. B., Riehl, E., Saavedra, J. E. and Urquiola, M. (2017). The big sort: College reputation and labor market outcomes. *American Economic Journal: Applied Economics*, **9** (3), 223–61.

Mullainathan, S. and Spiess, J. (2017). Machine learning: an applied econometric approach. *Journal of Economic Perspectives*, **31** (2), 87–106.

OECD (2014). Indicator C4 - Who studies abroad and where?

Peri, G., Shih, K. and Sparber, C. (2015). STEM workers, H-1B visas, and productivity in US cities. *Journal of Labor Economics*, **33** (S1), S225–S255.

Shih, K. (2016). Labor Market Openness, H-1B Visa Policy, and the Scale of International Student Enrollment in the United States. *Economic Inquiry*, **54** (1), 121–138.

Stuen, E. T., Mobarak, A. M. and Maskus, K. E. (2012). Skilled immigration and innovation: evidence from enrolment fluctuations in us doctoral programmes. *The Economic Journal*, **122** (565), 1143–1176.

Van Mol, C. (2014). *Intra-European Student Mobility in International Higher Education Circuits: Europe on the Move*. Palgrave Studies in Global Higher Education, Palgrave Macmillan UK.

Appendix A

Conceptual framework

Suppose that individuals are distinguished by an observed ability type $z \in [0, \bar{z}]$, drawn from a certain probability distribution with a cumulative distribution function $F(z)$. Denote their expected utility in case of student migration to Spain by $E[u^1] = \alpha^1 + \beta^1 z$. Denote their expected utility in case of non-migration by $E[u^0] = \alpha^0 + \beta^0 z$. Let the cost of migrating be given by $\kappa = \delta - \gamma z$, a reduced-form cost function such that the cost of migration is assumed to decrease with ability, to capture the lower cost of taking an additional international exam for high ability students. Students' migration decisions depend both on their ability level and on the comparative advantage in the returns to that ability. The benefit of migration is given by:

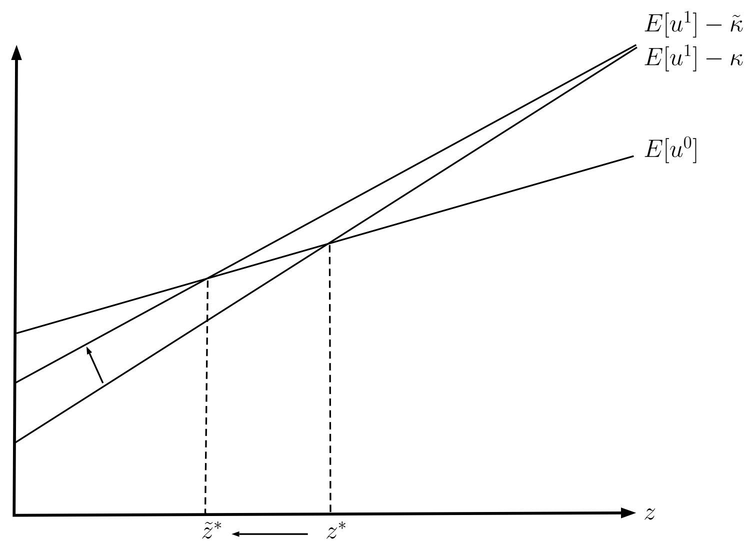
$$E[u^1] - \left(E[u^0] + \kappa\right) = (\alpha_1 - \alpha_0 - \delta) + (\beta_1 - \beta_0 + \gamma)\, z$$

Consider first the case where the payoff from migration increases with ability: $\beta^1 - \beta^0 + \gamma > 0$. If $\alpha_1 - \alpha_0 - \delta > 0$, all students migrate. If instead $\alpha_1 - \alpha_0 - \delta < 0$, there will be migration as long as $\frac{\alpha^0 - \alpha^1 + \delta}{\beta^1 - \beta^0 + \gamma} \leq \bar{z}$, and no migration if $\frac{\alpha^0 - \alpha^1 + \delta}{\beta^1 - \beta^0 + \gamma} > \bar{z}$. In case of an interior solution, only students with $z > z^* \equiv \frac{\alpha^0 - \alpha^1 + \delta}{\beta^1 - \beta^0 + \gamma}$ will migrate, and student migrants will be positively selected from the ability distribution of the home country. The expression for z^* shows that the ability threshold depends on the ratio of ability-unrelated over ability-related costs and benefits of migration. Intuitively, immigrants will be more positively selected if migrating is costly and if the benefits of migrating are ability-related rather than unconditional. The probability of migrating in this case is given by $1 - F(z^*)$.

Removing the requirement of taking the end-of-high-school exam entails both a reduction in the unconditional cost of migrating and in the host country comparative advantage in ability-related returns. The net benefit of migrating becomes larger due to the increase in Human Capital Portability, especially for those with a low z. For those with a high z, it could be quite small, if z is so high that they do not need to prepare for the exam. However, given the bureaucratic costs and fees, one would still expect that the net benefit of migration increases for them as well. Denote by $\tilde{\kappa} = \tilde{\delta} - \tilde{\gamma} z$ the post-reform cost, where $\tilde{\delta} < \delta$ and $\tilde{\gamma} < \gamma$, and such that $\tilde{\kappa} < \kappa, \;\; \forall z$ - or equivalently, such that $\Big((\delta - \tilde{\delta}) - (\gamma - \tilde{\gamma})\bar{z}\Big) > 0$.

Figure 6 depicts the effects of the reform in the case of $\beta^1 - \beta^0 + \gamma > 0$ and $\alpha_1 - \alpha_0 - \delta < 0$ and an interior solution, showing how it would lead to an increase in migration and a decline in student quality.[17] This is because the reform reduces the migration cost for everybody. This can be seen by combining the expressions for z^* and $\tilde{z}^*$, obtaining $z^* - \tilde{z}^* = \frac{(\delta-\tilde{\delta})-(\gamma-\tilde{\gamma})\tilde{z}^*}{(\beta_1-\beta_0+\gamma)}$. Because the reform reduces migration costs for everyone (i.e., $(\delta - \tilde{\delta}) - (\gamma - \tilde{\gamma})z > 0, \ \ \forall z \in [0, \bar{z}]$), the post-reform ability threshold is lower and student quality declines.

Figure 6: Effect of the reform on z^* under positive selection into migration



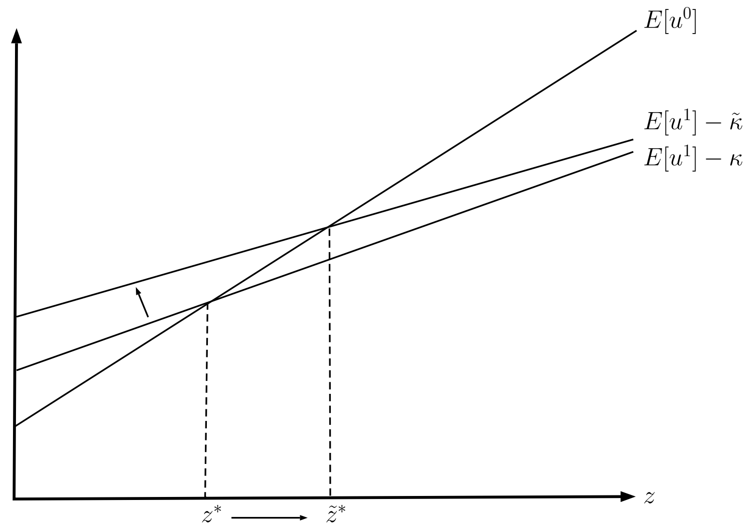
Consider second the case where the migration payoff decreases with ability: $\beta^1 - \beta^0 + \gamma < 0$. If $\alpha_1 - \alpha_0 - \delta > 0$, either $\frac{\alpha^0-\alpha^1+\delta}{\beta^1-\beta^0+\gamma} > \bar{z}$ and all students migrate; or all students with $z \leq z^* \equiv \frac{\alpha^0-\alpha^1+\delta}{\beta^1-\beta^0+\gamma}$ migrate, and the probability of migrating will be given by $F(z^*)$. In the interior solution case, student migrants will be negatively selected from the ability distribution of the home country. In this case, the reform reduces the unconditional migration cost but increases the host country comparative advantage in ability-related returns. This is depicted in figure 7, showing for an interior solution how in this case reducing Human Capital Portability frictions would increase migration and increase the ability threshold, and hence increase the quality of the migrants in the host country.[18] The the post-reform ability

[17]In the case of a corner solution with full migration, everyone still migrates, and there is no change in student quality. In a corner solution with no migration, the reform could lead to some migration.

[18]In the case of a corner solution with full migration, everyone still migrates, and there is no change in student quality. In a corner solution with no migration, the reform could lead to some migration.

threshold is higher in this case because the reform reduces migration costs for everyone and $z^* - \tilde{z}^* = \frac{(\delta-\tilde{\delta})-(\gamma-\tilde{\gamma})\tilde{z}^*}{(\beta_1-\beta_0+\gamma)}$, and thus student quality increases.

Figure 7: Effect of the reform on z^* under negative selection into migration



If instead $\beta^1 - \beta^0 + \gamma < 0$ but $\alpha_1 - \alpha_0 - \delta < 0$, no students migrate before the reform. If the cost reduction is large enough, the reform could lead to some student migration, which would be drawn from the bottom of the home country ability distribution (i.e, students with $z \leq z^* \equiv \frac{\alpha^0-\alpha^1+\tilde{\delta}}{\beta^1-\beta^0+\tilde{\gamma}}$.). Otherwise, there will continue to be no migration.

To sum up, the effects of the reform are not clear ex-ante, and depend on the initial pattern of student migrant selection with respect to the ability distribution in the home country. Note also that an immigrant can be positively selected with respect to her home country ability distribution, but negatively selected with respect to her host country ability distribution. z could be drawn from different distributions in every country of origin, and be distributed according to $F_c(z)$, where c indicates country of origin. This could be, for instance, due to cross-country differences in education quality and dropout rates in secondary and high school. Other parameters in the previous expressions are likely to change by country of origin c: differences in the expected utility in case of non-migration $E[u_c^0] = \alpha_c^0 + \beta_c^0 z$ and in the cost of migration $\kappa_c = \delta_c - \gamma_c z$ may lead to country-specific ability thresholds z_c^*. Hence, one might expect both different mappings between ability thresholds and the probability of migrating across countries of origin, as well as different ability thresholds by country.

Finally, note that student migrations have been presented as a one shot decision, while they are very frequently temporary migrations. This is motivated by the empirical exercise, that focuses on the quantity and the quality of student migrations, but cannot say anything about the length of migration, and is also motivated by the fact that most treated students could stay in Spain both before and after the reform, and hence one would not expect the reform to have important changes in return behaviour besides those arising from differences in selection. Dustmann and Glitz (2011), Dustmann *et al.* (2011), or Dustmann and Görlach (2016) present fully specified models (and empirical evidence) for temporary migrations like student migrations.

Appendix B

Table 11: Treated Countries

Austria	Greece	Norway
Belgium	Hungary	Poland
Bulgaria	Iceland	Portugal
China	Ireland	Romania
Cyprus	Italy	Romania
Czech Republic	Latvia	Slovakia
Denmark	Liechtenstein	Slovenia
Estonia	Lithuania	Sweden
Finland	Luxembourg	Switzerland
France	Malta	United Kingdom
Germany	Netherlands	

Table 12: Control Countries

Albania	Georgia	Other - Oceania
Angola	Guatemala	Pakistan
Argentina	Guinea Equatorial	Panamá
Armenia	Honduras	Paraguay
Australia	India	Perú
Belarus	Iran	Philippines
Belize	Israel	Puerto Rico
Bolivia	Japan	Qatar
Bosnia and Hercegovina	Jordan	Republica Dominicana
Brasil	Kazakhstan	Russia
Cameroon	Kuwait	Saint Christopher and Nevis
Canada	Lebanon	Salomo
Chad	Malaysia	South Africa
Chile	Mexico	South Korea
Colombia	Moldova	Swaziland
Costa Rica	Morocco	Syria
Croatia	Nepal	Trinidad and Tobago
Cuba	New Zealand	Tunisia
DRC	Nicaragua	Turkey
Ecuador	Nigeria	UAE
Egypt	North Korea	Ukraine
El Salvador	Other	Uruguay
Eritrea	Other - America	USA
Fiji	Other - Asia	Venezuela
Gabon	Other - Europe	

Table 13: Descriptive statistics, student enrolment

Variable Name	Treated, Pre	Control, Pre	Treated, Post	Control, Post
# Students	3.411	3.223	8.786	2.641
	(8.309)	(7.430)	(21.62)	(5.742)
log(1+#Students)	0.844	0.714	1.401	0.667
	(0.955)	(1.020)	(1.187)	(0.956)
% Students	0.0166	0.0157	0.0400	0.0122
	(0.0404)	(0.0362)	(0.0961)	(0.0270)
N	56	130	168	390
# of countries	28	65	28	65

Only immigrants. Every observation is a country × year. % of Students (Applicants) out of Total Students (Applicants).

Table 14: HK Portability and ISM, Placebo Test

	(1) #Students	(2) log(1+#Students)	(3) % Students
Treated Country × Post	6.395**	0.655***	0.0291**
	(2.732)	(0.155)	(0.0121)
Treated Country × Post 2006	-0.876	-0.102	-0.00424
	(0.709)	(0.137)	(0.00327)
Country FE	✓	✓	✓
Year FE	✓	✓	✓
Mean Dep. Var	4.188	0.854	0.0195
R^2 within	.025	.072	.026
N	744	744	744

Standard errors clustered at the country level in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 15: Descriptive statistics, High Income Control Countries

Variable Name	Pre	Post
# Students	4.410	3.397
	(8.336)	(5.992)
log(1+#Students)	0.951	0.840
	(1.118)	(1.040)
% Students	0.0215	0.0157
	(0.0404)	(0.0281)
N	78	234
# of countries	39	39

Only immigrants, from High Income Control Countries.

Every observation is a country × year. % of Students out of Total Students.

Table 16: HK Portability and ISM, High Income Control Countries

	(1) #Students	(2) log(1+#Students)	(3) % Students
Treated Country × Post	6.388**	0.667***	0.0292**
	(2.673)	(0.139)	(0.0119)
Country FE	✓	✓	✓
Year FE	✓	✓	✓
Mean Dep. Var	5.235	1.032	0.0243
R^2 within	.024	.09	.026
N	536	536	536

Standard errors clustered at the country level in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 17: HKP and ISM, High Income Control Countries, Placebo

	(1) #Students	(2) log(1+#Students)	(3) % Students
Treated Country × Post	6.505**	0.697***	0.0299**
	(2.759)	(0.165)	(0.0123)
Treated Country × Post 2006	-0.235	-0.0603	-0.00139
	(0.875)	(0.150)	(0.00401)
Country FE	✓	✓	✓
Year FE	✓	✓	✓
Mean Dep. Var	5.235	1.032	0.0243
R^2 within	.024	.09	.026
N	536	536	536

Standard errors clustered at the country level in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 18: HK Portability and ISM, excluding BLG and ROU

	(1) #Students	(2) log(1+#Students)	(3) % Students
Treated Country × Post	5.941**	0.603***	0.0269**
	(2.808)	(0.136)	(0.0124)
Country FE	✓	✓	✓
Year FE	✓	✓	✓
Mean Dep. Var	4.089	0.829	0.0190
R^2 within	.024	.069	.025
N	728	728	728

Standard errors clustered at the country level in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 19: HKP and ISM, excluding CHN

	(1) #Students	(2) log(1+#Students)	(3) % Students
Treated Country × Post	5.736**	0.562***	0.0260**
	(2.706)	(0.125)	(0.0120)
Country FE	✓	✓	✓
Year FE	✓	✓	✓
Mean Dep. Var	4.130	0.844	0.0192
R^2 within	.023	.064	.024
N	736	736	736

Standard errors clustered at the country level in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 20: HK Portability and ISM, controlling for unemployment

	(1) #Students	(2) log(1+#Students)	(3) % Students
Treated Country × Post	6.378**	0.647***	0.0291**
	(2.670)	(0.133)	(0.0118)
Country FE	✓	✓	✓
Year FE	✓	✓	✓
Unemployment Rates	✓	✓	✓
Mean Dep. Var	4.364	0.885	0.0203
R^2 within	.027	.08	.029
N	712	712	712

Standard errors clustered at the country level in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Unemployment rates include controls for the national unemployment rate and the one for the youth (15-24) in the country of origin

Table 21: HKP and ISM, 2006-2009

	(1) #Students	(2) log(1+#Students)	(3) % Students
Treated Country × Post	5.349***	0.688***	0.0250***
	(1.375)	(0.124)	(0.00631)
Country FE	✓	✓	✓
Year FE	✓	✓	✓
Mean Dep. Var	4.026	0.861	0.0193
R^2 within	.087	.131	.09
N	465	465	465

Sample: 2006-2009. Standard errors clustered at the country level in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 22: HK Portability and ISM - heterogeneous effects

	(1) #Students	(2) log(1+#Students)	(3) % Students
Treated Country × Post	2.860***	0.578***	0.0136***
	(0.822)	(0.144)	(0.00378)
Treated Country × Post × Latin-speaking	12.39	0.104	0.0533
	(9.307)	(0.276)	(0.0413)
Country FE	✓	✓	✓
Year FE	✓	✓	✓
Mean Dep. Var	4.188	0.854	0.0195
R^2 within	.054	.072	.053
N	744	744	744

Standard errors clustered at the country level in parentheses.

Latin-speaking: Italy, France, Portugal, Belgium, Romania, Switzerland, and Luxembourg.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 23: Descriptive statistics, Spanish vs. Treated and Control migrants

Variable Name	Spanish	All Immigrants	Treated	Control
Predicted Test Scores	0.107	0.137	0.366	-0.128
	(0.369)	(0.407)	(0.328)	(0.317)
Educated Father	0.314	0.493	0.469	0.520
	(0.464)	(0.500)	(0.499)	(0.500)
Educated Mother	0.319	0.459	0.443	0.476
	(0.466)	(0.498)	(0.497)	(0.500)
White Collar father	0.261	0.324	0.306	0.346
	(0.439)	(0.468)	(0.461)	(0.476)
White Collar Mother	0.210	0.251	0.257	0.244
	(0.407)	(0.434)	(0.437)	(0.430)
Spanish Proficiency Score	0.980	0.819	0.697	0.958
	(0.0991)	(0.352)	(0.416)	(0.175)
English Proficiency Score	0.553	0.639	0.661	0.613
	(0.416)	(0.442)	(0.439)	(0.446)
Catalan Proficiency Score	0.940	0.209	0.165	0.260
	(0.206)	(0.331)	(0.314)	(0.342)
Female	0.574	0.614	0.648	0.574
	(0.495)	(0.487)	(0.478)	(0.495)
STEM degree	0.479	0.548	0.626	0.458
	(0.500)	(0.498)	(0.484)	(0.498)
N	167584	3116	1667	1449
# of countries	1	93	28	65

Treated and Control are only Immigrants

Table 24: First Stage

	Entrance Grade		Entrance Grade
Primary Schooling (father)	0.960***	Primary Schooling (mother)	-0.107
	(0.224)		(0.260)
Secondary Schooling (father)	1.392***	Secondary Schooling (mother)	-0.120
	(0.229)		(0.211)
High School or Apprenticeship (father)	1.024***	High School or Apprenticeship (mother)	-0.0326
	(0.167)		(0.200)
Short College Degree (father)	0.950***	Short College Degree (mother)	-0.118
	(0.177)		(0.219)
PhD or Long College Degree (father)	1.060***	PhD or Long College Degree (mother)	0.0688
	(0.172)		(0.209)
Other (father)	1.070***	Other (mother)	0.0975
	(0.149)		(0.196)
Missing occupation (father)	0.158	Missing occupation (mother)	-0.125
	(0.194)		(0.140)
CEO (father)	-0.0578	CEO (mother)	0.0581
	(0.198)		(0.180)
White collar requiring college (father)	-0.0178	White collar requiring college (mother)	-0.0397
	(0.205)		(0.156)
Skilled worker, agriculture, livestock or fishing (father)	0.355	Skilled worker, agriculture, livestock or fishing (mother)	-1.208***
	(0.251)		(0.186)
Skilled worker, industry (father)	-0.384	Skilled worker, industry (mother)	0.404
	(0.222)		(0.293)
Skilled worker, construction or mining (father)	-0.0383		
	(0.253)		
Army (father)	-0.464	Army (mother)	-0.208
	(0.270)		(0.312)
Other (father)	-0.135	Other (mother)	0.0565
	(0.193)		(0.137)
Never worked (father)	-0.236	Never worked (mother)	0.307
	(0.232)		(0.248)
Catalan, very low level	0.00986	Spanish, very low level	0.293
	(0.117)		(0.286)
Catalan, low level	-0.0169	Spanish, low level	0.0328
	(0.0812)		(0.268)
Catalan, high level	-0.00801	Spanish, high level	-0.162
	(0.132)		(0.322)
Catalan, very high level	-0.271*	Spanish, very high level	0.00568
	(0.123)		(0.199)
English, very low level	0.121	English, high level	0.269
	(0.184)		(0.166)
English, low level	0.217	English, very high level	0.235**
	(0.129)		(0.0863)
Treated country	0.452***	Female	0.225***
	(0.0782)		(0.0670)

Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
$N = 610$ Kleibergen-Paap F: 776.7
Education base category: no education. Occupation base category: blue-collar.
Language base category: no knowledge. Sample: migrants, pre-treatment period.

Table 25: Predicted test scores and post-treatment test scores, Natives

	Correlation between Test Scores and Predicted Test Scores
Pre-treatment	0.3604
Post-treatment	0.3944

	Post-Treatment Test Scores
Predicted Test Scores	1.0448***
	(0.0065)
N	127,529

Only Natives.

Robust standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 8: Predicted Test Scores, natives

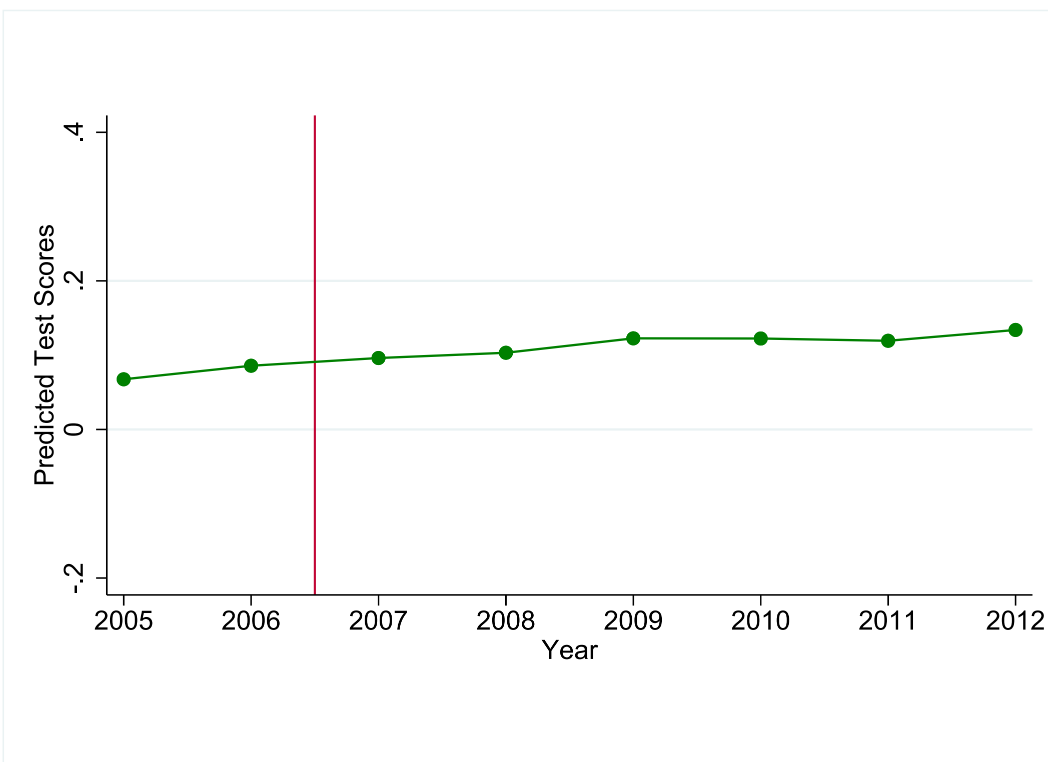
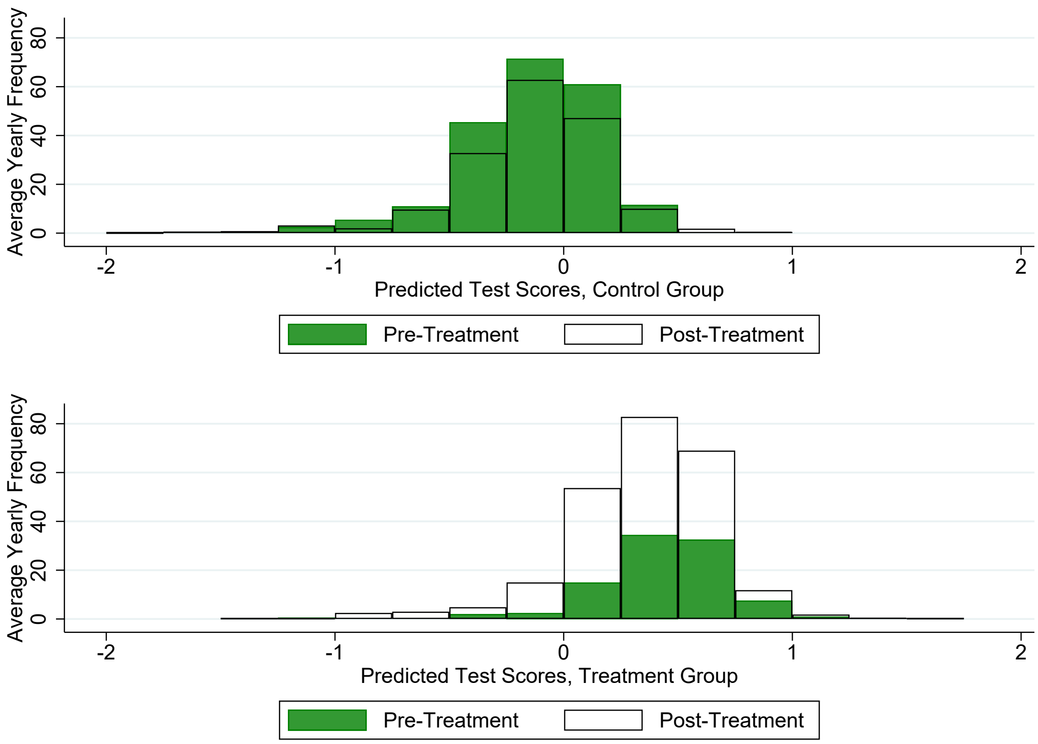


Figure 9: Frequency differences in predicted test scores, migrants



STEM enrolment

Previous research has emphasised that the innovation and economic dynamism gains from high skilled migration tend to come from STEM graduates (Hunt and Gauthier-Loiselle (2010), Kerr and Lincoln (2010), Peri *et al.* (2015)). In addition to the standard selection argument combined with STEM students being usually high ability students, there is another mechanism why one would expect changes in STEM enrolment. The Spanish end-of-high-school exam has a core and a field part. The field part depends on the high school track (arts, humanities, social sciences, engineering, science), and it determines to what degrees the students can apply for enrolment. STEM subjects (engineering, science) require less knowledge of Spanish, and hence the reform could be making more of a difference for students willing to enrol in humanities or social sciences, for which they would have needed a higher country-specific preparation before the reform. Figure 10 displays the average number of student migrants by country enrolling in a STEM degree by treatment group, and table 26 the corresponding point estimates. Unsurprisingly, because the reform is increasing student migration, it is also increasing STEM student migration.

However, it is also interesting to test if the new migrants are enrolling in STEM in similar proportions. Figure 11 displays the fraction of students enrolling in a STEM degree by treatment group. Migrants from treated countries were more likely to enrol in STEM degrees before the reform, but this enrolment gap seems to narrow after 2007.

Table 27 reports difference-in-differences estimates on the individual probability of enrolling in a STEM program. The difference-in-differences estimates captures the pattern in figure 11, and shows that on average, the propensity to enrol in STEM degrees declines from being 25pp larger for migrants from treated countries to being around 12.5pp larger. However, this effect is not precisely estimated. Nonetheless, student migrants from treated countries have higher STEM enrolment rates than natives (figure 12), meaning that the reform increased enrolment in STEM programs overall.

Figure 10: STEM Student Migration, treated vs. control countries

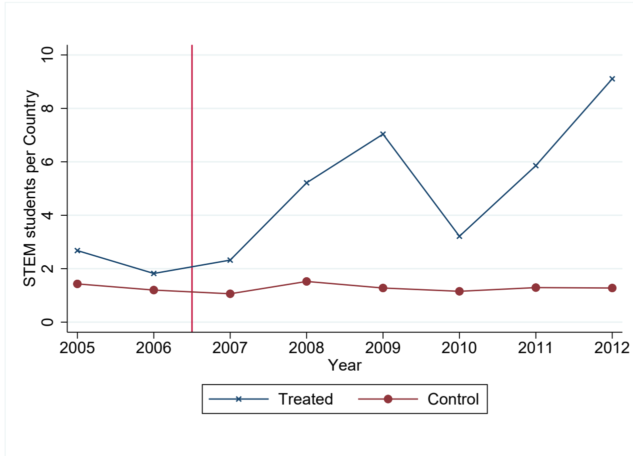


Table 26: STEM Student Migration

	(1) #STEM Students	(2) log(1+#STEM Students)
Treated Country × Post	3.260	0.299**
	(2.482)	(0.122)
Country FE	✓	✓
Year FE	✓	✓
Mean Dep. Var	2.294	0.509
R^2 within	.008	.023
N	744	744

Standard errors clustered at the country level in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 11: STEM enrolment propensity, student migrants

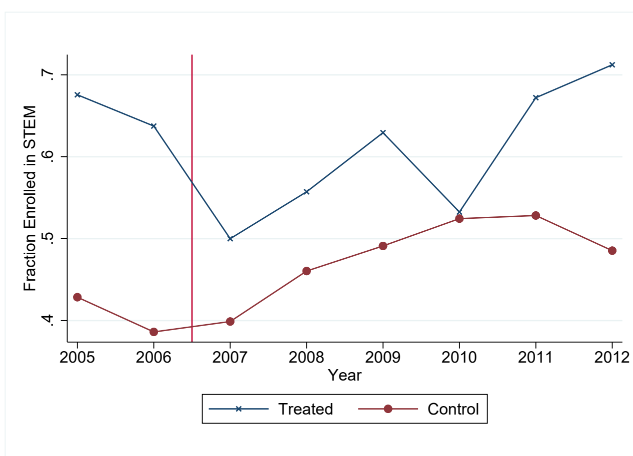


Table 27: STEM enrolment propensity, student migrants

	STEM enrollment	
	(1)	(2)
Treated Country	0.249	0.247
	(0.178)	(0.163)
Treated× Post	-0.122	-0.125
	(0.141)	(0.144)
Treated× Post 2005		0.00426
		(0.103)
Year FE	✓	✓
Mean Dep. Var	0.548	0.548
R^2 within	.023	.023
N	3116	3116

Standard errors clustered by country of origin in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 12: STEM enrolment propensity, natives