Three Essays on Higher Education

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THESIS

Submitted as partial fulfillment of the requirements
for the degree of Doctor of Philosophy in Economics
in the Graduate College of the
University of Illinois at Chicago, 2020

Chicago, Illinois

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I dedicate this dissertation to my late mother, Mok Wai Heng, without whom all of my accomplishments would never have been possible.

ACKNOWLEDGEMENTS

I am indebted to my advisor Ben Ost for his continued guidance and support. I am grateful to other members of dissertation committee – Erik Hembre, Faith Kares, Darren Lubotsky, Javaeria Qureshi, and Steven Rivkin – for their many valuable feedback. I thank Robert Kaestner for his continued encouragement. I also like to acknowledge the help and assistance of all the individuals whose names may not all be enumerated. Lastly, I would like to thank my family, especially my father and brother for their love and support.

CONTRIBUTION OF AUTHORS

Chapter 1 and Chapter 2 are both unpublished manuscript for which I was the primary author and major driver of the research. Chapter 3 represents a published manuscript (Khoo & Ost, 2018) for which I was a co-author along with my dissertation chair, Dr. Ben Ost. We independently developed this research idea before collaborating to design the model and perform the analysis. I generated all the tables and figures with critical input from Dr. Ben Ost, and I played a large role in the drafting of the manuscript. My work was critical to the conclusions of this manuscript because of the collaboration.

SUMMARY

This dissertation studies the determinants and consequences of higher education and its link to the labor market. First, I consider peers as an input to the production function of education for international students. Sharing a country of origin with peers can affect academic success by directly altering academic and social networks, as well as eliciting an indirect environmental response. The direct effect stems from how social bonds are formed based on similarities in culture and language, while the indirect environmental response encompasses responses from administrators, instructors, and domestic students. Using administrative data on higher education enrollment in Ohio, I document the first evidence of peer effects among international students in doctoral programs. I find that an increase in the share of peers from one's country of origin decreases the probability of persisting to the second year of the Ph.D. program. These country match effects are not driven by language or region match effects nor are they driven by the general effect of international students.

In my second paper, I study how immigration policies impact the schooling and major choices for international students. Specifically, I evaluate how the extension of Optional Practical Training (OPT) for science, technology, engineering, and mathematics (STEM) graduates affect the enrollment decision of international students into STEM fields in the US. I also examine the effect of the policy change on degree completion of foreign students. I find that there is an increase in the likelihood of enrollment and completion rate of foreign students in STEM fields after the OPT extension. This finding suggests that there is a premium for work experience in the US, and international students make schooling choices based on this opportunity. It also provides evidence

that OPT is an effective tool for changing the skill composition of students in higher education as well as a potential solution to the STEM shortage in the labor market.

The third paper, co-authored with Ben Ost, examines the effect of graduating with Latin honors (e.g. Cum Laude) on the earnings of recent college graduates (Khoo & Ost, 2018). We find that obtaining honors provides an economic return in the labor market, but this benefit only persists for two years. By the third year after college, we see no effect of having received honors on wages, suggesting that firms may use the signal for new graduates, but they do not rely on the signal for determining the pay of more experienced workers. This provides among the first pieces of evidence that firms respond to signals at the higher education level.

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1 A PEER LIKE ME? PEER EFFECTS AMONG INTERNATIONAL STUDENTS IN DOCTORAL PROGRAMS

1.1 Introduction

Working or learning with an individual who shares one's language and culture has the potential to affect performance because shared social capital can improve communication, foster trust and strengthen bonds. Evidence of such effects has been reported in a variety of contexts including primary school students (Dee, 2004), taxi drivers (Jackson and Schneider, 2011) and refugee immigrants in Sweden (Edin et al., 2003). For doctoral students, peers have the potential to be particularly important because of the immersive nature of Ph.D. programs in which cohorts of students take many courses together, study together, and socialize together. Despite the likely importance of peers, there is relatively little evidence on how doctoral student performance responds to their peers' characteristics. In this paper, I provide the first evidence on whether international doctoral students are affected by the presence of peers from their same country of origin. Specifically, I test whether persistence in the program changes as a response to having more same-country peers.

Sharing a country of origin with peers in graduate school can affect success directly by altering social networks and through an indirect institutional response. The direct effect stems from the fact that peers are likely an important determinant of academic success and peers from the same country of origin are a particular salient group in this regard. Peers from the same country of origin are likely to form a tight social network because of similarities in culture and language. This network can affect success by altering both academic and non-academic interactions. For example, a doctoral student with many same-country peers may socialize more and study less. Alternatively, a strong network might ease the process of social and academic integration, which has been shown to correlate positively with student retention.[1] Though integration is generally beneficial, if students only interact with peers from the same country of origin, these social ties might lead to isolation from the rest of the student cohort and a failure to assimilate to the environment may hinder performance (Andrade, 2006). In addition to the direct network effect, there may be institutional or environmental responses that can affect international students, as the share of peers from a particular country increases. For example, with more students from one country (culture) in a class, instructors may cater instruction to the typical learning styles of those students.

Given the possible pathways of influence, how same-country peers affect success, for example, persistence in the program, is an empirical question. To answer this question, I use unique administrative data from the state of Ohio that covers the universe of students enrolled in doctoral programs over a 7-year period.[2] Importantly, the administrative data allow me to identify the country of origin for each student. Therefore, I can construct a variety of measures of peer composition each year for each Ph.D. program. The data is longitudinal so it is possible to track how outcomes evolve for individual students and to compare outcomes across cohorts for whom the share of same country peers differ within a specific Ph.D. program. Using this within program, across cohort variation, I find that, a 10-percentage point increase in the share of peers from one's

[1] The study of student retention has spanned several fields including sociology and higher education studies. Spady (1970, 1971) recognized the two systems (academic and social) whereby students have to be fully integrated in order to persist in their academic institution. Since then, many student retention models have been built on these two factors, such as Tinto's Institutional Departure Model (1975, 1993), Bean's Student Attrition Model (1980, 1982), and the Student Retention Integrated Model (Cabrera, Nora, & Castaneda, 1993). For a comprehensive overview of student retention models, see Aljohani (2010).

[2] I use the term "program" to refer to a Ph.D. granting program at a particular institution e.g. Economics at Ohio State, or Integrated Bioscience at Akron University.

country of origin decreases the probability of persisting to the second year of the Ph.D. by two percentage point (2.35%). This suggest that an increase from 20% to 30% in share of peers from the same country of origin would see a reduction in first-to-second year persistence from 85% to 83%.[3]

The key empirical challenge to estimating the effect of peers on doctoral student persistence is that the types of students who enroll in each Ph.D. program is the result of systematic choices on the part of the students and the admission committee. As discussed in Manski (1993), this systematic sorting is likely correlated with potential outcomes and could create the appearance of peer effects where none exist.[4] For example, Chinese students are more likely to enroll in Economics Ph.D. programs than Sociology Ph.D. programs and the two types of programs have different time-to-completion rates. To address this type of concern, I include program, country-of-origin, and entry-year cohort fixed effects and exploit year-to-year variation in the fraction of students from different countries in a specific program. The country-of-origin fixed effects account for general differences in the persistence rates of different nationalities and the entry-year cohort fixed effects account for any changes over time in the cohorts that enroll in academic institutions.

Unlike the typical cross-cohort identification strategy, in my context, students from the same cohort and program may have different treatment exposure since treatment is also determined based on one's country of origin. This within program-by-cohort variation allows me to account

[3] For instance, say the cohort size for the Department of Economics at Ohio State is 10. Now, instead of having two peers from the same country, there is now three peers from the same country, the likelihood of persisting for the student has decreased by 2 percentage points.

[4] Manski (1993) also discusses two other threats to estimating peer effects, namely common shocks and reflection. Neither of these issues applies in my context since I study the effect of country of origin, an immutable peer characteristic.

for several more nuanced forms of sorting into programs. First, I can account for the possibility that the form of sorting into programs differs according to country of origin by including a program-by-country fixed effect. With this control, an example of the type of comparison I make is as follow. I compare Indian students in Economics at Ohio state from one cohort vs Indian students in Economics at Ohio state in a different cohort. Second, I can account for changes over time in the types of students who come from each country of origin using country-by-cohort (but not program) fixed effects. Finally, I can account for unobserved program-specific changes over time by including a program-by-cohort fixed effect. With all of these fixed effects included simultaneously, the remaining source of identifying variation is analogous to that of a triple difference specification where the differencing levels are cross cohort, cross country, and cross program.[5] I provide several pieces of evidence suggesting that conditional on the fixed effects discussed above, the remaining variation in country of origin is unlikely to be correlated with potential outcomes.

The paucity of research on the role of peers in international students' post-graduate education is partly due to a lack of data. My data is uniquely well suited for studying the effect of doctoral peers because having the universe of doctoral students allows me to construct the exact peer

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[5] Some may worry that with many high-dimensional fixed effects included simultaneously, there would be insufficient remaining variation in the country composition of cohorts. This is not an issue in practice, likely because there are many idiosyncratic forces shaping the composition of entering cohorts. For example, if program A admits 3 Chinese students each year, it is very possible that in some years all 3 Chinese students will come and in other years, none will attend. Furthermore, in some years, the top tier of applicants may include 3 Chinese students, whereas in other years, the top tier of applicants may include only 1 Chinese student. The general point is that there is considerable scope for idiosyncratic variation in cohort country composition that would not be predicted by program-by-country, cohort-by-country, or program-by-cohort fixed effects.

composition for every student at every Ph.D. program in Ohio. Furthermore, this detailed, administrative dataset tracks international students from their first term of enrollment to their completion of doctoral degree or until they exit the program without a degree. This facilitates studying both the short- and long-run effect of peers.

To explore further the mechanisms driving my main results, I first assess whether the own-country effect is simply driven by a general negative spillover among international students. I find that the effect of own-country peers is similar after accounting for the general effect of international peers. Second, I consider whether peers from one's same region (e.g. southeast Asia) have a similar effect as peers from one's own country. I find that conditional on the own-country effect, peers from one's broad region do not affect persistence. The own-country effect is virtually unchanged when controlling for the own-region effect suggesting that to the extent that the effect comes from cultural match, this match is based on country-specific factors. Third, I assess whether language commonality is a likely mechanism by constructing a peer language measure for each international student. Specifically, I use data from the CIA world factbook to identify primary and secondary languages for each country, and I then calculate the fraction of one's cohort that is likely to share the same language. I find that language match is unable to explain the own-country peer effect and there is no direct effect of having peers who speak the same native language. Finally, I examine whether sharing a country of origin with second year Ph.D. students affects the persistence of first-year Ph.D. students. I find that the effect of peers from one's country is much stronger if they are in the same cohort, with the cross cohort spillover being one quarter the size and not statistically significant.

My study fits at the intersection of several literatures. First, it contributes to the literature seeking to understand how doctoral student responds to peer characteristics. There is no existing evidence on the effect of peers from one's country of origin, but Bostwick and Weinberg (2018) studied the effect of gender composition and find that an increase in the share of women increases the probability of on-time graduation among female STEM doctoral students. Pezonni et al. (2016) examined graduate student publications, but find no relationship between the number of publication and the gender composition of the team.[6] Second, my study contributes to the large literature on peer effects in education settings. Most closely related are studies considering how peer racial composition affects students of different races in K-12 education (e.g. Hoxby, 2000; Hanushek et al., 2009). Third, my study contributes to the literature seeking to understand the determinants of international student success (Olivas & Li, 2006; Hyun et al., 2007; Sümer et al., 2008). Finally, my study contributes to the literature examining how economic outcomes are affected by cultural match (Jackson & Schneider, 2011; Edin et al., 2003).

1.2 <u>Literature Review</u>

There is a large literature on the effects of peers in an educational setting. Sacerdote (2011) provides a survey of this literature and documents how students' peers affect their performance in both primary, secondary, and post-secondary education. Most of the research in primary and secondary schools focuses on test scores as the main outcome variable,[7] while the research in post-secondary education is more likely to exploit the randomization of individuals to their peer groups—roommate, dormmate, and squadron member—to study how the characteristics and

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[6] There is also a related literature that considers the effect of having a female mentor on female doctoral students e.g. (Pezzoni et al. 2016; Neumark and Gardecki, 1998; Hilmer and Hilmer, 2007).

[7] Some of the research Sacerdote (2011) cites include Boozer and Cacciola (2001), Hanushek, Kain, and Rivkin (2003), Betts and Zau (2004), Lefgren (2004); Hoxby and Weingarth (2005), Vigdor and Nechyba (2007); Burke and Sass (2013).

background and behavior of peers have on individuals (Sacerdote, 2001; Zimmerman, 2003; Stinebricker & Stinebrickner, 2006; Carrell et al., 2008).

There are fewer studies that examine peer effects in doctoral programs and those that exist focus on gender effects. Most closely related to my paper is Bostwick and Weinberg (2018) who examine the effect of peer gender composition in STEM doctoral programs. They find that an increase in the share of women increases the probability of on-time graduation for female students. Pezzoni et al. (2016) studied graduate student publications for multiple Ph.D. cohorts in one university, focusing on students' gender, advisor's gender, gender pairing of advisor with student, and the gender composition of the team. While they did not find any evidence of a relationship between the number of publication and gender composition of the team, they did find that students who write with female advisors have a higher publishing rate.

The effect of student-peer match effects relates to a larger strand of literature that focuses on gender, race and ethnic pairing of students to their instructors. In doctoral programs, much of this research focuses on the gender match between the student and her advisor (Neumark and Gardecki, 1998; Hilmer and Hilmer, 2007, Pezzoni et al., 2016). For primary and secondary school students, researchers have studied the effect of instructor gender and race match effects and generally find that same race or gender matching is beneficial for students (Dee, 2004, 2005; Ehrenberg & Brewer, 1995; Klopfenstein, 2005; Gong et al., 2018). Similar to the literature on primary and secondary schools, several studies consider the effect of professor-student racial match in the post-secondary school setting. These studies generally find similar positive effects for minority students of having a minority instructor (Price, 2010, Fairlie et al., 2014). Although minority students gain from matching to teachers of the same race, minority students in the K-12 setting tend to do worse academically when the class share of peers of the same race increases (Hoxby, 2000; Hanushek et al., 2009).

As my review has revealed, there are no studies of the effect of same-country peers on academic success of Ph.D. students. This is a gap because in many fields, international students are a large share of students and the share of international students differs across universities even in the same field. Mine is the first paper to test for it.

1.3 Conceptual Framework

Economists, sociologists and psychologists have long studied the factors that affect student retention. The consensus reached is that students are more likely to persist in their academic institution if they are fully integrated in both the academic and social systems (Spady, 1970, 1971; Tinto, 1975, 1987 1993; Bean, 1980; Astin, 1984; Berry, 1997; Ward et al., 2005; Habley et al., 2012). There are various reasons to think that peers from the same country of origin might be an important peer group (network) in the process of integrating into both academic and social system. I describe two main dimensions of how country-of-origin network—social and institution—could potentially affect international students when considering their performance and success.

The first dimension is the social dimension, which refers to the changes in social ties that come with changes in the share of peers from the same country of origin. Within the social dimension, I identify two channels in which social ties can be form. People sort on similar interests and backgrounds (McPherson, 2001), and having grown up in a different culture, and possibly using a different language, it is considerably harder for international students to share common ground with natives (Rajapaksa and Dundes, 2002). It is easier to form a connection with people who share the same cultural background experience. Hence, one of the channels for social ties to form is through culture. Another channel is language. For many international students, English is not their first language. This language barrier is an obstacle to communicate or learn effectively with other students and instructors. Such barriers do not exist with peers who speak the same language, and same-country peers may make studying and learning easier as nothing gets lost in translation. The common culture and language as well as the experience of being in a foreign surrounding creates a sense of belonging in the doctoral program.

These social ties just described also make it easier and create more occasions to engage in social activities, which could have an adverse effect on their academic performance. One of the challenges that all students face is the tradeoff between socializing and studying.[8] International students with a larger share of peers from the same country of origin find themselves with greater and more rewarding social opportunities that crowd out studying and other academic efforts. therefore opt for more leisure than cohorts with lower share of peers from the same country of origin. The focus on more leisure may come at the expense of academic investment, which would be detrimental to their success in the program.

Ex-ante, it is unclear how social ties will affect the students' academic performance. On one hand, the network may enhance the sense of belonging and provide academic support for international students, which ease with both the academic and social integration.[9] However, as share of peers from the same country of origin increases, it could result in an increase in the risk of isolation from the rest of the cohort.[10] For example, a single Chinese student in a program may form a study group

[8] Andrade (2006, 2007) and Mamiseishvili (2012) documents this tradeoff for international students.

[9] As discussed earlier, these two factors correlate highly with student retention.

[10] Lazear's Model of Culture (1999) showed that assimilation is less likely to take place when there is a broad representation of an immigrant's culture and language in the new environment.

with students from other countries or domestic students, but a Chinese student with many Chinese peers may form a study group made up of primarily Chinese peers. This isolation could potentially hurt the process of adjusting into the environment at large as well as hinder learning when there is no flow of information from the rest of the cohort.

The second dimension of how same-country peers may affect academic success is an institutional response. The institutional response encompasses the notion that the surrounding environment may respond to changing cohort composition, which could potentially help or hurt students from the country with a growing share. For example, it is possible that instructors change their method of teaching as the share of students from the same country becomes significant; there could be more investment from the part of the instructors to make sure that the material is understood by the majority. Hoxby and Weingarth (2005) refer to this circumstance as consistent with a boutique model of education where institutions purposely serve similar students who then have better achievement because they when surrounded by peers with similar characteristics.

On the other hand, the environment may remain supportive of a minority person as long as they remain a minority. Once the minority reaches a critical mass, the institution is less likely to be as helpful or inclusive. Following the subculture theory (Becker, 1963), international students might be considered outsiders as they do not fit in with the values and norms of the rest of the cohort. A large share of peers from the same country could elicit animosity from the rest of the cohort as they now threaten to change the environment that works for the rest of the majority. Relatedly, instructors may be less likely to give extra attention to a group of students who are now a bigger share of the class as they presume that students will have sufficient help from their other peers, and instead, give their focus to other students who are still minorities in the cohort.

Empirically, it is not possible to separately identify institutional responses from the direct effect of peers so my estimates represent a reduced form combination of the two mechanisms. That said, I am able to provide indirect evidence on certain drivers of the peer mechanism. For example, I test for whether language sharing is a likely mechanism by investigating the effect of sharing the same language with other international students. Similarly, depending on the mechanisms at play, the effect of peers will be more strongly related to the number of peers from one's country or the share of the cohort from one's country. An additional peer results in a larger change in density in a small cohort compared to an additional peer in a big cohort. If same-country peers affect outcomes by providing a group of friends to socialize and study with, the effect of an extra same-country peer should be the same in a large program vs a small program. If, on the other hand, having more same-country peers alters group dynamics or generates an institutional response, the effect of an additional peer will be larger in a small program. In this case, the *share* of same-country peers is more relevant than the *number* of same-country peers. Though far from definitive, I provide indirect evidence on mechanisms by carefully considering these factors.

1.4 Data

My primary data are administrative records made available to researchers by the Ohio Education Research Center (OERC). The data include term-level data on admission, enrollment, demographics and field of studies, and they cover all students, including international students enrolled in public tertiary education in the state of Ohio from 2000 to 2015. Since information on the country of origin of international students are only available from 2009 onwards, I restricted my analysis to students enrolled in schools from 2009 onwards.[11]

[11] There are students who have information on their country of origin even though they enrolled before the year 2009. Some of them have such information as they had enrolled in programs later in the year. I chose to exclude students

I focus on international students enrolled in doctoral programs. There are 12 schools that offer doctoral programs in Ohio. The advantage of focusing on doctoral programs is twofold. First, peers in doctoral studies are more distinct and more easily identified than in undergraduate programs. Doctoral students are more likely to attend classes or progress in the program with peers who enter at the department at the same time as them. Cohorts of doctoral programs are much smaller than undergraduate cohorts, and network ties are more intensive among doctoral students. Second, given that international students make up a significant share of population in doctoral programs, especially in science, technology, engineering and mathematics (STEM) fields, this population seems to be more pertinent to study.[12] I define a cohort to be those entering a doctoral program in a specific school in the same year. I refer to these unique program-school combinations as departments.[13] [14] I restrict the analysis to departments that have more than 5 people enrolled.

who enrolled before 2009 as it might be that students only had information if they persisted into the program until 2009. I dropped less than 3% of the dataset with this restriction.

[12] According to NCES, in fall 2017, while the number of nonresident alien undergraduate students have doubled since fall 2000 from 288,000 to 575,000, nonresident alien only make up 3.5% of enrollment in undergraduate courses. In postbaccalaureate programs (include master's and doctoral programs, as well as professional doctoral programs such as law, medicine, and dentistry), nonresident alient make up around 14% of student enrollment. Out of the 3.0 million students enrolled in fall 2017, there are a total of 426,000 are nonresident alien students.

[13] Each student's doctoral program has a Classification Instructional Programs (CIP) code linked to it. Students who are enrolled with the same 6-digit CIP code at the same institution will be considered to be in the same department. For example, Department of Economics (CIP 45.0601) at University of Cincinnati and Department of Economics at Ohio State University are considered two separate departments.

[14] Students can have more than one department if they switch schools and or degree programs. A student who switches after enrolling in a department before completion is considered to have not persisted in the first department. Their second enrollment would be a new observation in the analysis. There are around 5% of students who switched departments.

Using administrative data has several advantages. The data provides a large sample size that is necessary given my focus on a department and an entering cohort into that department. Because of the extensive data on enrollment and academic performance, I can track international students once they enroll in a program and know if they complete, drop out or switch to another program or institution. One limitation of the data is the absence of background information on the students and any records of the students prior to attending their respective Ohio institutions. Hence, I will not be able to identify any network link that may exist between cohorts prior to entering the program. I am also unable to observe the composition of faculty members and their characteristics.

Table I shows the summary statistics for international students in doctoral programs. Female students make up 40% of the population, and students are around 28 years old when they first enrolled in their program of studies. An average cohort size for each department is around 14 people, where international students make up 65% of the cohort (around 9 students) while natives make up the rest of the 35% of the cohort size (around 5 students). The outcomes of interest are the graduation and/or persistence rate into the years of the doctoral program. Around 85% of the international students persist into the second year.[15] By the 4th year, around 60% of the students have graduated from their respective program or are still enrolled in program.

Note that there are different number of students for the measures of persistence into different years. I am able to observe the outcome, persistence into the second year, for cohorts that enroll in 2009

[15] Graduation and/or persistence into the years in doctoral program are indicator variables whereby it is a "1" if the student has already graduated from the doctoral program in that year or if they are still in the program. It is a "0" only if they had dropped out of the program. For the graduation and/or persistence into the second year, this primarily captures the persistence rate (or dropout rate) as students are not likely to obtain their doctoral degrees by the second year.

through 2014, which makes up 1206 observations. However, for persistence into the third year, I am only able to observe 1027 observations, which are for the cohorts that enroll in 2009 through 2013. This applies for the subsequent years where I observe fewer observations as I study persistence of students further into the program. For the later year (fourth to sixth year into the program), the outcome "graduated or still enrolled" is a dummy variable for the students for if they had graduated or are still in the program.

In my analysis, I sometimes define peers based on the proportion of same-country peers, the number of same-country peers, or whether there are any same-country peers. For all measures, person i is excluded from the calculation so, for example, the country of origin share is defined as

$$Country\ of\ Origin\ Share = \frac{Number\ of\ students\ from\ the\ same\ country\ of\ origin - i}{Total\ number\ of\ students - i}$$

The "number of students from the same country of origin – 1*"* would be the total number of students from the same country of origin as individual *i* excluding the individual *i* themselves in their cohort. The denominator for the proportion is the total number of students—both international and native students—in the cohort excluding individual *i.* The share is constructed for every cohort in each department. On average, approximately 20% of the cohort in the department is from the same country of origin, and there are about two other students from the same country of origin in each cohort. Across cohorts and departments, about 68% of students have at least one other peer from the same country of origin.

Table I also shows that the majority of the students are from East and South Asia. Asian students make up 76% of all international students in the sample, and the share of international students from all the regions are very similar in both large and small departments. The next largest group of international students (11%) are from the Middle East and North Africa. The remaining regions – Europe and Central Asia, Latin America and the Caribbean, North America, and Sub-Saharan African—make up the remaining 16%. It seems that most international students are in the natural sciences field, which is not surprising given that international students make up a significant share in STEM programs.

1.5 Empirical Approach

The challenge in establishing the causal effects of peers has been thoroughly analyzed. In his seminal work, Manski (1993) provides a framework for empirical analysis of peer effects and, more importantly, the empirical difficulties associated with estimating peer effects. First there is the reflection problem (Manski, 1993) named so for Manski's use of a mirror analogy—does the reflection in the mirror cause the person's movement or reflect them? The point of the mirror analogy is to illustrate the difficulty in disentangling the effect that the network has on an individual when the individual is also affecting the group simultaneously. The second empirical difficulty identified by Manski (1993) is the correlated effects (sometimes referred to as common shocks) where individuals in the whole group face similar institutional environments. The third issue is the tendency of individuals to self-select into networks that they share similarities with. The phenomenon—referred to as homophily—is a classic selection issue when it comes to establishing any types of causality.[16]

Carrell et al. (2009) reviewed the literature and strategies that social scientists have used to overcome the endogeneity and selection problem. Two types of strategies have been used to address the selection problem. Several studies exploit contexts where individuals are randomly

[16] McPherson et al. (2001) provides an overview on homophily in social networks.

assigned to their peer groups. The many instances of random assignment include roommate, dormmate, and squadron member.[17] Another strategy is to rely on the variation across classrooms or cohorts within a school that is plausibly random conditional on other covariates and fixed effects. My empirical approach is closely related to the second strategy. Using the variation in peer groups across cohorts within a department, I examine how peers from the same country of origin affects performance in doctoral programs. Specifically, I begin with a model that includes department, country and cohort fixed effects:

$$Y_{idct} = \alpha + \beta(Country\ of\ Origin\ Share_{dct}) + \gamma_d + \varphi_c + \delta_t + \epsilon_{idct} \quad (1)$$

In Equation 1, Y_{idct} denotes the probability that person I will persist in the program. This depends on the share of peers from the same country of origin, a department fixed effect, $\gamma_{d.}$, a country fixed effect, $\varphi_{c.}$ and a cohort fixed effect, δ_t.

The coefficient β captures the difference in Y for students exposed to a higher share of peers from their country of origin versus students exposed to a lower share of peers from their country of origin. An example of this variation would be comparing the relative outcomes for students from China in the department of Economics (high share) to the relative outcomes for students from China in the department of English Literature (low share). The model accounts for the possibility that Economics may generally have lower persistence than English Literature, but it does not account for the possibility that Chinese students in Economics may have relatively better

[17] This is a prominent strategy in establishing peer effects in higher education as there are many instances of individuals being randomly assigned to roommate, dormmate, and squadron member in that particular setting (Sacerdote, 2001; Zimmerman, 2003; Stinebricker and Stinebrickner, 2006; Carrell et al., 2008).

persistence than Chinese students in English Literature. These department-by-country differences have the potential to bias estimates based on equation 1.

To address this concern, I estimate a model with department-by-country fixed effect and cohort fixed effects:

$$Y_{idct} = \alpha + \beta(Country\ of\ Origin\ Share_{dct}) + \chi_{dc} + \delta_t + \epsilon_{idct} \quad (2)$$

In this equation, the department-by-country fixed effect is denoted by χ_{dc}. Here, my comparison groups will be cohorts with higher share of peers from the same country of origin to other cohorts with lower share within department. To address other potential sources of bias noted earlier, I also include field-by-cohort fixed effects and country-by-cohort fixed effects. I estimate these models for several dependent variables differing by the number of years of persistence in the department.

In order to assess the likelihood that variation in peer groups is correlated with potential outcomes, it is useful to consider the underlying source of this variation. Consider that the characteristics of a cohort are determined jointly by the choices of department admission committees and individual students. I first discuss the variation generated by the admission choices made by the department and then discuss variation generated by students' choices of whether to accept the offer.

The most straightforward approach to admissions is to maximize the expected quality of students who will ultimately enroll in their program. If departments follow this approach, variation in country composition will come from variation in how applicants from different countries compare to one another each year. In other words, when the applicants from India are relatively weak, the department admits fewer Indian students. At first glance, this appears to be a threat to internal validity since it generates a correlation between the proportion of Indian students admitted and the quality of the applicants. This will not create bias, however, because the systematic admission process means that the relative quality of *applicants* from different countries does not map to differences in the quality of *admitted* students from different countries. For example, suppose all of the Indian applicants are qualified, but only one of the Chinese applicants is qualified. The average quality of Indian applicants will be higher than the average quality of Chinese applicants, but there is no reason to expect that the admitted Chinese student is either more or less qualified than the average Indian admitted. In order to generate a systematic relationship between applicant quality and the number of admitted students, it is necessary to have a more nuanced admission process that specifically considers country of origin. Even if departments specifically consider country of origin, the process would need to be fairly involved and time-varying in order to generate bias. Hence, if departments use country of origin to infer applicant quality (statistically discrimination), this would not create any systematic relationship between the quality of individual applicants and the number of applicants from that country.

An example of behavior that would create bias is if departments statistically discriminate based on country of origin and use dynamic updating based on the outcomes of recently enrolled students. In other words, the admissions committee could decide to admit fewer students from a particular country because current students from that country have been performing poorly. This is similar to a violation of the difference-in-difference common trends assumption as it implies that the performance of past cohorts predicts the timing of treatment intensity. Though it is not possible to directly test the identifying assumptions of my model, it is possible to empirically assess whether past performance of students from a particular country predicts the share of students from that country in future cohort. I show this empirical test following the discussion of student choices of whether or not to accept an offer.

Students accept an admission offer only if it dominates all other offers and their non-graduate school options. There are many factors that would lead to differences across countries in the probability of accepting a given offer. For instance, countries like China has seen an increase in students going abroad to further their studies in the recent decade relative to other countries. Another example is the differential returns in obtaining a degree in certain fields and schools for different countries. Most factors would be captured by department-by-country fixed effects or the country-by-cohort fixed effects. Admission choices only generate bias if they are a function of the proportion of accepted applicants from that country and are also a function of the quality of those applicants. An example of behavior that would generate bias is if prospective students meet other prospective students during a campus visit and decide to attend a particular university because the quality of other prospective students from their country appears to be high. Though possible, it seems unlikely that this form of bias would be prevalent because prospective applicants decide whether or not to accept without knowing which other students will accept their offers. Furthermore, I suspect that the primary driver of enrollment decisions at a particular university is whether or not the applicant was admitted to better universities – not the quality of the other prospective students from their country.

A more likely source of bias is if prospective students speak to already enrolled students from their country of origin and make enrollment decisions based on whether those students are performing well. As an example, if a prospective student learns that the current first-year students from her country are performing poorly, they may decide not to attend this institution and the proportion of enrolled students from her country will fall, even if the proportion admitted students was constant. More generally, if prospective students base enrollment decisions on trends in retention of current students from their country, this has the potential to bias estimates.[18] Though it is not possible to directly test for this behavior, the implications of this behavior is similar to the implication from departments admitting students based on trends in quality. If students make acceptance decisions based on trends in retention of older cohorts, this will generate a relationship between the retention rate of the *t*-1 cohort and the fraction of students from that country who accept offers in year *t*.

Because I do not separately observe admissions and enrollments, it is not possible to separately test for bias stemming from department admission behavior and bias stemming from prospective student acceptance behavior. That said, for the purposes of my empirical analysis, I am interested in the combined effect of department and prospective student behavior. This is best captured by the ultimate enrollment information that is directly measured in my data. In order to assess the likelihood that combined departmental or prospective student behavior is based on past retention, I test whether the persistence into the second year for the t-1 cohort predicts the share of students enrolled from that country in year t. In order to focus on the same source of identifying variation as in my main analysis, this test is conditional on the cohort and department-by-country fixed effects as discussed above. Specifically, I estimate:

$$Persistence\ into\ Year\ 2_{idc,t-1}$$

$$= \alpha + \beta(Country\ of\ Origin\ Share_{dct}) + \chi_{dc} + \delta_t + \epsilon_{idct} \qquad (3)$$

Equation 3 provides an important specification test because, in addition to assessing the possibility of dynamically updating admissions or acceptances, it assesses whether treatment intensity is

[18] It is important to emphasize that if enrollment decisions are based on general trends in retention at the program, this will be captured by the department-by-cohort fixed effects.

generally related to trends in persistence. For example, if departments tend to admit more students from countries that have increasing trends in persistence, this would create bias, and it would also generate a positive relationship between persistence of cohort t – 1 and proportion admitted for cohort t. Similarly, if students make acceptance decisions based on recent trends in persistence, this would create bias and would create a correlation between persistence of cohort t – 1 and the proportion that accept offers. Figure 1 shows that there is zero correlation between persistence into the second year for the previous cohort and the share of country of origin in the current cohort.

The lack of correlation between persistence of the *t* – 1 cohort and the share from that country in year *t* is important evidence against various forms of potential bias, but it remains the case that there are various types of behavior that would bias estimates and not generate a correlation in Figure 1.[19] Though it is not possible to rule out every potential story that would lead to biased estimates, it is important to highlight that there are many sources of random variation in my context. Admission committees must admit a small percentage of hundreds of applicants, and there is likely to be some randomness in the exact set of admitted students. Similarly, attendance is a complicated function of admission decisions at other universities, and these are also subject to a degree of randomness. As such, there are many reasons why the proportion of peers from the same country would vary across cohorts within a department that would not be correlated with potential outcomes of the enrolled students.

[19] As an example, suppose departments set a minimum quota on the number of admitted students from each country of origin. This creates bias because in years where India has many high quality applicants, the number of admitted students would exceed the quota, whereas in years with very few high quality Indian applicants, the department would only admit at the quota, and the department may need to admit lower quality applicants in order to meet the quota. The proportion of admitted students would be uncorrelated with trends in performance and would thus not be identified by the test shown in Figure 1.

1.6 Results

1.6.1 The Effect of Share of Peers from the Same Country of Origin

Table II shows the results of using several different model specifications to estimate the effect of same-country peers on different measures of success. Each cell shows the results of a separate regression where the different rows correspond to different outcomes and the different columns correspond to different specifications. I first discuss the second-year persistence outcome (row 1) and then I discuss the remaining rows. In Column (1), I show the results of a simple OLS regression without controls. These results compare all international students with high country of origin share to other international students with low share of peers from the same country of origin. In Column (2), I add controls which include: gender, age of enrollment, size of cohort, total number of natives in the cohort, GDP growth as well as the unemployment rate of the country of origin. In the subsequent estimation, I added fixed effects sequentially, and the results show how the coefficient changes as I account for cohort, country and department fixed effects.

Estimates of the effects of same-country peers on persistence into the second year are presented in the first row. Before describing estimates, I will note that estimates from Columns (1) and (2), which exclude the department fixed effects, are quite different from other estimates. This is likely because they fail to account for the fact that departments with smaller international shares generally have lower rates of persistence. Once the department, country and cohort fixed effects are accounted for (Column 3 to 8), however, the coefficient is fairly robust to the inclusion of the two-way fixed effects. Also, while estimates in Columns (7) and (8) are similar to estimates in other columns (3 to 6), the standard errors are considerably larger. Given these consideraitons, I will focus the discussion around estimates in Column (6).

Estimates in Column (6) indicate that for each additional 10 percentage point increase of country of origin share of students, an international student will be approximately two percentage point less likely to persist into the second year. Again, the estimate does not change by much when I use more restrictive specifications that include field-by-cohort fixed effects and department-by-cohort fixed effects in Column (7) and Column (8) respectively. In rows 2 – 5, I explore how the same-country peer effect persistence into the later years. Here I define persistence to year T as a 1 if they appear in year T and a zero otherwise, so I am not conditioning on persisting to $T - 1$. If peers only determine retention from the first to the second year, I expect that the coefficient should be similar when the outcome is persistence to the second, third, fourth, fifth or sixth year. If on the other hand, peers affect retention each year, then the coefficients should grow as I consider retention to later years.

Because there are fewer cohorts that have longer term outcomes, there are fewer observations for the long-term outcomes and in some cases, the model runs out of degrees of freedom and cannot run at all. In cases where the model does run, the standard errors rise dramatically as the sample size falls and so I have limited ability to make strong conclusions regarding long-term persistence. Though the exact coefficient varies somewhat across outcomes, the general negative effect appears regardless of at what year I consider persistence. For persistence to year 3 or 4, there is no robust evidence of a growing effect as regardless of the specification, the estimates are statistically indistinguishable from the year 2 coefficients. For persistence to year 5 or 6, I have limited power to make strong conclusions, and I show these estimates just for completeness.

1.6.2 Using other Measures of Peer Effects for Robustness

Table III show the estimates when I use different measures of peer group: share of peers from the same country of origin (as was shown in Table II), number of peers from the same country (excluding one's self), and lastly, an indicator variable for having any peers from the same country. I estimate the model separately for each measure, and each row in table II shows the result for separate regressions. Because I am using measures with different scales, the interpretation of the estimates varies with the definition. However, it is important to note that the negative effect is present regardless of the different measure of cultural match. The different measures in table II alludes to different interpretation of how peers from the same county of origin affects international students. To state the obvious, studying the effect of country of origin share incorporates the notion that share matters. Though there are many reasons why share may matter more than the number of peers, institutional responses are a primary one. For example, if instructors change their method of teaching to cater to the plurality student body, the share of peers from one's country would be more relevant than the number.

The measure "number of peers" tests the effect of having more peers from the same country of origin. This measure alludes to the social and cultural dimension of peer effect, whereby there is an increased in interaction and opportunity for more teamwork learning as well as working together. My preferred specification (Column 6) shows a coefficient of -0.0193 in Column (6). The results indicate that having an additional peer from the same country of origin will reduce the likelihood of persisting into the second year by approximately 1.93 percentage points. It is important to note that country of origin share and number of peers are related. In fact, the number of peer measure is the denominator for the country of origin share measure. As reported in Column (6), the coefficient on country of origin share is 0.215. When scaled with the average cohort size (14 people), the coefficient is 0.015, a magnitude that in the ballpark figure of the coefficient of the number of peers, -0.0193 (0.215/14=0.015).

Lastly, “any peers” measures the importance of having at least one peer from the same country. This measure is based on the idea that having any peers from one’s country may be enough to provide a social network. The results for the “any peers” measure becomes less precise as I include more restrictive fixed effects in the estimation, but the point estimate in Column (6) still indicates a negative effect for having any peer from the same country of origin. Taken literally, the coefficient suggests that, by having at least a peer from the same country of origin, there is a -0.04 percentage point for persisting into the second year. Even though there is not enough power to have a conclusive interpretation for all the measures, it is still the case that, like table II, once basic fixed effects are controlled for, the estimates are relatively stable.

Table III establishes that the exact manner in which I model peer composition does not affect the qualitative findings. It is important to note, however, that, it does not assess what is the correct way to model peer composition since each measure is added to the regression separately. Adding all three measures simultaneously is not feasible because estimates become too imprecise. That said, it is possible to provide suggestive evidence on how peer composition should be modeled, and I do this in a later section.

1.6.3 Relaxing Assumption of Linearity

The estimates of the effect of number of peers in table III impose linearity such that the effect of an additional peer is the same regardless of how many peers already exist. On the other hand, the “any peers” measures imposes a highly non-linear functional form where the effect only operates through the difference between zero and one peers. In this section, I explore a more flexible functional form where I include separate dummies for each possibility between having one and eight or more peers from one’s country. Having no peers is the omitted group. Specifically, I estimate

$$Y_{idct} = \alpha + \sum_{j=1}^{j\geq 8} \beta_j Peer_{j,dct} + \chi_{dc} + \delta_t + \epsilon_{idct} \quad (2)$$

In Figure 2, I plot the coefficients for the effect of each additional peer from the same country of origin for international students.[20] The outcome is persistence to the second year, and I show three different specifications to help assess the robustness of each coefficient. Each point on the plot captures the effect of having "X" number of peer from the same country of origin relative to another international student who does not have any peers from the same country of origin. For example, having two peers from the same country reduces the probability of persisting into the second year by around -0.10—10 percentage points. There are several takeaways from Figure 2. First, as already shown in the tables, there is a general negative relationship between the number of peers from the same country on the persistence into the second year. Second, the relationship is reasonably well approximated by a linear functional form with perhaps some concavity after the 5th peer.

Figure 3 allows for the same flexible approach to capture the effect by the distribution of country of origin share. The plot shows a similarly linear graph. Relative to having no peers from the same country of origin, there is a negative effect in the persistence into the second year when peers from the same country of origin makes up 20% of the cohort and the effect gets larger as the share increases, consistent with the pattern shown in Figure 2. Together, Figures 2 and 3 suggest that modeling the effect linearly does not oversimplify the relationship between peers and persistence,

[20] I show the coefficient for having each additional peer, but the last coefficient is for having 8 or more students from the same country of origin.

and it is not centrally important whether one defines peer composition based on the share of the cohort or the number of peers.

Even though either measures—share of cohort or number of peers—can be used to define peer composition, it is unclear if the share of students in the cohort actually matters. Share of country of origin peer is a function of number of peers from the same country of origin. The results from country of origin share could be potentially estimating the effect of increasing number of peers, and there are no institutional response to having a majority share from one single country. Adding both measures simultaneously is also not feasible because estimates become too imprecise. Hence, to have a better understanding of the importance of share in a cohort, I assess how the effect of having additional number of peers from the same country of origin differ by the size of the cohort.

There are three predictions. First, if the effect of *number of peers* in a large cohort is greater than the effect in a small cohort, it would imply that having any peer is what that matters, and all the additional peers has the same effect as the first peer. Figure 2 and 3 as well as row 2 in table IV have already shown that this relationship is an unlikely case. The second case is such: if the effect of *number of peers* in a small cohort is greater than the effect in a large cohort, then it would imply that the share of peers from the same country of origin is important. In a small cohort, having one additional peer changes the density of the share of peers from the same country of origin, while in a large cohort, having one additional peer does not matter as much.

The last case would be if the effect of number of peers in a small cohort is equal to the effect in a large cohort. Here, share of peers would not matter as the effect of having an additional peer should be the same in both large and small cohort. The results of estimating the effect of the number of peers from the same country of origin for the two subsamples—large cohort and small cohort—

are shown in Column (2) and (3) in table IV. Table IV reports a bigger point estimate of -0.0339 in a small cohort and smaller point estimate of -0.0198 in a larger cohort. The coefficients are both statistically significant to the 95% level. The results confirms that share of peers does matter.

1.6.4 Heterogenous Effect of Country of Origin Share

Given the initiative in promoting persistence in STEM at all levels of education,[21] there are reasons to expect differential effects in those fields. Furthermore, it is common to have high stakes tests in doctoral program, which would determine if doctoral students can enter the dissertation phase after their required course work. Even though there is a variance to the timing and approaches to the examinations across disciplines, the high stakes tests in STEM fields are mostly taken at the end of the first year. These reasons motivate the analysis by STEM heterogeneity.

Table V shows the results for the separate analysis by STEM and non-STEM fields. The point estimate in Column (2) show a negative effect for having an increase in country of origin share on the persistence rate into the second year for international students in STEM fields. Though there is also a negative effect in non-STEM fields, the result for non-STEM fields are not statistically significant. It is also interesting to note that the point estimate of -0.235 for STEM fields is very similar to the effect for the whole sample population (-0.215 from Column 1). The results indicate that the effect is mostly driven by international students in STEM fields.

[21] The US government has a five-year plan for STEM literacy, innovation, and employment for all levels of education (U.S. Department of Education, 2018). More details on the plan can be found here: https://www.whitehouse.gov/wp-content/uploads/2018/12/STEM-Education-Strategic-Plan-2018.pdf.

1.6.5 Potential Mechanism of Country of Origin Share as Peer Effect

Persistence can be affected by directly altering social networks and through an indirect institutional response. International students form a social network based on similarities in experience. It is easy to form a bond with other peers who are trying to navigate the same difficult, and, at times, lonely process of acclimating to both the rigor of doctoral programs as well as a foreign environment. These ties provide a sense of belonging but they might also lead to an isolation from the rest of the cohort. More importantly, these mechanisms at play may not be exclusive to peers from the same country of origin, but to international students in general. Similarly, the institutional or environment responses that affect international students could be directly at the whole group of international students, regardless of country of origin. For instance, instructors may avoid using American slangs in their instruction when teaching a class with a large share of international students.

Accordingly, I assess whether the own-country effect is simply driven by a general negative spillover among international student peers. This specification allows for separate effect from share of same country of origin and share of international student. Column (2) in table VI shows the result from estimating the model. The coefficient for other international student share in Column (2) is not statistically different from zero. Furthermore, the coefficient for country of origin share, though less precisely estimated, still gives a very similar estimate as the result from Column (1), where I show the estimated the effect of country of origin share on the persistence of doctoral students into the second year without any augmentation.

Since it is unlikely that other international students is a possible channel for peer effects, I consider whether peers from one's region have a similar effect as peers from one's own country. Countries that cluster along national lines share geographic proximities, and with that, share linkages through religion, language, economic trade, historical experience as well as other cultural similarities.[22] I follow the same methodology and include "Region Share" into my specification. Column (3) in table VI shows the result from estimating the model. The coefficient on "Region Share" is small and insignificant while the point estimate from country of origin share still shows a negative effect that is statistically significant and very similar to Column (1).

Next, I assess if *language* could be the component that is working through the social mechanism. Using the World Factbook from the Central Intelligence Agency, I matched the international students to other international students who speak the same language.[23] The results in Column (4) in table VI shows the coefficient on language share to be small and insignificant. Like my previous analyses, the coefficient on same country of origin share is around -0.20. I find that conditional on the own-country effect, peers from one's broad region or peers who speak the same language do not affect persistence. The own-country effect is virtually unchanged when controlling for the own-region effect or same-language effect suggesting that to the extent that the effect comes from cultural match, this match is based on country-specific factors.

Lastly, I consider whether sharing a country of origin with second-year Ph,D. students affects the persistence of first-year Ph.D. students. There is a likelihood of a role model effect as second-year Ph.D. students can provide guidance and support to first-year Ph.D. students, especially to first-year Ph.D. students from their same country of origin. Second-year Ph.D. students can also alter

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[22] I used a general categorization of how countries are clustered by using geographic regions. Other researchers have talked about how it may be more nuanced (Simmie & Sennetm, 1999; Carroll, Reid & Smith, 2008; Martin & Sunley, 2003).

[23] The common language that the students share could be either their first or second language. For example, students from Cameroon speak both French and English would share a common language with a student from France.

the first-year students' academic performance by sharing tips and course materials on courses that they had just taken the year before.[24] I test for this potential channel, and the results are reported in Column (6). Since the data starts in 2009, the sample size is smaller as I exclude cohorts who entered in year 2009. I am unable to observe second year students for that particular cohort. In Column (5), I restricted the overall sample to be the same sample size as Column (6) for easier comparison. I find that the effect of peers from one's country is much stronger if they are in the same cohort, with the cross cohort spillover being one quarter the size and not statistically significant.

1.7 Conclusion

Using a unique administrative dataset that matches students to their peers from the same country of origin, I study the importance of cultural match among international students at the post-graduate level. The estimation uses year-to-year variation in the share of country of origin peers within doctoral programs to address concerns about systematic sorting into programs. I find that international students are two percentage points less likely to persist into the second year when there is a 10-percentage point increase in the share of peers from their country of origin. This equates to a reduction in first-to-second-year dropout from 17% to 15%.

My findings are subject to a number of qualifications. First, because all of the institutions I study are Ohio public universities, the results from my analysis may not generalize to other contexts. Ohio universities represent a wide swath of the higher education landscape in the United States including very selective Ph.D. programs (e.g. Ohio State) and less selective programs (e.g. Akron).

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[24] This channel would cause an omitted bias, which would mean that I am understating the peer effects from having a large share of peers from the same country of origin.

That said, the data do not include any private schools where resource allocation can be quite different. Importantly, Desrochers and Hurlburt (2016) report that stipends and student services spending tend to be lower at public universities and these factors are potential moderators of the effect of peers. Relatedly, the state of Ohio has a smaller share of immigrants compared to many other states and so international students may feel more isolated at an Ohio institution compared to say, a university in New York City. A second limitation of the analysis is that I lack data on time use or direct information on social or academic networks. This does not affect the reduced form interpretation of the results, but it limits my ability to investigate mechanisms directly.

Third, it is possible that the peer effects I document are driven by something correlated with country of origin rather than country of origin itself. I find no evidence that the effect operates through general international student status or language, but there are other factors that may be country-specific, but not caused by country of origin itself. For example, if students from the same country tend to study the same subfield, the effects I document could be driven by the effect of having more peers in one's subfield. Finally, when interpreting the effect on persistence, it is important to keep in mind that persisting in a Ph.D. program is not always optimal. Much of dropout between the first and second years of a Ph.D. program is involuntary based on qualifying exam failure, but some dropout is due to a student voluntarily leaving the program. It is possible that peers from one's country facilitate finding attractive jobs in one's home country and therefore increases dropout from the Ph.D. in a utility improving fashion. In other words, the students who are induced to dropout because of peer composition may not be worse off as a result of the dropout.

In closely related work, Botswick and Weingarth (2018) studied the effect of gender composition among female doctoral students in STEM fields. They documented that 10 percentage point increase in the share of women increases the probability of on-time graduation by 2.25 percentage points. My finding is similar in absolute value, but the effect goes in the opposite direction. Though it is not possible to fully understand why the effect of same-country peers differ from the effect of same-sex peers, there are two main theoretical possibilities. First, women in many STEM fields face the challenge that their field is stereotypically male and they may not feel that they belong in the program because of their sex. Though international students may similarly have difficulties integrating into their programs, there are not analogous stereotypes regarding the suitability of international students for their programs. As such, observing other women in one's program may alter perceptions of whether women fit into a particular program, but there is unlikely to be a similar mechanism at play for international students. Second, domestic female students are likely to have broader social networks that include individuals outside of their academic program. Hence, the gender of their cohort may operate more strongly through academic channels (e.g. study groups) rather than fundamentally altering their social network. For international students, the social network channel may be an important factor driving persistence.

My results imply that a more heterogenous (in terms of country of origin) cohort may help with the persistence of international students. Though encouraging the admissions committee to increase diversity across country of origin seems like a natural recommendation, there is need for a more detailed understanding on the mechanism underlying the peer effects from country composition. By documenting the first evidence of peer effects among international students in doctoral programs, my research provides the reduced form estimates that can provide the basis for further work on mechanisms. Future research might use a qualitative approach to better understand how international students perceive their same-country peers, or use more detailed data on networks to better understand mechanisms. Also, future research might investigate whether the negative effects documented in this study generalize to other contexts, either in Ph.D. programs in other states or in lower level degrees. Most of the literature on the effect of peers finds that having more peers like oneself is beneficial, but the negative effects found in this study suggest that the role of peers is more nuanced at the doctoral level.

2 IF YOU EXTEND IT, THEY WILL COME: THE EFFECTS OF THE STEM OPT EXTENSION?

2.1 Introduction

The Optional Practical Training (OPT) is a 12-month training program that allows foreign students, or more accurately, students in F-1 nonimmigrant status to work in a job that is related to the students' major of studies.[25] The key to this program is that foreign students can participate in the labor market without changing their nonimmigrant status. The value of the feature is even more apparent for both foreign students and employers when Congress drastically reduced the number of employment visas that were allowed annually in 2004. To meet the demand for high skilled workers, employers, particularly those in the science, technology, engineering and mathematics (STEM) fields, turned to the OPT program as source of workers. However, there is a growing concern that the US will become non-competitive in a global market, as employers face increasing difficulty recruiting and retaining high skilled workers coupled with the fact that other countries are adopting more liberal policies to attract these workers.[26] As a response to these concerns, the Department of Homeland Security (DHS) extended the OPT period from 12 months to an additional 17 months for STEM fields in 2008.

Since then, the OPT program has come under greater scrutiny. The extension of the OPT program for STEM graduates has prompted many debates, including a lawsuit that challenges its validity

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[25] For an overview of the OPT program, see https://cis.org/Report/History-Optional-Practical-Training-Guestworker-Program and https://www.cato.org/blog/facts-about-optional-practical-training-opt-foreign-students.

[26] For example, in 2007, the European Union proposed a "Blue Card" program, which allows skilled workers to obtain temporary work visa for employment in the region. This program is similar to the US H-1B visa program, except that the program will not have a cap on the number of workers that is granted the "blue card". The blue card program was approved in 2009. Other economies of the OECD countries are also increasing their opportunities for STEM scientists.

and, more recently, a proposal by the current administration to suspend the program.[27] In 2016, the STEM OPT extension was further extended from the 17-month period to a 24-month period. Foreign students have up to 3 years to utilize their OPT option. Proponents of stricter immigration policies argue that the program "increase[s] the number of economic competitors" and "expose[s] [native workers to] unfair competition",[28] while employers as well as academic institutions caution against the suspension, citing similar reasons that led to the OPT extension in the first place. Academic institutions contend that the OPT program has been an essential incentive for foreign students' enrollment especially in STEM fields, and with falling enrollment in recent years, there is increased apprehension that restrictions on the program will further affect enrollment. These discussions underscore the importance of understanding the implication of changes –whether it is expansion, restriction, or suspension—to the OPT program.

Specifically, I consider how OPT program, which eases foreign students into the US labor market and, in turn, provides an opportunity for expected earnings, affects the schooling decision of foreign students. Since its creation in 1992, the OPT program has evolved to become an important channel for temporary workers. So much so that in 2017, the number of individuals using their OPT to work in the US exceeds the number of workers on an H-1B visa.[29] Using administrative

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[27] This proposal to suspend the program has been triggered by the consequences of the Covid-19 pandemic but there has been many discussion prior to it. See https://www.insidehighered.com/news/2020/05/29/trump-administration-reportedly-considers-restrictions-foreign-student-work-program.

[28] One of proponents of stricter immigration policies is the Washington Alliance of Technology Workers, which is involved in an ongoing lawsuit to challenge the validity of the OPT extension. See https://www.courtlistener.com/opinion/2826700/washington-alliance-of-technology-workers-v-us-dep/. There is also a large literature examining the effect of college-educated foreign worker on the natives' labor market outcome, where the evidence is mixed.

[29] See https://cis.org/Report/History-Optional-Practical-Training-Guestworker-Program.

data from Student and Exchange Visitor Information System (SEVIS), Demirci (2019) shows that approximately 72% of foreign students in his sample use their OPT after completing their degree, and the proportion is even higher for students who complete fields that are eligible for the OPT extension. Given the high take up rate of the OPT extension, less is known on the impact of its extension on schooling choices.

My study evaluates how the OPT extension affected the enrollment decision of foreign students into STEM fields in the US and whether it had an impact on degree completion. Using exogenous variation in the timing of the OPT extension, I use a difference-in-difference approach to first compare the likelihood of foreign student enrolling in STEM fields before and after the extension. To account for the trend in labor demand for STEM graduates as well as general economic conditions for STEM occupations in the US, I use native students as a control group. I also examine the change in the composition of foreign students who are induced to study STEM fields in the US after the extension.

To implement the analysis, I use unique, administrative data from the state of Ohio that covers the universe of students enrolled in public higher education from the year 2004 to 2015. The longitudinal data follow multiple cohorts of students in each field of study, making it possible to track how academic outcomes evolve for individual students before and after the extension. I find that the OPT extension increases the likelihood of foreign students' enrollment into STEM fields. In particular, there is a 1.8 percentage point (5.4%) increase in the enrollment into STEM fields for Bachelor's program. Meanwhile, graduate programs see a smaller and statistically insignificant increase in the enrollment of STEM fields. My study also provides evidence of foreign student substituting into newly added STEM fields that qualify for the OPT extension in later years.

Even though there is an increase in the enrollment into STEM degree following the extension, it is less clear what the completion rate would be. To study the impact of the OPT extension on completion rate in STEM degree, I leverage the same exogenous variation to estimate the effect of the OPT extension on foreign students' probability of completing a STEM degree. There is a higher probability of completion by year 3 after the extension. In particular, there is a 0.7-percentage point (5.9%) increase in the probability of completion of a Bachelor's degree. The increase in completion rate comes from an increase in the hazard rate of completion in year three after enrollment. There is also a 1.3-percentage point (7.6%) and a 3.4-percentage point (11%) increase in completion rates (by year 2 and year 5) for both Master's and doctoral programs, respectively. Overall, there is an estimated positive effect of the OPT extension on the likelihood of enrolling and completing STEM degree. The increase in enrollment in response to the OPT extension suggests that foreign students place a large value on having work experience in the US after graduating. This results also provide evidence for the OPT is an effective tool for changing the skill composition of students in higher education as well as a potential solution to the STEM shortage in the labor market.

This study also relates to higher education literature that examine the determinants and completion of STEM majors. Given the importance of STEM fields and the increasing labor demand of STEM graduates, there has a surge in research to study the factors to increase STEM participation and retention. Those various factors include students' characteristics, academic preparedness and institutional support as well as innovation in teaching method.[30] Other studies focuses on the participation and persistence of female and minorities population in STEM fields. For example, Griffith (2010) studies institutional feature of the colleges and universities like graduate to undergraduate student ration as well the schools' allocation to research expenditures. There are increasingly more studies that look at how female role model impact the persistence of STEM majors for female students (Hoffmann and Oreopoulos, 2009; Griffith, 2010; Price, 2010; Bostwick and Weinberg, 2018). Some studies examines how grades in STEM and non-STEM classes affect persistence rates in college (Ost, 2010; Rask, 2010). My study focuses on the foreign student population and considers immigration policy as a determinant for participation and persistence in STEM majors.

Lastly, this study also joins a host of papers that leverage longitudinal data to study the changes in probability of the occurrences and timing of an event. For example, Clotfelter et al. (2008) compares the turnover patterns of teaches working in high poverty public secondary schools. Singer and Willett (1991) model the hazard of student dropping out and teacher attrition, while DesJardin and McCall (2010) examine the impact of financial aid on the probability of occurrences and timing student dropout, reenrollment and graduation. While estimated averages for completion rate is useful for reporting and policy purposing, they sometimes mask the dynamic nature of the education process as well as the importance of understanding the path towards the degree completion (Singer and Willett, 1993; 2003). This study also aims to provide a better understanding of foreign students' path towards the occurrence of completion in STEM degree.

[30] Ehrenberg (2010) provides a summary of some of the factors that influence persistence rates in STEM fields majors. Other examples include Whalen and Shelly (2010), Wang (2013) and Watkins and Mazur (2013).

2.2 Literature Review

Labor market opportunities have long been recognized as a driving force in foreign student's mobility,[31] and empirical evidence on their movement to higher income countries lends its support for this hypothesis (Suter and Jandl, 2006; Rosenzweig, 2008; She and Wotherspoon, 2013). It is not surprising then the US is a popular destination for foreign students, especially since a degree obtained from the US is highly valued in both the US and other parts of the world (Hanushek and Kimko, 2000; Clemens, 2013).[32] Obtaining a US degree is even more important for foreign students who consider higher education as a bridge to longer-term employment in the US. Several studies have shown that enrollment in college and universities decreases when immigration policy makes participation in the labor market more restrictive (Kato and Sparber, 2013; Shih, 2016). Given that foreign students' enrollment decisions are highly responsive to the labor market climate, it follows that they take into consideration the payoff to their field of studies. In fact, Bound et al. (2013) have shown that a more open labor market in the IT sector increases postsecondary enrollment in the associated fields.

This study largely relates to the literature on major choices, which examine the role of expected earnings in the choice of major. For instance, Freeman (1976) assesses how individuals base predictions of future earning opportunities on the starting salaries, which depend on the supply of new engineering graduates and the level of demand, when choosing their major. In more recent work, Beffy et al. (2012) examine how major choice of students in French universities respond to

[31] See the constrained domestic schooling model and the migration model posited by Rosenzweig (2006) illustrate foreign students' decision to study abroad.

[32] Over 24% of the foreign student population in the world is in the US. See https://www.migrationpolicy.org/article/foreign-students-united-states.

the relative returns of the different majors, which vary with the business cycle. They find that there is a low, but significant elasticity of major choice with respect to expected earnings. In the US context, Wiswall and Zafar (2015) find that students revise their beliefs and future major choice when provided with information on earnings. Using an opposite approach, Arcidiacono et al. (2012) leverage the misinformation that students have about labor market beliefs and find that students would have made different choices for their majors had they had full information on labor market returns. These studies show that students are forward looking with their expectation of their future earnings when considering their field of studies, and they respond accordingly to information on major-specific wage premia. My study contributes to the literature by considering foreign students and the role of expected earnings in their major choice.

To date, there is only one other study that I am aware of which provides some evidence of the effect of OPT extension on major choice. Amuedo-Dorantes et al. (2019) use multiple waves of National Survey of College Graduates to examine changes in the propensity of having a STEM degree. They compare the proportion of STEM degree holders among individuals who enter the US with a student visa to other individuals who enter the US with a permanent visa or an H-1B visa. Their analysis provides evidence that the OPT extension increases the propensity of holding a STEM degree for individuals who enter the US with a student visa by 18%. As discussed by the authors, the results could be driven by individuals who enter the US with student visas being induced to major in STEM. However, it is likely that part of the effect they find is mechanical due to the decrease in return migration among STEM graduates following the OPT extension. I view my study as complementary to this paper since I am able to directly study the effect of the OPT extension on enrollment into STEM major.

2.3 Background on Optional Practical Training

Foreign students who attend an academic institution in the US have several options after they graduate. First, they can leave the US. Second, students can transfer to another academic program to continue their studies, or they can also change to a different nonimmigrant status.[33] Lastly, students can use their OPT. The last option is a 12-month training program that allows foreign students, or more accurately, students in F-1 nonimmigrant status, to enter the labor market without changing their nonimmigrant status. The employment, however, has to relate to the students' field of studies.[34] Even though foreign students can also use the OPT (pre-completion) during their educational program, I focus on the OPT (post-completion) that students use after they graduate from their respective institutions.[35]

Since the OPT program does not require a change in academic status to participate in the labor market, foreign students have had a significant barrier to employment removed. One of the constraints in entering the US labor force is the difficulty of obtaining an H-1B visa,[36] which is a non-immigrant visa that allows US firms to employ workers to work in specialized occupations.[37]

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[33] For a list of nonimmigrant and immigrant visa categories, see: https://travel.state.gov/content/travel/en/us-visas/visa-information-resources/all-visa-categories.html.

[34] Foreign students can only work at E-verified firms and companies. These are entities that are enrolled in U.S. Citizenship and Immigration Services (USCIS) E-Verify employment verification program, which allows enrolled employer to confirm the eligibility of their employees to work the in United States.

[35] There are two types of OPT that F-1 students can obtain—pre-completion OPT where students can use during their educational program and post-completion OPT where students can use after they graduate. Any period of time of employment used before completing their degree will be deducted. For more information, see https://www.uscis.gov/opt.

[36] Lowell, B. L. (2000) describes the historical and legislative background of the H-1B visa.

[37] Only higher education institutions, nonprofit organization, and government research organization are exempt from the annual cap.

With an H-1B visa, employees can work up to three years with the option to be extended to a maximum of six years at the sponsoring firm. There is, however, an annual cap of 65,000 for the number of H-1B visas and an additional 20,000 for workers with advanced degree from the US that was imposed in 2004. Since then, the H-1B cap has been often binding every year and the program is oversubscribed. For instance, the filing period, which begins on April 1 before the start of each fiscal year on October 1, has been reached significantly earlier each year. In fact, for fiscal year 2008, the cap was reach on April 2, 2007—the first business day for filing, and the U.S. Citizenship and Immigration Services (USCIS) actually received more than twice the number of the approved cap.[38] Once the filing period is closed, USCIS will then randomly select from the petitions that were filed during the acceptance period. For petitions that are not selected, employers must then wait for the next fiscal year to be able to sponsor their foreign employees.

The OPT provides an opportunity to obtain working experience in the US without having to undergo the H-1B petition. Unlike the H-1B, foreign students who use their OPT are also not bound to be working with the sponsoring employer, and they can switch to any employment that is related to their field of studies. With the OPT program, foreign students have up to a full year to gain on-the-job experience, undergo training as well as form professional network in their field of interest without subjecting to tedious immigration processes. The opportunity—for on-the-job experience, training and professional network—would serve as an invaluable experience for a new graduate, regardless of the individuals' preferred location for their long-term employment. However, for foreign students who want to secure long-term employment in the US, the OPT is

[38] For comprehensive information on H-1B reach dates as well as number of petition that is in excess of annual limit, see https://redbus2us.com/h1b-visa-cap-reach-dates-history-graphs-uscis-data/. This is a popular website and forum that non-immigrants in the US refer to. I also cross tabulated the information with the USCIS website.

even more crucial. The 12-month period can be used to find a better match with employers as. Students can also gain firm-specific training that would make them a better candidate for employers to sponsor them for an H-1B visa. Analogously, firms can use the OPT as a screening period for foreign students before committing to the process of sponsorship.

On April 8, 2008, when the OPT period was extended from 12 months to 29 months for STEM fields, both the opportunity for work experience and the window for transition lengthened significantly for STEM graduates. Students can now work up to 29 months, and for those whose H-1B petition are not selected, they can remain at their current employment and their employers can file their petition for as long as the OPT is valid. On May 11, 2012, DHS expanded the list of STEM fields to include more majors. Students who graduate from fields like Quantitative Economics and Pharmaceutical Sciences now qualify for the 17-month extension as well. For the sake of clarity in this paper, I classify the fields of studies into three categories—*traditional* STEM fields, *newly added* STEM fields, and *non*-STEM fields—which students can complete their degree in. The classification follows how DHS designates degree programs into STEM fields.[39] Majors which are designated by DHS as STEM fields before 2012 are categorized as *traditional* STEM fields while majors that are included as STEM fields in 2012 are categorized as *newly added* STEM fields. All majors excluded from the list of STEM fields are then non-STEM fields. In 2016, the OPT extension for STEM graduates is changed from an additional 17 months to an additional 24 months.[40]

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[39] For the full list of STEM designated degree, see https://www.ice.gov/sites/default/files/documents/Document/2014/stem-list.pdf

[40] Since my data does not include years post 2015, I do not discuss the implication of the extension.

2.4 Conceptual Framework

Since the research question of interest is the effect of the extension of OPT on foreign students' major choice, it is beneficial to consider the value of the extension. When the OPT was extended from 12 months to an additional 17 months for graduates with STEM degree in 2008, the lengthened period allows a longer stay in their academic status while participating in the labor market. This provides a longer period to gain industry experience and training. Foreign students can also switch among employers to find a good fit or gain up to 29 months of firm-specific training. The extended length of OPT also increases the number of chances for an H-1B petition. Depending on when the student graduate, STEM graduates have multiple chances to apply for the petition. In sum, the OPT extension increases the length of time to obtain training and probability of securing both long term employment and working visa.

Theory predicts that enrollment into STEM degree should increase. There are two margins in which students will be induced to study a STEM degree in the US, and depending on which margin are more responsive to the policy change, the average quality of foreign students changes accordingly. The first margin is made up of students who would have studied STEM degrees in other countries but are induced to study in the US after the 2008 extension.[41] The 2008 extension can increase quality if low ability STEM foreign students have a very low probability of finding permanent employment even without the extension. Hence, the OPT extension would only have an effect on high ability STEM foreign students and not on students on the left tail of the ability distribution. One the other hand, it could decrease quality if low ability STEM foreign students

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[41] I adapt Kato and Sparber's (2010) theoretical model of foreign students' migration decision when considering foreign students who apply to US school to study STEM fields based on the perceived labor market prospects upon graduation.

now see the extension as an opportunity for longer firm-specific training, which would make them a better candidate for permanent employment. In this case, low ability foreign STEM students are induced to apply to the US, lowering the average ability of foreign students studying STEM fields. On this margin, the effect of the extension on average ability of foreign students in STEM fields is ambiguous.

The second margin consists of foreign students who would have studied non-STEM majors in the US and are now induced to study a STEM degree. There is an unambiguous decline in the average quality of foreign students studying STEM for this margin after the extension. Individuals differ in STEM ability, and occupation differ in wage premium.[42] Following the canonical Roy model (1951), individuals select field of studies based on their individual expectation of their own abilities and characteristics of the labor market. If all else is equal, high ability foreign students would then select into STEM fields since there is a higher wage premium in STEM related occupations. When the OPT period was extended for STEM graduates, it induces non-STEM foreign students who are in the higher distribution in STEM ability to select STEM fields. Even though the marginal students who are induced to select into traditional STEM majors are positively selected from non-STEM fields, the average ability of the marginal students is lower than the average ability of a foreign student who are always takers of STEM fields. This leads to a decrease of average ability of foreign students in STEM fields.

Since the foreign students select into fields of studies in accordance with their abilities and labor market returns, it is important to note that any potential effect of the OPT extension in affecting

[42] Evidence has shown that undergraduate degrees are concentrated in particular occupations, and there are heterogeneity in earnings across fields (Black et al., 2003; Ransom & Phipps, 2010; Altonji et al., 2014).

foreign students' major choice hinges on the implicit assumption that there are some degree of complementarity between skills for both STEM and non-STEM fields. To expand on the discussion on complementary of skills, I will use the classification of the field of studies – *traditional* STEM fields, *newly added* STEM fields, and *non*-STEM fields – which I have introduced in the previous section. When DHS expanded the list of designated STEM degree program in 2012 to include fields that rely heavily on analytical and quantitative skills but were otherwise listed as *non*-STEM fields prior to 2012, it follows that *newly added* STEM fields are positively correlated in skills with *traditional* STEM fields. In other words, there is a high complementary in STEM skills between *newly added* and *traditional* STEM fields.

This would imply that foreign students who would have otherwise chosen to study newly added STEM fields such as pharmaceutical sciences as well as econometrics and quantitative economics would be prime candidates of foreign students who would select into traditional STEM fields when the OPT were extended. Analogously, this would also mean that when the newly added STEM fields qualify for the OPT extension in 2012, marginal students who would have been induced to select traditional STEM degree would then substitute back to newly added STEM fields. In my analysis, I make the distinction between traditional and newly added STEM fields to examine the potential substitution of foreign students from traditional STEM fields into newly added fields that qualify for the OPT extension. I also account for the composition change of foreign students after the extension.

While enrollment rate for traditional STEM program should increase after the OPT extension, its effect on degree completion rate is unknown ex-ante. Even if individuals weigh the cost and benefit before choosing field of studies that maximize their welfare, they can only make decision based on their expectation of their abilities, and there is always a risk of overestimation.[43] Several studies in the literature of college completion have stressed on the role of uncertainty and shown that learning about schooling ability has accounted for college attrition (Altonji, 1993; Arcidiacono, 2004; Arcidiacono et al., 2016). Coupled with the potential change in the ability distribution of foreign students in traditional STEM fields, the effect of the OPT extension on the level of realized rate of completion in traditional STEM degree is ultimately an empirical question.

2.5 Data

My main source of data is a unique, administrative data set on higher education enrollment from the state of Ohio, which is made available to researchers by the Ohio Education Research Center (OERC).[44] The nature of the administrative dataset provides a large sample size and detailed information about the individuals. More importantly, the dataset allows for the tracking of students, including foreign students, who enroll in undergraduate and graduate programs at one of the 13 public four-year universities in Ohio from the year 2004 to 2015.[45] The data include information on students' demographic characteristics such as year of birth, gender, race/ethnicity, and citizenship status. The dataset also include term-level information on admissions, enrollment, program of study, and credentials earned. Because my analysis also considers the length of time

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[43] Altonji et al.'s (2016) in the Handbook of the Economics of Education presented a simple model of dynamic major choice that highlights the role of uncertainty of individuals as they decide on their field of studies.

[44] The Ohio Longitudinal Data Archive is a project of the Ohio Education Research Center (oerc.osu.edu) and provides researchers with centralized access to administrative data. The OLDA is managed by The Ohio State University's Center for Human Resource Research (chrr.osu.edu) in collaboration with Ohio's state workforce and education agencies (ohioanalytics.gov), with those agencies providing oversight and funding. For information on OLDA sponsors, see http://chrr.osu.edu/projects/ohio-longitudinal-data-archive.

[45] I restrict the same to after 2004 as there was a structural policy change in the H-1B, which could confound the estimates. Hence, I only focus on the period where the visa caps were already in place.

and the pathway of the student's education, I also restricted my sample to first-time degree-seeking students for their level of education. In particular, for undergraduate students, I do not consider transfer students, and I do not consider doctoral students who continue with their doctoral degree after obtaining a Master's degree. The sample is then drawn from 1,165,524 first-time degree-seeking students.

The unit of observations for my analysis to study the completion of degree is a schooling spell for each student enrolled in a program. I define the outcome of interest to be a dichotomous variable indicating whether the students completed—a traditional STEM, newly added STEM or non-STEM degree, which I consider for all the levels of degree program. Every student has a separate record in each year of their tenure in school before completing their degree. For example, a student who is enrolled in school for four years before completing their traditional STEM degree would have four separate record where the first three years of "Tenure" will have "0" associated with the outcome of interest and "1" for the 4^{th} year of "Tenure". If students begin a new program after completing their initial degree, I consider them to be a new unit of observation. To study enrollment into traditional or newly added STEM fields, I indicate the field of studies that individuals enroll into in their first term in graduate programs. As for undergraduate programs, I utilize the person period information, and I define an indicator variable for whether the student ever declared a traditional or newly added STEM field throughout their schooling spell. To examine how OPT extension affects enrollment into field of studies, I then collapse the data by the year of enrollment of each student to use the individual level information.

Using the person level data, Table VII shows the basic descriptive information on the analysis sample, and each panel shows the characteristics for different level of degree. Panel A shows the sample for students in the Bachelor's program sample, which make up 75% of the total sample, while panel B and C show the sample for students in the Master's and doctoral program sample, which consist of 21% and 4.1% respectively. Across the degree levels, there are more female native students enrolled compared to foreign students. Foreign students also tend to be older than native students in the undergraduate level, but that relationship switches at the graduate level. Table VII also indicates the share of students entering traditional STEM and newly added STEM fields for the different level of programs. It is not surprising that foreign students make up a larger proportion of enrolling in traditional STEM fields, and the share is even more significant in the graduate program. However, it is interesting that the share of foreign students is more comparable in newly added STEM fields for both Bachelor's and Master's program. Another important point to note about the low rate of completion rate is that the sample is drawn from students who enroll from 2004 to 2015. The seemingly low rate of completion is driven by students who enroll in later years and may not have completed their degree, rather than a high dropout rate. Appendix Table I and Appendix Table II show the proportion of students who complete traditional and newly added STEM degree, respectively, by each year since enrollment for all level of program.

2.6 Empirical Approach

I am interested in the effect of OPT on both the enrollment decision and completion of STEM degree. To study enrollment, I use a student-level analysis that predicts whether foreign students are relatively more likely to enroll in a STEM program following the policy. To study completion, I estimate a hazard model that investigates how the policy affects the yearly probability of persisting to the following year of one's program before completing the degree. The hazard model allows me to assess how the policy affects the cumulative probability of completion, and it facilitates decomposing the total probability of completion into a series of persistence probabilities at each year of one's program.

To identify the causal impact of the OPT extension on enrollment into traditional STEM fields, I use a difference-in-difference strategy whereby I will exploit the random timing of the OPT extension. Specifically, I will estimate a model of the following form:

$$y_{ifsct} = \alpha + \gamma F1_i + \lambda^{08}\, Post08_t + \varphi^{08}(F1_i \bullet Post08_t) + \lambda^{12}\, Post12_t$$

$$+\varphi^{12}(F1_i \bullet Post12_t) + \ \chi_i + \ \chi_f + \chi_{ft} + \chi_c + \ \chi_{ct} + \ \varepsilon_{ifsct} \quad (1)$$

where y_{ifsct} reflects the enrollment into major field *f* that is categorized as a traditional STEM field *s* for student *i* at school *c* in year *t*. $F1_i$ is an indicator variable for being a foreign student (has an F1 visa status) while $Post08_t$ is another indicator variable equal to one if the OPT extension has been passed by year of the students' enrollment *t*. The interaction term $(F1_i \bullet Post08_t)$ is the interaction between foreign student dummy and post treatment dummy. As previously discuss, because of the high complementary in skills between traditional and newly added STEM fields, I account for the possibility of students being drawn away from the STEM fields to the newly added STEM fields when they qualify for the extension in later years. Like $Post08_t$, $Post12_t$ is an indicator variable, but it is equal to one if the student *i* is enrolled in year *t* when the newly added STEM fields qualifies for the OPT extension. By including $Post12_t$ and $(F1_i \bullet Post12_t)$, I account for the possibility of foreign students being drawn away from the STEM fields to the newly added STEM fields.

I control for individual level characteristics such as age at enrollment and gender, which are represented by the vector χ_i. To account for difference across fields as well as unobserved field-specific changes over time, I include both field fixed effects, χ_f and field-by-year fixed effect, χ_{ft}. I do the same to alleviate the concern of differences in school quality and unobserved school-specific changes over time by including school fixed effects, χ_c and school-by-year fixed effects, χ_{ct}. The coefficient of interest is φ^{08}, which shows the difference-in-difference estimates of OPT extension effect on enrollment in traditional STEM fields.

The ideal experiment to identify the OPT extension impact would be to randomly assign the extension to foreign students. Without the random assignment in the OPT extension for students, this model uses native students as the counterfactual group. The key assumption of this strategy is that both foreign and native students would follow the same time trend absent of the treatment. Even if this policy extension is a result of increased demand from the science and tech industries, there are no reasons to think that foreign and native students would be responding differently to the labor demand prior to the OPT extension. In addition to that, I assume that foreign students and native students have parallel trends in their decision to major and complete traditional STEM degree and newly added STEM degree in the absence of the OPT extension in 2008.

To study completion of traditional STEM degree for foreign student, I utilize the longitudinal aspect of the dataset to map out the effect of the policy extension throughout the years the student is enrolled in a program. This means that I am expanding the person level data to utilize the person-period data information, where each individual student has their schooling spell associated with them. This approach allows the comparison of the pre- to post-2008 change in the probability of completing a traditional STEM degree for foreign student, conditional on the student being in school the year before. This is otherwise known as the hazard rate. The equation below provides the model specification:

$$y_{ifsct} = \alpha + \beta_t\, Tenure_{it} + \delta_t(Tenure_{it} \bullet F1_i)$$

$$+ \theta_t^{08}(Tenure_{it} \bullet Post08_t) + \theta_t^{12}(Tenure_{it} \bullet Post12_t)$$

$$+ \gamma_t^{08} \left(Tenure_{it} \bullet F1_i \bullet Post08_t\right) \ + \gamma_t^{12} \left(Tenure_{it} \bullet F1_i \bullet Post12_t\right)$$

$$+ \chi_i + \chi_i^{F1} + \chi_f + \chi_f^{F1} + \chi_{ft} + \chi_c + \chi_c^{F1} + \chi_{ct}$$

$$+ \ \varepsilon_{ifsct} \qquad (2)$$

where y_{ifsct} is an indicator variable for whether student *i* completed a major *f* that is categorized as a STEM field *s* in year *t*. $Tenure_{it}$ specifies the separate record in the time-period *t* that the student *i* is at risk of completing the outcome of interest. As specified before, $F1_i$ is an indicator variable for being a foreign student, and $(Tenure_{it} \bullet F1_i)$ is the interaction term for $F1_i$ and $Tenure_{it}$. Again, $Post08_t$ is an indicator variable equal to one if the OPT extension is in effect while student is enrolled in year *t*. However, note that I define the "treated" students to be students who are enrolled when the OPT extension is in effect instead of their enrollment year.[46] This is to account for the possibility of switching majors for foreign students who are already enrolled in school when the OPT extension came into effect. $(Tenure_{it} \bullet Post08_t)$ is then the interaction term of $Post08_t$ and $Tenure_{it}$ while $(Tenure_{it} \bullet F1_i \bullet Post08_t)$ is the interaction term of $Tenure_{it}$, $F1_i$, and $Post08_t$. By including $(Tenure_{it} \bullet Post12_t)$ and $(Tenure_{it} \bullet F1_i \bullet Post12_t)$, I account for the possibility of foreign students being drawn away from the STEM fields to the newly added STEM fields.

Like equation 1, I control for individual level characteristics, χ_i. Because there are differential in the gender and age of foreign and native students who choose to pursue STEM as shown in Table

[46] For example, a student who is enrolled in year 2006 and remains enrolled in school in year 2009 would have $Post08_t = 1$. For the previous analysis for enrollment, this distinction is not necessary as each individual student has an observation at the year of their enrollment.

VIII, I also included an interaction of χ_i with being a foreign students, defined by χ_i^{F1}. I include both field fixed effects, χ_f and field-by-year fixed effect, χ_{ft} to account for difference across fields as well as unobserved field-specific changes over time. I also include field-by-foreign student fixed effects, χ_f^{F1}, to account for the types of foreign students in different fields. I do the same to alleviate the concern of differences in school quality, unobserved school-specific changes over time, and selection of foreign students into different school by including school fixed effects, χ_c, school-by-year fixed effects, χ_{ct}, and school-by-foreign student fixed effects χ_c^{F1}. The coefficient of interest, γ_t^{08}, measures the impact of the OPT extension. More specifically, γ_t^{08} measures the probability of completing a traditional STEM degree in tenure *t*, conditional on the student enrolled in school in tenure *t – 1.* In other words, I estimating the impact of the OPT extension on foreign students' hazard rate of completing traditional STEM, relative to native students.

This analysis relies on the assumption that the OPT extension is not correlated with any other unobservable influencing the graduation rate for foreign students. An example of a violation of this assumption might be the appointment of a department faculty to recruit foreign students into STEM fields while enacting other changes to improve the new foreign students' outcomes (but not those foreign students who already chose the STEM major). The possibility of faculty members enacting changes that would improve outcomes only for new entrants but not for students who already chosen STEM is highly unlikely especially since faculty members would not be able to differentiate between students who would always choose STEM majors and students who are induced to choose the major.

In my conceptual framework, I discuss the potential difference in quality and types of foreign students who are induced by the policy extension to enroll in US academic institution to study

STEM degree. To examine this hypothesis, I estimate equation (1) including an interaction of school-by-year fixed effects with the foreign student status, χ_{ct}^{F1}. If γ_t^{08} from equation (2) does not change with the inclusion of $\chi_{ct}F1_i$, then it implies there is no changes in the types of students. However, if changing quality of student is positively correlated with the probability of completion in STEM degree, then there will be an upward bias in the estimated γ_t^{08} from equation (1). Analogously, if lower ability students are induced to enter the program, then I would have underestimated the size of the effect in γ_t^{08}.

2.7 Results

2.7.1 Effects of OPT Extension on Enrollment in STEM Program

Before discussing the results from equation 1, I plot an event student to examine the difference-in-difference estimates for the probability of enrolling in traditional STEM degree by year for each level of program. Figure 4 (a) shows the event study for bachelor's degree. The omitted year for these analysis is the year 2007, one year before the policy is in place. Figure 4 (b) and (c) show the event study for graduate programs, but the figures look to be a little imprecise to have a clear understanding of the effect of the OPT extension.

Table IX presents the estimated effect of the OPT extension on the probability of foreign students' enrollment into traditional STEM fields. The coefficient of interest for "Foreign by post '08" in Column (2) is estimated from the model specification in equation 1 for Bachelor's degree. There is an increase of 1.8 percentage point in the enrollment into STEM fields for foreign students after the extension. It is interesting to note that the coefficient is larger in magnitude in Column (2) than compared to Column (1). To examine the potential substitution from traditional STEM fields into newly added STEM fields when they qualify for the OPT extension, I estimated equation 1 without accounting for the policy change in 2012. The change in the magnitude of coefficient for "Tenure by post ‘08” suggests that foreign students in the undergraduate level are substituting to newly added STEM major when the fields qualify for the extension in 2012. Although the results suggest that there is a positive increase in enrollment of foreign students in the Master’s and Ph.D. program, I do not observe similar change for foreign students in the substitution to newly added STEM majors when they quality for the extension. This may be because it is more difficult to switch fields in the graduate level. The results, however, are statistically insignificant, and I do not have the precision for any conclusive inferences.

Since results in Table VIII, in particular for Bachelor’s degree, provides some evidence of foreign students substituting into newly added fields when they qualify for the OPT extensions in 2012, I further test this hypothesis by estimating the effect of the OPT extension on enrollment into newly added using the same specification in equation 1. Since newly added STEM fields do not qualify for the extension in 2008, I do not expect any positive estimates for “Foreign by post ’08”. The results for the estimation for Bachelor’s, Master’s and Ph.D. programs are shown in Table IX, and as expected, they are all negative in magnitude. Even though the coefficients on “Foreign by post ‘12” are not statistically significant to reject that the effect is different from zero, a visual inspection of Figure 5 shows some evidence of an overall upward trend for enrollment into newly added STEM fields after 2012.

2.7.2 Effects of OPT Extension on Completion of STEM Degree

The evidence on compositional change for the types of foreign students in traditional STEM fields is more apparent when examining the results for completion in traditional STEM fields. But, more importantly, I am able to exploit the rich longitudinal aspect of the dataset to study the pathway to completion of degree for foreign students. Appendix Table III presents the effect of the OPT extension on the hazard rate of completing traditional STEM degrees for foreign students in the undergraduate level. Namely, the coefficients of "Tenure by foreign by post '08" show the difference in difference estimates of the probability of completing a traditional STEM major in each respective tenure, conditional on foreign students not declaring the major the year before. To fully understand the dynamics of substitution into newly added STEM fields, I also estimate Equation 1 without accounting for the policy change in 2012. The results are in Column (1). Foreign students have negative risk of completing a traditional STEM degree throughout their schooling years after the extension. When accounting for the effect of newly added fields qualifying for OPT extension in Column (2), the hazard rate becomes slightly less negative. The change in the magnitude of the hazard rate suggests that some foreign students are substituting to newly added STEM major when the fields qualify for the extension in 2012.

In Columns (3) and (4), I explore the possibility of a compositional change in the types of foreign students who enroll in academic institutions after the extension. Columns (3) and (4) are analogous to Columns (1) and (2), but they are estimated with an inclusion of a school-by-foreign-by-year fixed effects to allow for differential effect for foreign and native students by the school and year of enrollment. The estimates for "Tenure by foreign by post '08" in both Column (3) and (4) change to be more positive, and they also differ from Column (3) and (4) in the same direction as Column (1) to (2). I focus on Column (4) as it is most comprehensive in its controls. Relative to native students, foreign students have a slightly higher risk of completing their traditional STEM degrees after the extension in 2008.

To illustrate the findings in Column (4), I show the graphical form of difference-in-difference analysis of the hazard rate for both foreign and native students in Figure 6 (a). As previewed in Appendix Table III, the share of traditional STEM degree is lower after the extension, and the hazard function of both native and foreign students show similar relationship, where they are lower after the extension. While native students have a lower risk of completing the traditional STEM degree compared to foreign students throughout the schooling spell, the more significant period is in the third year after enrollment. Foreign students have a positive risk of completing the degree from the third year while native students only face a high risk of completing after the fourth year. After the extension, the decrease in the hazard rate is also the smallest in the third year for foreign students.

Figure 6 (b) shows the corresponding estimates of the OPT extension effect on the hazard rates for completing traditional STEM major for foreign students. Note that the figure is also plotting the coefficient estimates for "Tenure by foreign by post '08" from Column (4) of Appendix Table III. For further clarity, I trace the path towards completion of degree by showing the probability of completion before and after the extension for both native and foreign students in Figure 6 (c). Figure 6 (d) shows the difference-in-difference estimates of the probability by each year. From Figure 6 (d), foreign students have 0.7 percentage point increase in completing the traditional STEM degree by the third year after the extension. This is about a 6 percentage point of increase from the probability in completion rate for foreign students.

Using the same empirical approach, I examine the OPT extension on the completion of Master's degree and Ph.D. degree in traditional STEM fields. Results from both table Appendix Table IV and Appendix Table V show that the effect of OPT extension is positive for Master's and Ph.D. degree, respectively, after accounting for the compositional change in types of foreign students as well as the potential substitution into newly added STEM fields. Since Master's program are usually two year long, it is not surprising that foreign students are at the greatest risk of completing their degree in the second year. It is also the year that records a positive hazard rate. While the effect of the OPT extension is less obvious for Bachelor's degree, its effect for the Master's program is significant. In Figure 7 (a), it is apparent that, conditional on not completing in the first year, foreign students are more likely to complete the degree in the second year after the extension. Figure 7 (c) and (d) show that there is a 7 percentage point increase in the probability of completing a traditional STEM degree for foreign students after the policy change.

The largest effect the OPT extension has, however, is on the Ph.D. program. From Figure 8 (c), the probability of foreign students completing in traditional STEM degree throughout the schooling spell has increased after the extension, where the highest risk of completion is in the fifth year since enrollment. The relationship is seen in Figure 8 (d), where the highest peak in probability is in the fifth year at around 0.034. Compared to the completion rate of foreign students before the extension, the increase in the probability is about 11 percentage point. In Figure 8 (a), it is interesting to note that, after the extension, the hazard rate for foreign students decreases after the 5th year. By simple interpretation of hazard rate, this means that, conditional on not graduating by the fifth year, foreign students now have a low risk of completing the degree.

Similar to understanding enrollment of foreign students in newly added STEM fields, I use equation 2 to student the completion of newly added STEM fields. The results for the estimation for Bachelor's, Master's and Ph.D. programs are in Appendix Table IV, V and VI, respectively. The point estimates for "Tenure by foreign by post '08" is either negative or close to zero. Furthermore, the coefficients on "Tenure by foreign by post '12" provides another evidence of response in major choices to the OPT extension. As newly added STEM degree qualify for the extension, there is a positive hazard rate in the later years for foreign students pursuing Bachelor's degree and Ph.D. degree, and the hazard rate for the second year of Master's degree is also positive.

Figure 9 (a) plots the estimated effect of newly added STEM in Bachelor's degree qualifying for the OPT extension in 2012 on the hazard rate for foreign students relative to native students. The solid line shows that the risk of completing newly added STEM fields increases after the third year when the fields qualify for the OPT extension. For ease of comparison as well as a check, I also plot the hazard rate for completion of newly added STEM fields for students who were enrolled in school before and after 2008. The dashed line shows that there is no effect of the 2008 OPT extension on the probability of foreign students completing newly added STEM fields. In fact, the hazard rate is close to zero for the first four years and start declining after.

In Figure 9 (b), I show the result for Master's degree. While the effect of newly added STEM fields qualifying for the OPT extension is very small in magnitude, the general pattern does show that the hazard rate increases in the second year. Furthermore, the small change induced by the qualification of the OPT extension in the second year for students enrolled after 2012 is slightly more apparent when compared with the declining rate of the hazard function, which models the 2008 change. The hazard function in Figure 9 (c) shows a steady increase in risk of completing newly added STEM fields for Ph.D. degree after the 4^{th} year of enrollment, and there is a sharp increase after the 6^{th} year. This results provide evidence that newly added STEM in Ph.D. degree qualifying for the OPT extension in 2012 has a positive effect on the probability of completion. However, it is important to note that there is an unexpected rise in the hazard rate in the 4^{th} year after the 2008 OPT extension. Since newly added STEM fields did not quality for 2008 change, there should not be any effect of the OPT extension. However, other than the 4^{th} year, it is reassuring the hazard rates are mostly zero or negative.

2.7.3 Effects of OPT Extension on Average Quality of Foreign Students in STEM Fields

Lastly, I provide more direct evidence of changes in the quality of foreign students who enroll in US academic institutions for traditional STEM fields after the policy extension. Given how the hazard rate changes after accounting for potential compositional change in foreign students who enroll in US academic institutions after the extension, I further examine the differential in quality and types of foreign students. Since I do not have information on pre-enrollment indicators on quality (i.e., SAT or GRE scores), I use first term GPA as the next best measure for students' academic quality. I use the same specification in equation (1) to compare the average academic quality of foreign students who enroll in US academic institutions for STEM degree before and after the extension. Table X shows the effect of the OPT extension on the first term GPA for foreign students who enroll in a traditional STEM program after the OPT extension for all the levels of program. The OPT extension led to a statistically significant decline of 0.02 in first term GPA for foreign students in the Master's program. Again, the results are too imprecise to conclude the effect of the OPT extension for foreign students in both the Bachelor's and Ph.D. program.

2.8 Conclusion

This study provides insight to the effect of OPT extensions on foreign student enrollment in STEM fields and their completion rate. I show that the OPT program is an effective tool to increase enrollment in STEM fields in higher education, especially for the undergraduate level. I find that foreign students are 1.8 percentage point (5.4%) more likely to enroll in traditional STEM fields after the extension. I find that foreign students are 6 percentage point more likely to complete a Bachelor's degree in traditional STEM after the OPT extension while the increase in Master's degree and Ph.D. degree are 7 percentage point and 11 percentage point, respectively.

The results also take into account the potential substitution of foreign students into newly added fields, which qualify for the extension in later years, as well as the potential compositional changes in the types of foreign students who enroll into academic institutions after the policy extension. I also explore how average academic quality changes for foreign students who were induced to choose STEM degree after the extension, and I find that there is a decline in the average first term CGPA. Further analysis suggests that foreign students who are induced into STEM degrees are foreign students who would have chosen the fields that eventually qualify for the extension in 2012. These results provide evidence that foreign students choose specific majors in accordance to their comparative advantage. Since the marginal benefit of a STEM degree has increases, foreign students in fields that have a high complementary in skills would now select into STEM fields. The decrease in the average quality of foreign students in STEM fields also aligns with the Roy model as the marginal students who are induced to choose a STEM degree are the higher-ability foreign student in fields that eventually qualify for extension who are now the lower-ability students in STEM degree.

In closely related work, Amuedo-Dorantes et al. (2019) studied the effect of the OPT extension on the propensity of holding a STEM degree for individuals who first enter the US with a student visa. They document a statistically significant increase of 16 percentage point for Master's degree and about 4 percentage point increase, though statistically insignificant, for Bachelor's degree. Surprisingly, they find a negative effect on the likelihood of doctoral graduates to hold a STEM degree. There are a few theoretical possibilities that my findings differ, which the authors discussed as well. First, their results are subject to selective outmigration. Due to the nature of their data, which sample college graduates living in the US at the time of the survey, the authors are not able to distinguish if the extension induced students to study STEM fields or if the extension just enable STEM degree graduates to prolong their stay in the US while on their student visa. Since I observe foreign students while they are enrolled in school, my results give a direct measure of the OPT extension for STEM degrees on foreign students' major choice.

The second reason could be the control group that is used in their analysis. Their comparison group are individuals who enter the US with a permanent visa or working visa (H-1B) before returning to pursue higher education. It is important to note that individuals with a permanent visa do not face similar constraint when searching for employment, where they are essentially considered as native. More importantly, individuals who enter with a H-1B visa are legally required to switch to a student visa in order to enter academic institution, which would underestimate any effect found. This could be the reason that they find a negative effect of the doctoral program while I find a positive effect.

Lastly, because all of the institutions I study are Ohio public universities, the results from my analysis may not generalize to the whole nation. Foreign students may be drawn to other states in the US and the effect of the OPT extension may induce more significant changes to enrollment and completion in those places. That said, the academic institutions in Ohio, which includes very selective universities (e.g. Ohio State) and less selective university (e.g. Akron) does provide a close representation of the landscape in the US. Furthermore, while California and New York host the most foreign students in the US, Ohio ranks 8th in the country for states with the most foreign students.[47]

[47] For statistics on international exchange in the US by state, see https://www.iie.org/Research-and-Insights/Open-Doors/Fact-Sheets-and-Infographics/Data-by-State-Fact-Sheets.

From a policy perspective, my study provides insight to the effect of OPT extensions. The consequences of any changes should not be assumed to be of negligible effect. In 2017, the number of foreign workers in the US using their OPT exceeds the number of workers using an H-1B visa to join the labor force. Given the growing concern of that the US is falling behind other countries in both the production of STEM degrees[48] and the competitiveness in the STEM workforce,[49] having a better understanding on how foreign students respond to change in immigration policies can help address the STEM shortages in both academia and labor force. My result imply that foreign students do value work experience in the US, and they make schooling decision on that dimension. Policies that restrict foreign students' ability to gain work experience will adversely affect their higher education enrollment in the US. These implications should be taken into consideration as policymakers consider suspending the OPT program.

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[48] For statistics on the 2018 Science and Engineering Indicators by the National Science Board, see https://www.nsf.gov/statistics/2018/nsb20181/figures.

[49] See National Academy of Sciences, National Academy of Engineering, and Institute of Medicine, Committee on Prospering in the Global Economy of the 21st Century: An Agenda for American Science and Technology, and Committee on Science, Engineering, and Public Policy, Rising Above the Gathering Storm: Energizing and Employing America for a Brighter Economic Future (Washington, DC: National Academies Press, 2007).

3 THE EFFECTS OF GRADUATING WITH HONORS ON EARNINGS

This chapter was previously published as Khoo, P., & Ost, B. (2018). The effect of graduating with honors on earnings. Labour Economics, 55, 149-162.

3.1 Introduction

In classical human capital models, education is directly productive and a firm pays educated workers a premium because it is able to directly observe workers' abilities. In these models, a firm does not use credentials to statistically discriminate because it is assumed to have perfect information on worker quality. Though there is little doubt that the human capital model accurately captures some dimension of the benefits to education, whether or not firms also use education as a signal is less clear. Despite its theoretic importance, there is a paucity of evidence on whether firms use credentials as a signal of quality, likely because of the substantial obstacles associated with identifying signaling effects empirically.

In this study we ask whether Latin honors serve as a signal to employers. Latin honors (e.g. *cum laude*) is a designation made by universities to reward students with exceptional academic performance. To study this question, we use unique data that matches administrative college records to administrative earnings information for the entire state of Ohio. We find that obtaining honors provides an economic return in the labor market, but this benefit only persists for two years. By the third year after college, we see no effect of having received honors on wages, suggesting that firms may use the signal for new graduates, but they do not rely on the signal for determining the pay of more experienced workers. This is consistent with Arcidiacano, Bayer and Hizmo (2010), which argues that workers without access to educational signals gradually reveal their quality as they gain experience.

A fundamental obstacle to estimating the signaling effect of Latin honors is that students who obtain honors are likely more productive than other students in unobservable ways. As such, these students are expected to have higher earnings, even if honors itself does not increase earnings. In an ideal setting, we would randomly assign Latin honors and then observe the resulting earnings differential between treatment and control. Our empirical strategy mimics this ideal by using a regression discontinuity design. In Ohio, students are awarded Latin honors solely based on their final cumulative GPA. Though the exact policies vary by school, all follow the same basic structure in which students above a particular GPA threshold are given honors and students below this threshold do not get honors. We use these policies to compare students with very similar GPAs, but one obtains honors because she is just above the threshold whereas the other does not get honors because she is just below the threshold.

A key concern in this context is the possibility that students manipulate their GPA in order to get just above the required threshold. We cannot definitively rule out this possibility, but we provide several pieces of evidence that suggest it is unlikely to explain the earnings effects that we see. In particular, we observe no discontinuities in any of the covariates that we test, including covariates that are strongly predictive of future earnings. If students sort strongly on unobservables, it would have to be done in a manner that is completely uncorrelated with a wide range of important observable characteristics. Second, if students manipulated their GPA in order to get just above the *cum laude* threshold, the histogram of the running variable would show a heap to the right of the threshold and a valley to the left of the threshold. We see no evidence of either a heap or a valley surrounding the *cum laude* threshold.

Though conceptually related, there are a few key differences between the signaling model developed in Spence (1973) and our context. First, it is possible that firms view Latin honors as a signal of ability, but this ability was learned in college as opposed to being an innate trait as in Spence's original model. As such, the signaling value of Latin honors could theoretically operate through a human capital effect. Second, in our context, firms could theoretically observe the underlying GPA that determines Latin honors, whereas in Spence's model, he assumes that firms cannot observe the underlying determinant of education. If firms perfectly observe (and understand) GPAs, observing that an individual has Latin honors may provide no new information to employers since it is simply a mechanical function of GPA. In the institutional background section, we provide a discussion of several institutional factors relevant to whether ex-ante, one expects Latin honors status to provide employers with new information or not. Ultimately, whether or not firms respond to Latin honors is an empirical question and our findings suggest that they do.

3.2 Related Literature

Although our study is the first to consider the effect of Latin honors, several papers have estimated signaling effects in other contexts.[50] The early literature on signaling looks for sharp increases in earnings associated with degree receipt and generally finds large signaling effects (Hungerford and Solon 1987; Jaeger and Page 1996; Kane and Rouse 1995). This literature relies on regression to control for confounding factors and cannot rule out the possibility that those who complete a degree differ from students who do not complete a degree in unobservable ways. In addition to potential unobserved heterogeneity, Flores-Lagunes and Light (2010) discuss various conceptual issues with that literature, and notes that holding fixed degree receipt, years of schooling is likely

[50] Nederhof and Van Raan (2005) compare the publication rates of *cum laude* doctorates in physics compared to ordinary doctorates, but their study is unable to control for unobservable differences between the two groups. They find that *cum laude* doctorates publish more articles than other doctorates.

negatively correlated with outcomes since those who take longer to complete the same degree are negatively selected.

At the high school level, we are aware of several studies that account for the possibility of unobserved heterogeneity in estimating signaling effects. Jepsen, Mueser and Troske (2016) study the labor market returns to a GED using a regression discontinuity design. Unlike earlier studies (e.g. Tyler Murnane and Willet (2000)), they find limited returns to the GED, though there is some evidence of a short-term increase in earnings for men. Clark and Martorell (2014) estimate the signaling value of a high school degree using a regression discontinuity design based on high-school exit exams. Clark and Martorell find no evidence of a signaling return to a high school degree.

A recent paper investigates the effect of honors degrees among law graduates in Germany (Freier, Schumann and Siedler 2015). We view our study as complementary to this paper, since we consider a different context (United States as opposed to Germany), we consider all college graduates as opposed to just law students, and we have a different identification strategy. Freier et al. (2015) use a difference-in-difference approach based on the fact that law students are awarded honors based on academic performance, but students in medicine and pharmacy do not have an honors distinction. The difference in earnings between high- and low-performing law students is assumed to reflect both the honors effect and skill differences, whereas the difference in earnings between high- and low-performing medicine/pharmacy students only reflects skill differences. Their key identifying assumption is that in the absence of an honors policy, the labor market returns to high academic performance in law would be the same as the labor market returns to high academic performance in medicine and pharmacy. They find a large earnings return to academic honors of 14%.

A recent working paper considers the closely related topic of degree class effects in the UK (Feng and Graetz 2016). Their analysis uses a fuzzy regression discontinuity design based on a series of grade cutoffs that determine which of the five degree classes a student receives.[51] Aside from considering a somewhat different question, there are several important differences between our study and Feng and Graetz (2016). First and most importantly, Feng and Graetz use survey data that lack a measure of earnings and so they cannot directly examine the effect of degree classes on earnings. Instead, they study the effect of degree classes on whether an individual is employed and whether an individual works in a high-earnings industry.[52] Second, Feng and Graetz focus on graduates of the London School of Economics and Political Science (LSE), whereas we use data from a set of less selective institutions -- many of which are completely non-selective. As such, in addition to studying a country with different educational and labor market institutions, we study a different population of college graduates. Feng and Graetz (2016) find that earning higher degree classes increases the probability of working in a high-wage industry six months after graduation by approximately 10 percentage points and has no effect on the probability of employment six months after graduation.

Our study also relates to a smaller literature on the effect of awards and prizes. Chan et al. (2014) use a synthetic control approach to study the effect of winning prestigious academic awards such as the John Bates Clark Medal on future publications and citations. They find that award receipt

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[51] Unlike Latin honors, degree classes are the only summative measure of college performance used in the UK since the GPA system is not used. Ex-ante, one might expect that degree classes would have a larger effect on labor market outcomes than Latin honors since its marginal information content is larger.

[52] Feng and Graetz (2016) note that their survey includes a question about annual salary, but the response rate was much too low to use this measure as an outcome. As an alternative, they include a supplemental analysis where the outcome of interest is imputed earnings based on industry, sex, and age.

increases both measures. This could be viewed as an effect of award receipt on productivity, but it could also reflect the signaling value of the award since both citations and publications are dependent on other researchers' perceptions of one's quality. Neckermann et al. (2014) study the effect of award receipt in call centers and find that winning an award leads to an increase in productivity in the following month. If Latin honors similarly affects productivity itself, then our estimates should be interpreted as the combined effect of the signaling value of academic honors and the productivity increased caused by the award receipt. We suspect, however, that there is unlikely to be direct productivity effects in our context because the award granting entity (the university) is distinct from the employer. As discussed in Neckermann et al. (2014), the theoretical mechanisms through which awards could affect productivity all operate through improved worker commitment to the company that provided them with the award.

3.3 Institutional Background of Latin Honor

Latin honors (e.g. *cum laude*) are awarded to graduates of 4-year schools based on academic performance. In Ohio, all 4-year public schools award Latin honors and receipt is determined by strict GPA cutoffs. Though most institutions in the United States use a fairly similar system to Ohio public schools, how GPA cutoffs are determined varies across institutions. The Ohio institutions that we study all use exact GPA cutoffs that vary by year, institution and major and range from 3.2 to 3.7. Though we only study the effect of Latin honors, all of the public 4-year schools in Ohio also grant other types of honors based on holistic evaluation taking into account extra-curricular activities, letters of recommendation and essays.

Several broader institutional factors are important to keep in mind when considering the likely effect of Latin honors on earnings. Importantly, these considerations are relevant for an ex-ante evaluation of the likely effect of Latin honors as opposed to being important for internal or external validity. As such, although we discuss a variety of factors that *could* potentially be relevant, we are not arguing that any of these factors necessarily occur in practice. Instead, we are establishing that there are some potential reasons that employers might value *cum laude* and the later sections empirically assess whether or not they do in practice.

First, the likely effect of Latin honors depends on the information set presented in student's resumes. If applicants send employers their cumulative GPA, Latin honors (based on cumulative GPA) may provide little new information. We have no direct data on what the college graduates we study list on their resume, so instead we discuss the advice that career coaches give to recent college graduates regarding what they *should* put on their resume. Based on examining online resume advice for recent college graduates, we identify several pieces of advice that are relevant to our research.[53] First, students are encouraged to list Latin honors on their resume if they have it. This suggests that at least career consultants *believe* that there is a labor market return to the signal. Second, some career advisors recommend listing GPAs only if it is above 3.0, while others recommend only listing very high GPAs such as 3.8 or above. If some students only list their cumulative GPA if it is 3.8 or above, *cum laude* provides employers with new information since the threshold for *cum laude* are always below a 3.8. Coaches that advise students to list a GPA, typically suggest that students present either their cumulative GPA or their major GPA, depending on which is higher.

A second institutional consideration is that, some institutions (outside of our sample) award Latin honors based on percentile rankings rather than exact GPA thresholds. For those institutions, Latin

[53] The advice we discuss is based on reading a variety of resume help websites, but all of the advice we discuss is found in the Forbes.com article "How to Write a Resume When You're Just Out of College" (Adams 2012).

honors provides additional information over and above the information content of cumulative GPA. Under the assumption that employers are not aware of the exact Latin honors policy at each school, employers may view honors as providing information on relative performance at one's schools as opposed to being a deterministic function of GPA. Relatedly, employers may not have perfect information regarding the interpretation of GPAs at different institutions and they are unlikely to know the particular *cum laude* thresholds used at different institutions. If an employer is uncertain about whether a GPA is good for a given institution, the *cum laude* label may provide new information.[54]

A final institutional consideration is that Latin honors is determined just before students graduate, and students may have already accepted a job prior to graduation. As such, for a subset of students, there is no way that Latin honors could affect their initial placement. This timing will reduce the likelihood that we would find an earnings return to Latin honors. A recent survey of college graduates estimates that approximately 17% of students secure employment by graduation (Svrluga 2015). Although this percentage is likely larger among high-performing students, it appears that a large enough fraction of students obtain employment after graduation so that cum laude could plausibly affect initial placement.[55]

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[54] If an employer does not know that a particular school uses a 3.4 threshold, the employer will not know that an applicant with a 3.39 barely missed *cum laude* nor that an applicant with a 3.41 GPA barely achieved *cum laude*.

[55] Even if *cum laude* has no effect on initial placements, it is possible that it could affect future employment. As such, *cum laude* status could affect earnings in the first few years, even among students who obtain their initial employment prior to graduating. That said, we suspect that employers are most likely to consider *cum laude* status for students with little work experience. Indeed, career coaches note that most employers do not care about academic performance once applicants have a few years of work experience (Adams 2012).

3.4 Data

Our primary data sources are administrative higher education data matched to administrative earnings records for the state of Ohio. These data are made available to researchers by the Ohio Education Research Center (OERC) and include data from the Ohio Longitudinal Data Archive (OLDA).[56] The earnings records come from unemployment insurance (UI) records and include weekly earnings for the years 2003-2013. These data cover most employees in the state of Ohio and only exclude self-employed workers, farmers and federal employees. The data include a separate observation for each quarterly payment made from a firm to a worker along with the number of weeks worked. The higher education data come from the Ohio Department of Higher Education and include term-level data on GPA (measured without rounding), admission data, demographics and college of attendance. These data cover all students enrolled in public tertiary education in the state of Ohio from 1999-2011.

The higher education data include a total of 36 higher educational institutions that can be categorized into three types. Six schools are four-year institutions with a selective application process. These schools are the type ranked by the US News and World Report where students who apply can be rejected based on academic quality (e.g. Ohio State University). Seven schools are four-year non-selective institutions, in which the vast majority of applicants are admitted. These schools are not ranked by the US News and World Report and are effectively open-

[56] The Ohio Longitudinal Data Archive is a project of the Ohio Education Research Center (oerc.osu.edu) and provides researchers with centralized access to administrative data. The OLDA is managed by The Ohio State University's Center for Human Resource Research (chrr.osu.edu) in collaboration with Ohio's state workforce and education agencies (ohioanalytics.gov), with those agencies providing oversight and funding. For information on OLDA sponsors, see http://chrr.osu.edu/projects/ohio-longitudinal-data-archive.

enrollment (e.g. Shawnee State University).[57] The remaining 23 schools are two-year non-selective institutions. Having a mixture of selective and open-enrollment public universities is typical of most states.

We are able to track students if they move across schools, but for the purposes of the current study, we categorize students entirely based on the degree-granting institution. Although it is theoretically possible to study *cum laude* effects at all three types of institutions, we exclude two-year schools for several reasons. First, two-year schools have a very low graduation rate such that fewer than 12% of enrollees obtain a degree within 6 years of first enrollment. Second, the students who do graduate tend to have relatively low GPAs. Finally, not all two-year schools grant *cum laude* honors. Together, these factors make it so that there is insufficient density around the *cum laude* threshold for two-year schools. Our baseline analyses combine all the four-year schools together, but we also show specifications in which we split the analysis according to selectivity tier. The 13 schools in our analysis sample are the universe of BA granting public institutions in the state of Ohio.[58]

We manually collected data from academic handbooks at each school regarding their *cum laude* policies. This measure is time-varying within a school, and our coding reflects any changes in these policies. In practice, there were very few policy changes during our timeframe. We spoke with registrar officials at each school to verify that we had accurate information regarding *cum*

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[57] We categorize schools based on the US News and World Report tier categorizations. We have verified that our results are not sensitive to using other selectivity categorizations.

[58] A detailed description of each school, can be found on the Ohio Department of Higher Education website at: https://www.ohiohighered.org/campuses.

laude thresholds during our timeframe and to confirm that the *only* determinant of *cum laude* at every school is cumulative GPA.

We make a number of sample restrictions to improve the reliability of our data.[59] First, we restrict the data to individuals with GPAs between 0 and 4 and with non-negative earnings outcomes. Second, we set earnings during graduate school to missing since these earnings are unlikely to reflect true earnings potential. In practice, this affects relatively few students. Third, as in past work using UI data, we exclude small payments from firms to workers to increase the likelihood that our earnings measure accurately captures labor earnings. We set the threshold so that we exclude observations that correspond to earning a weekly wage less than what a full-time worker would earn at the minimum wage. The exact threshold is somewhat arbitrary, and so we have verified that results are robust to more or less restrictive thresholds. Finally, we drop a handful (<0.001%) of extreme outliers who report *weekly* earnings in the typical *annual* earnings range. There are no other observations that come even close to this range and so we suspect that the extreme outliers are reported in error. Even after cleaning the earnings data, it is likely that some of the records that we categorize as earnings are actually other types of payments from firms to workers.[60] Similarly, we likely incorrectly drop some individuals who actually earn below the

[59] Though the data are administrative, there are still potential sources for error, particularly in the UI earnings data. The UI data includes any payment from a firm to an individual, which may include payment that do not constitute as earnings (e.g. legal settlements). Furthermore, individuals who are generally missing earnings information because they have left the state or are self employed may have a small portion of their earnings appear in the data (e.g. a worker who leaves the state but receives a few hours of back pay in the next quarter). See Webber (2015) and the cites therein for more discussion.

[60] This issue affects all studies that use UI earnings records and may bias estimates towards zero since *cum laude* is unlikely to affect non-earning payments.

minimum wage or are working few hours, but this is unlikely to bias our estimates. That said, it is important to keep in mind that our data are not representative of these types of individuals.

Relative to using survey data, the administrative data have several advantages for our research question and empirical design. First, large sample sizes are critical for estimating the regression discontinuity design, since the design focuses on observations near the threshold. Second, survey data on GPA (particularly retrospective data) has substantial measurement error (Takalkar et al. 1993). Using administrative data on cumulative GPA limits the attenuation bias associated with a noisy running variable.

Although our data is likely the best available for studying our question of interest, there are several limitations of these data for our purposes. First, we have no data on hours worked, so we cannot distinguish between working more hours per week and a higher wage rate. Theoretically, a productivity signal should affect wages as opposed to earnings and so it would be ideal to have a direct wage measure. That said, our administrative earnings measure may have less measurement error than survey questions regarding wages. Second, we cannot distinguish between non-employment, self-employment, federal employment and employment outside of Ohio. A substantial fraction of Ohio college graduates leave the state immediately after graduation and so this is an important data limitation. We are principally concerned with whether this attrition is systematically related to *cum laude* status, and we show in a later section that there is no discontinuity in missing earnings at the threshold.

Table XI shows basic descriptive statistics for the overall sample, the set of students who are above the *cum laude* threshold and the set of students who are below the *cum laude* threshold. Table XI shows that a large majority of the state of Ohio is white and this is even more true among students who obtain honors. Fewer than 2 percent of the sample are Hispanic and only 6 percent of the sample are black. Students who obtain honors complete the degree in slightly less time than other students and complete fewer credits. Not surprisingly, students who eventually obtain *cum laude* have substantially higher first-term GPAs compared to other students.

Table XI also shows that in the year following graduation, students who obtain honors earn approximately \$70 more per week compared to students who do not obtain honors.61 This difference could reflect the causal effect of honors on weekly earnings, but it could also reflect observed and unobserved differences between those with honors and those without. The final row of Table XI shows that we observe no earnings for approximately 44 percent of college graduates. This is similar to past work that uses UI data from a single state (Zimmerman 2014) and we show in a later section that there is no evidence of a discontinuity in the probability of missing earnings at the threshold.

3.5 Empirical Approach

Our analysis uses a sharp regression discontinuity design based on the fact that those above the threshold obtain honors whereas those below the threshold do not. To implement the analysis, we first construct our running variable, C_i as

$$C_i = GPA_i - cutoff_i \tag{1}$$

where GPA_i is student *i*'s graduating cumulative GPA and $cutoff_i$ is the relevant cutoff for *cum laude* honors based on student *i*'s college and major. Using this running variable, we then estimate

[61] Whenever we refer to the year after graduation, we define this in terms of the 4 quarters following degree receipt as opposed to starting with the next calendar year. This same definition applies to year $t + 2$ and year $t + 3$.

$$Y_i = \alpha C_i + \beta D_i + \gamma C_i D_i + \eta_i + X_i \delta + \varepsilon_i \qquad (2)$$

where Y_i denotes various outcomes, C_i is the running variable defined above and D_i is an indicator for whether C_i is above zero or not. η_i is a set of school-by-major-by-graduating-year fixed effects and X_i is a vector of student characteristics including age, gender and race indicators. Standard errors are heteroskedastic robust, though inference is similar if standard errors are cluster on the running variable as in Card and Lee (2008).

We include school-by-major-by-graduating-year fixed effects in all specifications to account for the fact that the composition of students differs across institutions and different institutions use different cutoffs. Even with the RD design, the school-major-year fixed effects are necessary because the normalized running variable is defined over slightly different ranges for different schools. For example, a school with a 3.7 cutoff has no observations at 0.4 above the cutoff since it is not possible to get a 4.1 GPA. Because some schools have cutoffs much lower than 3.7, the composition of schools represented changes discontinuously at $C_i = 0.3$ in this example. Though there is no compositional change at the threshold, the compositional change away from the threshold can lead to biased slope coefficients (α and γ), and this can bias estimates of the discontinuity.

We implement the RD analysis following the various recommendations from Imbens and Lemieux (2008). We begin the RD analysis by presenting all of our results in figure form with the data collapsed in to 0.1 point bins. To match the school-major-year fixed effects from the regression we plot outcomes conditional on school-major-year.[62] To obtain precise point estimates and

[62] This is similar to the recommendation of Imbens and Lemieux to show visual evidence based on conditional mean plots. Though testing for a discontinuity in the conditional mean plot is similar in spirit to controlling for the covariate

standard errors, we estimate local-linear regression for a variety of bandwidths. We use the IK optimal bandwidth described in Imbens and Kalyanaraman (2011), but we also explore the robustness of our results to a variety of other bandwidths. To assess the likelihood that differences in the characteristics of those just above and just below the threshold lead to biased estimates, we show that results are very similar when we control for covariates.

3.6 Specification Tests

A key concern is the possibility that students finely manipulate their GPA in order to land just above the honors threshold. This is a serious concern in our context because thresholds are publicly known, and students can theoretically alter their effort in order to obtain honors. That said, whether students are able to *finely* manipulate their GPA in order to obtain honors is an empirical question. Students may not view obtaining Latin honors to be sufficiently important to justify substantially altering their effort or they may lack the ability to finely manipulate GPA. We are able to provide substantial empirical evidence on this question both by visually inspecting the histogram of the running variable and by directly testing for whether there is sorting correlated with observable characteristics.

The first 11 plots in Figure 10 investigate whether there are discontinuities in any of the observable covariates at the threshold. Appendix Table IX shows local-linear RD estimates for each covariate using the IK optimal bandwidth. Across all specifications, none show a statistically significant discontinuity—even at the 10% level. We take these findings as suggesting that students do not manipulate GPA in a way that is correlated with observable characteristics. It is worth emphasizing

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in a regression, the two are not equivalent because the regression uses the covariate to residualize both the outcome and the running variable, whereas the figure can only residualize the outcome.

that many of the covariates that we test are likely to be related to a variety of unobservables that one might be concerned with. For example, the second to last row of Appendix Table IX shows no significant discontinuity in weekly earnings from the year prior to graduating.

As a summative way to investigate covariate imbalance, we use all of our covariates to predict weekly earnings in the year after graduation. We then examine whether there is a discontinuity in predicted earnings at the honors threshold. The last row of Appendix Table IX shows no evidence of a discontinuity in predicted earnings. This suggests that based on their observable characteristics, we have no reason to expect that those just above the honors threshold would earn more than those just below the honors threshold.

Figure 11 shows the histogram of final GPAs, with a line indicating the cum laude threshold. If students who are just below the cum laude threshold manipulate their GPA to get just above the cum laude threshold, we should observe a large valley just to the left of the threshold and a large heap just to the right of the threshold. The figures show no evidence of this pattern, providing further reassurance that students do not finely manipulate the running variable in this context.

3.7 <u>Effects of Latin Honors on Earnings</u>

Before studying the main outcomes of interest, we first consider whether sample selection is likely to substantially bias our estimates. Individuals can be missing earnings information for a variety of reasons, but the most common is that they have left the state of Ohio. Theoretically, if *cum laude* causes the very best (or worst) to leave the state, this will lead to biased estimates, even if there is no running variable manipulation. To investigate the likelihood that sample attrition biases our estimates, we estimate whether there is a discontinuity in the probability of missing earnings information at the *cum laude* threshold.[63]

Appendix Table X shows our estimates of the discontinuity in missing earnings in the year after graduation. Figures 12 (a) – (c) show the corresponding figures. Appendix Table X shows that the estimated discontinuities are statistically insignificant, are small in magnitude and switch signs across specifications. It is important to note that even if the estimates in Appendix Table X were all statistically significant, the magnitude is much too small to generate substantial bias since at most, the point estimates imply less than 1% differential attrition.

Figure 13 presents our main results of the effect of *cum laude* on weekly earnings in the year after graduation.[64,65] Figure 13 (a) shows the result for the overall sample, while Figure 13 (b) and Figure 13 (c) split the sample by the selectivity of the school. Overall, Figure 13 (a) shows evidence of a discontinuity in earnings right at the threshold. Based on Figures 13 (b) and (c), it appears that the overall discontinuity is driven by a discontinuity among the selective schools.

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[63] Though our main analysis considers weekly earnings in $t + 1$, in a later analysis we also consider earnings in $t + 2$ and $t + 3$. To maintain a consistent sample across specifications, we restrict all analyses to workers that are observed all the way until $t + 3$. The test for differential attrition is thus testing whether the probability of having earnings out to $t + 3$ differs at the threshold. The discontinuity estimates for the $t + 1$ outcomes are similar if we remove this restriction and there is no evidence of a discontinuity in missing earnings in $t + 1$ alone.

[64] Weekly earnings are measured conditional on having positive earnings since it is not possible to distinguish zero earnings from leaving Ohio or being self employed.

[65] We also explored the effect of *magna cum laude* and *summa cum laude* but with less density around these thresholds, these estimates are imprecise. The point estimates for the higher thresholds are statistically indistinguishable from zero, but we also cannot rule out sizeable positive effects (larger than the *cum laude* estimates).

Table XII provides exact discontinuity estimates along with standard errors for the overall sample and split by selectivity. In addition to showing estimates that use the IK optimal bandwidth and include controls, Table XII also shows robustness to a variety of bandwidth choices, with and without controls. Before discussing the results, it is worth noting that the stability of the estimates across bandwidth choices is reassuring that our preferred specification reasonably captures the visual discontinuities shown in Figures 13 (a), (b) and (c). Similarly, it is reassuring that the estimates are relatively stable when covariates are added.

For the overall sample, we estimate that the discontinuity is approximately $25 per week or roughly 3% of weekly wages. This estimate is statistically significant at the 5% level in most specifications. For the selective schools, the estimated discontinuity is larger in magnitude and also statistically significant at the 5% level in most specifications. Though the point estimates are generally similar across bandwidth choices, for the 0.25 bandwidth, the estimates are either insignificant, or only significant at the 10% level. We view this marginal significance as reflecting a lack of power as opposed to suggesting that there is not a *cum laude* effect. In particular, the general pattern of results shown in Figure 13 and Table XII seems inconsistent with the notion that the discontinuity is plausibly just sampling variation. For non-selective schools, none of the estimates are statistically significant but estimates are generally positive. Though we cannot statistically distinguish between the selective and non-selective discontinuity estimates, we only have clear evidence of an effect for the selective schools.

In addition to increasing wages, *cum laude* may also allow students to find a job more quickly and thus work more weeks during the year following graduation. This idea is based on a search model where workers who receive a positive signal face a tradeoff between increasing their reservation wage and decreasing unemployment duration. In Figures 14 (a) – (c), we show the estimated effect of *cum laude* honors on weeks worked in the year after college. Relative to the weekly earnings Figures 13 (a) – (c), the weeks worked figures are noisier and there is not a visually striking discontinuity. That said, the points to the right of the cutoff appear to be generally above trend, particularly for the non-selective schools. Table XIII shows the exact point estimates along with standard errors. All of the point estimates in Table XIII are positive, but the magnitude is largest for the non-selective schools, especially when using narrow bandwidths. These point estimates suggest that non-selective students may benefit from honors by finding a job more quickly as opposed to finding a better job. That said, the magnitude of the point estimate for non-selective schools is somewhat sensitive to bandwidth choices, ranging from 0.33 weeks to 1.01 weeks. As such, relative to our analysis of weekly earnings, we are less confident that we have identified the causal effect of honors on weeks worked because the estimates are more sensitive to the degree of linear extrapolation. Taken together, we interpret these results as suggestive of, but not definitive evidence of an effect of weeks worked and we leave it to future work to see the extent to which the effect replicates in other contexts.

In Figures 15 (a) – (c), we assess how weekly earnings and weeks worked combine to affect total earnings the year after graduation. Table XIV shows point estimates and standard errors for a variety of bandwidths. For the overall sample and the selective school sample, there is a visually clear discontinuity and these estimates are statistically significant at the 5% level except for the 0.25 bandwidth. For non-selective schools, though there is a statistically significant discontinuity for the IK bandwidth and the 0.25 bandwidth, the point estimates are sensitive to bandwidth choices and there is no visual evidence of a discontinuity.

The preceding results suggest that *cum laude* helps students signal ability in the first year after graduation, but it is possible that as students gain experience, employers begin to learn about true quality and no longer have to rely on the signal. In Figures 16 (a) – (c), we show the estimated effect of *cum laude* honors on weekly earnings 2 years after college. Figures 16 (a) – (c) show a similar pattern to the earlier results, where there is a visually apparent earnings discontinuity at the *cum laude* threshold, and this effect appears to be driven by students from selective schools. Table XV shows the exact point estimates along with standard errors. In terms of the point estimates, earnings in the second year after college are affected by *cum laude* very similarly to earnings immediately after college. The point estimates are somewhat larger, but they are also considerably noisier so that the coefficients for $t + 2$ are statistically indistinguishable from the coefficients for $t + 1$.

In Figures 17 (a) – (c), we show the estimated effect of cum laude honors on earnings 3 years after college. These figures show a very different pattern compared to the earlier results as there is no evidence of earnings discontinuities for any of the samples. Table XVI shows the exact point estimates along with standard errors. Earnings in the third year after college appears to be unrelated to cum laude. None of the specifications are statistically significant and the point estimates vary in sign across bandwidth choices. Combined with the earlier results, we interpret this finding as suggesting that Latin honors provides employers of recent graduates with a valuable signal, but after several years, employers do not rely on this signal because they have access to more directly relevant information such as work experience and job performance. Based on this theory, we expected that the year two earnings estimates would be smaller than the year one earnings estimates, but we did not see this pattern. Although we have no explanation for why there is no decline in the premium across the first two years, it is worth noting that the large standard errors of the second-year estimates prevent us from concluding that the premium did not decline across years. In particular, the 90% confidence interval for the second-year earnings effect includes many estimates that would be consistent with a declining premium.

3.8 Conclusion

We provide the first estimates of the effect of Latin honors on earnings. Our estimates are based on a regression discontinuity design that helps us to distinguish the Latin honors effect from unobserved differences between students who obtain honors and those who do not. We find that Latin honors increases weekly earnings in the year immediately after college graduation, but the earnings benefit does not persist.

Although our study is focused on the effect of Latin honors, it relates to the literature studying whether employers value academic performance more generally. Many studies have shown that there is a positive correlation between college GPA and earnings, but all of these studies face the major obstacle that college GPA is correlated with a variety of unobservables that are likely to directly affect earnings. Employers may value GPA either because they view academic performance as directly productive, or because academic performance is correlated with productive factors that the firm cannot observe. In either case, GPA would have a causal effect on earnings. Past research on the relationship between GPA and earnings, however, could be driven by a non-causal relationship between academic performance and earnings if there are characteristics that are observable to the employer, but unobservable to the econometrician. We are not aware of evidence that demonstrates that there is a causal relationship between academic performance and earnings in the United States. Our findings suggest that employers value academic performance at least among highly performing students. This result relates to recent studies suggesting that different colleges and majors provide very different labor market returns (Kirkeboen et al, 2016) Similar to their findings that not all college degrees are created equal, we find that even students with the same college and degree may derive different returns depending on their academic performance.

A few caveats are worth emphasizing regarding our results. First, the regression discontinuity design estimates the benefit of Latin honors for students at the margin. Because we are focused on students very near the *cum laude* threshold, our estimates apply to strong students, but not the very best. For our sample, this corresponds to roughly the 75^{th} percentile of graduates. It is possible that the top or bottom of the distribution would derive a larger or smaller benefit from Latin honors. Second, our estimates are based on the subset of students who remain in the state of Ohio and are not self-employed. We show that there is no differential attrition in terms of missing wages suggesting that we are able to credibly identify the wage effects among the set of students who are not missing earnings. In terms of external validity, however, it is important to keep in mind that our estimates may not apply to students who leave the state of Ohio or are self-employed.

Designating certain students as *cum laude* is a nearly costless policy that has the potential to help employers distinguish between candidates. For a college administrator deciding where to set the threshold, there are several factors to consider. Reducing the threshold necessary to obtain Latin honors provides the benefit of the signal to more students, but it has the potential to dilute, and perhaps destroy the signaling value of the designation. Our study provides evidence on the ceteris paribus effect of obtaining *cum laude* and does not incorporate this general equilibrium effect. Thus, our estimates should be taken as the effect of *cum laude* for an individual student, as opposed to indicative of the likely effect if institutions alter their thresholds.

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Figures

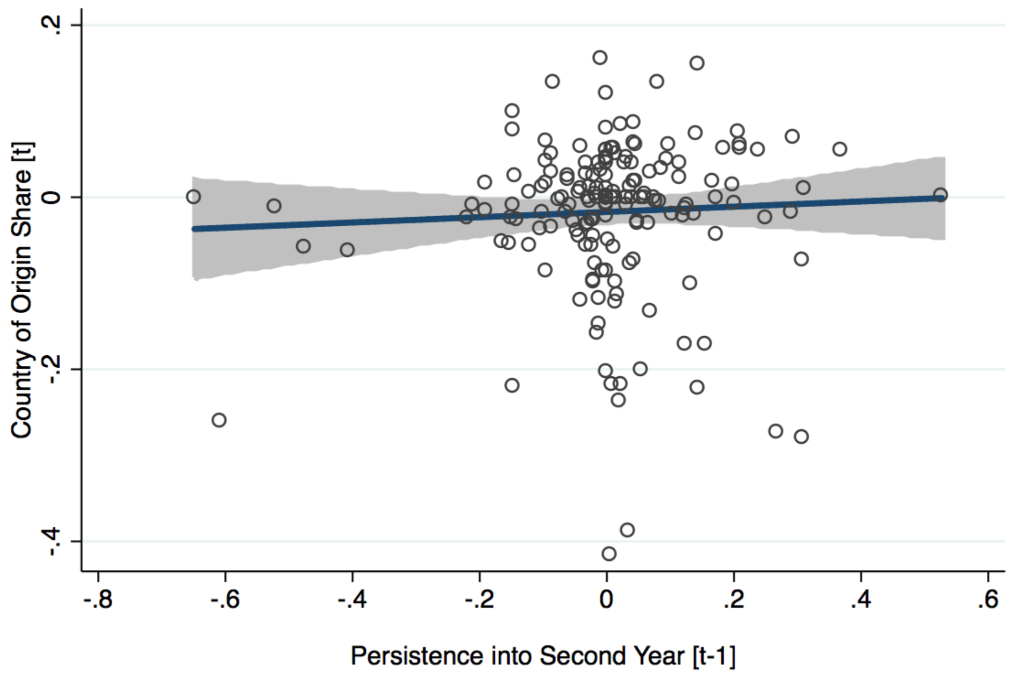


Figure 1. Persistence into the second year against the share of country of origin.

Notes: Conditional on controls, cohort and department-by-cohort fixed effects, I plot the residualized persistence into the second year for older cohort against the residualized country of origin share for the current cohort. The fitted line of the scatter plot shows a 95% confidence interval. The coefficient of the fitted line is -0.005 with a standard error of 0.031.

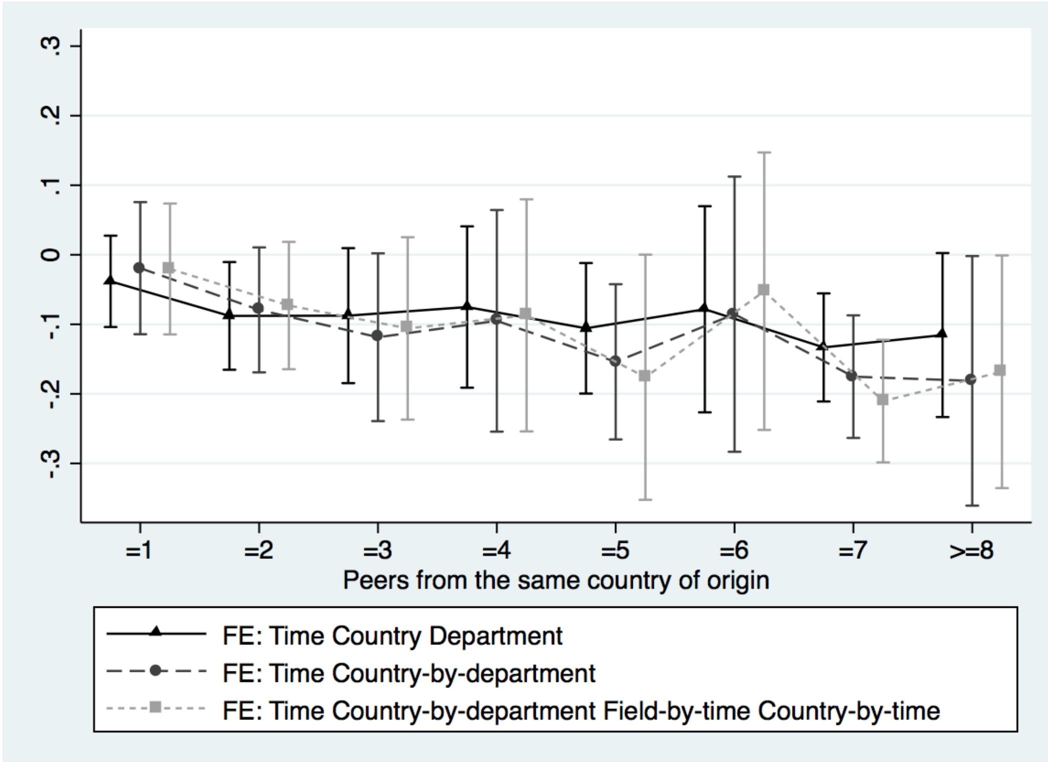


Figure 2. Effect of having different number of peers in cohort.

Notes: Each point on the plot are the coefficients for the effect of having "X" number of peer from the same country of origin relative to another international student who does not have any peers from the same country of origin. The dependent variable is an indicator variable for persisting into the second year of the program, and I show three different specifications for robustness.

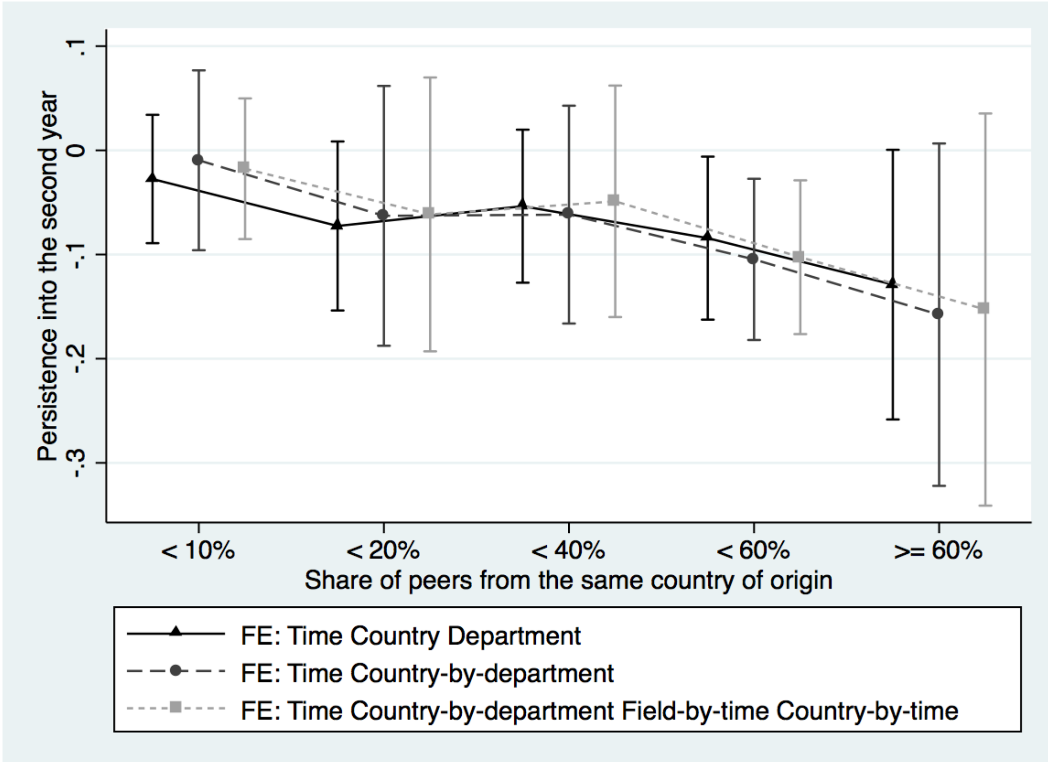


Figure 3. Effect of being in different share levels of peers in cohort.

Notes: Each point on the plot are the coefficients for the effect of having different share of peers from the same country of origin relative to another international student who does not have any peers from the same country of origin. The dependent variable is an indicator variable for persisting into the second year of the program, and I show three different specifications for robustness.

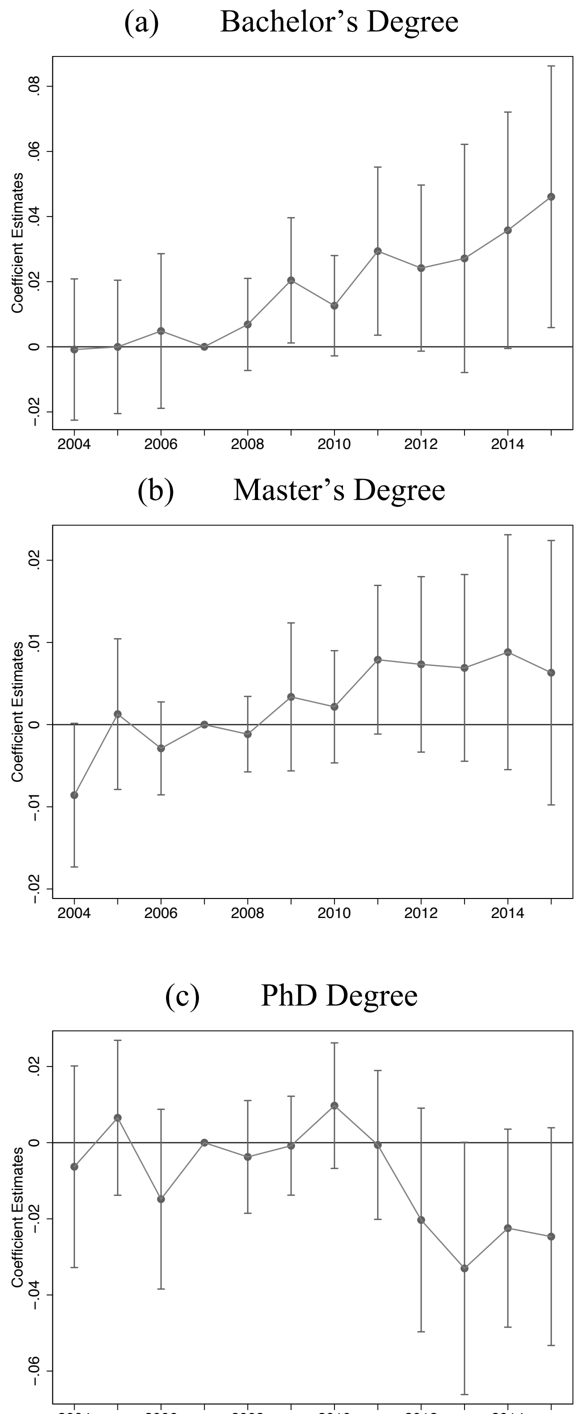


Figure 4. Event-study estimates of the OPT extension effects on enrolling in traditional STEM program

Notes: The figure plots the coefficient on the indicator variables of year relative to the OPT extension. The year before the OPT extension is omitted, and the associated coefficient is standardized to 0.

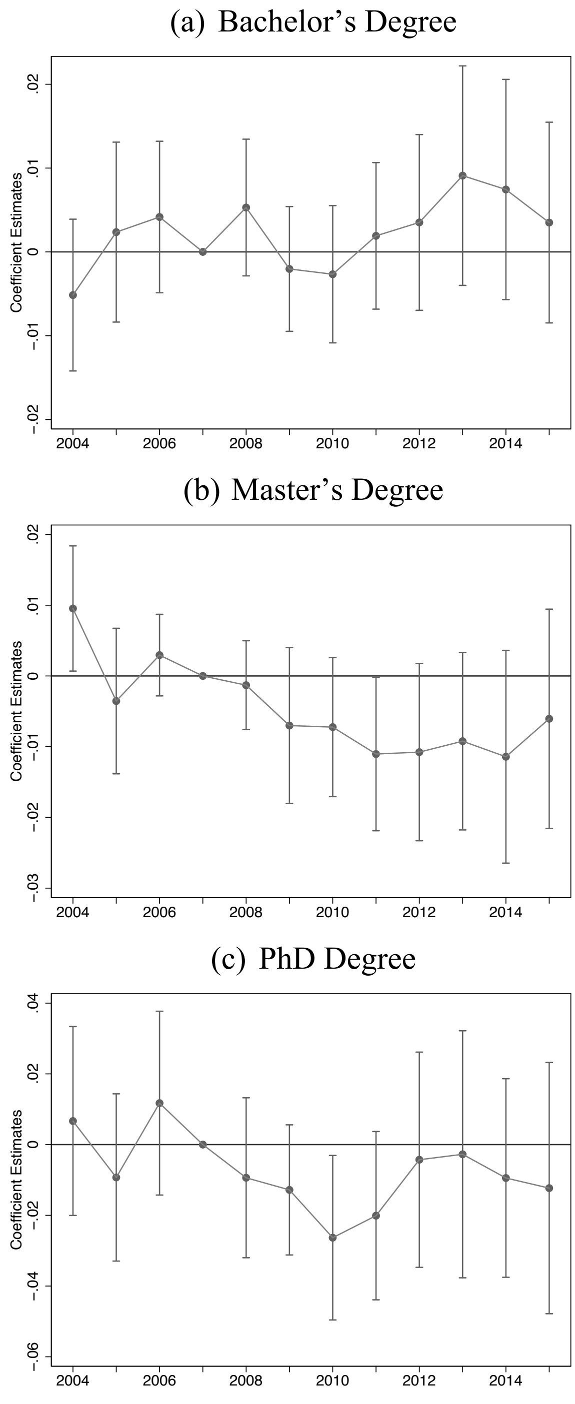
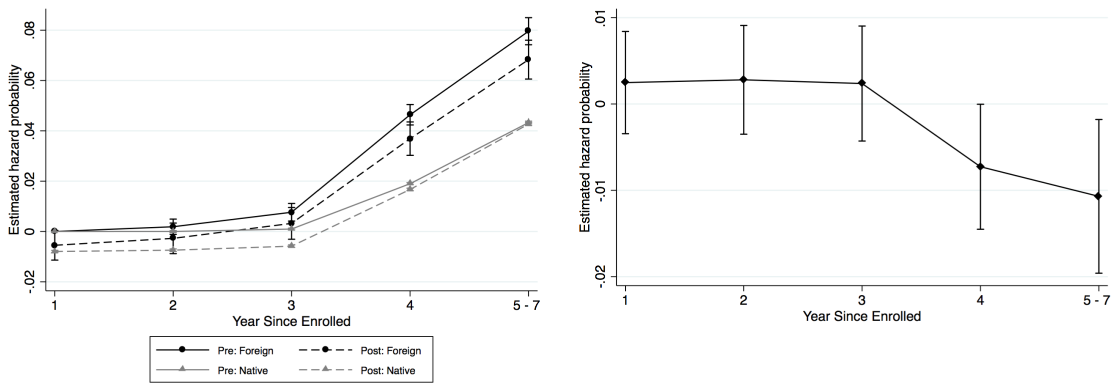


Figure 5. Enrolling in newly added STEM program by year.

Notes: The figure plots the coefficient on the indicator variables of year relative to the OPT extension. The year before the OPT extension is omitted, and the associated coefficient is standardized to 0.

Hazard function (Completing traditional STEM degree)

(a) Hazard function before and after OPT extension (b) “Difference-in-Difference” hazard function



Probability of completing traditional STEM degree by year

(c) Before and after OPT extension (d) “Difference-in-Difference”

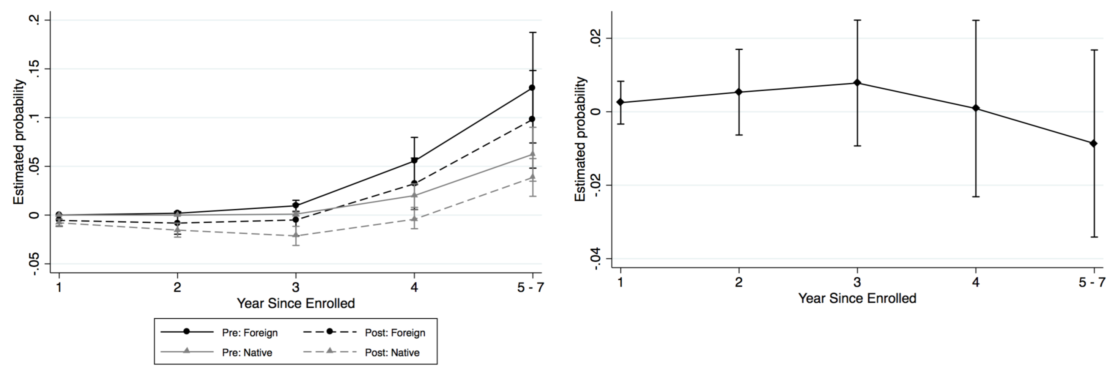
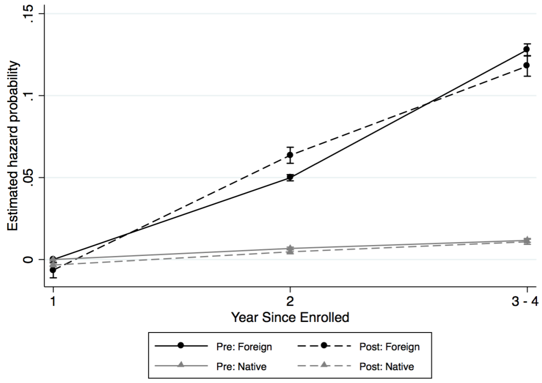


Figure 6. Hazard function and probability of completing traditional STEM degree by year since enrolled for undergraduate level.

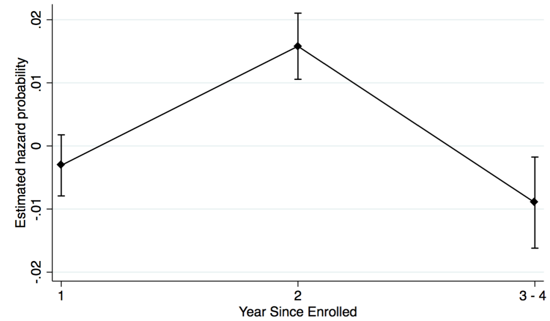
Notes: The figures plot estimates from the specifications in column 4 of Appendix Table III. Data are administrative data on higher education enrollment from the state of Ohio, made available to researchers by the Ohio Education Research Center (OERC). Sample is from year 2000 to 2015 and restricted to first-time degree seekers. The unit of observation is a schooling spell for each student enrolled in a program. The dependent variable is completion in traditional STEM degree for Bachelor’s degree.

Hazard function (Completing traditional STEM degree - Master's)

(a) Hazard function before and after OPT extension

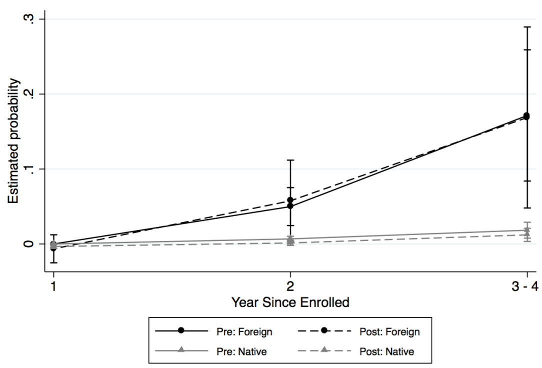


(b) "Difference-in-Difference" hazard function



Probability of completing traditional STEM degree by year - Master's

(c) Before and after OPT extension



(d) "Difference-in-Difference"

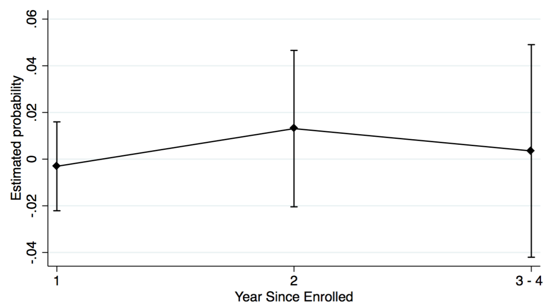
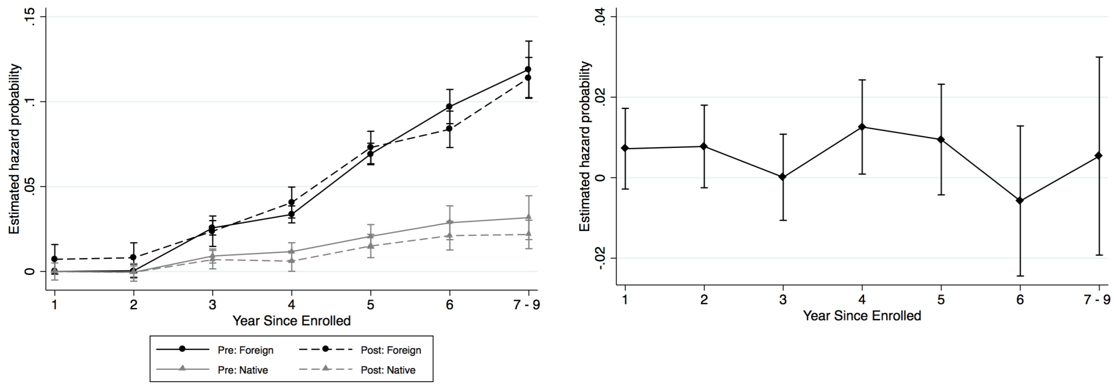


Figure 7. Hazard function and probability of completing traditional STEM degree by year since enrolled for graduate level (Master's degree).

Notes: The figures plot estimates from the specifications in column 4 of Appendix Table IV. Data are administrative data on higher education enrollment from the state of Ohio, made available to researchers by the Ohio Education Research Center (OERC). Sample is from year 2000 to 2015 and restricted to first-time degree seekers. The unit of observation is a schooling spell for each student enrolled in a program. The dependent variable is completion in traditional STEM degree for Master's degree.

Hazard function (Completing traditional STEM degree - Ph.D.)

(a) Hazard function before and after OPT extension (b) "Difference-in-Difference" hazard function



Probability of completing traditional STEM degree by year - Ph.D.

(c) Before and after OPT extension (d) "Difference-in-Difference"

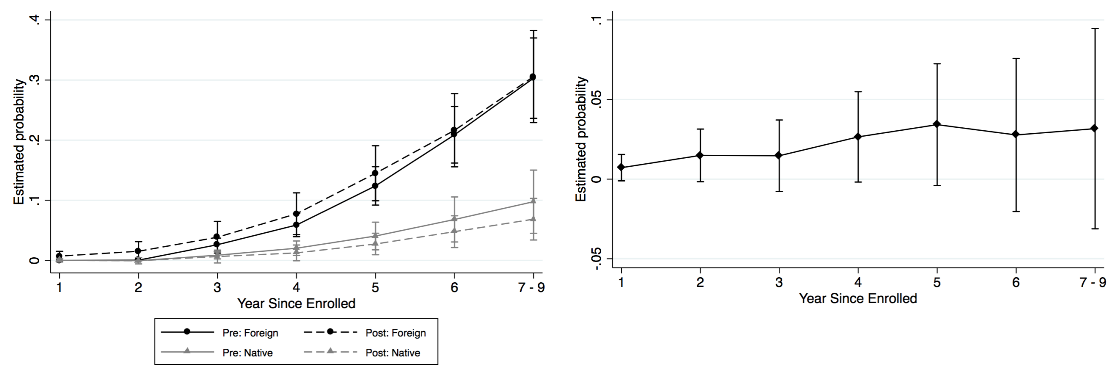
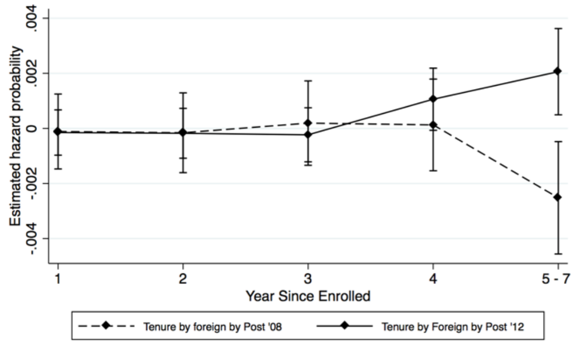


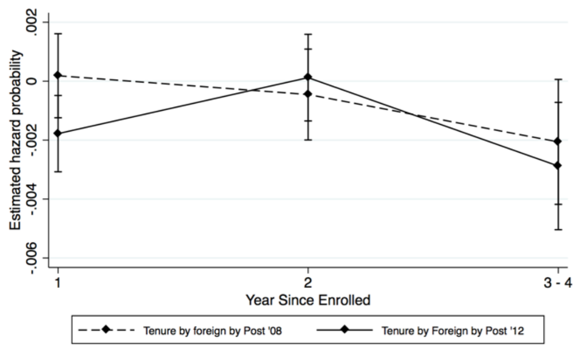
Figure 8. Hazard function and probability of completing traditional STEM degree by year since enrolled for graduate level (PhD degree).

Notes: The figures plot estimates from the specifications in column 4 of Appendix Table V. Data are administrative data on higher education enrollment from the state of Ohio, made available to researchers by the Ohio Education Research Center (OERC). Sample is from year 2000 to 2015 and restricted to first-time degree seekers. The unit of observation is a schooling spell for each student enrolled in a program. The dependent variable is completion in traditional STEM degree for Ph.D. degree.

(a) Bachelor's Degree



(b) Master's Degree



(c) PhD Degree

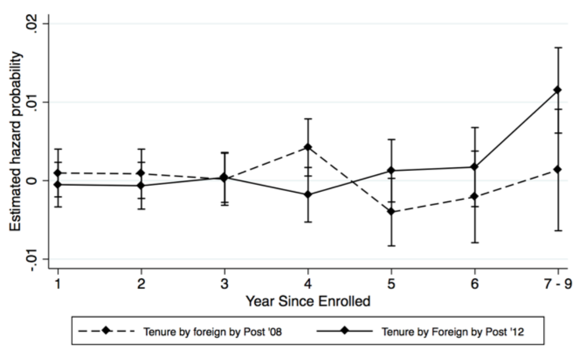


Figure 9. Hazard function for completion of newly added STEM degree of foreign relative to native students by year since enrolled by program level.

Notes: The figures plot estimates from the specifications in column 4 of Appendix Table VI, VII, and VIII.. Data are administrative data on higher education enrollment from the state of Ohio, made available to researchers by the Ohio Education Research Center (OERC). Sample is from year 2000 to 2015 and restricted to first-time degree seekers. The unit of observation is a schooling spell for each student enrolled in a program. The dependent variable is completion in newly added STEM degree for different level of programs.

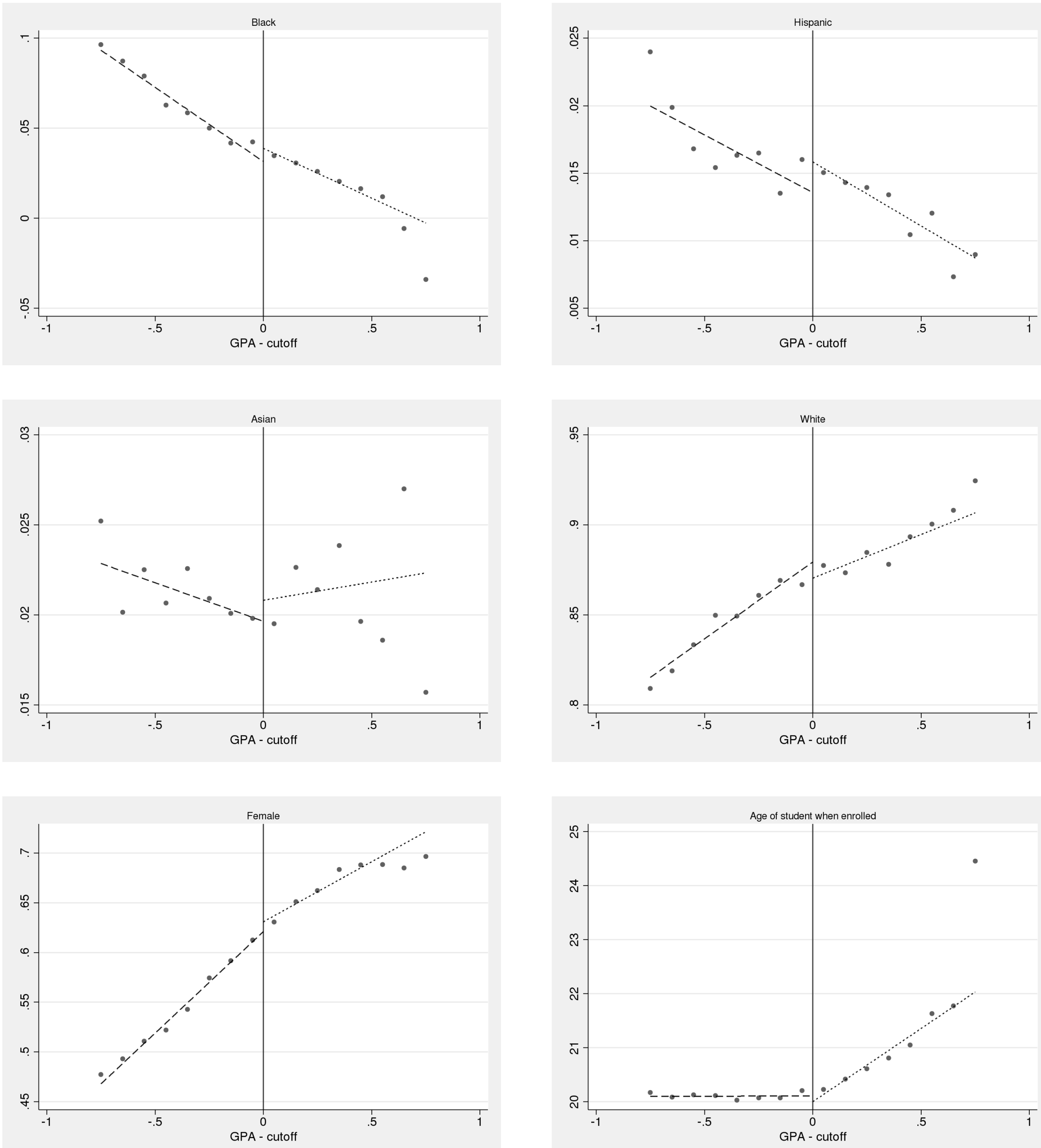
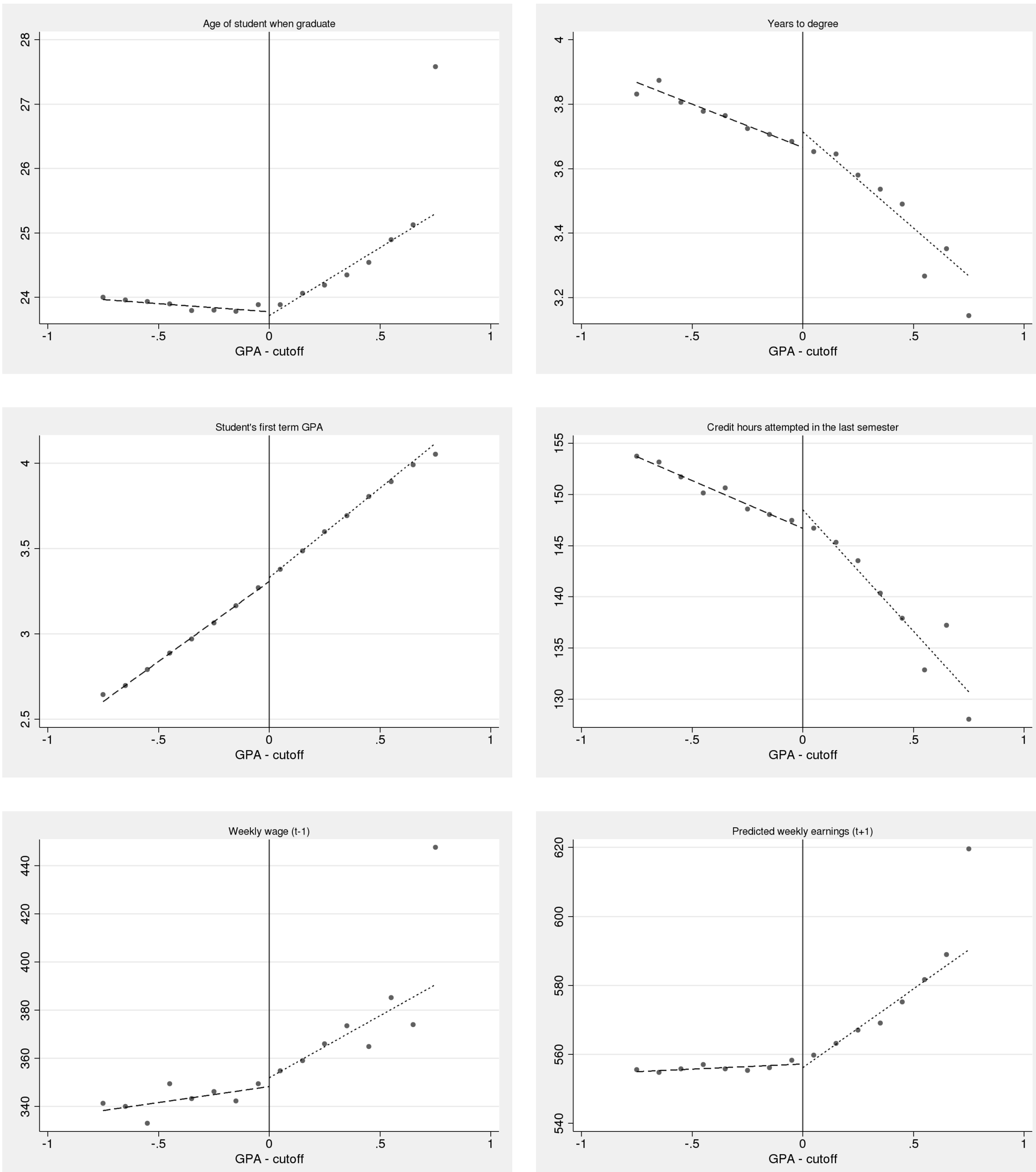


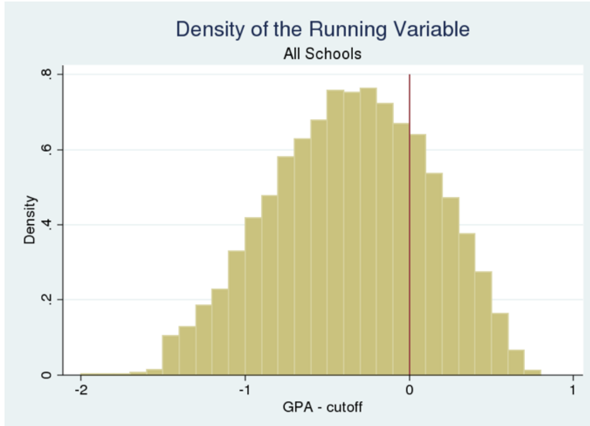
Figure 10. Tests of covariate balance for all schools.

[Continued from Figure 10]

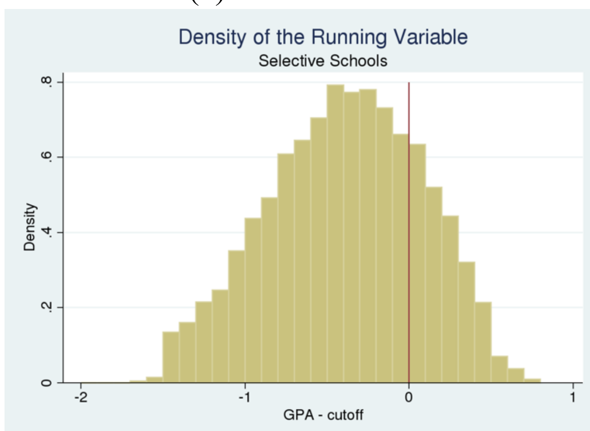


Notes: These figures provide evidence on whether a variety of covariates are smooth through the threshold. As in the main specification, the running variable is cumulative GPA minus the cum laude threshold and the outcome variable plots are conditional on school-major-year fixed effects. All figures show data for a fixed bandwidth of 0.75, and the superimposed lines are based on estimating equation (2) with no controls where the outcome is various student characteristics conditional on school-major-year fixed effects.

(a) All Schools



(b) Selective Schools



(c) Non-Selective Schools

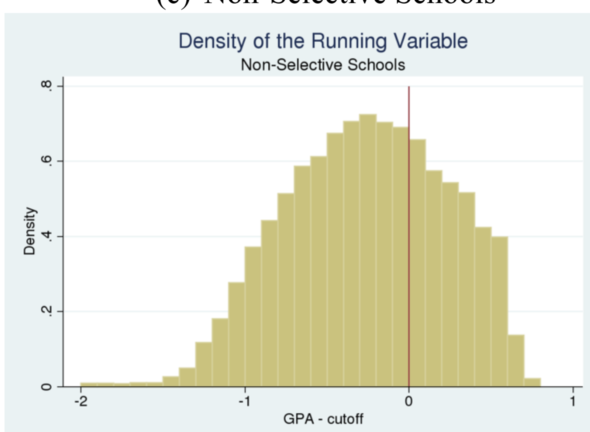
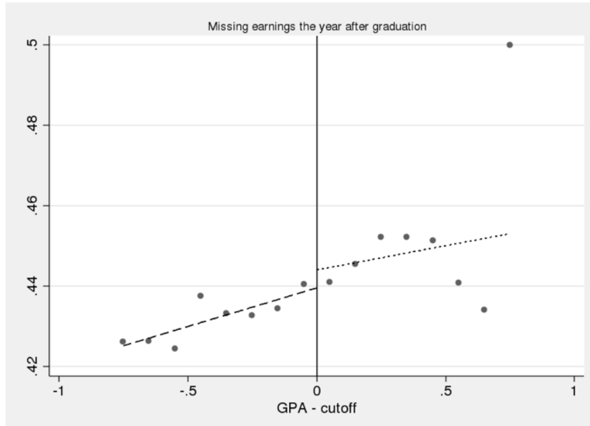


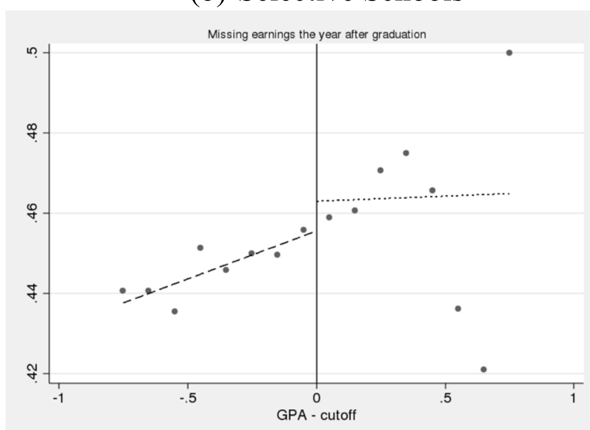
Figure 11. Density for the running variable for all schools.

Notes: Subfigure (a) depicts the density of the running variable (cumulative GPA minus the cum laude threshold) overall. Subfigures (b) and (c) show the same density split by selectivity.

(a) All Schools



(b) Selective Schools



(c) Non-Selective Schools

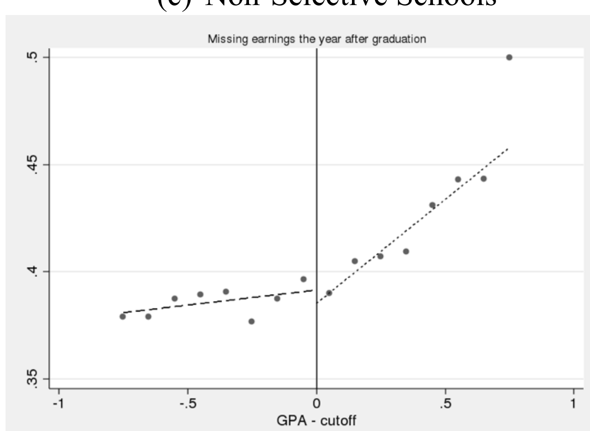
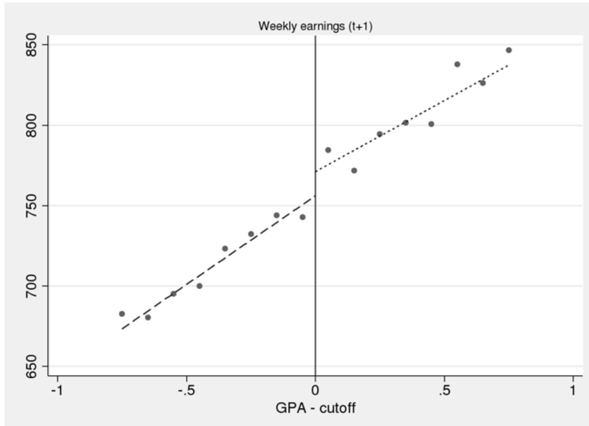


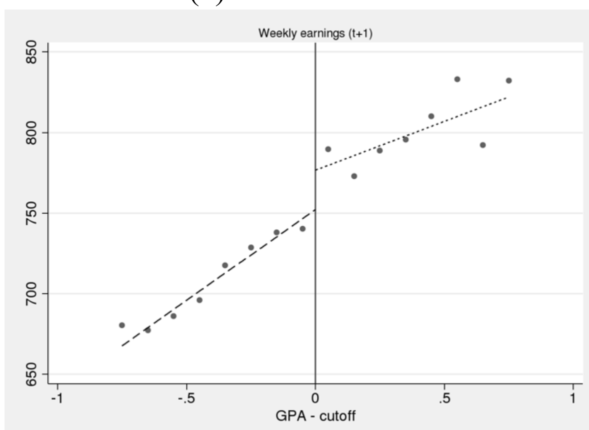
Figure 12. Discontinuity in probability of missing earnings in the year after graduation.

Notes: Subfigure (a) depicts the probability of missing earnings in the year following graduation for all schools and subfigures (b) and (c) split according to school selectivity. The running variable is cumulative GPA minus the cum laude threshold and the outcome variable plots are conditional on school-major-year fixed effects. All figures show data for a fixed bandwidth of 0.75.

(a) All Schools



(b) Selective Schools



(c) Non-Selective Schools

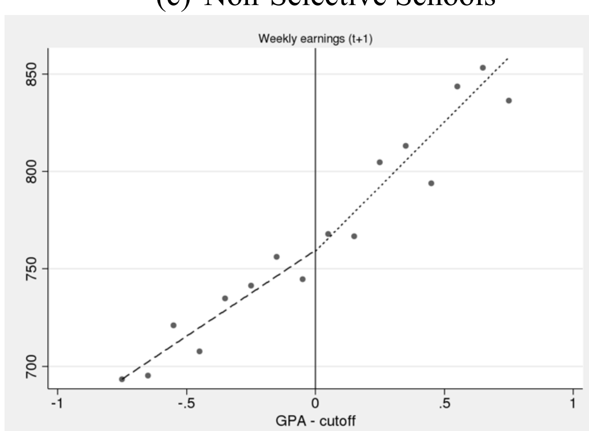
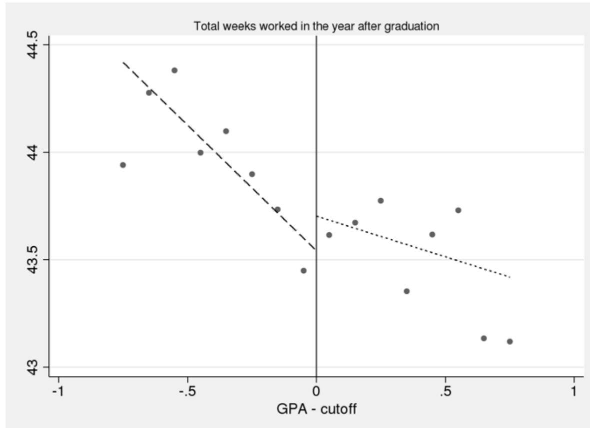


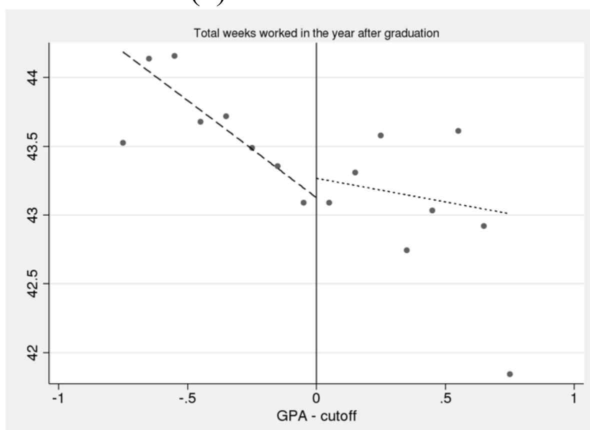
Figure 13. Discontinuity in average weekly earnings the year after graduation.

Notes: Subfigure (a) depicts weekly earnings in the year following graduation for all schools and subfigures (b) and (c) split according to school selectivity. See notes on Figure 12 for data restrictions and the superimposed lines. The coefficients corresponding to the 0.75 bandwidth are shown in the rightmost column of Table XII.

(a) All Schools



(b) Selective Schools



(c) Non-Selective Schools

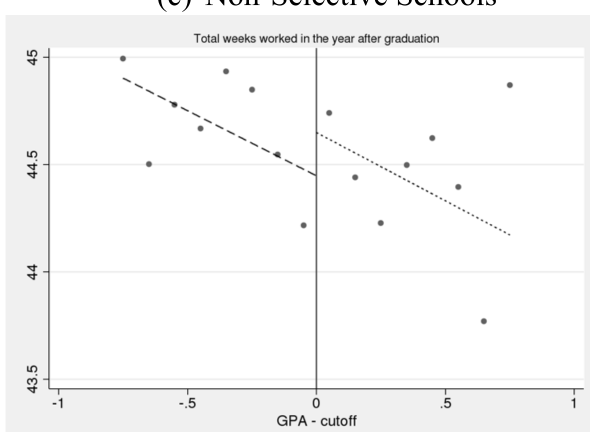
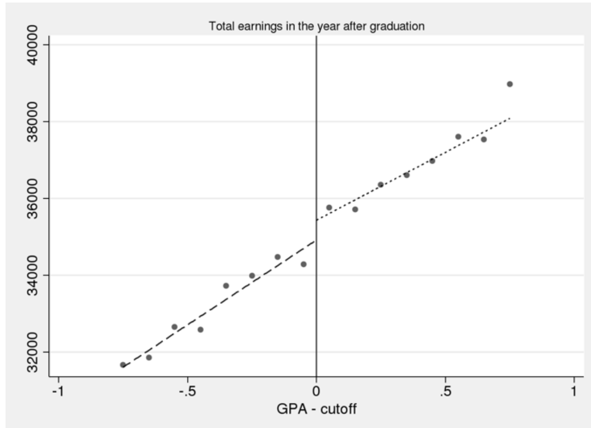


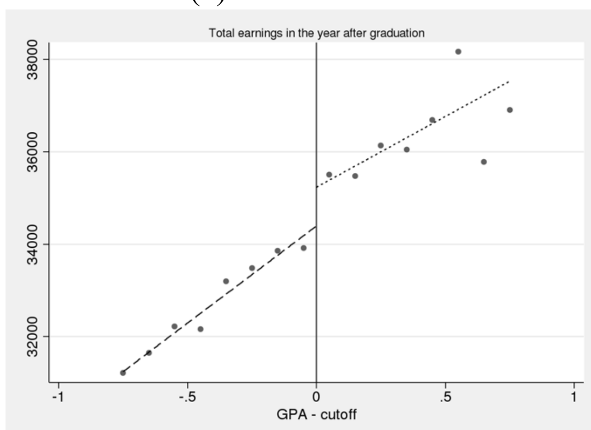
Figure 14. Discontinuity in total weeks worked the year after graduation.

Notes: Subfigure (a) depicts weeks worked in the year following graduation for all schools and subfigures (b) and (c) split according to school selectivity. See notes on Figure 12 for data restrictions and the superimposed lines. The coefficients corresponding to the 0.75 bandwidth are shown in the rightmost column of Table XIII.

(a) All Schools



(b) Selective Schools



(c) Non-Selective Schools

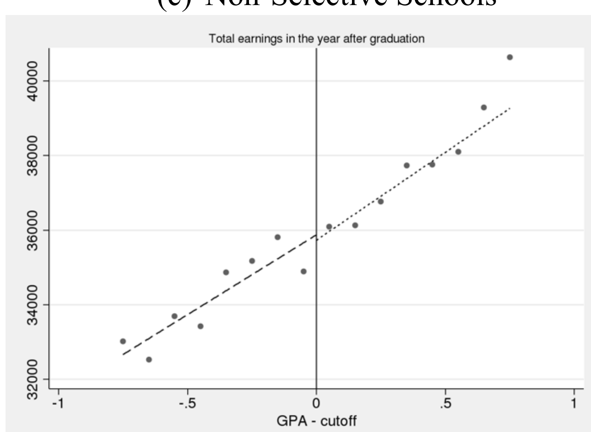
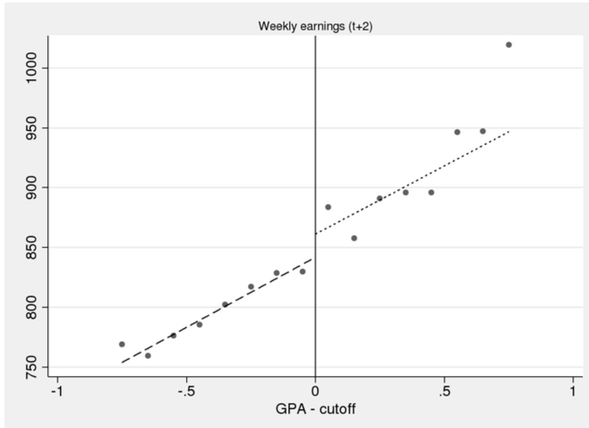


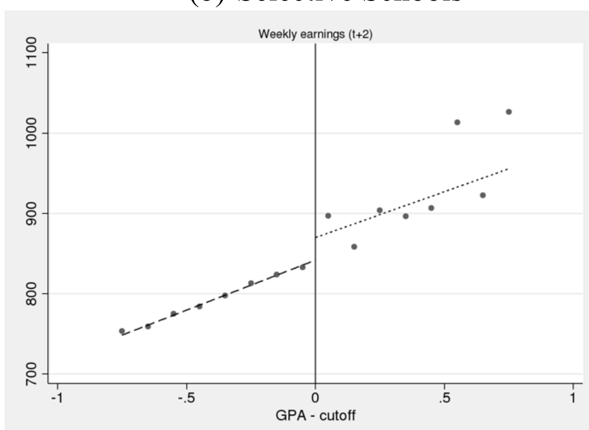
Figure 15. Discontinuity in total earnings the year after graduation.

Notes: Subfigure (a) depicts total earnings in the year following graduation for all schools and subfigures (b) and (c) split according to school selectivity. See notes on Figure 12 for data restrictions and the superimposed lines. The coefficients corresponding to the 0.75 bandwidth are shown in the rightmost column of Table XIV.

(a) All Schools



(b) Selective Schools



(c) Non-Selective Schools

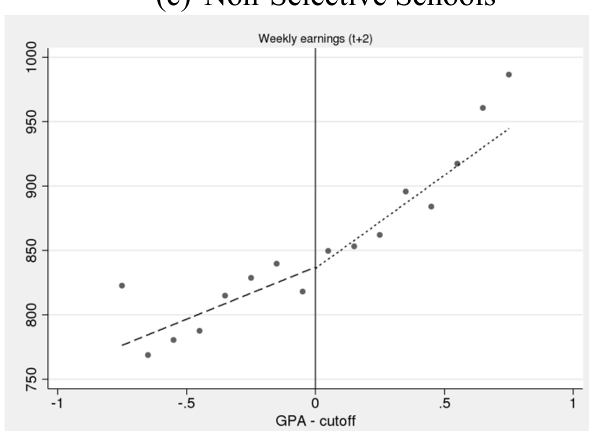
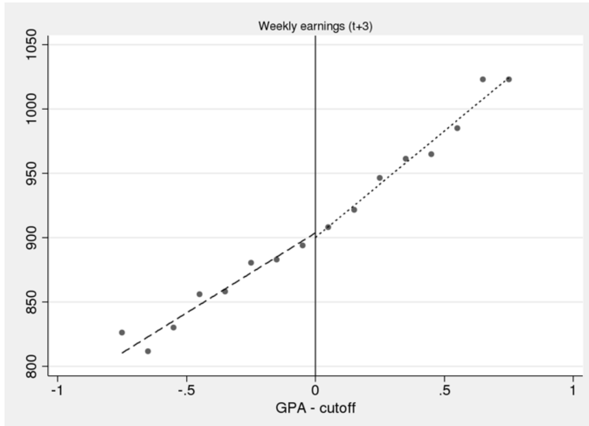


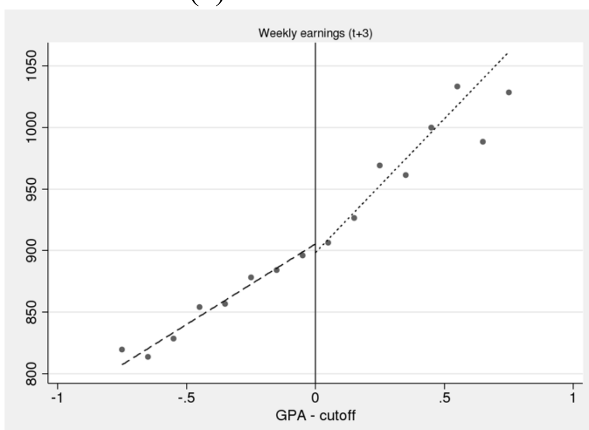
Figure 16. Discontinuity in average earnings two years after graduation.

Notes: Subfigure (a) depicts weekly earnings in the second year following graduation for all schools and subfigures (b) and (c) split according to school selectivity. See notes on Figure 12 for data restrictions and the superimposed lines. The coefficients corresponding to the 0.75 bandwidth are shown in the rightmost column of Table XV.

(a) All Schools



(b) Selective Schools



(c) Non-Selective Schools

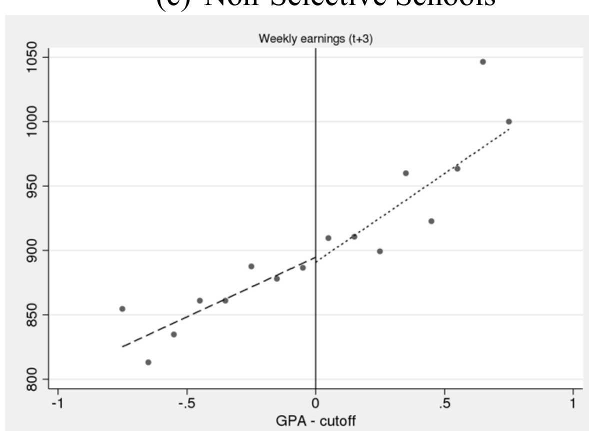


Figure 17. Discontinuity in average weekly earnings three year after graduation.

Notes: Subfigure (a) depicts weekly earnings in the third year following graduation for all schools and subfigures (b) and (c) split according to school selectivity. See notes on Figure 12 for data restrictions and the superimposed lines. The coefficients corresponding to the 0.75 bandwidth are shown in the rightmost column of Table XVI.

Tables

TABLE I. DESCRIPTIVE STATISTICS OF THE REGRESSION SAMPLE

	Observations	Mean	S.D.
Female	1,206	0.49	(0.49)
Age of Enrollment	1,206	27.55	(4.49)
Size of cohort	1,206	14.05	(9.89)
Total international students in cohort	1,206	9.21	(5.98)
Total number of natives in cohort	1,206	4.84	(7.45)
Country of origin share [t]	1,206	0.20	(0.22)
Number of peers	1,206	2.20	(2.52)
Any peers	1,206	0.67	(0.47)
Persistence into year 2	1,206	0.85	(0.36)
Persistence into year 3	1,027	0.73	(0.45)
Graduated or still enrolled in year 4	825	0.68	(0.47)
Graduated or still enrolled in year 5	562	0.62	(0.49)
Graduated or still enrolled in year 6	367	0.52	(0.50)
Graduated or still enrolled in year 7	210	0.44	(0.50)
Share of students from region:			
East Asia & Pacific	1,206	0.57	(0.50)
Europe & Central Asia	1,206	0.07	(0.26)
Latin America & Caribbean	1,206	0.04	(0.19)
Middle East & North Africa	1,206	0.11	(0.31)
North America	1,206	0.01	(0.12)
South Asia	1,206	0.19	(0.39)
Sub-Saharan Africa	1,206	0.02	(0.13)
Share of students in field:			
Natural Sciences	1,206	0.64	(0.48)
Social Sciences	1,206	0.20	(0.40)
Business Administration/Management	1,206	0.05	(0.22)
Humanities/Fine Arts	1,206	0.11	(0.31)

Notes: Data are administrative data on doctoral program enrollment from the state of Ohio, made available to researchers by the Ohio Education Research Center (OERC). Sample is restricted from year 2009 to 2015. The unit of observation is cohort-by-department, where a cohort defined as individuals entering a doctoral program in a specific school in the same year and a department refers to unique program-school combination.

TABLE II. EFFECT OF PROGRAM'S COUNTRY OF ORIGIN COMPOSITION ON PERSISTENCE

Persistence into year 2	0.0182	-0.03	-0.105**	-0.170**	-0.161**	-0.215***	-0.288***	-0.243***
	(0.0508)	(0.0539)	(0.0515)	(0.0722)	(0.0694)	(0.0817)	(0.0931)	(0.0923)
Observations	1,206	1,206	1,206	1,206	1,206	1,206	1,206	1,206
R-squared	0	0.024	0.191	0.248	0.253	0.409	0.431	0.446
Persistence into year 3	0.0422	-0.00242	-0.0623	-0.138	-0.126	-0.199	-0.259*	-0.233
	(0.0812)	(0.0865)	(0.0835)	(0.0953)	(0.0948)	(0.123)	(0.15)	(0.203)
Observations	1,027	1,027	1,027	1,027	1,027	1,027	1,027	1,027
R-squared	0	0.024	0.22	0.279	0.28	0.454	0.468	0.476
Persist or graduate in year 4	0.108	0.0439	-0.0882	-0.175*	-0.16	-0.369***	-0.256	-0.281
	(0.0958)	(0.101)	(0.0887)	(0.104)	(0.102)	(0.132)	(0.186)	(0.216)
Observations	825	825	825	825	825	825	825	825
R-squared	0.003	0.033	0.22	0.293	0.296	0.47	0.491	0.506
Persist or graduate in year 5	0.123	0.0675	-0.0108	-0.113	-0.0894	-0.437**	-0.0654	
	(0.11)	(0.11)	(0.106)	(0.164)	(0.162)	(0.175)	(0.438)	
Observations	562	562	562	562	562	562	562	
R-squared	0.003	0.034	0.229	0.323	0.326	0.492	0.518	
Persist or graduate in year 6	-0.13	-0.224*	-0.25	-0.135	-0.191	-0.399	-0.799	
	(0.114)	(0.129)	(0.187)	(0.307)	(0.288)	(0.42)	(0.828)	
Observations	367	367	367	367	367	367	367	
R-squared	0.002	0.02	0.203	0.323	0.327	0.473	0.504	
Control		Yes	Yes	Yes	Yes	Yes	Yes	Yes
Department FE			Yes	Yes	Yes			
Country FE				Yes	Yes			
Cohort FE					Yes	Yes	Yes	Yes
Department-by-country FE						Yes	Yes	Yes
Field-by-cohort FE							Yes	Yes
Department-by-cohort FE								Yes

Notes: The unit of observation is cohort-by-department. Each row is a separate regression with different outcomes. Controls included in the regressions are individual demographics (gender and age of enrollment), size of cohort as well as country demographics (unemployment rate and GDP growth). Standard errors, in parentheses, are clustered at the department level. ** *$p < 0.05$*

TABLE III. OTHER MEASURES OF PEER EFFECTS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Country of Origin share	0.0182	-0.03	-0.105**	-0.170**	-0.161**	-0.215***	-0.288***	-0.243***
	(0.0508)	(0.0539)	(0.0515)	(0.0722)	(0.0694)	(0.0817)	(0.0932)	(0.0924)
Number of peers	-0.00594	-0.00707	-0.00978***	-0.0141**	-0.0131**	-0.0193**	-0.0193**	-0.0173**
	(0.0048)	(0.0043)	(0.0032)	(0.0057)	(0.0051)	(0.0084)	(0.0088)	(0.0072)
Any peers	-0.0366	-0.0498**	-0.0419*	-0.0578*	-0.0592*	-0.0454	-0.0654	-0.0453
	(0.0229)	(0.0237)	(0.0248)	(0.0328)	(0.0317)	(0.0449)	(0.046)	(0.0397)
Control		Yes	Yes	Yes	Yes	Yes	Yes	Yes
Department FE			Yes	Yes	Yes			
Country FE				Yes	Yes			
Cohort FE					Yes	Yes	Yes	Yes
Department-by-country FE						Yes	Yes	Yes
Field-by-cohort FE							Yes	Yes
Department-by-cohort FE								Yes
Observations	1,206	1,206	1,206	1,206	1,206	1,206	1,206	1,206

Notes: The unit of observation is cohort-by-department. The dependent variable is an indicator variable for persisting into the second year of the program. Each row is a separate regression with the respective independent variable listed. Controls included in the regressions are individual demographics (gender and age of enrollment), size of cohort as well as country demographics (unemployment rate and GDP growth). Standard errors, in parentheses, are clustered at the department level. ** $p < 0.05$

TABLE IV. EFFECT OF NUMBER OF PEERS FROM SAME COUNTRY OF ORIGIN

	(1)	(2)	(3)
	All	Large cohort	Small cohort
Number of peers	-0.0193**	-0.0198**	-0.0339**
	(0.0084)	(0.00883)	(0.0167)
Observations	1,206	873	333
R-squared	0.408	0.311	0.713

Notes: The unit of observation is cohort-by-department. The dependent variable is an indicator variable for persisting into the second year of the program. All regressions include controls, cohort fixed effects, and department-by-country fixed effects. Controls used are individual demographics (gender and age of enrollment), size of cohort, as well as country demographics (unemployment rate and GDP growth). To categorize departments to departments with large or small cohort, each of department's average cohort size is compared to the median all the department's averages (mean of cohort size over time for each department). A large cohort are departments that have a higher average compared to the median of all the departments' averages. Standard errors, in parentheses, are clustered at the department level. ** $p < 0.05$

TABLE V. HETEROGENOUS EFFECT OF COUNTRY OF ORIGIN SHARE BY STEM

	(1)	(2)	(3)
	All	STEM	Non-STEM
Country of origin share	-0.215***	-0.235***	-0.135
	(0.0817)	(0.0570)	(0.128)
Observations	1,206	710	496
R-squared	0.409	0.293	0.564

Notes: *Data are administrative data on doctoral program enrollment from the state of Ohio, made available to researchers by the Ohio Education Research Center (OERC). Sample is restricted from year 2009 to 2015. The unit of observation is cohort-by-department. The dependent variable is an indicator variable for persisting into the second year of the program. All regressions include controls, cohort fixed effects, and department-by-country fixed effects. Controls used are individual demographics (gender and age of enrollment), size of cohort, as well as country demographics (unemployment rate and GDP growth). Standard errors, in parentheses, are clustered at the department level.* $^{**}p < 0.05$

TABLE VI. EFFECT OF COUNTRY OF ORIGIN SHARE CONDITIONAL ON OTHER SHARES

	(1)	(2)	(3)	(4)	(5)	(6)
Country of origin share [t]	-0.215***	-0.195	-0.228**	-0.197	-0.189*	-0.201*
	(0.0817)	(0.125)	(0.109)	(0.157)	(0.0974)	(0.109)
Other international student share		0.0241				
		(0.146)				
Region share			0.0204			
			(0.136)			
Language share				-0.0158		
				(0.119)		
Country of origin share [t-1]						-0.0469
						(0.113)
Observations	1,206	1,206	1,206	1,206	996	996
R-squared	0.409	0.409	0.409	0.409	0.418	0.418

Notes: Data are administrative data on doctoral program enrollment from the state of Ohio, made available to researchers by the Ohio Education Research Center (OERC). Sample is restricted from year 2009 to 2015. The unit of observation is cohort-by-department. The dependent variable is an indicator variable for persisting into the second year of the program. All regressions include controls, cohort fixed effects, and department-by-country fixed effects. Controls used are individual demographics (gender and age of enrollment), size of cohort, as well as country demographics (unemployment rate and GDP growth. The dependent variable is persistence into the second year in the doctoral program. The sample size is smaller in column (6) as cohorts who entered in year 2009 would not have second year students as peers since the data starts in year 2009. In column 5, I restricted the overall sample to be the same sample size as column 6 for easier comparison. Standard errors, in parentheses, are clustered at the department level. $^{**} p < 0.05$

TABLE VII. SUMMARY STATISTIC BY DEGREE LEVEL

	All	Foreign	Native
Panel A: *Bachelors Degree*	0.75	0.33	0.78
Traditional STEM fields			
Share entered program	0.22	0.33	0.21
Female	0.32	0.27	0.32
Age of enrollment	18.7	19.7	18.7
Share completed degree	0.32	0.27	0.32
Years to complete degree	4.71	4.35	4.72
Newly added STEM fields			
Share entered program	0.015	0.017	0.015
Female	0.38	0.32	0.38
Age of enrollment	19.8	21.6	19.8
Share completed degree	0.0032	0.003	0.0032
Years to complete degree	4.49	4.46	4.49
Observations	873135	25113	848022
Panel B: *Master Degree*	0.21	0.58	0.18
Traditional STEM fields			
Share entered program	0.19	0.55	0.12
Female	0.32	0.3	0.33
Age of enrollment	25	23.8	26.5
Share completed degree	0.06	0.19	0.032
Years to complete degree	2.07	1.93	2.26
Newly Added STEM fields			
Share entered program	0.016	0.018	0.015
Female	0.57	0.52	0.57
Age of enrollment	28.6	27.1	28.9
Share completed degree	0.0051	0.0053	0.0051
Years to complete degree	2.27	2.18	2.29
Observations	244488	43736	200752

TABLE VII: SUMMARY STATISTIC BY DEGREE LEVEL - CONTINUED

	All	Foreign	Native
Panel C: *Doctoral Degree*	0.014	0.054	0.037
Traditional STEM fields			
Share entered program	0.11	0.54	0.037
Female	0.34	0.31	0.41
Age of enrollment	27.8	27.5	28.7
Share completed degree	0.044	0.22	0.014
Years to complete degree	4.81	4.78	4.88
Newly Added STEM fields			
Share entered program	0.014	0.054	0.0075
Female	0.48	0.46	0.55
Age of enrollment	28.5	28	29.9
Share completed degree	0.0043	0.027	0.0012
Years to complete degree	5.09	5.2	4.74
Observations	47901	7167	40734

Notes: Data are administrative data on higher education enrollment from the state of Ohio, made available to researchers by the Ohio Education Research Center (OERC). Sample is from year 2000 to 2015 and restricted to first-time degree seekers. The sample for summary statistics is a person by program level data.

TABLE VIII. ESTIMATES OF THE EFFECT OF OPT EXTENSION ON ENROLLMENT IN TRADITIONAL STEM FOR DIFFERENT LEVEL OF PROGRAMS

	(1)	(2)	(3)	(4)	(5)	(6)
Variables	Bachelor's Degree		Master's Degree		Doctoral Degree	
Foreign student	0.0226***	0.0226***	0.00211	0.0019	-0.00735	-0.00746
	(0.0086)	(0.00865)	(0.0071)	(0.0071)	(0.0075)	(0.0075)
Post '08	0.0127***	0.0132***	0.00175	0.00259	-0.00741	-0.00345
	(0.0028)	(0.0031)	(0.0018)	(0.0020)	(0.0048)	(0.0046)
Foreign by post '08	0.0137**	0.0176***	0.00513	0.00337	0.00789	0.00631
	(0.0065)	(0.0056)	(0.0038)	(0.0037)	(0.0055)	(0.0065)
Post '12		0.00178		0.00104		0.00770
		(0.0017)		(0.0013)		(0.0050)
Foreign by post '12		-0.00664		0.00309		0.00327
		(0.0083)		(0.0039)		(0.0064)
Observations	778,007	778,007	232,982	232,982	14,975	14,975

*Notes: Data are administrative data on higher education enrollment from the state of Ohio, made available to researchers by the Ohio Education Research Center (OERC). Sample is from year 2000 to 2015 and restricted to first-time degree seekers. The unit of observation is person by program. The dependent variable is an indicator variable for enrolling in traditional STEM fields. Controls included in the regressions are individual demographics fixed effects (gender and age of enrollment) as well as the H-1B cap change in 2004. All regressions also include field fixed effects, field-by-year fixed effects, school fixed effects, and school-by-year fixed effects. Standard errors, in parentheses, are clustered at the department level. ** $p < 0.05$*

TABLE IX. ESTIMATES OF THE EFFECT OF OPT EXTENSION ON ENROLLMENT IN NEWLY-ADDED STEM FOR DIFFERENT LEVEL OF PROGRAMS

	(1)	(2)	(3)	(4)	(5)	(6)
Variables	Bachelor's Degree		Master's Degree		Doctoral Degree	
Foreign student	-0.00338	-0.00335	0.00967*	0.00980*	0.0187*	0.0192*
	(0.0029)	(0.0029)	(0.0051)	(0.0052)	(0.0099)	(0.0101)
Post '08	0.00226*	0.00184	-0.00172	-0.00181	0.00118	-0.000213
	(0.0013)	(0.0012)	(0.0013)	(0.0012)	(0.0038)	(0.0045)
Foreign by post '08	-0.00222	-0.0011	-0.00991**	-0.00868*	-0.0182*	-0.0152*
	(0.0022)	(0.0024)	(0.0049)	(0.0045)	(0.0102)	(0.00811)
Post '12		-0.000938		0.000785		0.00272
		(0.0007)		(0.0015)		(0.0043)
Foreign by post '12		-0.00181		-0.00222		-0.00588
		(0.0019)		(0.0040)		(0.0068)
Observations	778,007	778,007	232,982	232,982	14,975	14,975

Notes: Data are administrative data on higher education enrollment from the state of Ohio, made available to researchers by the Ohio Education Research Center (OERC). Sample is from year 2000 to 2015 and restricted to first-time degree seekers. The unit of observation is person by program. The dependent variable is an indicator variable for enrolling in newly added STEM fields. Controls included in the regressions are individual demographics fixed effects (gender and age of enrollment) as well as the H-1B cap change in 2004. All regressions also include field fixed effects, field-by-year fixed effects, school fixed effects, and school-by-year fixed effects. Standard errors, in parentheses, are clustered at the department level. ** $p < 0.05$

TABLE X. ESTIMATES OF THE EFFECT OF OPT EXTENSION ON THE FIRST TERM GPA AMONG STUDENTS IN TRADITIONAL STEM FOR DIFFERENT LEVEL OF PROGRAM

VARIABLES	Bachelor's Degree	Master's Degree	PhD Degree
DID estimates	-0.00945	-0.0232**	0.00359
	(0.0137)	(0.0091)	(0.0197)
Observations	808,117	101,989	38,213

*Notes: Data are administrative data on higher education enrollment from the state of Ohio, made available to researchers by the Ohio Education Research Center (OERC). Sample is from year 2000 to 2015 and restricted to students enrolling in traditional STEM fields. The unit of observation is person by program. The dependent variable is first term GPA. Controls included in the regressions are individual demographics fixed effects (gender and age of enrollment) as well as the H-1B cap change in 2004. All regressions also include field fixed effects, field-by-year fixed effects, school fixed effects, and school-by-year fixed effects. Standard errors, in parentheses, are clustered at the department level. ** $p < 0.05$*

TABLE XI. SUMMARY STATISTICS BY HONOR STATUS

	All Mean (sd)	Honors Mean (sd)	No Honors Mean (sd)
Black	0.064	0.028	0.078
	(0.24)	(0.17)	(0.27)
Hispanic	0.018	0.015	0.019
	(0.13)	(0.12)	(0.14)
Asian	0.027	0.027	0.027
	(0.15)	(0.16)	(0.16)
White	0.82	0.85	0.80
	(0.36)	(0.36)	(0.40)
Female	0.54	0.64	0.50
	(0.50)	(0.48)	(0.50)
Age of student when enrolled	20.4	20.8	20.2
	(4.41)	(5.46)	(3.90)
Age of student when graduate	24.4	24.7	24.3
	(4.85)	(5.82)	(4.40)
Years to degree	4.04	3.87	4.10
	(2.71)	(2.63)	(2.73)
Majored in STEM	0.36	0.40	0.35
	(0.48)	(0.49)	(0.48)
Student's first term GPA	3.02	3.59	2.82
	(0.67)	(0.44)	(0.62)
Enrollment in postgraduate schools	0.039	0.065	0.030
	(0.19)	(0.25)	(0.17)
Total credit hours	150.7	140.9	154.2
	(47.5)	(46.5)	(47.4)
Weekly earnings the year after graduation	709.0	760.7	691.9
	(517.4)	(715.3)	(431.2)
Missing earnings the year after graduation	0.44	0.45	0.43
	(0.50)	(0.50)	(0.50)
Observations	309699	81185	228514

Notes: Data are administrative data on higher education enrollment from the state of Ohio, made available to researchers by the Ohio Education Research Center (OERC). Sample is from year 1999 to 2011 and restricted to graduates of public 4-year institution. Sample sizes listed at the bottom of the table are for the total number of students, but some outcomes (e.g. earnings) have smaller sample sizes due to missing data. The "Honors" column includes all students with GPAs above the cum laude threshold and the "No Honors" column includes all students with GPAs below the threshold.

TABLE XII. ESTIMATES FOR WEEKLY EARNINGS THE YEAR AFTER GRADUATION WITH DIFFERENT BANDWIDTHS

	Weekly earnings (t+1)							
All	IK Bandwidth		Bandwidths					
	0.6114	0.6114	0.25	0.25	0.50	0.50	0.75	0.75
Above Cutoff	26.28***	25.02**	31.90*	33.01*	28.02**	27.49**	23.90***	22.29**
	(10.09)	(9.94)	(18.46)	(18.26)	(11.35)	(11.19)	(9.117)	(8.982)
Observations	44,849	44,849	20,364	20,364	38,389	38,389	51,399	51,399
Control	NO	YES	NO	YES	NO	YES	NO	YES
Selective	IK Bandwidth		Bandwidths					
	0.6810	0.6810	0.25	0.25	0.50	0.50	0.75	0.75
Above Cutoff	36.51***	30.28**	37.35	42.01*	37.80**	36.36**	36.51***	30.29**
	(12.07)	(11.91)	(25.21)	(24.97)	(15.18)	(15.00)	(12.08)	(11.93)
Observations	35,234	35,234	14,049	14,049	26,309	26,309	35,184	35,184
Control	NO	YES	NO	YES	NO	YES	NO	YES
Non-Selective	IK Bandwidth		Bandwidths					
	0.3808	0.3808	0.25	0.25	0.50	0.50	0.75	0.75
Above Cutoff	8.386	8.605	21.65	19.86	9.106	9.184	5.176	7.302
	(15.22)	(14.82)	(19.66)	(19.13)	(14.46)	(14.07)	(12.7)	(12.38)
Observations	10,862	10,862	6,315	6,315	12,080	12,080	16,215	16,215
Control	NO	YES	NO	YES	NO	YES	NO	YES

Notes: Data are administrative data on higher education enrollment from the state of Ohio, made available to researchers by the Ohio Education Research Center (OERC). Sample is from year 1999 to 2011 and restricted to graduates of public 4-year institution. The dependent variable is weekly earnings in the year following graduation. Reported coefficients are based on estimating equation (2) from the text using local linear regression with a triangular kernel and bandwidths that vary according to column. To maintain a consistent sample across specifications, the analysis is restricted to individuals with non-missing earnings in the first three years after graduation. Figures 13 (a) – 13 (c) show the corresponding visual evidence using the 0.75 bandwidth from the rightmost column of this table. Controls refer to age, sex and race indicators. All specifications include school-by-major-by-graduation-year fixed effects. Heteroskedastic-robust standard errors reported in parentheses. ** $p < 0.05$

TABLE XIII. ESTIMATES FOR TOTAL WEEKS WORKED THE YEAR AFTER GRADUATION WITH DIFFERENT BANDWIDTHS

	Total Weeks Worked (t+1)							
All	IK Bandwidth		Bandwidths					
	0.6114	0.6114	0.25	0.25	0.50	0.50	0.75	0.75
Above Cutoff	0.340*	0.368**	0.359	0.390*	0.278*	0.308*	0.18	0.241*
	(0.185)	(0.184)	(0.231)	(0.228)	(0.169)	(0.167)	(0.147)	(0.146)
Observations	31,558	31,558	20,364	20,364	38,389	38,389	51,399	51,399
Control	NO	YES	NO	YES	NO	YES	NO	YES
Selective	IK Bandwidth		Bandwidths					
	0.6810	0.6810	0.25	0.25	0.50	0.50	0.75	0.75
Above Cutoff	0.145	0.144	0.125	0.128	0.149	0.143	0.168	0.165
	(0.215)	(0.213)	(0.284)	(0.281)	(0.209)	(0.207)	(0.184)	(0.183)
Observations	24,950	24,950	14,049	14,049	26,309	26,309	35,184	35,184
Control	NO	YES	NO	YES	NO	YES	NO	YES
Non-Selective	IK Bandwidth		Bandwidths					
	0.3808	0.3808	0.25	0.25	0.50	0.50	0.75	0.75
Above Cutoff	0.909***	0.876***	1.008***	0.947**	0.633**	0.633**	0.326	0.353
	(0.324)	(0.323)	(0.391)	(0.389)	(0.28)	(0.279)	(0.243)	(0.242)
Observations	9,054	9,054	6,315	6,315	12,080	12,080	16,215	16,215
Control	NO	YES	NO	YES	NO	YES	NO	YES

*Notes: Data are administrative data on higher education enrollment from the state of Ohio, made available to researchers by the Ohio Education Research Center (OERC). Sample is from year 1999 to 2011 and restricted to graduates of public 4-year institution. The dependent variable is total weeks worked in the year following graduation. Reported coefficients are based on estimating equation (2) from the text using local linear regression with a triangular kernel and bandwidths that vary according to column. To maintain a consistent sample across specifications, the analysis is restricted to individuals with non-missing earnings in the first three years after graduation. Figures 14 (a) – 14 (c) show the corresponding visual evidence using the 0.75 bandwidth from the rightmost column of this table. Controls refer to age, sex and race indicators. All specifications include school-by-major-by-graduation-year fixed effects. Heteroskedastic-robust standard errors reported in parentheses. ** $p < 0.05$*

TABLE XIV. ESTIMATES FOR TOTAL EARNINGS THE YEAR AFTER GRADUATION WITH DIFFERENT BANDWIDTHS

	Total Earnings (t+1)							
All	IK Bandwidth		Bandwidths					
	0.4080	0.4080	0.25	0.25	0.50	0.50	0.75	0.75
Above Cutoff	1,143**	1,207**	1,062	1,149*	1,035**	1,048**	846.0**	838.8**
	(486.7)	(476)	(681.5)	(668.8)	(428.2)	(418.1)	(345.4)	(336.5)
Observations	32,278	32,278	20,364	20,364	38,389	38,389	51,399	51,399
Control	NO	YES	NO	YES	NO	YES	NO	YES
Selective	IK Bandwidth		Bandwidths					
	0.4911	0.4911	0.25	0.25	0.50	0.50	0.75	0.75
Above Cutoff	1,236**	1,190**	892	1,109	1,235**	1,180**	1,275***	1,017**
	(570.2)	(559)	(915.3)	(901)	(564)	(552.8)	(452.5)	(442.9)
Observations	25,952	25,952	14,049	14,049	26,309	26,309	35,184	35,184
Control	NO	YES	NO	YES	NO	YES	NO	YES
Non-Selective	IK Bandwidth		Bandwidths					
	0.3572	0.3572	0.25	0.25	0.50	0.50	0.75	0.75
Above Cutoff	1,213*	1,127*	1,634**	1,472*	744	748.9	307.8	433.8
	(685.8)	(656.1)	(816.4)	(781)	(585.5)	(559.8)	(500.7)	(478.9)
Observations	8,938	8,938	6,315	6,315	12,080	12,080	16,215	16,215
Control	NO	YES	NO	YES	NO	YES	NO	YES

*Notes: Data are administrative data on higher education enrollment from the state of Ohio, made available to researchers by the Ohio Education Research Center (OERC). Sample is from year 1999 to 2011 and restricted to graduates of public 4-year institution. The dependent variable is total earnings the year following graduation. Reported coefficients are based on estimating equation (2) from the text using local linear regression with a triangular kernel and bandwidths that vary according to column. To maintain a consistent sample across specifications, the analysis is restricted to individuals with non-missing earnings in the first three years after graduation. Figures 15 (a) – 15 (c) show the corresponding visual evidence using the 0.75 bandwidth from the rightmost column of this table. Controls refer to age, sex and race indicators. All specifications include school-by-major-by-graduation-year fixed effects. Heteroskedastic-robust standard errors reported in parentheses. ** $p < 0.05$.*

TABLE XV. ESTIMATES FOR WEEKLY EARNINGS TWO YEARS AFTER GRADUATION WITH DIFFERENT BANDWIDTHS

	Weekly Earnings (t+2)							
All	IK Bandwidth		Bandwidths					
	0.6219	0.6219	0.25	0.25	0.50	0.50	0.75	0.75
Above Cutoff	29.93**	28.32**	36	36.89	32.89**	31.95**	26.97**	24.90**
	(12.98)	(12.87)	(25.27)	(25.12)	(14.97)	(14.85)	(11.7)	(11.59)
Observations	45,409	45,409	20,364	20,364	38,389	38,389	51,399	51,399
Control	NO	YES	NO	YES	NO	YES	NO	YES
Selective	IK Bandwidth		Bandwidths					
	0.6810	0.6810	0.25	0.25	0.50	0.50	0.75	0.75
Above Cutoff	41.00**	36.44**	39.57	45	42.34**	41.02**	40.52**	34.80**
	(17.15)	(17.03)	(35.29)	(35.1)	(20.78)	(20.64)	(16.25)	(16.14)
Observations	32,991	32,991	14,049	14,049	26,309	26,309	35,184	35,184
Control	NO	YES	NO	YES	NO	YES	NO	YES
Non-Selective	IK Bandwidth		Bandwidths					
	0.3808	0.3808	0.25	0.25	0.50	0.50	0.75	0.75
Above Cutoff	19.39	17.88	31.15	27.88	13.32	12.58	1.368	2.987
	(16.91)	(16.45)	(21.4)	(20.86)	(14.83)	(14.42)	(12.63)	(12.28)
Observations	9,516	9,516	6,315	6,315	12,080	12,080	16,215	16,215
Control	NO	YES	NO	YES	NO	YES	NO	YES

Notes: *Data are administrative data on higher education enrollment from the state of Ohio, made available to researchers by the Ohio Education Research Center (OERC). Sample is from year 1999 to 2011 and restricted to graduates of public 4-year institution. The dependent variable is weekly earnings in the second year following graduation. Reported coefficients are based on estimating equation (2) from the text using local linear regression with a triangular kernel and bandwidths that vary according to column. To maintain a consistent sample across specifications, the analysis is restricted to individuals with non-missing earnings in the first three years after graduation. Figures 16 (a) – 16 (c) show the corresponding visual evidence using the 0.75 bandwidth from the rightmost column of this table. Controls refer to age, sex and race indicators. All specifications include school-by-major-by-graduation-year fixed effects. Heteroskedastic-robust standard errors reported in parentheses. ** $p < 0.05$.*

TABLE XVI. ESTIMATES FOR WEEKLY EARNINGS THREE YEARS AFTER GRADUATION WITH DIFFERENT BANDWIDTHS

	Weekly Earnings (t+3)							
All	IK Bandwidth		Bandwidths					
	0.4212	0s.4212	0.25	0.25	0.50	0.50	0.75	0.75
Above Cutoff	2.189	2.63	0	1.759	3.88	3.175	2.623	0.576
	-9.763	-9.534	-12.99	-12.7	-9.012	-8.798	-7.773	-7.593
Observations	33,161	33,161	20,364	20,364	38,389	38,389	51,399	51,399
Control	NO	YES	NO	YES	NO	YES	NO	YES
Selective	IK Bandwidth		Bandwidths					
	0.3043	0.3043	0.25	0.25	0.50	0.50	0.75	0.75
Above Cutoff	-8.598	-5.608	-12.29	-8	2.845	1.651	5.788	-0.141
	-12.64	-12.31	-13.97	-13.61	-10.16	-9.898	-8.828	-8.601
Observations	17,004	17,004	14,049	14,049	26,309	26,309	35,184	35,184
Control	NO	YES	NO	YES	NO	YES	NO	YES
Non-Selective	IK Bandwidth		Bandwidths					
	0.4128	0.4128	0.25	0.25	0.50	0.50	0.75	0.75
Above Cutoff	8.245	8.595	29.9	29.22	4.128	4.292	-7.203	-4.926
	-20.18	-19.74	-28.14	-27.62	-18.19	-17.78	-15.6	-15.26
Observations	10,214	10,214	6,315	6,315	12,080	12,080	16,215	16,215
Control	NO	YES	NO	YES	NO	YES	NO	YES

*Notes: Data are administrative data on higher education enrollment from the state of Ohio, made available to researchers by the Ohio Education Research Center (OERC). Sample is from year 1999 to 2011 and restricted to graduates of public 4-year institution. The dependent variable is weekly earnings in the third year following graduation. Reported coefficients are based on estimating equation (2) from the text using local linear regression with a triangular kernel and bandwidths that vary according to column. To maintain a consistent sample across specifications, the analysis is restricted to individuals with non-missing earnings in the first three years after graduation. Figures 17 (a) – 17 (c) show the corresponding visual evidence using the 0.75 bandwidth from the rightmost column of this table. Controls refer to age, sex and race indicators. All specifications include school-by-major-by-graduation-year fixed effects. Heteroskedastic-robust standard errors reported in parentheses. ** $p < 0.05$*

APPENDIX

APPENDIX TABLE I: LIFE TABLE DESCRIBING THE NUMBER OF YEARS UNTIL COMPLETING TRADITIONAL STEM DEGREE

			Enrolled before 2008				Enrolled after 2008			
	All		Foreign		Native		Foreign		Native	
Years in School	N	Proportion	N	Proportion	N	Proportion	N	Proportion	N	Proportion
Panel A: *Bachelors Degree*										
1	1,778,742	0.000017	11,838	0	833,086	0.0000024	41,292	0	892,526	0.000031
2	1,269,319	0.00032	6,377	0.002	560,796	0.00017	26,919	0.0012	675,227	0.00039
3	1,068,933	0.0014	4,701	0.0089	440,066	0.0011	20,053	0.0096	604,113	0.0012
4	851,968	0.027	3,129	0.049	319,324	0.019	12,580	0.054	516,935	0.03
5 - 7	465,720	0.056	1,519	0.083	147,553	0.045	5,503	0.093	311,145	0.061
Panel B: *Master Degree*										
1	636,196	0.0036	49,337	0.0039	224,433	0.0015	63,620	0.0099	298,806	0.0037
2	284,322	0.027	16,956	0.057	95,823	0.0079	24,576	0.16	146,967	0.013
3 - 4	93,509	0.03	3,219	0.15	34,550	0.013	4,356	0.15	51,384	0.024
Panel C: *Doctoral Degree*										
1	42,757	0	9,061	0	10,136	0	10,440	0	13,120	0
2	33,112	0.00036	6,947	0.00072	7,199	0.00014	8,484	0.00059	10,482	0.000095
3	26,020	0.018	5,335	0.026	4,878	0.0098	7,163	0.026	8,644	0.01
4	17,818	0.025	3,411	0.036	2,631	0.013	5,682	0.04	6,094	0.011
5	11,050	0.05	1,827	0.072	1,337	0.022	3,858	0.076	4,028	0.023
6	5,479	0.069	616	0.099	599	0.03	1,996	0.11	2,268	0.038
7 - 9	4,515	0.05	213	0.12	350	0.031	1,359	0.099	2,593	0.021

Notes: Data are administrative data on higher education enrollment from the state of Ohio, made available to researchers by the Ohio Education Research Center (OERC). Sample is from year 2000 to 2015 and restricted to first-time degree seekers. The unit of observation is a schooling spell for each student enrolled in a program.

APPENDIX TABLE II: LIFE TABLE DESCRIBING THE NUMBER OF YEARS UNTIL COMPLETING NEWLY ADDED STEM DEGREE

			Enrolled before 2008				Enrolled after 2008			
	All		Foreign		Native		Foreign		Native	
Years in School	N	Proportion	N	Proportion	N	Proportion	N	Proportion	N	Proportion
Panel A: *Bachelors Degree*										
1	1,778,742	5.6E-07	11,838	0	833,086	0	41,292	0	892,526	0.0000011
2	1,269,319	0.000013	6,377	0	560,796	0.000029	26,919	0	675,227	0.0000015
3	1,068,933	0.00038	4,701	0.00043	440,066	0.00055	20,053	0.00045	604,113	0.00026
4	851,968	0.0013	3,129	0.00064	319,324	0.00096	12,580	0.0023	516,935	0.0015
5 - 7	465,720	0.0022	1,519	0.0053	147,553	0.0023	5,503	0.0036	311,145	0.0021
Panel B: *Master Degree*										
1	636,196	0.00027	49,337	0.0002	224,433	0.00036	63,620	0.00024	298,806	0.00021
2	284,322	0.0021	16,956	0.0012	95,823	0.0011	24,576	0.0038	146,967	0.0026
3 - 4	93,509	0.0027	3,219	0.0059	34,550	0.0022	4,356	0.0039	51,384	0.0027
Panel C: *Doctoral Degree*										
1	42,757	0	9,061	0	10,136	0	10,440	0	13,120	0
2	33,112	0	6,947	0	7,199	0	8,484	0	10,482	0
3	26,020	0.00081	5,335	0.0011	4,878	0.0012	7,163	0.0007	8,644	0.00046
4	17,818	0.0021	3,411	0.0015	2,631	0.0019	5,682	0.0035	6,094	0.0013
5	11,050	0.0062	1,827	0.013	1,337	0.00075	3,858	0.0098	4,028	0.0017
6	5,479	0.0078	616	0.011	599	0.0017	1,996	0.013	2,268	0.004
7 - 9	4,515	0.0053	213	0.0094	350	0.0029	1,359	0.014	2,593	0.00077

Notes: See notes on Appendix Table I.

APPENDIX TABLE III: ESTIMATES OF THE EFFECT OF OPT EXTENSION ON THE COMPLETION OF TRADITIONAL STEM DEGREE FOR UNDERGRADUATE LEVEL

	(1)		(2)			(3)		(4)	
	Tenure	Tenure by foreign	Tenure	Tenure by foreign		Tenure	Tenure by foreign	Tenure	Tenure by foreign
2	0.0006***	0.0016	0.0005**	0.0014	2	0.0006***	0.0007	0.0005**	0.0005
	(0.0002)	(0.0021)	(0.0002)	(0.0021)		(0.0002)	(0.0021)	(0.0002)	(0.0021)
3	0.0023***	0.0088***	0.0021***	0.0087***	3	0.0023***	0.0068***	0.0021***	0.0066***
	(0.0002)	(0.0022)	(0.0002)	(0.0022)		(0.0002)	(0.0022)	(0.0002)	(0.0022)
4	0.0220***	0.0290***	0.0218***	0.0289***	4	0.0221***	0.0267***	0.0218***	0.0265***
	(0.0002)	(0.0023)	(0.0002)	(0.00231)		(0.0002)	(0.0024)	(0.0002)	(0.0024)
5 - 7	0.0451***	0.0378***	0.0446***	0.0377***	5 - 7	0.0451***	0.0351***	0.0446***	0.0351***
	(0.0003)	(0.0029)	(0.0003)	(0.0029)		(0.0003)	(0.0030)	(0.0003)	(0.0030)
	Tenure by post '08	Tenure by foreign by post '08	Tenure by post '08	Tenure by foreign by post '08		Tenure by post '08	Tenure by foreign by post '08	Tenure by post '08	Tenure by foreign by post '08
1	-0.0093***	-0.0031**	-0.0089***	1.45e-07	1	-0.0094***	0.0012	-0.0090***	0.0036
	(0.0002)	(0.0015)	(0.0003)	(0.0016)		(0.0002)	(0.0032)	(0.0003)	(0.0032)
2	-0.0096***	-0.0034*	-0.0088***	0.0009	2	-0.0098***	0.0025	-0.0089***	0.0054
	(0.0002)	(0.0018)	(0.0003)	(0.0020)		(0.0002)	(0.0034)	(0.0003)	(0.0035)
3	-0.0094***	-0.0030	-0.0087***	-0.0022	3	-0.0096***	0.0042	-0.0088***	0.0038
	(0.0003)	(0.0019)	(0.0003)	(0.0022)		(0.0003)	(0.0035)	(0.0003)	(0.0037)
4	0.0002	-0.0074***	-0.0059***	-0.0119***	4	1.53e-05	0.0006	-0.0060***	-0.0051
	(0.0003)	(0.0021)	(0.0003)	(0.0026)		(0.0003)	(0.0036)	(0.0003)	(0.0039)
5 - 7	0.0085***	-0.0080***	-0.0027***	-0.0151***	5 - 7	0.0083***	0.0010	-0.0028***	-0.0083*
	(0.0003)	(0.0029)	(0.0004)	(0.0038)		(0.0003)	(0.0041)	(0.0004)	(0.0048)
			Tenure by post '12	Tenure by foreign by post '12				Tenure by post '12	Tenure by foreign by post '12
1			-0.0091***	-0.0046***	1			-0.0092***	0.0005
			(0.0003)	(0.0011)				(0.0003)	(0.0019)
2			-0.0094***	-0.0049***	2			-0.0095***	0.0004
			(0.0003)	(0.0013)				(0.0003)	(0.0021)
3			-0.0090***	0.0003	3			-0.0091***	0.0053**
			(0.0003)	(0.0016)				(0.0003)	(0.0023)
4			0.0043***	0.0033	4			0.0042***	0.0081***
			(0.0003)	(0.0021)				(0.0003)	(0.0026)
5 - 7			0.0156***	0.0022	5 - 7			0.0155***	0.0080**
			(0.0004)	(0.0032)				(0.0004)	(0.0036)
School FE			√		School FE			√	
School-by-Year FE			√		School-by-Year FE			√	
School-by-Foreign FE			√		School-by-Foreign FE			√	
School-by-Foreign-Year FE					School-by-Foreign-Year FE			√	
Observations			4,749,363		Observations			4,749,363	

*Notes: The unit of observation is a schooling spell for each student enrolled in a program. The dependent variable is completion in traditional STEM degree. Reported coefficients are based on estimating equation (2) from the text. Standard errors, in parentheses, are clustered at the department level. ** $p < 0.05$*

APPENDIX TABLE IV: ESTIMATES OF THE EFFECT OF OPT EXTENSION ON THE COMPLETION OF TRADITIONAL STEM DEGREE FOR GRADUATE LEVEL (MASTER'S DEGREE)

	(1)		(2)			(3)		(4)	
	Tenure	Tenure by foreign	Tenure	Tenure by foreign		Tenure	Tenure by foreign	Tenure	Tenure by foreign
2	0.0083***	0.0539***	0.0083***	0.0536***	2	0.0084***	0.0512***	0.0083***	0.0508***
	(0.0005)	(0.0014)	(0.0005)	(0.0014)		(0.0005)	(0.0014)	(0.0005)	(0.0014)
3 - 4	0.0136***	0.1460***	0.0135***	0.1460***	3 - 4	0.0138***	0.1410***	0.0137***	0.1410***
	(0.0007)	(0.0024)	(0.0007)	(0.0024)		(0.0007)	(0.0025)	(0.0007)	(0.0025)
	Tenure by post '08	Tenure by foreign by post '08	Tenure by post '08	Tenure by foreign by post '08		Tenure by post '08	Tenure by foreign by post '08	Tenure by post '08	Tenure by foreign by post '08
1	-0.0073***	-0.0330***	-0.0048***	-0.0144***	1	-0.0048***	-0.0320***	-0.0032***	-0.0018
	(0.0005)	(0.0010)	(0.0006)	(0.0011)		(0.0005)	-0.0025	(0.0006)	(0.0026)
2	-0.0046***	0.0597***	-0.0052***	-0.0057***	2	-0.0023***	0.0626***	-0.00360***	0.00953***
	(0.0006)	(0.0014)	(0.0007)	(0.0017)		(0.0007)	(0.0027)	(0.0007)	(0.0028)
3 - 4	0.0006	-0.0414***	-0.0035***	-0.0394***	3 - 4	0.0027***	-0.0341***	-0.00203*	-0.0216***
	(0.0009)	(0.0029)	(0.0011)	(0.0034)		(0.0009)	(0.0038)	(0.0011)	(0.0041)
			Tenure by post '12	Tenure by foreign by post '12				Tenure by post '12	Tenure by foreign by post '12
1			-0.00414***	-0.0268***	1			-0.0026***	-0.0440***
			(0.0006)	(0.0010)				(0.0006)	(0.0023)
2			0.0014**	0.1050***	2			0.0029***	0.0872***
			(0.0007)	(0.0016)				(0.0007)	(0.0026)
3 - 4			0.0081***	-0.0006	3 - 4			0.0094***	-0.0173***
			(0.0010)	(0.0034)				(0.0010)	(0.0041)
School FE				√	School FE				√
School-by-Year FE				√	School-by-Year FE				√
School-by-Foreign FE				√	School-by-Foreign FE				√
School-by-Foreign-Year FE					School-by-Foreign-Year FE				√
Observations				858,095	Observations				858,095

*Notes: See notes on Appendix Table III. ** $p < 0.05$.*

APPENDIX TABLE V: ESTIMATES OF THE EFFECT OF OPT EXTENSION ON THE COMPLETION OF TRADITIONAL STEM DEGREE FOR GRADUATE LEVEL (DOCTORAL DEGREE)

	(1)		(2)			(3)		(4)	
	Tenure	Tenure by foreign	Tenure	Tenure by foreign		Tenure	Tenure by foreign	Tenure	Tenure by foreign
2	-0.0002	0.0009	-0.0002	0.0008	2	-0.0002	0.0008	-0.0002	0.0008
	(0.0023)	(0.0034)	(0.0023)	(0.0034)		(0.0023)	(0.0034)	(0.0023)	(0.0034)
3	0.0106***	0.0128***	0.0106***	0.0128***	3	0.0106***	0.0129***	0.0106***	0.0129***
	(0.0025)	(0.0035)	(0.0025)	(0.0035)		(0.0025)	(0.0036)	(0.0025)	(0.0036)
4	0.0131***	0.0201***	0.0130***	0.0203***	4	0.0132***	0.0190***	0.0131***	0.0190***
	(0.0029)	(0.0040)	(0.0029)	(0.0040)		(0.0029)	(0.0040)	(0.0029)	(0.0040)
5	0.0216***	0.0490***	0.0217***	0.0493***	5	0.0215***	0.0473***	0.0215***	0.0473***
	(0.0037)	(0.0050)	(0.0037)	(0.0050)		(0.0037)	(0.0050)	(0.0037)	(0.0050)
6	0.0297***	0.0696***	0.0301***	0.0698***	6	0.0294***	0.0677***	0.0295***	0.0676***
	(0.0053)	(0.0074)	(0.0053)	(0.0074)		(0.0053)	(0.0075)	(0.0053)	(0.0075)
7 - 9	0.0325***	0.0850***	0.0332***	0.0852***	7 - 9	0.0318***	0.0875***	0.0320***	0.0872***
	(0.0068)	(0.0109)	(0.0068)	(0.0109)		(0.0068)	(0.0110)	(0.0068)	(0.0110)
	Tenure by post '08	Tenure by foreign by post '08	Tenure by post '08	Tenure by foreign by post '08		Tenure by post '08	Tenure by foreign by post '08	Tenure by post '08	Tenure by foreign by post '08
1	-0.0002	-0.0107***	0.0034	-0.0071**	1	-0.0027	0.00321	-1.27e-05	0.0063
	(0.0022)	(0.0030)	(0.0025)	(0.0034)		(0.0024)	(0.0050)	(0.0027)	(0.0053)
2	-0.0003	-0.0107***	0.0031	-0.0063*	2	-0.0028	0.0033	-0.0003	0.0070
	(0.0024)	(0.0032)	(0.0027)	(0.0037)		(0.0025)	(0.0052)	(0.0029)	(0.0055)
3	-0.0018	-0.0062*	-0.0002	-0.0098**	3	-0.0043	0.0069	-0.0036	0.0030
	(0.0026)	(0.0035)	(0.0030)	(0.0041)		(0.0028)	(0.0053)	(0.0032)	(0.0057)
4	-0.0039	0.0005	-0.0036	0.0016	4	-0.0066**	0.0143**	-0.0070*	0.0148**
	(0.0032)	(0.0041)	(0.0036)	(0.0048)		(0.0033)	(0.0057)	(0.0037)	(0.0062)
5	-0.0014	-0.0036	-0.0033	-0.0035	5	-0.0040	0.0109*	-0.0065	0.0100
	(0.0040)	(0.0053)	(0.0046)	(0.0061)		(0.0041)	(0.0066)	(0.0047)	(0.0072)
6	0.0057	-0.0077	-0.0056	-0.0196**	6	0.0033	0.0075	-0.0085	-0.0057
	(0.0057)	(0.0080)	(0.0064)	(0.0089)		(0.0058)	(0.0089)	(0.0065)	(0.0097)
7 - 9	-0.0096	-0.0113	-0.0079	-0.0050	5 - 7	-0.0116	-0.0004	-0.0102	0.0045
	(0.0071)	(0.0115)	(0.0076)	(0.0123)		(0.0071)	(0.0121)	(0.0077)	(0.0129)
			Tenure by post '12	Tenure by foreign by post '12				Tenure by post '12	Tenure by foreign by post '12
1			0.0011	-0.0061*	1			-0.0022	-0.0034
			(0.0025)	(0.0034)				(0.0027)	(0.0050)
2			0.0014	-0.0075**	2			-0.0018	-0.0046
			(0.0027)	(0.0037)				(0.0028)	(0.0052)
3			0.0049*	0.0063	3			0.0017	0.0086
			(0.0029)	(0.0040)				(0.0030)	(0.0054)
4			0.0072**	-0.0020	4			0.0038	0.0010
			(0.0034)	(0.0046)				(0.0035)	(0.0059)
5			0.0108***	-4.52e-05	5			0.0074*	0.0041
			(0.0041)	(0.0056)				(0.0042)	(0.0067)
6			0.0273***	0.0276***	6			0.0238***	0.0325***
			(0.0053)	(0.0076)				(0.0054)	(0.0084)
7 - 9			0.0034	-0.0129	7 - 9			5.02e-06	-0.0080
			(0.0051)	(0.0083)				(0.0052)	(0.0091)
School FE				√	School FE				√
School-by-Year FE				√	School-by-Year FE				√
School-by-Foreign FE				√	School-by-Foreign FE				√
School-by-Foreign-Year FE					School-by-Foreign-Year FE				√
Observations				367,104	Observations				367,104

*Notes: See notes on Appendix Table III. ** $p < 0.05$.*

APPENDIX TABLE VI: ESTIMATES OF THE EFFECT OF OPT EXTENSION ON THE COMPLETION OF NEWLY ADDED STEM DEGREE FOR UNDERGRADUATE LEVEL

	(1)		(2)			(3)		(4)	
	Tenure	Tenure by foreign	Tenure	Tenure by foreign		Tenure	Tenure by foreign	Tenure	Tenure by foreign
2	7.10e-05	0.0001	6.63e-05	9.57e-05	2	7.13e-05	0.0001	6.66e-05	0.0001
	(5.00e-05)	(0.0005)	(5.00e-05)	(0.0005)		(5.00e-05)	(0.0005)	(5.00e-05)	(0.0005)
3	0.0008***	7.25e-05	0.0008***	7.00e-05	3	0.0008***	8.49e-05	0.0008***	7.03e-05
	(5.21e-05)	(0.0005)	(5.21e-05)	(0.0005)		(5.21e-05)	(0.0005)	(5.21e-05)	(0.0005)
4	0.0012***	-0.0005	0.0012***	-0.0005	4	0.0012***	-0.0005	0.0012***	-0.0005
	(5.48e-05)	(0.0005)	(5.48e-05)	(0.0005)		(5.48e-05)	(0.0005)	(5.48e-05)	(0.0005)
5 - 7	0.0024***	0.0023***	0.0024***	0.0023***	5 - 7	0.0024***	0.0022***	0.0024***	0.0022***
	(6.87e-05)	(0.0007)	(6.88e-05)	(0.0007)		(6.87e-05)	(0.0007)	(6.88e-05)	(0.0007)
	Tenure by post '08	Tenure by foreign by post '08	Tenure by post '08	Tenure by foreign by post '08		Tenure by post '08	Tenure by foreign by post '08	Tenure by post '08	Tenure by foreign by post '08
1	-0.0002***	-0.0001	-0.0002***	1.07e-05	1	-0.0002***	-0.0004	-0.0003***	-0.0001
	(5.23e-05)	(0.0003)	(5.85e-05)	(0.0004)		(5.26e-05)	(0.0007)	(5.88e-05)	(0.0007)
2	-0.0003***	-0.0002	-0.0003***	-3.74e-05	2	-0.0003***	-0.0004	-0.0003***	-0.0002
	(5.60e-05)	(0.000406)	(6.36e-05)	(0.000452)		(5.62e-05)	(0.000777)	(6.38e-05)	(0.000798)
3	-0.0008***	-2.69e-05	-0.0008***	0.0002	3	-0.0008***	-0.0002	-0.0008***	0.0002
	(5.79e-05)	(0.0004)	(6.59e-05)	(0.0005)		(5.81e-05)	(0.0008)	(6.61e-05)	(0.0008)
4	6.52e-05	0.0011**	-0.0003***	0.0003	4	6.00e-05	0.0010	-0.0003***	0.0003
	(6.12e-05)	(0.0005)	(6.97e-05)	(0.0006)		(6.14e-05)	(0.0008)	(6.99e-05)	(0.0009)
5 - 7	-0.0005***	-0.0008	-0.0008***	-0.0024***	5 - 7	-0.0005***	-0.0007	-0.0008***	-0.0023**
	(7.72e-05)	(0.0007)	(8.71e-05)	(0.0009)		(7.74e-05)	(0.0009)	(8.72e-05)	(0.0011)
			Tenure by post '12	Tenure by foreign by post '12				Tenure by post '12	Tenure by foreign by post '12
1			-0.0004***	-0.0002	1			-0.0004***	-8.94e-05
			(5.87e-05)	(0.0002)				(5.89e-05)	(0.0004)
2			-0.0004***	-0.0002	2			-0.0004***	-0.0001
			(6.45e-05)	(0.0003)				(6.47e-05)	(0.0005)
3			-0.0004***	-0.0002	3			-0.0004***	-0.0002
			(6.72e-05)	(0.0004)				(6.74e-05)	(0.0005)
4			0.0002***	0.0010**	4			0.0002***	0.0011*
			(7.14e-05)	(0.0005)				(7.16e-05)	(0.0006)
5 - 7			0.0002**	0.0020***	5 - 7			0.0002**	0.0021**
			(8.77e-05)	(0.0007)				(8.79e-05)	(0.0008)
School FE			√		School FE			√	
School-by-Year FE			√		School-by-Year FE			√	
School-by-Foreign FE			√		School-by-Foreign FE			√	
School-by-Foreign-Year FE					School-by-Foreign-Year FE			√	
Observations			4,749,363		Observations			4,749,363	

*Notes: The unit of observation is a schooling spell for each student enrolled in a program. The dependent variable is completion in newly added STEM degree. Reported coefficients are based on estimating equation (2) from the text. Standard errors, in parentheses, are clustered at the department level. ** $p < 0.05$.*

APPENDIX TABLE VII: ESTIMATES OF THE EFFECT OF OPT EXTENSION ON THE COMPLETION OF NEWLY ADDED STEM DEGREE FOR GRADUATE LEVEL (MASTER'S DEGREE)

	(1)		(2)			(3)		(4)	
	Tenure	Tenure by foreign	Tenure	Tenure by foreign		Tenure	Tenure by foreign	Tenure	Tenure by foreign
2	0.0008***	0.0003	0.0008***	0.0003	2	0.0007***	0.0005	0.0007***	0.0005
	(0.0002)	(0.0004)	(0.0002)	(0.0004)		(0.0002)	(0.0004)	(0.0002)	(0.0004)
3 - 4	0.0015***	0.0031***	0.0015***	0.0031***	3 - 4	0.0014***	0.0035***	0.0014***	0.0035***
	(0.0002)	(0.0007)	(0.0002)	(0.0007)		(0.0002)	(0.0007)	(0.0002)	(0.0007)
	Tenure by post '08	Tenure by foreign by post '08	Tenure by post '08	Tenure by foreign by post '08		Tenure by post '08	Tenure by foreign by post '08	Tenure by post '08	Tenure by foreign by post '08
1	-0.0013***	-0.0004	-0.0013***	-4.61e-05	1	-0.0014***	0.0002	-0.0014***	0.0005
	(0.0002)	(0.0003)	(0.0002)	(0.0003)		(0.0002)	(0.0007)	(0.0002)	(0.0008)
2	0.0005***	0.0003	0.0002	-0.0006	2	0.0004**	0.0006	0.0002	-0.0002
	(0.0002)	(0.0004)	(0.0002)	(0.0005)		(0.0002)	(0.0008)	(0.0002)	(0.0008)
3 - 4	-0.0001	-0.0025***	-0.0004	-0.0005	3 - 4	-0.0001	-0.0025**	-0.0005	-0.0004
	(0.0003)	(0.0009)	(0.0003)	(0.0010)		(0.0003)	(0.0011)	(0.0003)	(0.0012)
			Tenure by post '12	Tenure by foreign by post '12				Tenure by post '12	Tenure by foreign by post '12
1			-0.0003**	-0.0007**	1			-0.0004**	-0.0013*
			(0.0002)	(0.0003)				(0.0002)	(0.0007)
2			0.0002	0.0012**	2			0.0001	0.0004
			(0.0002)	(0.0005)				(0.0002)	(0.0008)
3 - 4			0.0003	-0.0040***	3 - 4			0.0003	-0.0047***
			(0.0003)	(0.0010)				(0.0003)	(0.0012)
School FE			√		School FE				√
School-by-Year FE			√		School-by-Year FE				√
School-by-Foreign FE			√		School-by-Foreign FE				√
School-by-Foreign-Year FE					School-by-Foreign-Year FE				√
Observations			858,095		Observations				858,095

*Notes: See notes on Appendix Table VI. ** $p < 0.05$.*

APPENDIX TABLE VIII: ESTIMATES OF THE EFFECT OF OPT EXTENSION ON THE COMPLETION OF NEWLY ADDED STEM DEGREE FOR GRADUATE LEVEL (DOCTORAL DEGREE)

	(1)		(2)			(3)		(4)	
	Tenure	Tenure by foreign	Tenure	Tenure by foreign		Tenure	Tenure by foreign	Tenure	Tenure by foreign
2	1.10e-05	-0.0002	1.26e-05	-0.0002	2	4.36e-05	-0.0002	4.10e-05	-0.0002
	(0.0007)	(0.0011)	(0.0007)	(0.0011)		(0.0007)	(0.0011)	(0.0007)	(0.0011)
3	0.0015*	-0.0003	0.0015*	-0.0003	3	0.0015*	-0.0003	0.0015*	-0.0003
	(0.0007)	(0.0011)	(0.0008)	(0.0011)		(0.0008)	(0.0011)	(0.0008)	(0.0011)
4	0.0023**	-0.0011	0.0023**	-0.0011	4	0.0023**	-0.0010	0.0023**	-0.0010
	(0.0009)	(0.0013)	(0.0009)	(0.0013)		(0.0009)	(0.0013)	(0.0009)	(0.0013)
5	0.0009	0.0112***	0.0009	0.0112***	5	0.0010	0.0114***	0.0010	0.0114***
	(0.0012)	(0.0016)	(0.0012)	(0.0016)		(0.0012)	(0.0016)	(0.0012)	(0.0016)
6	0.0017	0.0101***	0.0017	0.0101***	6	0.0018	0.0106***	0.0018	0.0106***
	(0.0017)	(0.0024)	(0.0017)	(0.0024)		(0.0017)	(0.0024)	(0.0017)	(0.0024)
7 - 9	0.0027	0.0081**	0.0027	0.0081**	7 - 9	0.0028	0.0077**	0.0028	0.0077**
	(0.0021)	(0.0035)	(0.0022)	(0.0035)		(0.0021)	(0.0035)	(0.0022)	(0.0035)
	Tenure by post '08	Tenure by foreign by post '08	Tenure by post '08	Tenure by foreign by post '08		Tenure by post '08	Tenure by foreign by post '08	Tenure by post '08	Tenure by foreign by post '08
1	-0.00045	-0.0002	-0.0001	-3.52e-05	1	-0.0003	0.0005	-0.0002	0.0010
	(0.0007)	(0.0010)	(0.0008)	(0.0011)		(0.0008)	(0.0016)	(0.0009)	(0.0017)
2	-0.0002	-0.0003	1.01e-05	-1.21e-05	2	-0.0001	0.0005	-0.0001	0.0011
	(0.0008)	(0.0010)	(0.0009)	(0.0012)		(0.0008)	(0.0016)	(0.0009)	(0.0017)
3	-0.0012	-0.0004	-0.0005	-0.0007	3	-0.0011	0.0005	-0.0007	0.0004
	(0.0008)	(0.0011)	(0.0010)	(0.0013)		(0.0009)	(0.0017)	(0.0010)	(0.0018)
4	-0.0013	0.0025*	-0.0019	0.0034**	4	-0.0011	0.0032*	-0.0020*	0.0044**
	(0.0010)	(0.0013)	(0.0012)	(0.0015)		(0.0010)	(0.0018)	(0.0012)	(0.0020)
5	0.0006	-0.0039**	0.0013	-0.0048**	5	0.0007	-0.0034	0.0012	-0.0039*
	(0.0013)	(0.0017)	(0.0015)	(0.0020)		(0.0013)	(0.0021)	(0.0015)	(0.0023)
6	0.0022	-0.0017	0.0014	-0.0026	6	0.0023	-0.0015	0.0013	-0.0021
	(0.0018)	(0.0025)	(0.0020)	(0.0028)		(0.0018)	(0.0028)	(0.0021)	(0.0031)
7 - 9	-0.0016	0.0058	-0.0013	-8.29e-05	5 - 7	-0.0016	0.0070*	-0.0015	0.0014
	(0.0022)	(0.0036)	(0.0024)	(0.0039)		(0.0022)	(0.0038)	(0.0024)	(0.0041)
			Tenure by post '12	Tenure by foreign by post '12				Tenure by post '12	Tenure by foreign by post '12
1			-0.0002	-0.0001	1			-0.0003	-0.0005
			(0.0008)	(0.0011)				(0.0009)	(0.0016)
2			-5.19e-05	-0.0003	2			-0.0001	-0.0009
			(0.0008)	(0.0012)				(0.0009)	(0.0016)
3			-0.0007	0.0009	3			-0.0008	0.0005
			(0.0009)	(0.0013)				(0.0010)	(0.0017)
4			0.0015	-0.0014	4			0.0014	-0.0018
			(0.0011)	(0.0015)				(0.0011)	(0.0019)
5			-0.0008	0.0018	5			-0.0010	0.0014
			(0.0013)	(0.0018)				(0.0013)	(0.0021)
6			0.0019	0.0022	6			0.0017	0.0019
			(0.0017)	(0.0024)				(0.0017)	(0.0027)
7 - 9			-5.75e-05	0.0119***	7 - 9			-0.0003	0.0117***
			(0.0016)	(0.0026)				(0.0016)	(0.0029)
School FE				√	School FE				√
School-by-Year FE				√	School-by-Year FE				√
School-by-Foreign FE				√	School-by-Foreign FE				√
School-by-Foreign-Year FE					School-by-Foreign-Year FE				√
Observations				367,104	Observations				367,104

*Notes: See notes on Appendix Table VI. ** $p < 0.05$.*

APPENDIX TABLE IX: TEST OF COVARIATE BALANCE

VARIABLES	Discontinuity estimate
Black	0.000505
	(0.0044)
Observations	33928
Hispanic	0.00123
	(0.00244)
Observations	41713
Asian	-0.00219
	(0.00368)
Observations	23446
White	-0.00385
	(0.00712)
Observations	36951
Female	0.0267
	(0.054)
Observations	46280
Age at enrollment	-0.107
	(0.107)
Observations	26193
Age at graduation	-0.161
	(0.128)
Observations	41031
Years to degree	-0.027
	(0.0219)
Observations	34273
Student's first term GPA	0.00744
	(0.0117)
Observations	34431
Total credit hours	0.875
	(1.165)
Observations	24450
Average weekly earnings the year before graduation (t-1)	-1.998
	(9.034)
Observations	38055
Predicted weekly earnings (t+1)	-1.564
	(1.994)
Observations	31225

*Notes: Table presents discontinuity estimates for a variety of covariates around the critical GPA threshold. We use the optimal IK bandwidth, which varies by covariate. See table XII for details regarding specification and sample restrictions. Heteroskedastic-robust standard errors reported in parentheses. ** $p < 0.05$.*

APPENDIX TABLE X: ESTIMATES FOR MISSING EARNINGS THE YEAR AFTER GRADUATION WITH DIFFERENT BANDWIDTHS

	Missing Earnings (t+1)							
All	IK Bandwidth		Bandwidths					
	0.4786	0.4786	0.25	0.25	0.50	0.50	0.75	0.75
Above Cutoff	0.00166	0.000517	0	0.00309	0.00204	0.000427	0.00435	-0.000459
	-0.00386	-0.00351	-0.0052	-0.00474	-0.0038	-0.00346	-0.00335	-0.00304
Observations	180,646	180,646	100,122	100,122	186,675	186,675	243,439	243,439
Control	NO	YES	NO	YES	NO	YES	NO	YES
Selective	IK Bandwidth		Bandwidths					
	0.3951	0.3951	0.25	0.25	0.50	0.50	0.75	0.75
Above Cutoff	8.03E-05	0.00163	-0.000539	0	0.00195	0.00117	0.00418	4.41E-05
	-0.00492	-0.00453	-0.00608	-0.00561	-0.00447	-0.00412	-0.00399	-0.00366
Observations	113,334	113,334	74,297	74,297	137,496	137,496	178,112	178,112
Control	NO	YES	NO	YES	NO	YES	NO	YES
Non-Selective	IK Bandwidth		Bandwidths					
	0.4322	0.4322	0.25	0.25	0.50	0.50	0.75	0.75
Above Cutoff	-0.00545	-0.00199	-0.00864	0.00179	-0.00493	-0.00264	-0.00603	-0.00449
	-0.00757	-0.0067	-0.0099	-0.00878	-0.0071	-0.00629	-0.00616	-0.00547
Observations	43,377	43,377	25,825	25,825	49,179	49,179	65,327	65,327
Control	NO	YES	NO	YES	NO	YES	NO	YES

*Notes: Data are administrative data on higher education enrollment from the state of Ohio, made available to researchers by the Ohio Education Research Center (OERC). Sample is from year 1999 to 2011 and restricted to graduates of public 4-year institution. The dependent variable is missing earnings in the year following graduation. Reported coefficients are based on estimating equation (2) from the text using local linear regression with a triangular kernel and bandwidths that vary according to column. To maintain a consistent sample across specifications, the analysis is restricted to individuals with non-missing earnings in the first three years after graduation. Figures 17 (a) – 17 (c) show the corresponding visual evidence using the 0.75 bandwidth from the rightmost column of this table. Controls refer to age, sex and race indicators. All specifications include school-by-major-by-graduation-year fixed effects. Heteroskedastic-robust standard errors reported in parentheses. ** $p < 0.05$.*

5 VITA

Pauline Khoo

EDUCATION

2020, Expected	Ph.D. in Economics, University of Illinois at Chicago
2015	M.A. in Economics, University of Illinois at Chicago
2012	B.Sc. in Mathematics, Hollins University

PUBLICATIONS

Khoo, P., & Ost, B. (2018). The effect of graduating with honors on earnings. *Labour Economics*, *55*, 149-162.

WORKING PAPERS

A Peer Like Me? Peer Effects Among International Students in Doctoral Programs.
How Do International Students Affect Native Students in Doctoral Programs?
If We Extend It, They Will Come: The Effects of the STEM OPT Extension.

RESEARCH EXPERIENCE

2016 – 2017	Research Assistant for Professor Javaeria Qureshi, UIC
2015 – 2016	Research Assistant for Professor Steven Rivkin, UIC
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2012	Research Assistant for Dr. Kim Hwa Lim, Penang Institute
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TEACHING EXPERIENCE

Spring 2020	Teaching Assistant, Principles of Microeconomics, UIC
Fall 2019	Teaching Assistant, Intermediate of Macroeconomics, UIC
Spring 2019	Course Instructor, Economics of Education, UIC
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CONFERENCE PRESENTATIONS

2019	APPAM California Regional Student Conference
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The effect of graduating with honors on earnings

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Publication: Labour Economics

Publisher: Elsevier

Date: December 2018

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