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Essays on Migration

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ESSAYS ON MIGRATION

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Abstract

This dissertation covers two evaluations on migration policies and their impact on distinct groups of immigrants.

The first chapter deals with a recent policy in the German state of Baden-Württemberg, which started to charge tuition fees from international students in 2017. Tertiary education remains free of charge in all other 15 states, which warrants a difference-in-differences approach. My analysis with publicly available data focuses on enrollment rates and provides additional insights into the geographical distribution of changes in enrollment behavior. I find that the international enrollment share decreased by about 2 percentage points at treated institutions, largely thanks to changes in the enrollment behavior of Asian and African students. The policy has however not yet materialized in an improvement of foreign failure shares of final exams, i.e. there is no empirical evidence for a quality effect.

Chapter 2 assesses the labor market effects on different immigrant groups under a restrictive immigration regime. I focus on the Johnson-Reed Act in the US, which curbed immigration after 1924, in particular from Asia as well as Southern and Eastern Europe. Filipinos were exempt from these restrictions because they immigrated from a US territory until gaining independence. This differential treatment creates a well-defined group for a difference-in-difference analysis on relative changes in labor market outcomes. My findings, derived from full US census data between 1910 and 1940, support the hypothesis that ongoing Filipino immigration lowered the relative occupational standing of their compatriots who migrated earlier. Moreover, Filipinos became more likely to seek and find employment in the same period. The effects are particularly strong for 1930 before Filipinos were subject to restrictions themselves,

and in California, the main destination of Filipinos. Panel estimates partially support these results.

To my family and friends

Acronyms

ARWU Academic Ranking of World Universities

EEA European Economic Area

EU European Union

GDP Gross Domestic Product

IPUMS Integrated Public Use Microdata Series

MLP Multigenerational Longitudinal Panel

NUTS Nomenclature of Territorial Units for Statistics

UK United Kingdom

US United States

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1. THE EFFECT OF BADEN-WÜRTTEMBERG'S TUITION FEES ON INTERNATIONAL STUDENT OUTCOMES[†]

Despite the increasing number of students learning abroad, little is known about the way international students migrate and how policies influence their decision. This article evaluates one German state's recent policy to charge tuition from international students, while tertiary education remains free elsewhere. For my difference-in-differences analysis, I collect and combine publicly available records for German higher education institutions since 1998. I find that the international enrollment rate decreases by 2 percentage points at treated institutions, driven by African and Asian students. In contrast to state government motivations, I find no evidence for a short-term decrease in exam failure rates.

[†]I am very grateful for valuable comments by conference participants at WEAI and BIEN Jahrestagung. Also, I thank John Gardner for continuous feedback as well as Lydia Kolano, Lawrence Ogbeifun and Timo Schreck for their insights in the early stages. All remaining errors are my own.

1.1. Introduction

Recent decades have seen an unprecedented increase in the number of students abroad. In 2018, almost six million students enrolled outside of their home country, an increase by 68 percent since 2008. Despite this trend, economic research on the decision-making of students and host societies is only emerging (Beine et al., 2014; Beine et al., 2020; Bound et al., 2020; Dustmann and Glitz, 2011).

This paper adds to our understanding by analyzing a recent policy change for international students in Germany, their main destination in Europe and the forth-largest exporter of tertiary education worldwide. Germany is also the largest host country where English is not the primary language and stands out from other major destinations because tuition fees for public education are largely absent. This is an attractive feature of German education, especially for students from poorer regions worldwide.[1] A policy change in one German state (out of sixteen), charging tuition from international students at public institutions only, allows a closer look how tuition fees change education outcomes and migration flows.

To ease comparison throughout this paper, I refer to students outside of Europe as *international* students, whereas *foreign* students include all without German citizenship. Likewise, I use the term *institution* to avoid a distinction between different types of higher education in Germany such as universities, universities of applied sciences (*Fachhochschulen*), academies or *Hochschulen*, and between separate campuses in different locations.

In 2017, the German state of Baden-Württemberg introduced a fee of 3,000 euro per year, explicitly targeting students from outside the European Economic Area (EEA). Similar policies have been adopted in various European countries since the 1980s, especially after 2006 (Cai and Kivistö, 2013).[2] The specific targeting of international students raises important questions regarding education and immigration that this

[1] All numbers above are taken from Heublein and Hutzsch (2021), Fig. 2. Note that there are no official records for China. As discussed in more detail below, several German states charged tuition for all students between 2006 and 2014.

[2] As of 2022, the EEA includes all current European Union (EU) member states as well as Iceland, Liechtenstein and Norway.

article aims to answer: What is the policy's impact on migration flows? Who exactly is affected if the price of higher education increases? Are educational outcomes affected?

The differential treatment, deviating from free education at public institutions in only one state, provides well-defined treatment and control groups for a difference-in-difference analysis.[3] The data used herein are collected from publicly available sources, the Federal Statistical Office and the majority of states' statistical offices. Although both data sets stem from the same original sources, the institutions themselves, they vary in their depth and units of observation. For example, one state reports data for each campus of an institution separately, whereas another state may lump them together by location or institution. Institutional mergers, campus closures, and institutions operating in multiple states further complicate a comparison of the two data sets, if they are treated in different ways by the offices. To assess the impact of these reporting differences, I analyze each estimation sample separately, obtaining similar results.

I examine the short-run impact of tuition fees on international enrollment and final exam failure rates at the institutional level. My results indicate that the introduction of tuition fees in the state of Baden-Württemberg leads to a drop of about 2 percentage points in the international enrollment rate there. The policy disproportionally affects students from Africa and Asia in the sense that their shares fall significantly on Baden-Württemberg's campuses. Estimates for the Americas are ambiguous and depend on the specified model. Moreover, upon its introduction, state officials endorsed the tuition policy on the basis that fees might improve the academic performance of international students, who have historically performed worse than their native peers. Looking at foreign students' failure rate of final examinations, I do not find any evidence for an improvement in the short term.

The results in this paper are robust to placebo tests on the policy's location and time, but also alternative specifications. These tests add further credence to my

[3] Additionally, the Free State of Saxony allows a similar measure for its institutions. To the best of my knowledge, only *Hochschule für Musik und Theater Leipzig* has taken advantage of the possibility by charging non-EU citizens for most courses since 2013. I exclude this institution from my analysis.

findings on the impact of the policy on international students' enrollment decision and performance.

The main contributions of this paper are an empirical analysis of a recent state policy on the enrollment decisions and educational outcomes of a narrowly targeted group. In particular, the geographical distribution of changes in student migration flows adds to our understanding of the interconnectedness of regional educational policies and international students' decision-making. In contrast to previous studies, the policy that I analyze can be considered as exogenous, and takes place in a major destination for international students. Additionally, this paper highlights the growing importance of education export in non-English speaking countries, as well as the sensitivity of outcomes to the imposition of tuition. Thereby, I provide an estimate for similar policies and whose decisions may be affected.

While my analysis of the impact of the policy on the geographical distribution of students at German institutions is novel, the potential change in enrollment rates due to similar policies has produced a lot of research with contradicting results. To understand the source of the theoretical ambiguity, consider the following thought experiment: Before the policy change, institutions fill all vacancies with approved applicants from Germany and abroad. The introduction of tuition fees might change a poor international student's mind such that she applies to another German institution. Her decision leads to a vacant spot, which is allocated to a domestic student, and so the international enrollment rate decreases.

Alternatively, the vacancy could be filled by a more affluent international student whose credentials surpass domestic applicants. Given that student's approval, his institution is presumably able to finance his spot each semester upon payment. If his tuition covers the spot fully, no other student is affected. In this scenario, no native student is involved as all vacancies transfer from one international student to another. Such transactions may even be without consequences for continental enrollment shares if both affected students hail from the same continent. For a modest amount of tuition charged, it is also possible that the institution is able to increase revenue without trade-offs. Hence, it is possible that there is no effect of tuition on enrollment rates.

For well-renowned universities, the tuition charged could result in additional funds without stark changes in student numbers. This circumstance provides an incentive to broaden the university's international outreach, possibly at the expense of natives, especially if its pool of applicants is large. Less popular institutions, in contrast, may see a decrease in enrollment. In the extreme, international students apply elsewhere, thus causing less revenue, while being replaced by e.g. native students with lower skills. The institution in this scenario ends up with lower revenue and a decline in quality. Thus, the policy change might lead to a redistribution of students to institutions, possibly displacing higher-skilled natives.

There are other channels through which fees may affect enrollment. Expecting adverse consequences as a perceived lower probability of admission, some native students may be discouraged from enrolling in the treated state at all, similarly to findings in Hübner (2012) and Dwenger et al. (2012).[4] In contrast, if tuition fees signal quality, international enrollment rates may increase.

To summarize, the adoption of tuition fees puts a markup onto a previously free, but restricted, good. Following the previous thought experiment, a reduction in international enrollment seems plausible, but not necessary, if institutions continue to restrict admission as before and applications exceed available spots. Thus, in theory, the potential effects of the policy change are ambiguous.

Empirical results from existing studies reflect this ambiguity. Previous research on different fees has focused primarily on the United States (US) and the United Kingdom (UK), with mixed evidence.[5] Apart from Beine et al. (2020), who examine international enrollment in Italy, most studies focus on comparison across countries, thus neglecting regional variation within a single country and potentially raising en-

[4] Both of these papers focus on student mobility of German high school graduates under a universal fee. For the same policy, Alecke et al. (2013) and Bruckmeier and Wigger (2014) find no such effect.

[5] See Beine et al. (2020) for more details and Bound et al. (2021) for a review of current trends in US Higher Education.

dogeneity concerns. This paper adds to this strand of the literature by focusing on state-level policy changes on outcomes at the institutional level. Beine et al. (2020) find a strong negative effect on international enrollment when universities increase their tuition. My findings are consistent with theirs and also show similar effects for state-mandated tuition policies.

My work is also closely related to research on the introduction of state-wide universal tuition fees in seven German states in the early 2000s, which generated a considerable amount of research, however contradictory in their findings. Hübner (2012), Bruckmeier and Wigger (2014), Dietrich and Gerner (2012) and Dwenger et al. (2012) focus on the implications of these policies for the enrollment and application decisions of high school graduates.[6] Hübner (2012) finds a strong negative and significant impact (−2.7 percentage points) on high school graduates' enrollment rates, even −4.7 percentage points considering spillover effects (as tuition may affect high school graduates in non-fee states). For the same data, applying additional controls and bearing variation in states and over time in mind, research by both Bruckmeier and Wigger (2014) and Alecke et al. (2013) finds no negative effect from tuition on the enrollment for high school graduates. My study differs from these papers in several ways. I analyze a state policy at the institutional level, and emphasize the impact on foreign and international enrollment rates as well as possible implications for both international and native students.

For the same policy, Dwenger et al. (2012) investigate the impact of tuition for medicine and dentistry applicants, both programs being centralized in Germany; they find that the probability of applying in one's home state decreases if tuition is charged. Moreover, their results indicate that the composition of students across states changes because better high school graduates are more likely to apply to universities in their home state. Bietenbeck et al. (2020) also evaluate the enrollment of current and prospective students in Germany on previous fees, when switching from free education

[6] As in the papers mentioned, I do not consider second-round migration decisions as in Lange (2013).

to charging tuition fees. They find that tuition accelerates graduation, and generates financial gains for universities, but does not affect educational attainment.

1.2. Tuition Fees for International Students

In contrast to other developed countries, tuition fees are not common for public education in Germany. Apart from *Hörergeld* of 150 Deutschmarks per semester until 1970, tertiary education was free for all students at universities until the 2000s. Education is the domain of individual states, and so, beginning in 2006, seven states implemented fees to motivate long-term university students (those exceeding the standard period of study by more than four semesters) to graduate. The typical fee was 500 euro per semester.[7] Persistent public dislike led to their overall abolishment by 2014.

In March 2017, the state of Baden-Württemberg passed a new law (*Gesetz zur Änderung des Landeshochschulgebührengesetzes und anderer Gesetze*) to charge 1,500 euro per semester from students outside of the EEA at public institutions. Exemptions are made for those with a German high school degree, also obtained abroad (*Bildungsinländer*), those married to or a child of an EEA citizen, as well as refugees and residents of Germany for five years or more. Further, institutions can negotiate bilateral exchange agreements and support talented students from the least developed countries. Likewise, exchange students are not subject to payments. The law also recommends against charging disabled students or those facing emergency situations. Lastly, the fee does not apply to institutions for Public Service (*Hochschulen für den öffentlichen Dienst*) such as administration and policing. According to newspaper reports, about half of international students meet the criteria for exemptions.[8]

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[7] Exceptions were possible at the university level in the states of Bavaria and North Rhine-Westphalia, but only used in two cases. As I only focus on the presence of fees below, I ignore the lower fees at the universities in Bielefeld and Münster. To the best of my knowledge only the University of Münster deferred tuition until 2007, which is considered in the analysis below. It should also be noted that German higher education is relatively selective as only about half of the native population is entitled to enroll.

[8] The large increase in Syrian, and thus Asian, refugees in 2015 should not qualitatively change the results below as there is no plausible impact on the decision where to study given the exemption. There is also a considerable delay until they start with tertiary education. For more information on exemptions granted

Reports of a first announcement regarding this policy change circulated in October 2016 and insinuated cost-saving effects. In the following spring the state parliament passed the law, which became effective in the 2017/18 winter term. The delay in legal adoption gave international students ample time, if aware of the prospective policy change, to reevaluate their potential applications or continue their studies at another institution within Germany. Although there may exist strong regional preferences for attending a university in Baden-Württemberg (such as higher reputation, access to local networks or amenities), international students who desire to graduate in Germany have plenty of alternatives. Early reports expected the fee to apply to 7,000 currently enrolled students and generate revenue of up to 45 million euro annually by 2022.

The responsible state ministry claims that the tuition guarantees improved conditions, in particular by reducing the excessive dropout rates of international students and by providing better support for the continuous growth of international student numbers. According to law, 300 euro of a student's semester fee remain at the institution to cover and further improve the general conditions for international students. The remaining amount of 1,200 euro flows into the state budget. At the time of implementation, the state ministry referred to similar fees in other countries, which ranged from 1,500 euro in Austria up to 20,000 euro more in the Netherlands and the United Kingdom. Thus, in comparison the tuition fee discussed herein is relatively modest.

1.3. Data

For my analysis, I collect and combine publicly available data, covering institutions of higher education, from two sources – the Federal Statistical Office of Germany (Statistisches Bundesamt (Destatis), 2020b) and the states' statistical offices. Institutions report to the state's entity, which then forwards these numbers to the federal level, so both sources are indirectly linked.

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see https://www.stuttgarter-nachrichten.de/inhalt.semestergebuehren-in-baden-wuerttemberg-nur-jeder-zweite-internationale-student-zahlt.19c5e4d6-65bf-482c-93c5-c2906032af48.html.

State and federal data are not aggregated at the same level, differ in their depth of information, and thus they are not readily comparable.[9] Most importantly, the data from the Federal Statistical Office lump foreign students into one category, while the states provide additional information on citizenship by continents. Hence, the *federal* data exclusively refers to *foreign* students and the *states'* data to *international* students.

These federal data include observations on students' gender (male/female) and nationality (German/foreign) at campus levels between 1998 and 2019. I merge the Federal student statistics with an additional data set from the same source covering final examinations before graduation to evaluate student performance (Statistisches Bundesamt (Destatis), 2020a). Descriptive statistics in Table 1.1 show that failure in final examination is a rare event, but somewhat more likely for foreigners. Unfortunately, data for dropout rates were not available. To deal with the imprecision in students' origins, arising from the binary value, I have also collected data from the states' statistical offices.[10] Those data identify foreign students' continents of origin, which allows me to observe changes in migration patterns.

Given mergers and openings of new campuses and institutions, resulting in different units of observation, I refrain from assembling all information into one data set and run two separate analyses on the policy's effect on both foreign and international students. Doing so allows me to draw more insightful conclusions on enrollment rates by continent of origin and student success, which is not included in the states' data.

Because most students from European countries outside of Germany will be covered by bilateral agreements (as in exchange programs like Erasmus), I exclude those from *international* students.[11]

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[9]To match reports with summarized data only, I combine campuses affected by mergers, renaming or recoding in the same region to one observation. A full list of such instances can be found in Appendix A.1.

[10]Existent data protection regulations prevent a usage from educational institutions in Rhineland-Palatinate. Additionally, Lower Saxony, Mecklenburg-Western Pommerania, and Saxony did not reply to data requests. For Bremen, only years since 2000 are included, while data for Berlin, Brandenburg, and Saxony-Anhalt cover only years from 2007 onward. Hamburg and Schleswig-Holstein round the number of enrolled students up or down to threes.

[11]Doing so also mitigates the problem of having a foreign citizenship, but completed education in Germany, which mostly applies to Turks, but also Bosnians and Kosovars. One potential confounder

Both data sets include years from 1998 to 2019 to cover the number of foreign and international students. For any year, student data, such as enrollment numbers and the share of males, refer to the beginning of the winter semester, starting in October. Many institutions only accept new enrollment for the winter semester. I also identify public institutions of tertiary education separately in case students value these differently than private institutions.

To account for other factors relevant to migration decisions, I obtained state and regional Gross Domestic Product (GDP) per capita from the statistical portal of the Federal and states' offices (Arbeitskreis Volkswirtschaftliche Gesamtrechnungen der Länder, 2020), and unemployment rates from Eurostat (2020). For Germany, regional data for government regions falls under the European geocode standard Nomenclature of Territorial Units for Statistics (NUTS) 2. Additionally, I control for the share of foreigners within each state, taken from the Federal Statistical Office, and an institution's Shanghai Academic Ranking of World Universities (ARWU) alumni rank, which is a measure for institutional quality.[12] Lastly, high-school reforms (*G-8 Abitur*), which temporarily influence the number of graduates within a state, enter as a binary variable per state and year. All of these variables are included in both samples.

In total, my panel for the federal data includes 10,603 observations for 698 institutions or locations. The second sample, collected from the states themselves, contains 6,921 observations for 466 institutions. Given campus mergers, openings and closures of institutions between 1998 and 2019, the panels are unbalanced. Data provision by the states' statistical offices explains large parts of the gap. Table 1.1 lists the descriptive statistics for relevant variables in the analysis.

remaining is the relatively high percentage of Tunisians and Moroccans with a German high school degree, i.e. *Bildungsinländer*, who account for around one fourth of international students. In 2019, the 21,400 Erasmus students in Germany made up 6.7 percent of all international and less than 20 percent of European students. Baden-Württemberg had the highest ratio of international students with 10.6 percent (Deutsches Zentrum für Hochschul- und Wissenschaftsforschung and Deutscher Akademischer Austauschdienst, 2021).

[12]Due to data limitations, I use the available *Nobel* rank for the year of 2003. The values for rank follow Beine et al. (2020)'s approach, where I cap ranks at 500 to allow for consistent comparison over all years and calculate the value for ARWU by $(500 + 2) - ranking$.

	(1) Federal data		(2) States' data	
	mean	sd	mean	sd
Number of students	4,694.41	8,422.83	5,434.99	9,240.09
Foreign student share	0.12	0.13	0.13	0.13
International student share	—	—	0.07	0.09
Share of males	0.52	0.17	0.52	0.17
Share of failed exams: foreign	0.03	0.08	—	—
Share of failed exams: German	0.02	0.04	—	—
ARWU rank	18.90	73.59	21.57	79.59
Public institution	0.68	0.46	0.66	0.47
Previous tuition fee	0.15	0.36	0.16	0.37
Share of foreigners in state	0.10	0.04	0.11	0.04
GDP per capita (in log)	11.04	0.18	11.09	0.15
Unemployment rate	8.25	3.87	7.28	2.98
n	698		466	
N	10,603		6,921	

Table 1.1.: **Descriptive Statistics for Both Samples on Variables Used.** Column (1) includes data from the Federal Statistical Office, whereas column (2) considers data from the states' statistical offices on campuses of German institutions of higher education.

For all key variables, both samples are comparable as means are within one standard deviation. If the states' sample was complete, it would overlap entirely with the federal one. The different means in Table 1.1 for unemployment rates and GDP indicate that economically stronger years or states are slightly overrepresented in the states' sample, as are campuses with more students.[13] The slightly higher number on ARWU rank in column (2) signals that prestigious campuses are more likely to be included in the state sample. Despite the slight differences between the two samples, student characteristics are similar in both.

International student numbers in Germany have been increasing in recent years. Fig. 1.1 illustrates that both the number and share of foreign students go up in comparison to 1998.[14] After an initial decline in the share, both the number and the share have increased monotonically since 2012. The graph also illustrates that the

[13] The different samples prevent a statistical analysis of differences across those.

[14] Mostly because of the COVID-19 pandemic, and so limited issuance of visa, excessive drops in international enrollment were expected for 2020 with a recovery possible (Heublein and Hutzsch, 2021).

increase in international student numbers is driven by foreign students, but not at the expense of Germans. Even though foreign enrollment has been more than doubling, the overall fraction remains relatively low.[15] One reason for the increase is the Bologna Process, which streamlined the educational system within Europe, thus facilitating recognition of foreign degrees.

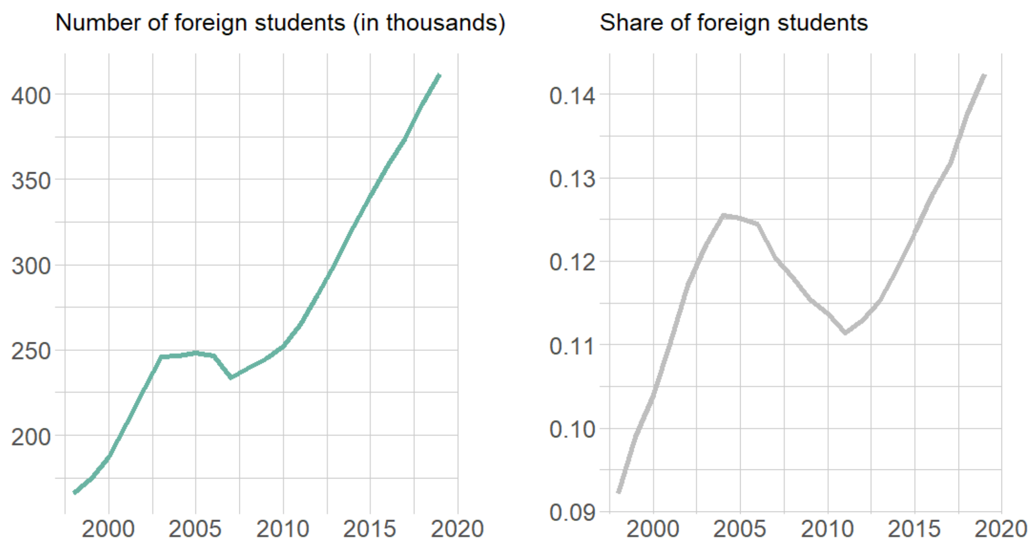


Figure 1.1.: **Number and Share of Foreign Students at German Institutions of Higher Education (1998–2019).** Both total number and share of foreign students have been increasing relatively to 1998.
Data source: Statistisches Bundesamt (Destatis), 2020b.

1.4. Identification and Estimation

1.4.1. Event Study

As outlined above, the data do not report exemptions at the individual level, hence estimations below focus on average effects.[16] To provide preliminary evidence on the

[15] According to *Statistisches Bundesamt; DZHW-Berechnungen*, the fraction of *Bildungsinländer* is remarkably stable, fluctuating between 2.87 and 3.5 percent between 1998 and 2019.

[16] Income differences across continents potentially discourage students, who would have applied in the absence of a fee. I argue that visa regulations to permit work after immigration as an international student ought to assuage such concerns. These assumptions are supported by the most recent figures from 2016 as employment rates for low and lower middle, high, and upper middle income countries all fall between 46 and 52 percent (Apolinarski and Brandt, 2018, p. 45).

effects and validity of the identification strategy, I first conduct an event study of the dynamic effects of the policy on foreign and international students, at the institutional level, controlling for the usual unit and time fixed effects, as well as a vector of time-varying observable characteristics with the following model:

$$international_{it} = \sum_{p=1}^{p_1} \theta^p lead_{it}^p + \sum_{q=1}^{q_1} \delta^q lag_{it}^q + \rho_i state_i + \tau_t year_t + \gamma \mathbf{X}_{it} + u_{it} \quad (1.1)$$

Equation (1.1) captures the effects of tuition fees on the international enrollment rate, the dependent variable $international_{it}$, at treated institutions. The coefficients on the *lead* represent tests for pretrends, and those on the *lag* capture the dynamic effects of the policy. The vector $\mathbf{X}_{it}$ consists of all the covariates that are listed in Table 1.1. These include the share of male students to account for gender disparities in the student body or among migrant flows, and whether the institution had implemented a tuition fee previously in a particular year. Student numbers and shares are taken or derived from the original samples.[17] Standard errors are clustered at the institutional level i.

I focus on enrollment rates for two reasons: First, the total of enrolled students differs widely between institutions, even more so for international students. Most institutions are rather small, but may attract a large fraction of international students, which would be obscured when looking at the mere total. Second, using a logarithmic scale eliminates observations with zero students, which distorts the analysis. In contrast, as shown in Fig. 1.1, the share of foreign students is fairly stable and reflective of changes within the student body. The shares also include observations for year-institution pairs with no international students enrolled. Nevertheless, I present supporting graphs on the total numbers below.

Corresponding to the event study specification in Eq. (1.1), Fig. 1.2 illustrates the impact of the policy change in foreign enrollment rates, using the federal data,

[17]The inclusion of an index for the cost of living does not substantially affect the results presented herein, but reduces the sample size to 10 percent of the used sample (data source: numbeo.com). Its estimates are statistically equivalent to zero. In consequence, I do not consider the cost of living further.

with and without controls.[18] The year prior to implementation (2016) serves as a benchmark, and is thus omitted from the regression. Whenever data are available, I include all post-periods, i.e. $q_1 \leq 3$ in Eq. (1.1). The first leading variable includes all years from 1998 to 2009, and so $p_1 = 7$.

The foreign enrollment rate in Baden-Württemberg is larger than in control states for years prior to the introduction of tuition fees, but the difference diminishes over time. Without adjusting for time-variant covariates, there appear to be pre-trends differing across states. Yet, the difference in foreign enrollment is statistically indistinguishable from zero for all years save 2010, thus being consistent with the parallel trend assumption when controls are considered. After international students are subject to tuition, the share of foreign enrollment decreases with a change in trend after implementation. The gap also becomes larger over the years. According to Fig. 1.2, the tuition fee reduces foreign enrollment by 1.0–2.3 percentage points. The deviation from the previous trend is highly statistically significant for all lags.

For international enrollment, using the states' data, the dynamics shown in Fig. 1.3 mirror the previous findings. The inclusion of controls results in pre-trend estimates that are statistically indistinguishable from zero. After tuition is charged, international enrollment rates decrease significantly by 0.9–2.8 percentage points with similar trends as above for the foreign share.

Both graphs support the hypothesis that a deviation from zero price education for international students reduces their enrollment rates. Despite the smaller and incomplete sample but higher precision, both results are reassuringly similar.

Further validity for these preliminary findings may be provided by replacing enrollment shares by student numbers. Fig. 1.4 and 1.5 depict similar trends for the total numbers of foreign and international students. Therefore, I focus on enrollment rates below, which also facilitate comparison across institutions of different types and sizes.

[18] As data for regional GDP per capita is unavailable for 2019, there exists a trade-off regarding its inclusion as a control. Fig. 1.2 and 1.3 show that an exclusion does not sufficiently alter estimates when other controls enter the regression. In consequence, state GDP serves as a control in further estimations.

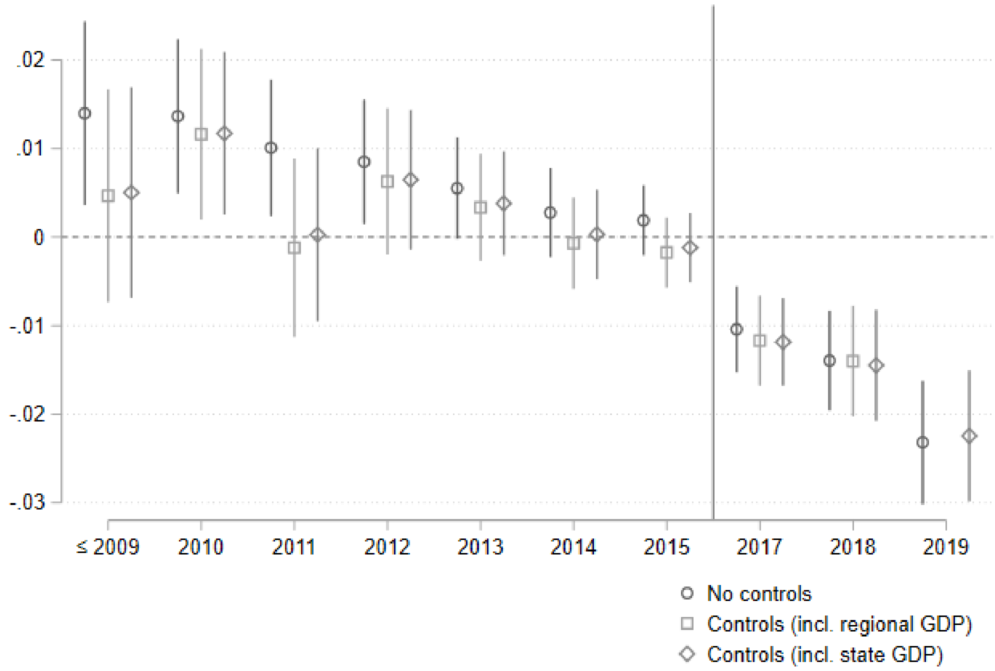


Figure 1.2.: **Dynamic Effects of Foreign Enrollment Rates in German Institutions of Higher Education.** Foreign students include all students without German citizenship. The data cover the period between 1998–2019, years until 2009 being included in the first estimate. The year before the policy change (2016) serves as benchmark. Treated institutions are public and located in the state of Baden-Württemberg, all others serve as control group. The graph pictures the estimates with a 95 percent confidence interval. Estimations include the full set of covariates: number of students, share of males, ARWU rank, public institution, previous tuition fee, share of foreigners in state, GDP per capita and unemployment rate. Specifications differ by the inclusion of log GDP per capita as controls. Regional controls refer to NUTS 2.
Data source: Statistisches Bundesamt (Destatis), 2020b.

1.4.2. Difference-in-differences Analysis

For my main analysis, I apply a conventional multivariate panel regression to estimate the effects of the policy with the following model:

$$international_{it} = \beta Post \times Fee_{it} + \rho_i state_i + \tau_t year_t + \gamma \mathbf{X}_{it} + u_{it}, \qquad (1.2)$$

where $international_{it}$ refers to the variable of interest, e.g. international enrollment rate, while $Post$ is a binary variable on the post-treatment period starting in 2017. Fee_{it} indicates public institutions in Baden-Württemberg, i.e. the treated institutions,

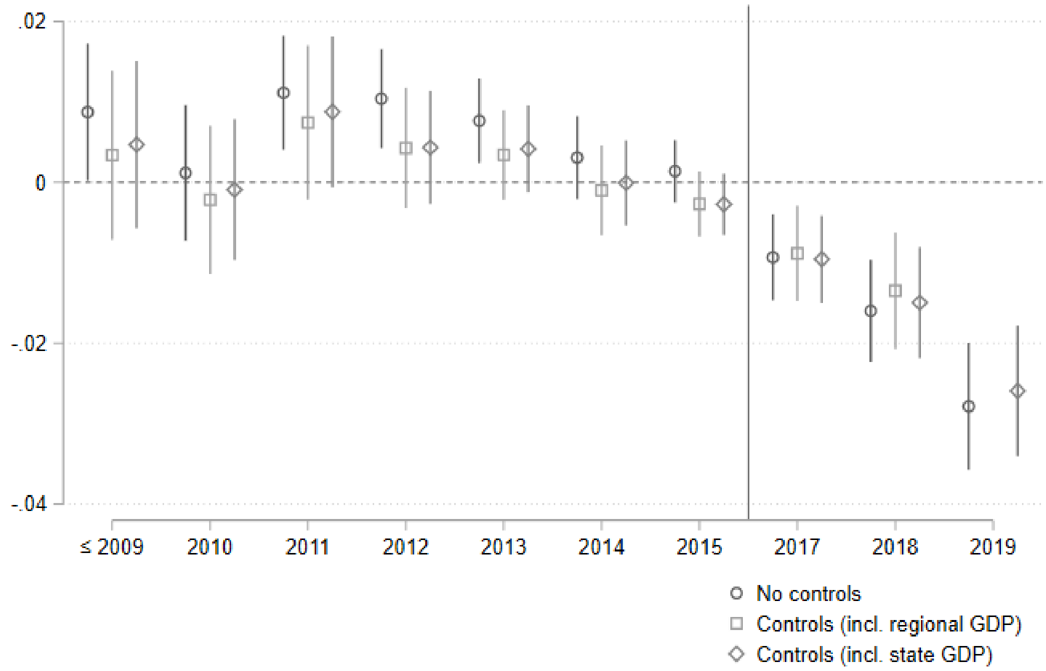


Figure 1.3.: **Dynamic Effects of International Enrollment Rates in German Institutions of Higher Education.** International students include all students from outside of Europe. The data cover the period between 1998–2019, years until 2009 being included in the first estimate. The year before the policy change (2016) serves as benchmark. Treated institutions are public and located in the state of Baden-Württemberg, all others serve as control group. The graph pictures the estimates with a 95 percent confidence interval. Estimations include the full set of covariates: number of students, share of males, ARWU rank, public institution, previous tuition fee, share of foreigners in state, GDP per capita and unemployment rate. Specifications differ by the inclusion of log GDP per capita as controls. Regional controls refer to NUTS 2.
Data source: States' statistical offices, 2020.

$state_i$ and $year_t$ are location and time fixed effects and $\mathbf{X}_{it}$ is a vector of all other covariates as outlined above. The main coefficient of interest is β, which describes the effect of the policy on international enrollment into higher education in Baden-Württemberg.

In further analyses, I apply Eq. (2.1) to evaluate geographical variation for *international* students' regions of origins as the outcome variable. Likewise, I estimate the impact of tuition on student performances in terms of failed final exams of *foreign* students.

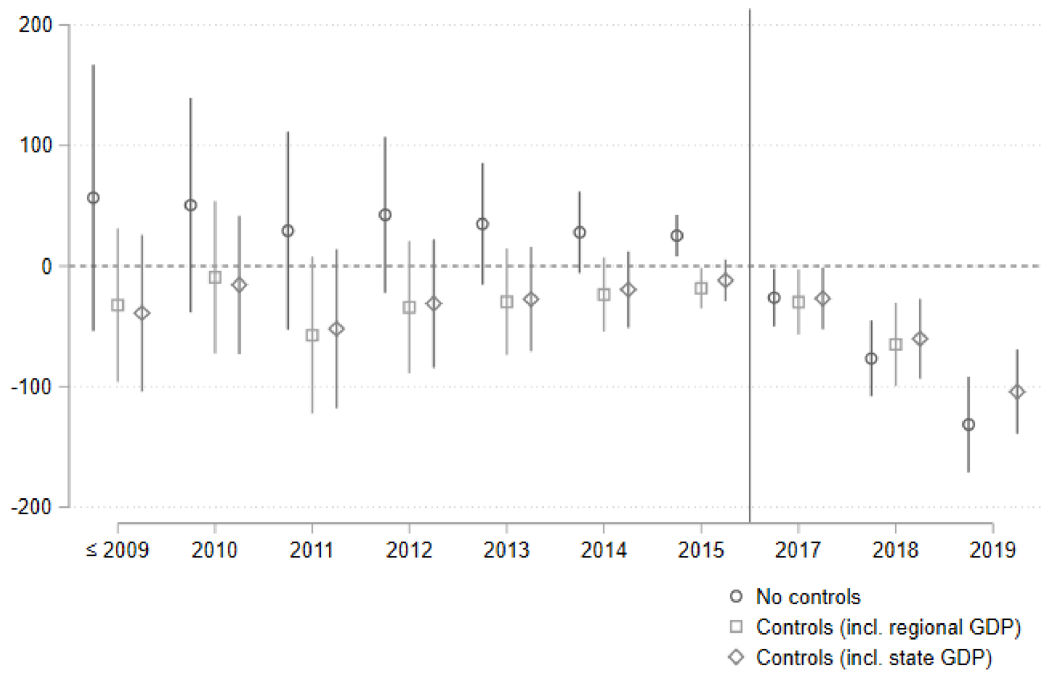


Figure 1.4.: **Dynamic Effects of Foreign Total Numbers in German Institutions of Higher Education.** Foreign students include all students without German citizenship. The data cover the period between 1998–2019, years until 2009 being included in the first estimate. The year before the policy change (2016) serves as benchmark. Treated institutions are public and located in the state of Baden-Württemberg, all others serve as control group. The graph pictures the estimates with a 95 percent confidence interval. Estimations include the full set of covariates: number of students, share of males, ARWU rank, public institution, previous tuition fee, share of foreigners in state, GDP per capita and unemployment rate. Specifications differ by the inclusion of log GDP per capita as controls. Regional controls refer to NUTS 2.
Data source: Statistisches Bundesamt (Destatis), 2020b.

Enrollment Rates

Under parallel trends, estimates of β in Eq. (2.1) represent the effect of fees on international enrollment rates. As no other state introduced such a policy, their institutions and private ones in Baden-Württemberg form a natural control group.

Table 1.2 summarizes regression results in separate panels for foreign and international students, listing the results for the federal and states' data respectively. The estimates indicate a highly significant drop in foreign enrollment rates in Baden-Württemberg after tuition fees were introduced for international students. Despite

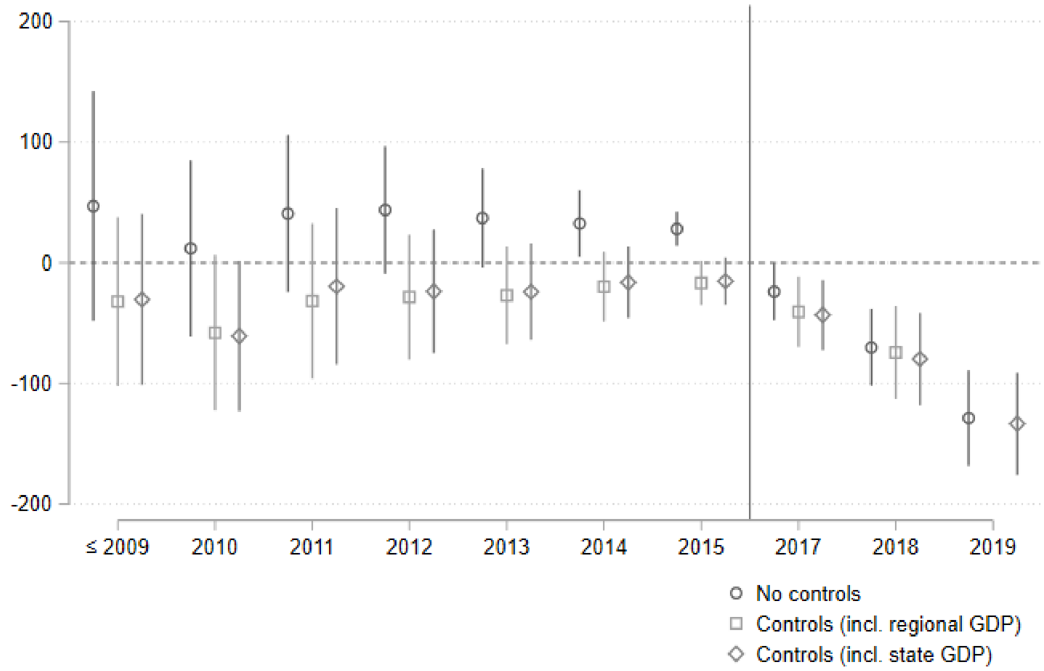


Figure 1.5.: **Dynamic Effects of International Total Numbers in German Institutions of Higher Education.** International students include all students from outside of Europe. The data cover the period between 1998–2019, years until 2009 being included in the first estimate. The year before the policy change, 2016, serves as benchmark. Treated institutions are public and located in the state of Baden-Württemberg, all others serve as control group. The graph pictures the estimates with a 95 percent confidence interval. Estimations include the full set of covariates: number of students, share of males, ARWU rank, public institution, previous tuition fee, share of foreigners in state, GDP per capita and unemployment rate. Specifications differ by the inclusion of log GDP per capita as controls. Regional controls refer to NUTS 2.
Data source: States' statistical offices, 2020.

the use of a different, and much smaller, sample, estimates are similar for both foreign and international students.

This finding also holds true when only considering public institutions in the last two columns of each panel. Charging tuition from international students is associated with a decrease in their share of the student body by about 2 percentage points in the short run. For all specifications, estimates are similar and highly statistically significant.

In line with Fig. 1.2 and 1.3, the enrollment rates of treated students are declining over the sample period, whereas the descriptive statistics and Fig. 1.1 show that they are increasing on average in Germany throughout the period. The insignificant and sometimes negative estimates for *rank* are contrary to previous findings in the literature, but may be absorbed by other covariates or reflect the relatively high international enrollment rates across Germany.[19] Reforms, which reduced the years of schooling to twelve years (*G-8 Abitur*), temporarily increased the enrollment of native students in a state the same year or prior. These reforms do not affect the share of foreign and international students at statistically significant levels. An increasing number of foreign high school graduates may explain why these coefficients remain positive. Lastly, Table 1.2 highlights preferences for stronger economic and more culturally diverse regions for students' enrollment decisions.

Geographical Distribution of Changes in Enrollment Rates

As one may expect, international students react to financial incentives. The states' data provide an opportunity to consider continents of origin and assess whose behavior the policy affects, and how. Unfortunately, more detailed data on countries were not available for all states considered. Although continents are obviously a crude measure, as they may ignore differences across countries within the same continent such as trade links and colonial past, they also provide clearly and exogenously defined groups for comparison by difference-in-differences. This allows me to examine heterogeneity in the effects of international fees on confined groups by place of origin.

Table 1.3 lists the changes in continental shares upon the policy change in tuition. Controls are not reported here. According to the underlying hypothesis, shares of the student population follow similar trends over time. Shares for international students do not differ at treated institutions in Baden-Württemberg, as indicated by *Tuition fee*. None of the continents experienced a statistically significant divergence in the *Post* period in control states.

The claim for lower enrollment rates due to the policy cannot be rejected for Africa, the Americas and Asia. Coefficient estimates indicate a drop by 0.4 percentage points

[19] A potential confounder is the non-consideration of the prestigious Humboldt University in Berlin in the ranking. Enserink (2007) offers insights on the underlying quarrel.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Foreign students				International students			
Tuition fee	0.010	-0.014	0.11***	0.12***	0.0031	-0.011	0.044***	0.050***
	(0.02)	(0.03)	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)
Post	0.072***	-0.013	0.075***	0.023	0.063***	0.0024	0.063***	0.019
	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)
Tuition fee × Post	-0.024***	-0.017***	-0.023***	-0.013***	-0.023***	-0.018***	-0.022***	-0.018***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Number of students (in thousands)		-0.0015***		-0.0016**		-0.0012***		-0.00091**
		(0.00)		(0.00)		(0.00)		(0.00)
Public institution		0.028**		0.17		0.014		0
		(0.01)		(0.67)		(0.01)		(.)
ARWU rank (/1000)		0.0031		-0.011		-0.0034		-0.017
		(0.01)		(0.01)		(0.02)		(0.01)
Share of males		0.019		0.00086		0.049		0.013
		(0.03)		(0.03)		(0.03)		(0.02)
Previous tuition fee		0.0050**		0.0031		-0.00098		-0.0024
		(0.00)		(0.00)		(0.00)		(0.00)
Year before G-8 Abitur in state		0.0039		0.0024		-0.0017		-0.00071
		(0.00)		(0.00)		(0.00)		(0.00)
G-8 Abitur in state		0.0018		0.0014		0.00038		0.00028
		(0.00)		(0.00)		(0.00)		(0.00)
GDP per capita (in log)		0.043		-0.021		0.027		0.039
		(0.06)		(0.06)		(0.06)		(0.06)
Unemployment rate		-0.0057***		-0.0067***		-0.0067***		-0.0052***
		(0.00)		(0.00)		(0.00)		(0.00)
Share of foreigners in state		1.06***		0.62*		0.86***		0.35
		(0.32)		(0.37)		(0.31)		(0.34)
Constant	0.075***	-0.48	-0.024***	0	0.027	-0.35	-0.018***	-0.46
	(0.02)	(0.68)	(0.00)	(.)	(0.02)	(0.66)	(0.01)	(0.61)
Only public institutions			X	X			X	X
n	698	679	403	387	466	461	253	251
N	10,581	9,708	7,250	6,537	6,921	6,481	4,602	4,259

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.2.: **Panel Regression Results for Foreign and International Enrollment Rates.** The columns show the estimates for foreign enrollment rate (columns 1-4) and share of international students (columns 5-8) on a set of independent variables. Students with citizenship other than German are considered as *foreign*, while *international* students include those from outside of Europe. *Tuition fee* refers to public institutions in Baden-Württemberg, which introduced tuition fees for international students in 2017. Observations cover the years 1998–2019. Columns 3, 4, 7 and 8 only consider public institutions.

for African students and a (less statistically relevant) decline by 0.2 percentage points from Americans. The impact is strongest for Asians, whose post-treatment enrollment share fell by about 1.1 percentage points. The estimates are approximately equivalent to a decrease by 37.7 percent (total international share), 59.2 percent (African share), 20.3 percent (Americas) and 29.9 percent (Asia).[20]

European students are not subject to the policy and thus are unresponsive to the policy introduction, thereby providing further credibility of the policy's impact. The absence of effects for Oceania and Other (i.e. unknown origin and stateless students) likely reflects the small sample sizes or greater variance in enrollment rates, which is supported by the panels in Fig. 1.6. These illustrate the per-year changes in reference to the year of 2016, i.e. before the introduction of tuition fees. The graphs align with the significant changes found in Table 1.3, except for the Americas. None of the per-year estimates is statistically significant after tuition was charged, whereas pre-trends diverge significantly from 2016 on, thus casting doubt on a particular impact of the policy on American students.

The larger impact of tuition on international students could potentially be explained by a gravity model, wherein students migrating at higher social costs prefer less financial costs (Bessey, 2012; Beine et al., 2014). As the education in a foreign country comes with higher social costs, students may aim to study at lower financial costs. Thus, tuition fees may provide an important disincentive if students have weak location preferences.

For the state of Baden-Württemberg, non-accessible data by country (with the exception of China and selected European countries) prevent a more detailed analysis on compositional effects. Over the post-period, the total number of Chinese students increased by exactly 17, which contrasts the increasing numbers elsewhere. This may

[20] Anecdotal evidence shows a drastic decline in the African share in Heilbronn, Karlsruhe and Reutlingen. See https://www.stuttgarter-zeitung.de/inhalt.studiengebuehren-fuer-auslaendische-studierende-studenten-aus-afrika-bleiben-weg.73e213fb-4a51-4806-90e8-b6e100974e31.html and https://www.forschung-und-lehre.de/lehre/einige-hochschulen-leiden-unter-studiengebuehren-1240/.

	(1) Africa	(2) Americas	(3) Asia	(4) Europe	(5) Oceania	(6) Other
Post	-0.017	0.0065	0.014	0.0054	-0.00056	-0.00059
	(0.01)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)
Tuition fee	0.0029	-0.0012	-0.013	-0.0068	0.00033	0.00012
	(0.00)	(0.00)	(0.02)	(0.01)	(0.00)	(0.00)
Post × Tuition fee	-0.0038***	-0.0018*	-0.011***	-0.0031	-0.00014	-0.00034
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
n	461	461	461	461	461	461
N	6,481	6,481	6,481	6,481	6,481	6,481

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.3.: **Changes in Continental Shares of Total Student Populations of Institutions.** *Tuition fee* refers to public institutions in Baden-Württemberg, which introduced tuition fees for international students in 2017. All estimations include the full set of covariates: number of students, share of males, ARWU rank, public institution, previous tuition fee, share of foreigners in state, GDP per capita and unemployment rate.

be of particular importance. As noted above, the enrollment of young Syrian refugees should not be relevant as they are exempt from paying tuition.

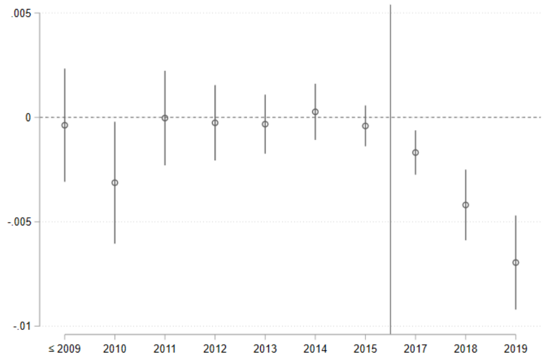
Failure Rates of Final Exams

As pointed out previously, Baden-Württemberg's government claimed to improve the performance of international students by making them pay for education. To test this claim, I apply Eq. (2.1) with the failure rate of foreign students in final exams before graduation as the dependent variable. Typically, these include bachelor and master theses.

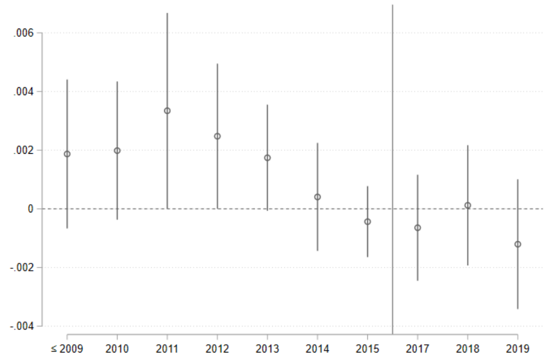
Facing a switch from no to any fee may provide students with an incentive to change behavior such as dropping out, switching to a non-fee institution or accelerating graduation.[21] Unfortunately, there are no publicly available data on drop out rates by institutions, which means that only students approaching graduation are present in the data.[22] Also keep in mind that the federal sample used includes only information

[21] Bietenbeck et al. (2020) find that mid-2000s tuition fees accelerated the time of graduation for enrolled students, but decreased the overall rate of enrollment.

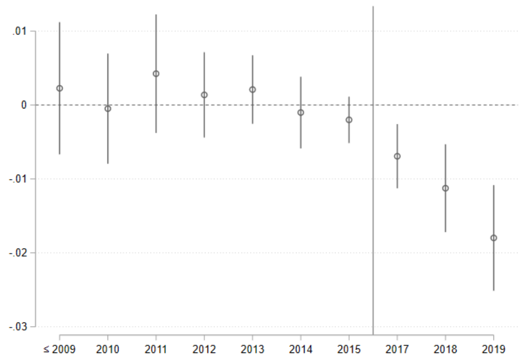
[22] Dropout rates are particularly high for international bachelor students. Most recent numbers for 2014 reveal that almost half end their studies without a degree, whereas up to 30 percent of Germans do so. For master programs, dropout rates for 2016 are 26 percent for internationals and



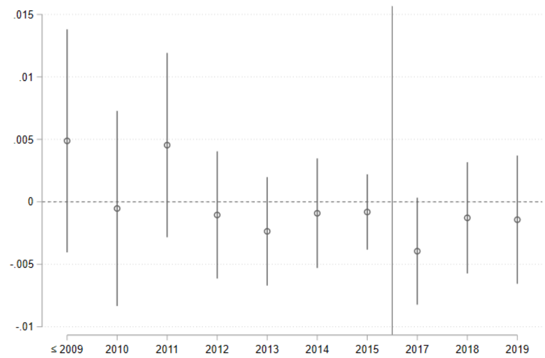
(a) Africa



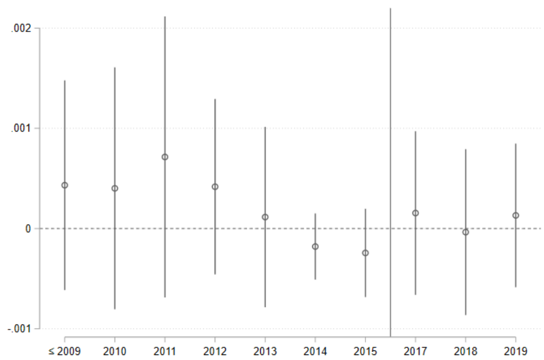
(b) Americas



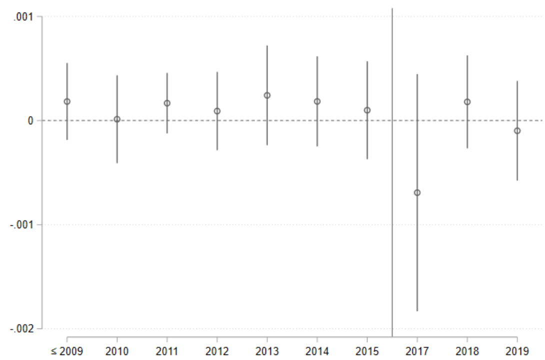
(c) Asia



(d) Europe



(e) Oceania



(f) Other

Figure 1.6.: **Event Studies for International Enrollment Rates in Baden-Württemberg by Continents.** The horizontal reference line at zero represents values for 2016. All estimations include the full set of covariates: number of students, share of males, ARWU rank, public institution, previous tuition fee, share of foreigners in state, GDP per capita and unemployment rate.
Data source: States' statistical offices, 2020.

on *foreign* students. All institution-year pairs with at least one observation are considered, whereas those with no specific value on final exams are treated as missing.[23]

Recall that the descriptive statistics in Table 1.1 show that foreigners are slightly more at risk of failing final exams. Fig. 1.7 indicates an initial increase and subsequent decline in foreign students' failure rate since the implementation of tuition fees, though this is not statistically different from the reference or prior years. The deviation from zero for foreign students' failure rates at Baden-Württemberg's public institutions in 2009 signifies that not all variation is picked up by the model.

The difference-in-differences specification, which pools over all periods, provides inconsistent results on average during the post-implementation period. Estimates are listed in the first two columns of Table 1.4. If controls are included, the interaction term becomes statistically significant, albeit marginally at the ten percent significance level. In this sense, foreign students are less likely to fail their final exams at treated institutions in the post-period, in line with the state government's claims.

However, the event study graphs point towards a more relevant change after 2012, not being causally related with the discussed imposition of a tuition fee. In fact, the event study estimates suggest that subsequent to the introduction of tuition fees, the share of failed final examinations of foreign students increases at first, but then decreases again. A possible explanation for this pattern could be that already enrolled students, attempting to avoid payments by graduating early, increase the failure rate temporarily.

Apart from such short-run consequences, this finding suggests that a sizable improvement in international students' performances, as anticipated by the state government at the time of implementation, has yet to realize. So far, there is no strong empirical support for the state's claim that the raised funding leads to a quality effect.

17 percent for Germans. The rates are similar across continents, with the exception of remarkably high dropout rates of up to 46 percent of African master students (Heublein and Hutzsch, 2021, p. 55).

[23] If there is any value reported for an institution in a specific year, I assume that the unreported numbers of passed and failed exams are zero.

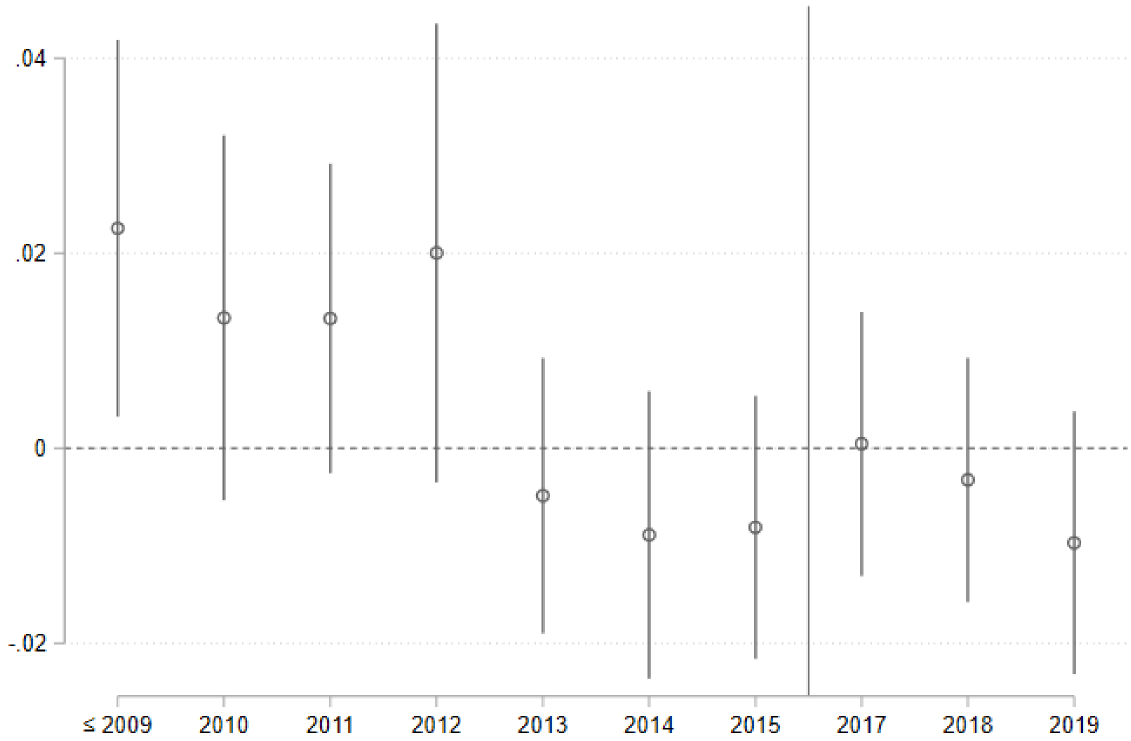


Figure 1.7.: **Dynamic Effects of Foreign Students' Failure Rate in Final Examinations.** The data cover the period between 1999–2019 with the excluded year of 2016 as the reference line. Treated institutions are located in the state of Baden-Württemberg, all others serve as control group. The graph pictures the estimates with a 95 percent confidence interval. All estimations include the full set of covariates: number of students, share of males, ARWU rank, public institution, previous tuition fee, share of foreigners in state, GDP per capita and unemployment rate.
Data sources: Statistisches Bundesamt (Destatis), 2020b; Statistisches Bundesamt (Destatis), 2020a.

One potential explanation could be that the fees discourage foreign students despite of their quality without an impact on student performance. In addition, the reallocated money per student (300 euro) may be inadequate for quality improvement. Given the decrease in international student rates altogether, less funds are raised and spent than anticipated, and thus do not advance institutional support. Alternatively, international students need to spend more hours working to cover the tuition than prior cohorts. In that sense, learning conditions differ for pre- and post-periods, which could obscure a potentially positive quality effect as well. For a better understanding, individual-level data analysis is necessary to evaluate student performances in more detail.

	(1)	(2)	(3)	(4)
	Failed exams: foreign		$\tilde{P}$	
Tuition fee	-0.0020	-0.0057	-0.16**	-0.078
	(0.01)	(0.01)	(0.07)	(0.08)
Post	0.034***	-0.059***	0.034***	-0.042
	(0.01)	(0.02)	(0.01)	(0.04)
Tuition fee × Post	-0.00095	-0.011*	0.0071	-0.0020
	(0.01)	(0.01)	(0.01)	(0.01)
n	615	605	284	282
N	8,104	7,759	2,201	2,139

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.4.: **Changes in Foreign Students' Final Exam Failure Rates.** Columns (1) and (2) consider the failure rate of foreign students in final exams as the dependent variable, whereas columns (3) and (4) use $\tilde{P} = ln(share)/(1 - ln(share))$. *Tuition fee* refers to public institutions in Baden-Württemberg, which introduced tuition fees for international students in 2017. The estimations in columns (2) and (4) include the full set of covariates: number of students, share of males, ARWU rank, public institution, previous tuition fee, share of foreigners in state, GDP per capita and unemployment rate.

1.5. Robustness

I use several strategies to judge the robustness of my estimates to alternative interpretations. To help assess whether the difference-in-differences estimates are causal, I generate placebo treatments in states where no fees were charged. If my results are consistent with the underlying theory, I would not expect any visible effect on these placebo treatments. It is possible that students enroll into other states' institutions after Baden-Württemberg implemented fees. Such a substitution may lead to positive estimates in other states. On the other hand, negative estimation coefficients may challenge the robustness of my findings.

No other state introduced international tuition fees. Appendix Fig. A.2.1 and A.2.2 summarize the analysis above for the other states, when data are available.[24] However, none of these non-fee states shows the same pattern as the fee state in the pre- and post-period. Bavaria, e.g., is arguably the most similar in terms of size, population, economy and political preferences to Baden-Württemberg. If anything, the post-treatment estimates imply an increase, e.g. a substitution to non-fee institutions in Bavaria.

Post-period trends are decreasing in the states of Hesse, Lower Saxony, and Schleswig-Holstein. Yet, there are pre-trends for at least one specification in each state and outcomes are nowhere near as pronounced as for Baden-Württemberg, especially for both foreign and international student shares. For average rates, only Schleswig-Holstein observes negative estimates for both foreign and international enrollment shares. These are, however, not reflected in the event studies, in contrast to Baden-Württemberg's.[25] Thus, I consider these checks as evidence that the change in tuition policy led to a drop in Baden-Württemberg's foreign and international enrollment rates.

I have also conducted a synthetic control approach, where the other institutions replicate the foreign share at treated ones. Fig. 1.8 shows the changes in foreign share over the years. As one may expect, the synthetic control continues to increase, whereas the foreign share at fee institutions levels once the policy is in place. I consider this divergence of trends, which coincides with the policy change, as further evidence for my hypotheses.

To allow for non-linearities in the share and other determinants, I replace the share with the log odds of being a foreign student. The dependent variable in each case is constructed by $\tilde{P} = \frac{ln(share)}{1-ln(share)}$. Table 1.4 indicates that there are no systematic deviations for enrollment rates and continental shares (not reported). Students' failure

[24] A similar placebo test for non-treated private institutions in Baden-Württemberg gives pre- and post-treatment estimates that are statistically equal to zero, possibly reflecting small sample sizes. Using alternative years as another placebo treatment does not challenge the findings above.

[25] As Lower Saxony is not included in the states' data, I cannot draw further conclusions here. The event study for the foreign student share does not indicate a clear negative trend.

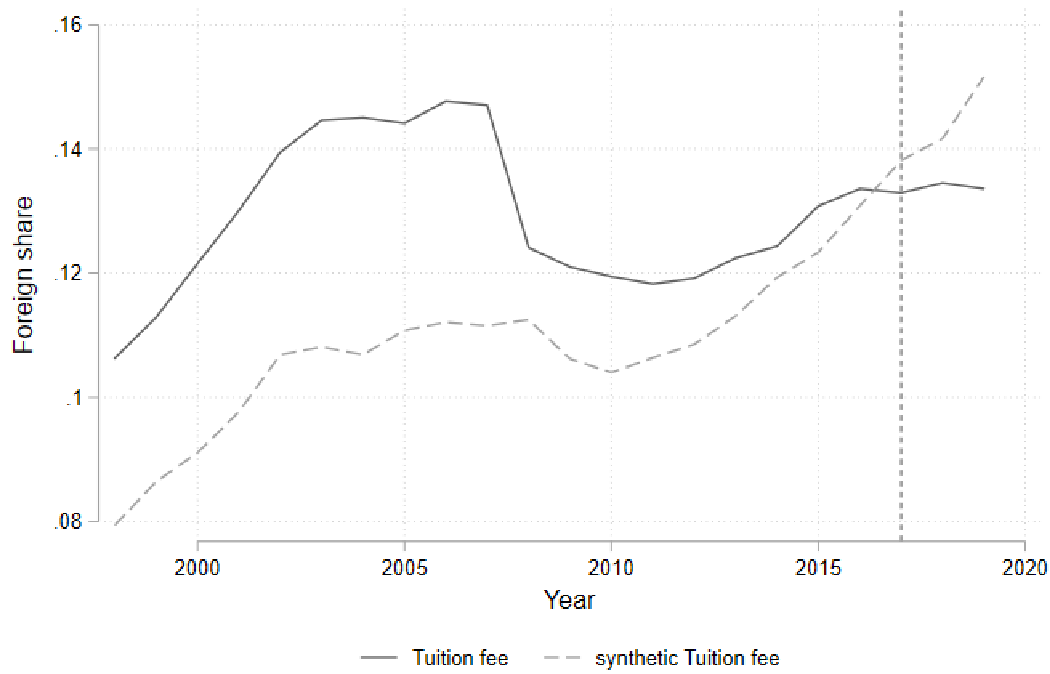


Figure 1.8.: **Synthetic Control Approach.** The figure depicts the foreign enrollment share at treated institutions (black line) and a synthetic Tuition fee, which is derived from all other institutions. The dashed vertical line represents the policy change in 2017.
Data source: Statistisches Bundesamt (Destatis) (2020b).

rates are clearly statistically insignificant, strengthening the case for no impact of the tuition fee on student performance.

1.6. Conclusion

In this chapter, I link the policy of tuition fees, affecting only international students in higher education in the German state of Baden-Württemberg, to a decrease in international enrollment rates, i.e. a price effect. My estimates are consistent with those in both Hübner (2012) and Dwenger et al. (2012), although theirs target a more narrow group. Qualitatively, my estimates are comparable to Beine et al. (2020) for Italy in the sense that there exists a negative relationship between tuition fees and enrollment shares of international students.

There are sizable differences across continents of origin in the effect of charging tuition fees on international students. The enrollment of African and Asian students exhibits the most sensitivity to the implementation of fees, which suggests that their geographical preferences are weak enough that the imposition of tuition fees causes them to substitute from Baden-Württemberg to institutions elsewhere. In contrast to the official purpose, a quality improvement has not yet translated into students' performances in their final exams, which is consistent with experiences in Denmark (Cai and Kivistö, 2013). However, it is perhaps too premature to draw general conclusions on students' overall performance. These results are robust across a battery of placebo estimations.

The findings in this paper have implications for education policy. Estimates suggest that charging only international students significantly changes the composition of the student body. Even the modest amount of 3,000 euro per year disincentivizes international students. Potentially, schools can offset the price effects by visible quality improvement to counter the found price effect. Such a quality effect would be necessary to attract the immigration of highly qualified students and to facilitate their integration. Further research is needed to assess the policy's long-term outcomes.

2. THE IMPACT OF THE JOHNSON-REED ACT ON FILIPINO LABOR MARKET OUTCOMES[†]

Immigration restrictions to the US are rather modern policies. One of the most significant policy changes, the Johnson-Reed Act of 1924, drastically limited the number of new immigrants per year, especially from Asia. In combination with the Emergency Quota Act of 1921, immigration per country was capped at 2 percent of the respective population in the 1890 census. In this paper, I examine to what extent exemptions from immigration restrictions affected relative labor market outcomes of prior migration cohorts. Using decennial census data, I apply a difference-in-difference estimation, considering that restrictions initially did not apply to the Philippines, then a US territory. My findings indicate that initial immigration restrictions impacted Filipinos that were exempt from the policy, more severely, highlighting the impact of competition on their economic assimilation. In comparison with other migrants, relative log occupation scores of Filipinos declined, while their labor force participation and employment status increased. These findings corroborate previous studies that emphasize the relevance of substitutability within immigrant cohorts. The effects are particularly strong for the year of 1930 and in California, which coincides with the timing of immigration policies and Filipinos' main destination. Individual panel data analysis partially supports the findings in the cross-sectional evaluation.

[†]I want to thank participants at the PSE Summer School on Migration, the Data-Intensive Research Conference, the MVEA Annual Meeting and seminar participants at the University of Mississippi for their valuable feedback and comments. I am also very grateful for comments from John Gardner and Marques Kitchen on earlier drafts. All remaining errors are my own.

2.1. Introduction

In recent years immigration has become a controversial topic for public and policy debate in the US and the EU. Opinions diverge on whether to restrict immigration by enhancing border security. Likewise, the debate on the paths to entry and citizenship covers almost the whole spectrum of policy initiatives—from open borders to closed doors. For the US this debate is nothing new. A century ago restrictions prevented immigration almost universally. Immigration restrictions are politically motivated, as a result, their economic ramifications are not usually the primary focus, particularly in relationship to previous immigrant cohorts that lack political representation.

My paper attempts to answer the question: Are immigration restrictions more harmful to exempt groups? I focus on the Johnson-Reed Act of 1924 (also known as Immigration Act of 1924), which established quotas permanently, thereby prohibiting immigration from Asia. The policy did not apply to the Philippines, then a US territory, which serves as the treated group with which the impact of the restrictions can be measured.[1] Only Filipinos did not face barriers to entry for intercontinental migration routes. I argue that this differential treatment at the border increases competition for Filipinos relative to other immigrants. In turn, Filipinos' labor market outcomes are more adversely affected by the immigration restrictions.

This case study can help us understand immigration policies and their impact on prior cohorts of immigrants, in particular among those most similar to recent arrivals. This study's focus on the labor substitution within a narrowly defined immigrant group may be useful to modify future policies. Modern-day exemptions often exist for refugees, highly-skilled workers and students, or family reunifications. The analysis of historical restrictions provides two additional advantages by covering the full foreign-born population and allowing to gauge the long-term consequences.

I use this natural experiment to apply a difference-in-differences analysis of the foreign-born population that considers full-count census data from 1910–1940. I also

[1] Similar exceptions also applied to citizens of independent countries in the Americas (*non-quota immigrants*), which are in consequence excluded from the analysis below. Note that colonizations were included in the quota of the colonial power.

employ panel data by tracking people across censuses, a novel matching method in this literature. In my analysis, I focus on the relative differences in labor market outcomes (log occupation score as a proxy for lifetime earnings, employment status, and labor force participation) of the entire foreign-born population in the US. In a separate step, I estimate the policy's impact on California, the main destination of Filipinos.

My results indicate an impact on labor market outcomes for Filipinos, whose immigration remained unrestricted. When compared to other immigrant groups, relative log occupation scores for Filipinos declined by 0.2 log points or about 22 percent. Likewise, Filipinos' relative employment went up by ca. 7 percentage points and relative labor force participation increased about 10 percentage points. Apart from labor force participation, the effects are more pronounced when looking at previous immigrant cohorts separately from recent arrivals. Moreover, a dynamic estimation approach shows greater effects in 1930, which is consistent with the timing of immigration policies that also restricted Filipinos in the 1930s.

I test the same hypotheses with individual-level panel data, tracking immigrants over time. Panel estimates show no significant change in either of the variables after the policy changes but align in direction with the cross-sectional estimates and mostly also in size. However, the statistical insignificance may be a consequence of the limited sample size that arises from challenges tracking people across censuses.

The policy impact on Filipinos seems to be particularly strong in California, their major destination within the US. Again, this result suggests that greater competition from incoming immigrants mostly hurts similar incumbents, also in more narrowly defined localities. This implication is robust across several checks.

The main contribution of this paper is an evaluation of unintended, secondary effects of stricter immigration regimes on immigrant workers. The less favorable economic conditions for Filipinos, who compete against new arrivals from their homeland, highlight the importance of substitutability among immigrants, a finding also documented by Ottaviano and Peri (2012). A more direct competition from the same source country translates into stronger in-group substitution. This result may also partially explain the support among modern-day immigrants in the US for more restrictive immigration policies, in particular in border regions.

My estimates also complement the literature on the effect on natives by similar policies in the US, thereby adding to the recently expanding literature on immigration restrictions. Abramitzky et al. (2019) find that the Emergency Quota Act of 1921 decreased earnings for natives in regions, which are more affected by such immigration quotas. For the same policy, population and productivity growth declined, as did marriage and fertility rates for first or second-generation immigrants, especially women. Also, white natives experienced lower earnings, whereas African Americans gained from the restrictions, and so, narrowed the earnings gap (Ager and Hansen, 2017; Xie, 2017). Greenwood and Ward (2015) assess that immigration quotas increased the length of stay and reduced emigration rates.

Similar to Massey (2016), Xie (2017), and Tabellini (2020), I focus on the Johnson-Reed Act, which had the most comprehensive, binding, and permanent consequences for immigration. Massey (2016) explores the change in the selection of Canadian migrants after the Johnson-Reed Act and assesses increased skill levels among restricted migrants. Furthermore, Xie (2017) illustrates that wages in manufacturing increased, but that restrictions also discouraged industrial development. Tabellini (2020) finds economic benefits of immigration for natives, even in sectors with large numbers of immigrants employed. Despite these positive economic ramifications, immigration generated political backlash, especially for immigrants with different beliefs and cultures more distant from the native population; examples include Catholics, Jews, and non-English speakers.

This paper also provides new insights on the inflow of immigrants from Asia with the hopes of addressing a literature gap. In particular, I consider the effects on Filipinos, who were exempt from the initial quota acts in the 1920s. Closer cultural proximity, size of immigration, and data availability may explain the relative abundance of research on transatlantic immigration. In comparison to their European and American counterparts, Asian immigrants experience larger costs for migrating. Psychological distress from xenophobia further adds to their social costs. Asian migration started primarily in the West in the early 1900s, thereby differing in their migration pattern and dynamics from European migrants. Results obtained by Massey (2016) indicate that immigration quotas had a more pronounced impact on migrants at the American west coast because changes in selection improved their average skill levels.

My focus on Asian immigration herein may also help to fill a void in the existing literature, and also offers insights into the effects of immigration to the US West. Doing so complements prior research, as markets are emerging, but also because there are differences across regions in migrant flows (Chen, 2015; Massey, 2016). A distinct evaluation for Filipinos in California adds to this strand of literature.

This paper also shows differences in labor market outcomes that may be linked to racial discrimination, given the relatively low rate of legal and permanent immigrants, in particular of non-whites. In a similar study, Chen (2015) investigates the impact of the Chinese Exclusion Act of 1882 on the skill levels of restricted and unrestricted immigrants and finds that the average occupational standing of Chinese immigrants declined.

The differential treatment across migration cohorts by the Johnson-Reed Act allows detailed analysis of the long-run effect of the quotas on both the immigrant and native populations. In this sense, immigration from Asia in the early twentieth century is of distinct interest, as the exception for Filipinos was both temporary and contrary to strict anti-Asian measures (cf. Ngai, 1999; Okrent, 2020). Understanding the Filipino experience may offer an opportunity to disentangle racial and ethnic discrimination from anti-immigration policies and the resulting reactions of affected labor market participants in future research.

2.2. Immigration Restrictions to the US in the Early Twentieth Century

2.2.1. The Path to Restricting Immigration to the US

The Page Act of 1875, which banned only Chinese women from entry, and the Chinese Exclusion Act of 1882 were the first major anti-immigration bills signed into law (Chen, 2015). Although nativist rhetoric permeated American politics, immigration remained unrestricted and thus flourished from western and northern Europe throughout the nineteenth century.[2]

Between 1890 and 1920, the share of US residents from these traditional source countries decreased from 80 to 40 percent. Countries located in southern and eastern Europe accounted for larger shares of immigrants than before, which altered the demographics of new immigrants. Their beliefs, but also cultural, linguistic, and socioeconomic differences met staunch opposition from nativist policymakers and the press (Tabellini, 2020).

Prejudicial attitudes of policymakers motivated the implementation of restrictions to enter, driven by a desire to maintain a predominantly white and Protestant population (Higham, 1988; Ngai, 1999; Okrent, 2020; Yang, 2020; Shah, 2021). Initial restrictive measures, starting in the late nineteenth century, screened individual skills. Literacy was the main concern in the public debate in regards to basic requirements for new immigrants (Goldin, 1994). Proposals to implement such legislation either failed in Congress or were ultimately vetoed by Presidents Cleveland, Taft, and Wilson. Both congressional chambers overrode Wilson's second veto and established reading and writing requirements for immigrants in the Literacy Act (also known as Immigra-

[2]The rise of the anti-immigrant movement is emblematic in the success of the anti-Catholic *Know-Nothing Party* around 1850 (cf. Alsan et al., 2020). Policies implemented to reduce the number of immigrants singled out classes, which were deemed, among others, insane, anarchist or involved in prostitution (Immigration Act of 1903), followed by disabled and ill persons (Immigration Act of 1907). More details on the legislative history of immigration to the US can be found in Hutchinson (1981). On the eugenicist motivation of immigration restrictions, see Okrent (2020).

tion Act of 1917) while barring most Asians from relocating to the US.[3] The passage of the Literacy Act marks the beginning of the end of the Age of Mass Migration (Abramitzky and Boustan, 2017).

Given the high levels of literacy by 1910, the Literacy Act is commonly deemed ineffective in mitigating European immigration (Hutchinson, 1981; Tabellini, 2020; Higham, 1988).[4] Contemporaneous assessment corresponds with this point of view as the perceived ineffectiveness of previous policies ignited the support of stricter measures. Despite being provisional, the Emergency Quota Act of 1921 drastically reduced the number of migrants allowed into the US (Tabellini, 2020). Immigration from southern and eastern Europe was largely prevented and came to a halt for Asians. However, no such restrictions were set in place for immigration from the Americas (Abramitzky et al., 2019; Ager and Hansen, 2017; Massey, 2016).

Nevertheless, ongoing public discontent and anti-immigrant sentiments demanded more comprehensive, permanent restrictions, thus leading to the Johnson-Reed Act (also known as Immigration Act of 1924), which modified previous laws.[5] Economic crises in peak immigration years may have intensified the calls for restrictive policies. The measure also had the support of white farmers and business owners in the West, who claimed to be threatened by immigration from East Asia (Goldin, 1994; Tabellini, 2020).

The Johnson-Reed Act included the Asian Exclusion Act and the National Origins Act, which adjusted the existent quotas to determine the number of immigrants allowed into the US by using 1890 as the reference year. Changing the year of reference

[3]The special status of the Philippines, a main focus of this paper, was considered in the Literacy Act and exemptions granted. Besides the Philippines, the Empire of Japan, which disallowed emigration by the informal Gentlemen's Agreement in 1907, was excluded, at least in theory. Further, the policy did not apply to Asians working in certain professions and their families (Hutchinson, 1981). As the US entered into the First World War, multiple exemptions from the Literacy Act were also granted for Mexican workers in farming, mining and railroads. Lastly, refugees were exempt from the initial Literacy Act (Okrent, 2020).

[4]Goldin (1994) offers a more detailed and somewhat challenging view on the Literacy Act and its history. Along this line, by using body height as a measure, Spitzer and Zimran (2018) find evidence for more positive selection among Italians under the Literacy Act.

[5]Coolidge (1921) offers insights into the mindset of the incoming Harding administration, distinguishing between assimilation by Nordics and others. President Coolidge himself signed the Johnson-Reed Act into law on May 24, 1924.

to 1890 essentially barred Asians from migrating to the US altogether, with the exception of Filipinos, then US nationals (cf. Okrent, 2020; Yang, 2020). Key details were left for later adjustment to implement the policy swiftly (Higham, 1988; Ngai, 1999). By 1929, quotas based on an immigrant's nation of origin capped the maximum numbers at 150,000 immigrants per year, thereby shifting the share of immigrants to the detriment of southern, eastern, but also central European countries, while favoring British immigrants (Ngai, 1999; Shah, 2021).

2.2.2. A Brief History of Filipino Americans

The history of Filipino Americans is relatively short, and almost entirely takes place in the twentieth century.

The Chinese Exclusion Act had increased the immigration from Japan because of labor shortages, the Gentlemen's Agreement with the Japanese Empire (which prohibited Japanese citizens from emigration) resulted in job vacancies in the US West. There were no restrictions of entry for immigrants, however, the transfer in colonial power from Spain to the US provided two motives for Filipinos to emigrate. Since 1901, the US Army and American teachers provided public education in English, and as a result, disseminated US-American values in the Philippines. On the other hand, the dependency on exports, coupled with the loss of life and famine in the Philippine-American war, provided push factors to emigrate (Sharma, 1984).

Filipinos settled in Hawaii (excluded for the sake of this paper) for agricultural work, while students mostly moved to California. The 1920s saw a drastic increase in Filipino immigration, with their primary occupations being concentrated in the fields of agriculture, salmon canning, and services. Furthermore, Filipinos served in the US Navy, although they had no path to citizenship until 1940 (Posadas, 1999).

Widespread participation among Filipinos in unions and the labor movement was a product of poor working conditions and discrimination in all aspects of life. Hostility against Filipino immigrants, particularly related to the opposition of intimate relations between Filipinos and white women, resulted in multiple riots in the late

1920s, culminating in the 1930 Watsonville riots. Calls for political action to mitigate discord resulted in anti-miscegenation laws and policy considerations to bar Filipinos from entry (Daniels and Kitano, 1970, pp. 66–68).[6]

A law that would ban US nationals from entry, was deemed inappropriate by many policymakers in the US and the Philippines. One way to circumvent such a moral dilemma was to grant independence, thus allowing for restrictions on immigration in return (Bonacich, 1984). According to Hutchinson (1981), legal proceedings to limit the immigration of Filipinos emerged in 1931 but were not acted on. Two years later, the Hare-Hawes-Cutting Act passed Congress but was rejected by the Philippine Senate, mostly in opposition to trade barriers.

Starting in 1934, specific restrictions for the Philippines were introduced. The Tydings-McDuffie Act (or Philippine Independence Act) drastically reduced the number of immigrants per year allowed to 50 (from almost 4,600 under open borders in 1929).[7] To motivate their return, the Filipino Repatriation Act was ratified in 1935, which allowed for free one-way travel to the Philippines.[8] The Luce-Celler Act, which was passed just two days before Philippine independence on July 4 in 1946, doubled the annual immigration quotas for Filipinos to 100 immigrants.[9]

The policies discussed herein largely remained in place until the major overhaul of immigration policies in 1965, which also abolished the reference to national origins in earlier censuses (Yang, 2020). My data exclude years later than 1940 from the analysis, thus ignoring drastic demographic changes among Filipino and international migrants.[10] According to estimates by the 2018 American Community Survey, almost

[6]In 1948, laws to prohibit interracial marriages were ultimately ruled unconstitutional by the California Supreme Court. The US Supreme Court followed in 1967.

[7]The Tydings-McDuffie Act also reclassified Filipinos in the US from *nationals* to *aliens* for immigration purposes.

[8]This act expired in 1938 with a total of 2,190 repatriates (less than five percent of the 1930 stock; Posadas, 1999, p. 24) and was considered a flop by contemporaneous officials, see http://content.time.com/time/subscriber/article/0,33009,760236,00.html. Ultimately, the US Supreme Court ruled the Repatriation Act to be unconstitutional.

[9]Under the Luce-Celler Act, Filipinos could naturalize as American citizens, which had been possible through military service since 1940. The same measure applied to Indians. The Immigration and Nationality Act of 1952 (McCarran-Walter Act) further extended eligibility to naturalization to all other Asian immigrants.

[10]Examples include the conscription of thousands of Filipinos into the US Army (of whom 10,737 were naturalized) and so-called war brides and mutual children (up to 108,000 immigrants) as well

4.1 million Filipinos and Filipino Americans live in the US, forming the third-largest group of Asian descent.

2.3. Theory

Unsurprisingly, immigration restrictions reduce the future inflow of labor into a country. Over time, this policy slows the growth of labor supply for given wages because the cost of migration increases. According to neoclassical economic theory, labor supply shifts to the left. Less competition from new arrivals allows for the bargaining of higher wages or taking more attractive job offers. In that sense, on average, immigration restrictions are expected to improve the income situation and occupational standing of prior immigrant cohorts (Abramitzky et al., 2019; Ager and Hansen, 2017).

However, it seems plausible that these implications vary for different groups in the country. Ultimately, the impact of such a labor market shock depends on assumptions made about the substitutability between native and immigrant labor, skill levels, and returns to scale (Ager and Hansen, 2017; Borjas, 2014).

In this paper, I focus on the substitutability within the foreign-born population, assuming that identical birthplaces approximate a high degree of substitutability over time. Other characteristics that determine substitutability across immigrant groups are addressed below (cf. Ottaviano and Peri, 2012). If we consider imperfect substitution between different groups of immigrants, quotas to entry improve the occupational standing of those in close competition to would-be migrants. As an example, an experienced carpenter in the Midwest from Germany may be threatened most by a similar craftsman who could undercut his wage at the same place.

In contrast to natives and other immigrant groups, Filipinos will still face competition from incoming compatriots because Filipinos face language and social barriers to employment, higher than those of e.g. Canadians; moreover, Filipinos and other Asian immigrant groups did not have a legal path to citizenship before 1946 (see fn.

as nursing graduates. None of these entered the US through the quotas (Posadas, 1999). For further information on US immigration policies after 1924, see Yang (2020).

9). According to this reasoning, greater Filipino immigration relative to Canadian immigration puts downward pressure on the *relative* wages of Filipinos. The absolute effect on Filipino wages might still be positive, but in case of occupational downgrading the results could even be negative. This rationale provides a first hypothesis to test empirically.

The impact of immigration restrictions on other labor market outcomes is less straightforward. The Johnson-Reed Act may also affect employment or labor force participation if continuous immigration from the Philippines threatens employed workers from any other country. If both groups have a high degree of substitutability, firms may prefer hiring Filipinos to reduce costs. The higher degree of competition by close substitutes may suggest for a need to find employment among Filipinos, arguing for a relative increase in their employment status and labor force participation.

There are many factors which could determine these outcomes of Filipinos. Negative selection may arise by open borders and migration networks as increased immigration may attract less-skilled Filipinos to move to the US. Open borders could motivate more Filipinos to emigrate as long as vacancies prevail and returns to skills are higher in the US than at home. An increasing number of Filipinos also establishes migration networks, lowering migration costs. In both scenarios, negative selection leads to lower average skill levels, and thus a decline in Filipinos' incomes. If such a trend exists, it should be visible in the outcomes of more recent immigrants.

Group interaction merits a closer look. In general, the native population could influence the assimilation of immigrants in two ways. First, increased immigration could provoke a backlash, in particular for groups that are culturally more distant such as the Filipinos (e.g. by religious denomination or language). Social backlash might lead to a higher degree of discrimination, less prestigious jobs, retributions against unionization among Filipinos, and social exclusion (cf. Tabellini, 2020). Secondly, assimilation and acceptance into the native society might advance over time (cf. Steinmayr, 2021).

As a result of such ongoing assimilation, incumbent migrant workers become more substitutable with natives as time goes by. However, if previous migrant cohorts remain vulnerable to being replaced by incoming Filipinos, they can prevent competition by moving away from regions where new immigrants have also moved to or by looking for better jobs. If previous immigrants respond in this way to new arrivals, the effect of differential restrictions on employment may be attenuated, which motivates a nationwide analysis (cf. Massey, 2016). In the long run, the total impact of reactions by natives and previous immigrant groups on Filipino immigration is unclear.

The Johnson-Reed Act potentially improved the relative position of restricted migrants by accelerating their rate of assimilation. In contrast, incoming Filipinos were in need of employment, which strengthens the argument for a positive effect on their employment status and labor force participation, when compared to other immigrant groups. Ultimately, the impact of restrictions on employment, and similarly labor force participation, remains an empirical question to answer.

2.4. Data, Estimation Strategy and Results

2.4.1. Data

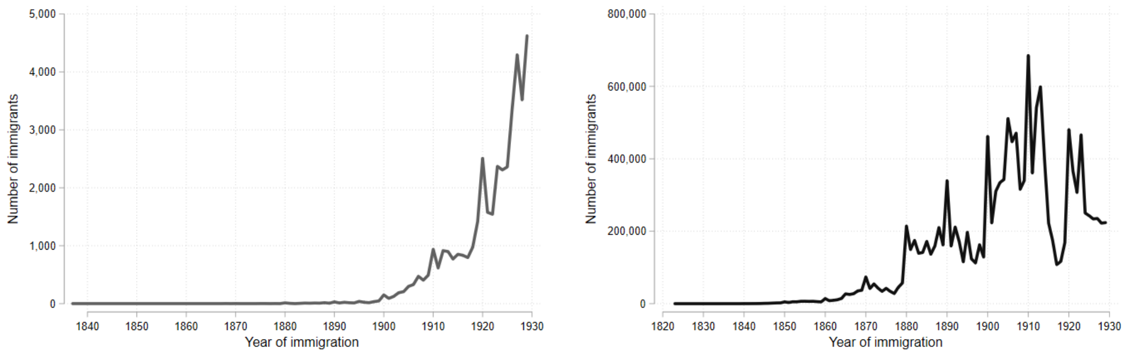
In my analysis, I focus on foreign-born men to evaluate the impact of immigration restrictions. Because the Johnson-Reed Act affects immigrant groups differently, I distinguish between Filipinos and other immigrant groups by place of birth.

I use the full-count census data from 1910–1940. Income and wage data are only recorded for 1940 onward, while employment is not included in the 1920 census. As a consequence, I focus on years for which data are available and rely on proxies for relevant outcomes. Since income is not observed, I use the occupation scores provided by the Integrated Public Use Microdata Series (IPUMS), as commonly done in the literature (Abramitzky et al., 2014; Chen, 2015; Tabellini, 2020).[11] The occupation

[11] Limited variation is a potential problem with occupation scores, especially within occupations (Abramitzky et al., 2012; Chen, 2015). Spitzer and Zimran (2018) list further issues on the variable. However, Sobek (1996) finds that the 1950 occupational income is a good approximation for 1890.

score is calculated as the median total income within an occupation in hundreds of 1950 dollars. Residents of group quarters and natural-born citizens, i.e. those who were born abroad to US-American citizens, are excluded from the data used in the estimations below. To avoid imprecise estimates, I also exclude immigrants from American countries of birth. More information on the variables used in this paper is listed in Appendix Table B.1.1.

Fig. 2.1 shows the total numbers of Filipinos and all other foreign born by year of immigration in the 1930 census, the latest to report that variable. While the number of Filipino immigrants peaked in 1929 at about 4,600, immigration in general decreased from 1910 onwards, and even more after restrictions were implemented.



(a) Filipinos (b) Others

Figure 2.1.: **Total Numbers of Foreign Born over Year of Immigration in the 1930 Census.** The figure considers both men and women of the full-foreign born population. Only years until 1929 are represented because the 1940 census does not specify years of immigration.
Data source: Ruggles et al. (2021).

Descriptive statistics, separated by an indicator on birthplace in the Philippines, are listed in Tables 2.1 and B.2.1. Table 2.1 further distinguishes between the full foreign-born population (save Alaska and Hawaii) and the sample of prime-working aged men, i.e. between 15 and 65 years old. For both population and sample, the descriptive statistics reveal an abundance of young and unmarried men among Filipinos. Their years in the US are clearly below the average of other groups. This difference coincides with a lag in year of migration, as outlined above, but may also be linked with the timing of restrictions and possibly higher return migration.

Labor market outcomes, the main variables of interest in this paper, differ between Filipinos and other immigrants. For the full population, the shares of both employed and people in the labor force are greater for Filipinos, but very close for the sample considered. In contrast, log occupation scores are lower for Filipinos than for other immigrants. Potential reasons may be the more recent arrival in the US and the younger population.

	Full Foreign-born Population				Sample			
	(1) Filipinos		(2) Others		(3) Filipinos		(4) Others	
	mean	sd	mean	sd	mean	sd	mean	sd
Male	0.91	0.28	0.55	0.50	1.00	0.00	1.00	0.00
Age	29.93	9.81	43.15	16.83	31.47	8.50	41.53	12.43
Married	0.15	0.36	0.61	0.49	0.21	0.41	0.67	0.47
Year of immigration	1919.97	9.17	1898.82	16.20	1919.40	9.22	1900.20	13.73
Years in the US	8.44	7.60	21.56	15.18	9.66	8.57	18.34	12.62
In labor force	0.86	0.35	0.56	0.50	0.93	0.25	0.94	0.23
Employed	0.79	0.41	0.48	0.50	0.85	0.36	0.80	0.40
Log occupation score	2.50	0.47	3.04	0.49	2.55	0.51	3.13	0.39
N	94,048		53,743,731		49,151		20,950,262	

Table 2.1.: **Summary Statistics of Key Variables for Foreign-born Population (Cross-sections, 1910–1940).** This table considers the full foreign-born population in columns (1) and (2); the latter two columns describe the sample used in the analysis below, i.e. men between 15 and 65 years old.

Table 2.2 reports descriptive statistics by group and census year; Appendix Table B.2.1 does so for the full population. Average ages and the percentage of married persons increase in the time span covered by the table. Labor force participation and employment status of Filipinos increase after 1920. Both variables are relatively stable for other immigrants, with a small downward trend in labor force participation. Log occupation scores temporarily fall for Filipinos, diverging from the continuous increase for everyone else.

Whether these trends can be linked empirically to the restrictive policies is at the heart of the next section.

Census year	1910		1920		1930		1940	
	(1) Filipinos mean/sd	(2) Others mean/sd	(3) Filipinos mean/sd	(4) Others mean/sd	(5) Filipinos mean/sd	(6) Others mean/sd	(7) Filipinos mean/sd	(8) Others mean/sd
Male	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Age	29.96	37.55	26.70	39.83	27.14	42.61	35.01	47.50
	12.60	12.68	7.37	12.14	6.97	11.74	7.79	10.77
Married	0.27	0.58	0.17	0.65	0.13	0.71	0.27	0.75
	0.44	0.49	0.37	0.48	0.34	0.45	0.44	0.43
Year of immigration	1898.58	1893.19	1910.81	1900.18	1921.02	1906.70	1935.00	1935.00
	11.96	13.14	7.33	11.90	7.47	12.11	0.00	0.00
Years in the US	11.42	16.81	9.19	19.82	.	.	.	.
	11.96	13.14	7.33	11.90	.	.	.	.
In labor force	0.89	0.96	0.86	0.95	0.94	0.95	0.94	0.90
	0.31	0.20	0.35	0.22	0.24	0.21	0.24	0.30
Employed	0.70	0.75	.	.	0.85	0.84	0.85	0.82
	0.46	0.43	.	.	0.36	0.37	0.36	0.39
Log occupation score	2.51	3.08	2.73	3.12	2.50	3.15	2.57	3.18
	0.67	0.40	0.57	0.38	0.51	0.38	0.51	0.40
N	688	5,490,734	2,554	5,669,227	18,968	5,495,380	26,941	4,294,921

Table 2.2.: **Summary Statistics of Key Variables for Foreign-born by Census Year.** This table considers the sample on men between 15 and 65 years of age, which is used in the empirical analysis of the paper. For the full foreign-born population, see Table B.2.1.

2.4.2. Estimation Strategy

In my baseline model, I compare Filipinos whose ability to migrate was not restricted by immigration restrictions with other immigrants.[12] This enables a difference-in-differences evaluation of the Johnson-Reed Act and subsequent policies, focusing on changes in the Philippine-born population. However, data scarcity and the rapid increase in Filipinos' employment raise concern to which extent other immigrants are a valid control group; this issue is addressed below.

The underlying assumption is that both immigrant groups, Filipino and other immigrants, follow similar trends before the restrictions come into effect. I apply the following model for estimation of various outcome variables, y_{igt}:

$$y_{igt} = \beta_0 + \beta_1 JohnsonReed_t + \beta_2 Filipino_g + \beta_3 JohnsonReed_t \times Filipino_g \\ + \epsilon_{igt}, \tag{2.1}$$

where the subscripts refer to immigrant i in group g at time t. The interaction between $JohnsonReed_t$ and $Filipino_g$ provides the main coefficient of interest, β_3, the difference-in-differences estimate for Filipinos in the post-treatment periods. The estimates can be interpreted as the relative change in status and log occupation scores between Filipinos and other immigrant groups. Following the theory outlined above, I expect a lower relative log occupation score for Filipinos, and possibly higher relative shares of employment and labor force participation.

Similar to Massey (2016), I do not include control variables in the base model. The policy changes may alter the selection of immigrants, which could also impact such controls, e.g. age and sex of immigrants, and so induce selection bias (cf. Spitzer and Zimran, 2018). Alternatively, I control for age, marital status, and state indicators. I also include indicators for census years and country of birth. All these variables offer potential determinants for substitution across demographic groups. Given likely

[12] In contrast to Massey (2016), I cannot detect the place of residence prior to migrating to the US. Before 1922, such migrants may be considered e.g. as Canadian. Hence, for the purpose of this study, I assume the place of origin as identical to one's birthplace (if not born abroad to US-American parents).

geographical autocorrelation and the large number of observations, I refrain from clustering standard errors.

2.4.3. Cross-sectional Results

Table 2.3 reports cross-sectional estimates of Eq. (2.1) for log occupation score. Odd-numbered columns refer to the unconditional model, while even-numbered columns control for age, marital status, and state of residence. Columns (1) and (2) consider the full sample, while the latter four separate this by recent and previous cohorts with five years in the US as the relevant threshold in the data. Missing information on the year of immigration explains most of the gap in observations.

According to the estimates, (log) occupation score increases over time, reflected by positive coefficients on *Johnson-Reed Act*. Throughout the specifications, Filipinos experience a lower score than other foreign-born men, which is further exacerbated after the implementation of restrictions, implied by the negative interaction terms. The full sample estimates vary between 0.17 and 0.21 log scores. The effect on Filipinos' log occupation score is sizable, translating into a relative decline by about 18.4–23.4 percent.

Overall, the absolute effect is negative for Filipinos because their log occupation score declines over time with values ranging from 0.3 to 0.6 log scores (or 35–82 percent below the control group). These numbers indicate a drastic downgrading in Filipinos' occupational ranking after the Johnson-Reed Act. These results match the economic theory outlined above that open borders for Filipinos affect their occupational standing negatively.

Generally, the two subsamples mirror the previous assessment because estimates for recent and previous cohorts are similar in absolute value. However, the inclusion of controls decreases the coefficients' magnitude, in particular for recent immigrants. A possible explanation is that recent cohorts lack place-specific human capital and are highly substitutable across immigration groups.

	Log occupation score					
	Full sample		Recent immigrants		Previous cohorts	
	(1)	(2)	(3)	(4)	(5)	(6)
Johnson-Reed Act	0.0683***	0.0790***	0.0879***	0.145***	0.0472***	0.0504***
	(0.000)	(0.000)	(0.001)	(0.002)	(0.000)	(0.000)
Filipino	-0.421***	-0.334***	-0.476***	-0.325***	-0.367***	-0.288***
	(0.013)	(0.013)	(0.022)	(0.022)	(0.015)	(0.015)
Johnson-Reed Act × Filipino	-0.205***	-0.169***	-0.221***	-0.119***	-0.235***	-0.193***
	(0.013)	(0.013)	(0.023)	(0.022)	(0.016)	(0.016)
Age		-0.000862***		0.000627***		-0.000989***
		(0.000)		(0.000)		(0.000)
Married		0.120***		0.0862***		0.110***
		(0.000)		(0.001)		(0.000)
Constant	3.099***	3.114***	3.037***	2.941***	3.110***	3.136***
	(0.000)	(0.003)	(0.000)	(0.009)	(0.000)	(0.004)
State controls	No	Yes	No	Yes	No	Yes
N	17,045,421	17,045,421	1,686,519	1,686,519	11,481,940	11,481,940

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.3.: **Repeated Cross-sectional Regression Results on Log Occupation Score.** The sample only includes prime working age men with foreign birthplace and non-American parents. Recent immigrants refer to those migrating within the last five years before each census. The pre-period includes 1910 and 1920, whereas 1930 and 1940 form the post-period.

Previous Filipino cohorts seem to be more vulnerable to the restrictions when compared to other immigrants. In that sense, the ongoing immigration from the Philippines hurts the relative standing of existing cohorts of Filipinos despite their advantage of earlier arrival. Other established immigrants do not experience such a large-scale downgrading.

The slightly smaller magnitude between recent and previous cohorts, and overlapping total effects, might argue against negative selection among Filipinos. This result may partly rely on the Tydings-McDuffie Act, which drastically restricted Filipino immigrants beginning in 1934 and is addressed in more detail in subsection 2.4.4 below.

Tables 2.4 and 2.5 follow the previous pattern for employment status and labor force participation. As I outlined in the theory section, the anticipated results are less straightforward.

For employment status, the overall share of employed men increases over the observed period (although not necessarily for recent arrivals). On average, Filipinos have a consistently lower likelihood to be employed by about 5.6–7.6 percentage points in the full sample. After the passage of the Johnson-Reed Act, there seems to be a relative increase in the employment rate for Filipinos, up to 7.7 percentage points. According to these estimates, Filipinos have a higher propensity to be employed after the Johnson-Reed Act in relative terms, whereas the absolute effect is ambiguous. Again, the impact seems to be mostly on previous cohorts, both in terms of magnitude and statistical significance, in line with the hypothesized imperfect substitutability across immigrant groups.

	Employed					
	Full sample		Recent immigrants		Previous cohorts	
	(1)	(2)	(3)	(4)	(5)	(6)
Johnson-Reed Act	0.0765***	0.0905***	0.0408***	-0.0361***	0.0961***	0.113***
	(0.000)	(0.000)	(0.001)	(0.002)	(0.000)	(0.000)
Filipino	-0.0557***	-0.0759***	-0.0420*	-0.0545**	-0.0765***	-0.0934***
	(0.018)	(0.017)	(0.025)	(0.025)	(0.024)	(0.024)
Johnson-Reed Act × Filipino	0.0766***	0.0673***	0.0655**	0.0413*	0.0880***	0.0661***
	(0.018)	(0.017)	(0.026)	(0.025)	(0.024)	(0.024)
Age		-0.00329***		0.000539***		-0.00282***
		(0.000)		(0.000)		(0.000)
Married		0.0917***		0.0132***		0.0761***
		(0.000)		(0.001)		(0.000)
Constant	0.753***	0.895***	0.777***	0.836***	0.745***	0.875***
	(0.000)	(0.003)	(0.000)	(0.008)	(0.000)	(0.003)
State controls	No	Yes	No	Yes	No	Yes
N	15,327,632	15,327,632	1,869,082	1,869,082	9,218,632	9,218,632

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.4.: **Repeated Cross-sectional Regression Results on Employment Status.** The sample only includes prime working age men with foreign birthplace and non-American parents. Recent immigrants refer to those migrating within the last five years before each census. The pre-period includes 1910 and 1920, whereas 1930 and 1940 form the post-period.

It should be noted that the estimates may also reflect the lower initial employment numbers of Filipinos in 1910, and a subsequent catching up to other immigrants. In this case, the parallel trends assumption may be violated. I address other potential issues below. Looking at labor force participation may provide better insights than for employment, given availability of data for 1920, the most recent census before the Johnson-Reed Act.

Unlike employment, labor force participation falls over time within the full sample (columns 1 and 2 in Table 2.5). The large negative estimates for Filipinos align with those on employment status, whereas the post-period estimates do not. While the estimates are largely negative for the first two rows, the interaction term is consistently positive. After the Johnson-Reed Act, Filipinos may have a greater incentive to work than before, which could explain the relative increase of around 10 percentage points on average. The magnitude of this estimate also offsets Filipinos' initially lower propensity to participate in the labor force. In contrast to previous results, the estimates are largest for recent immigrants. This may largely rely on the high participation rates among Filipinos in the pre-period, and opposing trends across groups thereafter.[13]

To summarize this first set of results: the difference-in-differences analysis finds a negative impact of immigration restrictions on unrestricted Filipinos' relative occupational standing. While Filipinos are more likely to seek and find employment, they experience a downgrading in their occupational ranking, and thus likely in incomes. A possible explanation for the relative increase of employment and labor force participation under Johnson-Reed is that the match of potential jobs declined, especially for Filipinos, and labor force participation increased out of sheer necessity. The coinciding Great Depression may lend additional support to this hypothesis, if it affected Filipinos differently than anyone else. Alternatively, with less immigration on the national level, Filipinos may face more vacancies in local labor markets.

2.4.4. Dynamic Approach

In an additional step, I take a closer look at the labor market dynamics in the post-period. This also allows for taking partial effects of the Tydings-McDuffie Act into

[13]As a robustness check, I excluded the year 1920 to mimic the lack of data for employment. The estimates in Appendix Table B.3.1 are remarkably similar, giving credence to those in the baseline model of Table 2.5.

	In labor force					
	Full sample		Recent immigrants		Previous cohorts	
	(1)	(2)	(3)	(4)	(5)	(6)
Johnson-Reed Act	-0.0234***	-0.0563***	-0.0426***	-0.152***	0.00152***	-0.00765***
	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)
Filipino	-0.0877***	-0.0832***	-0.152***	-0.123***	-0.0515***	-0.0482***
	(0.006)	(0.006)	(0.011)	(0.011)	(0.007)	(0.007)
Johnson-Reed Act × Filipino	0.0959***	0.102***	0.161***	0.122***	0.0394***	0.0401***
	(0.006)	(0.006)	(0.012)	(0.011)	(0.007)	(0.007)
Age		-0.00171***		0.00188***		-0.00145***
		(0.000)		(0.000)		(0.000)
Married		0.0686***		0.0296***		0.0596***
		(0.000)		(0.000)		(0.000)
Constant	0.953***	1.001***	0.959***	0.916***	0.952***	0.992***
	(0.000)	(0.001)	(0.000)	(0.004)	(0.000)	(0.002)
State controls	No	Yes	No	Yes	No	Yes
N	20,999,413	20,999,413	2,091,777	2,091,777	14,667,718	14,667,718

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.5.: **Repeated Cross-sectional Regression Results on Labor Force Participation.** The sample only includes prime working age men with foreign birthplace and non-American parents. Recent immigrants refer to those migrating within the last five years before each census. The pre-period includes 1910 and 1920, whereas 1930 and 1940 form the post-period.

account, which restricted immigration from the Philippines since 1934. To do so, I modify the previous equation:

$$y_{igt} = \beta_0 + \sum_{p=1}^{p_1} \beta_1^p year_t^p + \beta_2 Filipino_g + \sum_{q=1}^{q_1} \beta_3^q post_t^q \times Filipino_g \qquad (2.2)$$
$$+ \epsilon_{igt}.$$

Eq. (2.2) includes separate year indicators for the census years, i.e. $p_1 = 4$. Indicator variables for the years in the post-period and $q_1 = 2$ are interacted with $Filipino_g$. Naturally, 1910 serves as the base year.

The estimates in Table 2.6 show a steady increase in log occupation score over the years, and reversing trends for employment (which is positive) and labor force participation status (negative). On average, Filipinos fare worse than their foreign-born peers for all outcome variables. The estimation results on the interaction term reveal a greater relative impact on Filipinos' log occupation score in 1930 than in 1940 (e.g. -0.23 compared to -0.18 in column 1). This occupational downgrading is consistent with the later restrictions by the Tydings-McDuffie Act in 1934, and thus more similar treatment regardless of place of birth.

	(1)	(2)	(3)	(4)	(5)	(6)
	Log occupation score		Employed		In labor force	
1920	0.0429***	0.0358***			-0.0119***	-0.0158***
	(0.000)	(0.000)			(0.000)	(0.000)
1930	0.0764***	0.0614***	0.0875***	0.102***	-0.00662***	-0.00984***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
1940	0.104***	0.0790***	0.0626***	0.0904***	-0.0587***	-0.0564***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Filipino	-0.433***	-0.334***	-0.0557***	-0.0759***	-0.0843***	-0.0832***
	(0.013)	(0.013)	(0.018)	(0.017)	(0.006)	(0.006)
1930 × Filipino	-0.225***	-0.191***	0.0662***	0.0530***	0.0703***	0.0728***
	(0.013)	(0.013)	(0.018)	(0.017)	(0.006)	(0.006)
1940 × Filipino	-0.181***	-0.156***	0.0902***	0.0774***	0.121***	0.123***
	(0.013)	(0.013)	(0.018)	(0.017)	(0.006)	(0.006)
Age		-0.000862***		-0.00329***		-0.00171***
		(0.000)		(0.000)		(0.000)
Married		0.120***		0.0917***		0.0686***
		(0.000)		(0.000)		(0.000)
Constant	3.077***	3.114***	0.753***	0.895***	0.959***	1.001***
	(0.000)	(0.003)	(0.000)	(0.003)	(0.000)	(0.001)
State controls	No	Yes	No	Yes	No	Yes
N	17,045,421	17,045,421	15,327,632	15,327,632	20,999,413	20,999,413

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.6.: **Repeated Cross-sectional Regression Results on Labor Market Variables, Including Year Dummies (Full Sample).** The sample only includes prime working age men with foreign birthplace and non-American parents. Here, the post-period is considered by separate year dummies, and interacted with a binary variable on Filipinos.

In contrast, the relative impact on Filipinos is increasing over the years for both employment (from around 6 percentage points to 8) and labor force participation status (from 7 to 12). This means that Filipinos are more likely to seek and find employment after the Johnson-Reed Act, further increasing their likelihood to exceed the figures of other foreign born by 1940. These increasing trends are not entirely coherent with the theory because immigration by all groups covered in the sample has slowed down, and so presumably pressure to seek employment. However, it is possible that while the restrictions on Filipinos had such an impact, their absence in the early 1930s further enhanced the pressure and is not fully countered until 1940. A back-of-the-envelope calculation based on Table 2.2 implies that there were at least 7,973 Filipinos migrating between 1930 and 1940 (approx. 42 percent of the 1930 stock). According to Appendix Table B.2.2, only 838 of these entered under Tydings-McDuffie, which suggests that an alleviation of the negative impact in 1940 is possible but not guaranteed.

Separate estimations for recent and established immigrants provide more details on the impact of the Johnson-Reed Act. The estimates for previous immigrants in Table 2.7 report enhancing trends over the post-period for all variables. According to these estimates, previous immigrant Filipino cohorts continue to face downward pressure on their relative occupation scores and have a greater propensity to seek employment in 1940.

The dynamics for most recent Filipino immigrants may also support the idea of an impact of the policy change as the estimates in Table 2.8 differ from those for the whole sample and established immigrants. The estimates for 1930 largely align in size, but not in terms of statistical significance. When restrictions apply to all immigrant groups in 1940, immigrant groups become statistically indistinguishable, which suggests a high degree of substitutability among recent arrivals. In turn, this suggests that the effects in the whole sample stem from established immigrants, which includes everyone migrating in or before 1934, i.e. the year of implementation of the Tydings-McDuffie Act.

The general results, a large negative relative decline in occupation score and a small increase in employment status and labor force participation for established Filipinos, are consistent with the economic theory outlined above. The subsiding immigration of Filipinos slightly improves the relative occupational standings for recent arrivals from the same country of origin. There appears to be some degree of substitution between Filipinos and other groups, but also over time. A greater economic impact on Filipinos in 1930 reflects the change in immigration regime, which reduces the inflow from all places outside the Americas, and so, the degree of competition from

	(1)	(2)	(3)	(4)	(5)	(6)
	Log occupation score		Employed		In labor force	
1920	0.0286***	0.0237***			-0.00924***	-0.0116***
	(0.000)	(0.000)			(0.000)	(0.000)
1930	0.0633***	0.0529***	0.0961***	0.113***	-0.00378***	-0.00494***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
1940	0.0870***	0.0683***	0.0727***	0.104***	-0.0557***	-0.0501***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Filipino	-0.374***	-0.297***	-0.0765***	-0.0901***	-0.0493***	-0.0514***
	(0.015)	(0.015)	(0.024)	(0.024)	(0.007)	(0.007)
1930 × Filipino	-0.228***	-0.196***	0.0880***	0.0682***	0.0372***	0.0390***
	(0.016)	(0.016)	(0.024)	(0.024)	(0.007)	(0.007)
1940 × Filipino	-0.240***	-0.206***	0.112***	0.0922***	0.0880***	0.0899***
	(0.016)	(0.015)	(0.024)	(0.024)	(0.007)	(0.007)
Age		-0.00122***		-0.00350***		-0.00198***
		(0.000)		(0.000)		(0.000)
Married		0.117***		0.106***		0.0722***
		(0.000)		(0.000)		(0.000)
Constant	3.094***	3.152***	0.745***	0.885***	0.957***	1.009***
	(0.000)	(0.003)	(0.000)	(0.003)	(0.000)	(0.001)
State controls	No	Yes	No	Yes	No	Yes
N	15,358,825	15,358,825	13,458,466	13,458,466	18,907,552	18,907,552

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.7.: **Repeated Cross-sectional Regression Results on Labor Market Variables, Including Year Dummies (Previous Cohorts).** The sample only includes prime-working age men with foreign birthplace and non-American parents, who did not migrate in the last five years before each census. Here, the post-period is considered by separate year dummies, and interacted with a binary variable on Filipinos.

immigration. The statistically insignificant estimates for recent, restricted Filipinos support the hypothesis on imperfect substitutability. Alleviation of the Great Depression, an alternative explanation, does not fit with the continuing competition among established cohorts.

2.4.5. Challenges to Identification

Apart from the aforementioned issues arising from the lack of observations on employment status, there are several challenges to my identification strategy. The parallel trends assumption may be violated if either immigrant group changes preferences of location, reflected by states, before the policy change. Alternatively, networking could influence the labor market outcomes of immigrant groups in different ways. It is also

	(1)	(2)	(3)	(4)	(5)	(6)
	Log occupation score		Employed		In labor force	
1920	0.0329***	0.0560***			-0.0397***	-0.0463***
	(0.001)	(0.001)			(0.001)	(0.001)
1930	0.0677***	0.0460***	0.0619***	0.0727***	-0.0280***	-0.0374***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)
1940	0.182***	0.144***	-0.0463***	-0.0356***	-0.130***	-0.151***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Filipino	-0.496***	-0.325***	-0.0420*	-0.0545**	-0.127***	-0.123***
	(0.022)	(0.022)	(0.025)	(0.025)	(0.011)	(0.011)
1930 × Filipino	-0.186***	-0.122***	0.0529**	0.0457*	0.126***	0.127***
	(0.023)	(0.022)	(0.026)	(0.025)	(0.012)	(0.011)
1940 × Filipino	-0.0739*	-0.0458	-0.0429	-0.0562	0.0180	0.0168
	(0.042)	(0.041)	(0.036)	(0.035)	(0.026)	(0.026)
Age		0.000627***		0.000539***		0.00188***
		(0.000)		(0.000)		(0.000)
Married		0.0862***		0.0132***		0.0296***
		(0.001)		(0.001)		(0.000)
Constant	3.033***	2.941***	0.777***	0.836***	0.964***	0.916***
	(0.000)	(0.009)	(0.000)	(0.008)	(0.000)	(0.004)
State controls	No	Yes	No	Yes	No	Yes
N	1,686,519	1,686,519	1,869,082	1,869,082	2,091,777	2,091,777

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.8.: **Repeated Cross-sectional Regression Results on Labor Market Variables, Including Year Dummies (Recent Immigrants).** The sample only includes prime-working age men with foreign birthplace and non-American parents, who migrated in the last five years before each census. Here, the post-period is considered by separate year dummies, and interacted with a binary variable on Filipinos.

possible that the effect of the Great Depression (or other events) was heterogeneous across groups and thus influences the estimation results. Likewise, technological advance could lead to differences that are not fully picked up by the model.

Fig. 2.2 pictures the trends of key variables for Filipinos as well as other immigrants over census years by visualizing the means in Table 2.2. The solid vertical lines represent the year of implementation for the Johnson-Reed and the Tydings-McDuffie Act, i.e. policies restrictive for others and Filipinos. Given the data structure, the pre-period includes the years 1910 and 1920 (only 1910 for *Employed*). As the trend is also moving for the control group, it is essential to focus on the divergence of trends across the two groups.

For labor force participation and log occupation score, the pre-trends move in similar ways for Filipinos and the reference group. These trends change in the post-period by widening the gap to the control group for log occupation score or reversing in the case of labor force participation (and employment). The relative disruption in log occupation score is particularly remarkable, since it deviates from the roughly linear increase for other immigrants after 1924.

In contrast to the labor market outcomes, the control variables for age and marital status move in different directions before the Johnson-Reed Act. These diverging pre-trends may threaten the approach's validity, if either control itself explains a change in selection and thus captures the concurrent changes in labor market outcomes. Panel data analysis may help to address the underlying mechanisms.

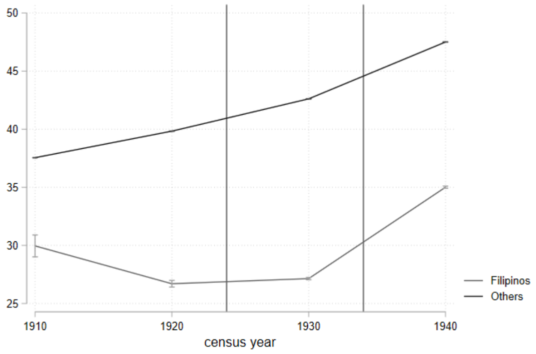
2.5. Panel Data Analysis

The panel structure may detect changes in selection of immigrants, which can be linked to immigration restrictions. In theory, implementing restrictions for most other countries, and creating vacancies on the job market, potentially incentivizes Filipinos, who would not have migrated otherwise. This again may affect selection, if the average human capital for migrants declines, i.e. quality decreases (cf. Borjas, 1985). Panel analysis may be warranted by more complete information on selection, but also because it offers more in-depth information in certain variables, e.g. year of immigration before 1930 and internal migration within the US.

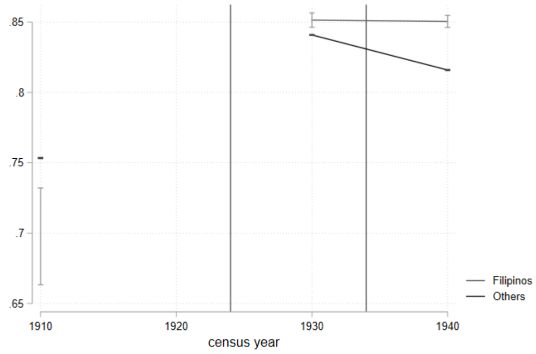
2.5.1. Set up

The following paragraphs describe the steps to create the panel data set in detail. This is to provide a clear picture to facilitate replication and highlight the decisions made in the process.

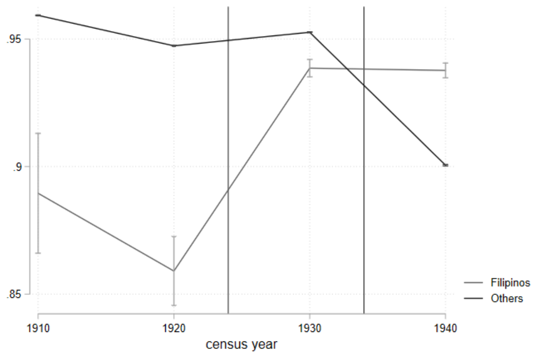
First, I use the full count data (only between 1920 and 1940) and split them into two subsamples: all foreign born in the 1920 and 1930 censuses and another one



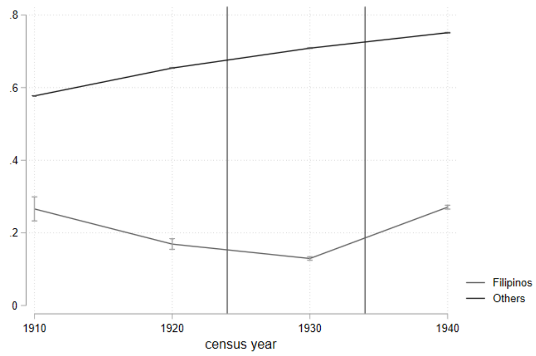
(a) Age



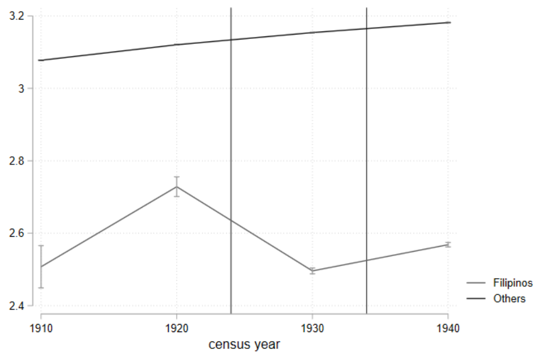
(b) Employed



(c) In labor force



(d) Married



(e) Log occupation score

Figure 2.2.: **Trends of Key Variables over Time for Different Immigrant Groups.** The graph pictures the means and the 95 percent confidence intervals derived from the sample of foreign-born men between 15 and 65 years. The vertical lines represent the passage of the Johnson-Reed Act in 1924 (restricting Others) and the Tydings-McDuffie Act in 1934 (restricting Filipinos). Note that there are no data on *Employed* in the 1920 census.
Data source: Ruggles et al. (2021).

for 1930–1940.[14] Next, I match men across the three censuses, following Helgertz et al. (2020a). Matches occur along the dimensions of the household serial number, person number, consistent historical identifier, state, age, and birthplace. Notably, the year of immigration is missing from this list. In a further step, all household members are linked, thus likely overrepresenting married men. This procedure links up to three observations for each person in the data, an unbalanced Multigenerational Longitudinal Panel (MLP) derived from Helgertz et al. (2020b). I drop observations without a historical identifier.

Concerns on the applicability of the constructed MLP are visible in Table 2.9. While the share of men is still larger for Filipinos, it is almost 20 percentage points less than in Table 2.1. Likewise, there are 44 percent married Filipinos in the panel, compared to just 15 for the cross-sections. Around 59.3 percent of foreign-born men can be matched over the three censuses, but only 7.8 percent of Filipinos, which is below the linking rates of similar historical studies (cf. Biavaschi et al., 2021, p. 7).

	(1) Filipinos		(2) Others	
	mean	sd	mean	sd
Male	0.72	0.45	0.56	0.50
Age	31.74	12.45	47.59	14.23
Married	0.44	0.50	0.82	0.38
Year of immigration	1919.44	7.84	1904.69	13.07
Years in the US	10.10	7.43	22.14	12.92
Employed	0.57	0.50	0.47	0.50
In labor force	0.61	0.49	0.53	0.50
Log occupation score	2.84	0.57	3.14	0.42
N	3,857		12,415,614	

Table 2.9.: **Summary Statistics of Key Variables for Foreign-Born Population (Panel 1920–1940).**

The divergences from the cross-sectional data in Table 2.2 are naturally present in Table 2.10, which lists the same variables for the entire foreign-born population over the years. Even more concerning is the difference in the outcome variables of the sample in Table B.2.3, labor force participation and log occupation score, whose means

[14]Only 34 Filipinos can be matched across the 1910 and 1920 data, and only 14 men of prime working age. A full panel for 1910–1940 only includes eleven Filipinos with multiple observations.

are well above the ones in the cross-sectional data. This may lead to underestimated results, and thus issues in statistical inference.

	(1)	(2)	(3)	(4)	(5)	(6)
Census year	1920		1930		1940	
	Filipinos mean/sd	Others mean/sd	Filipinos mean/sd	Others mean/sd	Filipinos mean/sd	Others mean/sd
Male	0.57	0.57	0.55	0.57	0.77	0.55
	0.50	0.50	0.50	0.50	0.42	0.50
Age	22.93	38.11	21.28	48.08	34.98	51.73
	12.11	12.95	9.34	13.07	11.37	13.22
Married	0.56	0.83	0.38	0.82	0.51	0.82
	0.50	0.37	0.48	0.39	0.50	0.39
Year of immigration	1910.19	1900.65	1919.88	1906.56	1935.00	1935.00
	8.43	11.96	7.37	13.14	0.00	0.00
Years in the US	9.81	19.35	10.12	23.44	.	.
	8.43	11.96	7.37	13.14	.	.
In labor force	0.36	0.55	0.54	0.54	0.70	0.51
	0.48	0.50	0.50	0.50	0.46	0.50
Employed	.	.	0.51	0.48	0.64	0.47
	.	.	0.50	0.50	0.48	0.50
Log occupation score	2.94	3.10	2.80	3.13	2.87	3.16
	0.57	0.42	0.57	0.42	0.57	0.42
N	120	2,329,899	2,005	5,029,394	1,732	5,056,321

Table 2.10.: **Summary Statistics of Key Variables for Foreign-born Population by Census Year (Panel 1920–1940).**

2.5.2. Panel Results

Table 2.11 lists the results from the panel regressions. Note that the lack of employment figures for 1920 prevents a closer look for this variable, as there is no observation in the pre-period.

The estimates on log occupation scores correspond for the panel and the repeated cross-sections (in Table 2.3) in direction and, apart from the interaction term, mostly in magnitude. While the estimates imply a decline for Filipinos' occupation scores in the post-period by about 0.04–0.05 log scores, there is not enough variation for statistical robustness. If there are significant changes on log occupation scores after the implementation of the Johnson-Reed Act, as Table 2.3 suggests, these did not primarily affect Filipinos in the panel data set.

For labor force participation, the results on timing and Filipinos are somewhat similar to previous cohorts in Table 2.5 for the post-period and Filipinos. Both estimates have the same direction, and are of low magnitude. This may partially be

	(1)	(2)	(3)	(4)
	Log occupation score		In labor force	
Johnson-Reed Act	0.0313***	0.0479***	-0.0278***	-0.0363***
	(0.000)	(0.000)	(0.000)	(0.000)
Filipino	-0.232***	-0.206**	-0.0996***	0.0497
	(0.057)	(0.093)	(0.026)	(0.087)
Johnson-Reed Act × Filipino	-0.0467	-0.0430	-0.00434	0.00873
	(0.058)	(0.057)	(0.027)	(0.026)
Age		-0.00143***		-0.00169***
		(0.000)		(0.000)
Married		0.0964***		0.112***
		(0.000)		(0.000)
Constant	3.152***	3.211***	0.955***	0.805***
	(0.000)	(0.074)	(0.000)	(0.083)
State controls	No	Yes	No	Yes
n	3,583,774	3,583,774	3,884,921	3,884,921
N	5,842,574	5,842,574	7,088,910	7,088,910

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.11.: **Panel Regression Results on Labor Market Outcomes.** The estimates are for men in prime-working age (15–65 years).

so, because of the shorter time span covered by the panel and change in the year of reference. However, the interaction term on labor force participation is ambiguous in Table 2.11. At least for those Filipinos tracked over censuses, there do not seem to exist significant changes in their propensity to participate in the labor force when compared to other immigrants.

Despite the statistical insignificance for the main variables of interest, the panel estimates partially support the findings above, but emphasize the challenges of the panel approach to statistical inference. The limited numbers of Filipinos that can be tracked even before 1920 may reduce the size and composition of the sample decisively. In particular, the underrepresentation of singles by the tracking method could affect the estimation approach. Alternative linking methods, e.g. described by Abramitzky et al. (2021), suffer from the relatively low number of Filipinos that can be tracked.

Reconciling the conclusions so far, there seems to be empirical evidence for a negative impact on Filipinos' labor market outcomes by ongoing immigration. A potential explanation for the impact among migrant cohorts would be that those who migrated early retain their positions and changes do not differ from those of other immigrants. Among Filipinos, less established cohorts may face greater competition from incoming compatriots, which could explain the more pronounced impact on previous cohorts altogether. If so, a decline in the quality of human capital seems unlikely. Moreover, less established cohorts will tend to move around for jobs or return to their home country, which complicates their tracking across censuses.

2.6. Californian Labor Market

Since 1920, California has had the largest share of Philippine-born immigrants among the states. By 1940, the vast majority (65 percent) of Filipinos lived in California, which had experienced a fivefold increase of its population since the turn of the century.[15] In consequence, a more distinct analysis of the Californian labor market is warranted, also to evaluate the consistency of the results and to address potential confounders on the national level. There is a substantial literature on the distinction between local and national analyses, since internal migration could mitigate effects or spill these over to other areas. In a brief overview of this debate on labor markets (and crime), Watson and Thompson (2022) consider the impact of technological change, trade and diverging paths of immigrant groups.

2.6.1. Baseline Model for California

In this section, I consider only Californian residents of foreign origin. For the analyses below, I use this Californian sample and estimate Eq. (2.1) as above. The same limitations to data and methodology apply.

Table 2.12 presents the estimates for all outcome variables within the Californian sample, corresponding to the first two columns in each of Tables 2.3–2.5. By and large,

[15] Other population centers of Filipinos were in Illinois, New York, Oregon, Pennsylvania and Washington. However, only in California Filipinos surpassed more than 1,000 people in 1920 and increased their population share until 1940. Once again, Hawaii is not considered within this analysis.

the estimates support the previous findings that Filipino labor market outcomes were particularly affected by immigration restrictions with the absolute effect in California being of similar size than in the US. Apart from labor force participation the interaction terms, i.e. relative effects, are greater for Californian Filipinos. The relative log occupation score in California falls by more than the national average of 0.2 log points. Likewise, employment becomes more likely for Californian Filipinos after 1920 with a relative increase around 11 percentage points. For labor force participation, the estimates of ca. 0.083 in columns (5) and (6) are somewhat below those in Table 2.5, but clearly offset the group's general lower propensity.

The stronger estimates for California alone imply a greater impact of the Johnson-Reed Act on this state than elsewhere or on the national level. This finding is plausible, given its high share of Filipinos, and supports the claim for better opportunities for Filipinos. Looking at changes on the county level may offer additional insights on the spatial impact of the Johnson-Reed Act.

	(1)	(2)	(3)	(4)	(5)	(6)
	Log occupation score		Employed		In labor force	
Johnson-Reed Act	0.120***	0.0978***	0.106***	0.0920***	-0.0260***	-0.0715***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)
Filipino	-0.346***	-0.340***	-0.0452	-0.0772**	-0.0395***	-0.0383***
	(0.021)	(0.020)	(0.039)	(0.039)	(0.009)	(0.009)
Johnson-Reed Act × Filipino	-0.284***	-0.240***	0.0915**	0.109***	0.0827***	0.0838***
	(0.021)	(0.020)	(0.039)	(0.039)	(0.009)	(0.009)
Age		-0.00112***		-0.00316***		-0.00166***
		(0.000)		(0.000)		(0.000)
Married		0.204***		0.0873***		0.0552***
		(0.001)		(0.001)		(0.001)
Constant	2.948***	2.941***	0.726***	0.826***	0.939***	1.006***
	(0.001)	(0.003)	(0.001)	(0.003)	(0.000)	(0.002)
State controls	No	Yes	No	Yes	No	Yes
N	970,251	970,251	918,987	918,987	1,202,329	1,202,329

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.12.: **Regression Results on Labor Market Outcomes for California.** Only prime working age men with foreign birthplace and non-American parents. The pre-period includes 1910 and 1920, whereas 1930 and 1940 form the post-period.

2.6.2. Across counties

Apart from the state preference, Filipinos predominantly settled in only a few counties; first primarily in the Greater Bay Area, and later in Santa Barbara and Los Angeles. This pattern may emphasize the importance of networks and first settlements. To gain further knowledge on the policy impact on localities, I use the variation in Filipino shares on the county level over time with the following model:

$$
\begin{aligned}
y_{ct} = \beta_0 + \beta_1 JohnsonReed_t + \beta_2 shareFilipino_{c,1910} \\
+ \beta_3 JohnsonReed_t \times shareFilipino_{c,1910} + \epsilon_{ct},
\end{aligned}
\tag{2.3}
$$

where $shareFilipino_{c,1910}$ refers to the share of all Californian Filipinos within county c in 1910. The percentage share of Filipinos in a particular year is held constant over time. Thus, I interpret the coefficients of β_3 as the impact on the foreign-born population's log occupation score within a county that can be associated with the growth of its Filipino share. All other variables follow the definitions in Appendix Table B.1.1. Standard errors are clustered at the county level.

Given changes in Filipino shares over time, 1910 may not be convincing as more than half of Filipino migrants to California settled in San Francisco, which had just about 11 percent of their share in 1940. Over the following decades, the Bay Area lost its hegemony as the primary entry port to Los Angeles. Furthermore, the total numbers of Filipinos to California were significantly lower before 1920, and networks only emerging. Therefore, I include specifications with 1920 as year of reference for the same model, while ignoring prior years. This also serves as a first robustness check.

Tables 2.13–2.15 report regression estimates on all three main variables. Throughout the specifications, the interaction between a county's share of Filipinos and the passing of the Johnson-Reed Act only becomes highly statistically relevant for log occupation score with 1920 as the reference year. At a ten percent significance level, the probability for employment is increasing with 1910 as the reference year. The large values emphasize the relatively low shares for many county-year pairs. With the exception of the (yet insignificant) negative interaction coefficients on labor force participation for 1910, the other estimates align with the previous results and demonstrate the spatial impact on dominant Filipino localities.

	(1)	(2)	(3)	(4)
	Log occupation score			
Reference year	1910		1920	
Johnson-Reed Act	0.0705***	1.060***	0.0827***	0
	(0.020)	(0.280)	(0.020)	(.)
% Share of Filipinos in 1910	12114.9***	14193.7***		
	(42.540)	(1422.544)		
Johnson-Reed Act × % Share of Filipinos in 1910	-100.8	-92.15		
	(85.080)	(90.178)		
% Share of Filipinos in 1920			75.86***	82.23***
			(1.053)	(17.669)
Johnson-Reed Act × % Share of Filipinos in 1920			-5.488***	-5.647***
			(1.580)	(1.923)
(mean) age		0.00884		0.00203
		(0.006)		(0.006)
(mean) married		-0.180		-0.0972
		(0.144)		(0.276)
Constant	1.548***	0	2.722***	2.744***
	(0.010)	(.)	(0.012)	(0.258)
Observations	232	232	174	174

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.13.: **Panel Analysis Estimates on Log Occupation Score for Californian Counties.** The estimates are for men in prime-working age (15–65 years), following Eq. (2.3). *Johnson-Reed Act* applies to census years 1930 and 1940.

From the estimations, the 1920 Filipino share seems to impact only the counties' log occupation scores among the foreign born. To a lesser extent, this assessment pertains to employment after 1910, which may also attribute to the results in the baseline model in Table 2.3. The changes in Filipino strongholds provide a fitting explanation for these findings.

2.7. Robustness

To test the validity of my results, I run several robustness checks. First, I estimate the baseline results for other countries without temporary exemptions from immigration

Reference year	(1) Employed 1910	(2)
Johnson-Reed Act	0.0596***	0.123**
	(0.016)	(0.050)
% of Filipinos in 1910	1006.3***	-115.5
	(33.191)	(1295.418)
Johnson-Reed Act × % of Filipinos in 1910	84.34*	71.75*
	(49.787)	(41.521)
(mean) age		-0.00760
		(0.005)
(mean) married		0.0923
		(0.121)
Constant	0.556***	0.929***
	(0.009)	(0.274)
Observations	174	174

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.14.: **Panel Analysis Estimates on Employment Status for Californian Counties.** The estimates are for men in prime-working age (15–65 years), following Eq. (2.3). *Johnson-Reed Act* applies to census years 1930 and 1940. An estimation, which uses 1920 as the reference year would lack a pre-period and is thus not considered.

restrictions. Japan and China, the two largest initial source countries of Asian immigration, serve as natural comparison groups.[16] I further check for the patterns for immigrants from (otherwise excluded) Canada and Mexico, as they were the largest among the unrestricted countries throughout the entire period. For all estimations, I exclude Filipinos.

Almost all of the estimates listed in Appendix Tables B.4.1–B.4.4 are of statistical relevance, but none of the panels shows the consistency of results for Filipinos. The interaction estimates on Chinese immigrants' relative occupation score in Table B.4.1 is mostly negative, however of small magnitude, when compared within the foreign-born population. In contrast, Table B.4.2 reports a positive relative growth for the same variable. Both estimates differ from the results for Filipinos, as one would predict. It is possible that the earlier introduction of immigration restrictions on China have substantial ramifications on the sign and size of the estimates. However, in line with the large relative impacts on Filipinos found above, I consider these

[16] The data continue to include only years until 1940, which means that revisions of the Page Act and the Chinese Exclusion Act have not had materialized yet.

	(1)	(2)	(3)	(4)
	Labor force participation			
Reference year	1910		1920	
Johnson-Reed Act	-0.0381***	0.0195	-0.0532***	0
	(0.012)	(0.030)	(0.014)	(.)
% Share of Filipinos in 1910	432.8***	159.6		
	(24.382)	(1122.218)		
Johnson-Reed Act × % Share of Filipinos in 1910	-6.474	-10.26		
	(48.764)	(45.690)		
% Share of Filipinos in 1920			5.273***	2.974
			(0.421)	(10.518)
Johnson-Reed Act × % Share of Filipinos in 1920			0.688	0.610
			(0.631)	(0.831)
(mean) age		-0.00599		-0.00281
		(0.004)		(0.004)
(mean) married		0.00789		0.0303
		(0.102)		(0.158)
Constant	0.855***	1.104***	0.897***	0.966***
	(0.006)	(0.233)	(0.009)	(0.173)
Observations	232	232	174	174

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.15.: **Panel Analysis Estimates on Labor Force Participation for Californian Counties.** The estimates are for men in prime-working age (15–65 years), following Eq. (2.3). *Johnson-Reed Act* applies to census years 1930 and 1940.

estimates as support for my previous results that a reduction of similar workers over time benefits incumbent immigrant workers of the same group.

In contrast, the estimates differ for unrestricted Canadians and Mexicans, at least in terms of log occupation scores and employment status. Estimated relative occupation scores are positive for Canada, whereas they are negative for Mexicans. Apart from employment status, the estimates for Mexicans mirror the outcomes for Filipinos, however they are smaller in magnitude, perhaps because back-and-forth migration was less expensive for Mexicans.

Apparently, Canadians were able to avoid occupational downgrading after the Johnson-Reed Act, but Mexicans were worse off. The impact on Canadians' employment status and labor force participation is minor. When compared within the foreign-born population, the additional effect of the Johnson-Reed Act is small, but of similar sign for log occupation score and labor force participation. The estimates on employment status are ambiguous.

Overall, although the direction and size of the estimates differ across these immigrant groups, they are in line with the economic theory presented. If Canadians are better substitutes for the native population than other immigrants, they might not face the same degree of competition from their countrymen as Filipinos and Mexicans. The same native language, similar denominations, and presumably less discrimination would argue for a high degree of substitutability between Canadians and US Americans. Back-and-forth migration and a more negative migration selection among Mexicans provide further potential explanations.

Until now, I have focused on the average effects for immigrant groups by census years. Additionally, I can test the impact of immigration restrictions by year of immigration. Again, the Johnson-Reed Act distinguishes the treated from the untreated group. Filipinos immigrating after 1924 are compared with their peers in the same immigration cohort. In other words, Filipinos receive the treatment of open borders after 1924 in this scenario. Note, that missing data for immigration years between 1930 and 1935 limits the analysis as well as a comparison with the results above.

The relevant estimations can be found in Appendix Table B.5.1. It is apparent, that the estimates are smaller than those in Tables 2.3–2.5, and even negative for employment status without covariates. While the restrictions largely affect labor market outcomes of those migrating under the stricter policies, there is also a sizable impact on Filipinos entering without restrictions, especially for log occupation scores. As I have found before, the total effect is not only due to changes in the more recent years within the sample but emphasizes that prior Filipino cohorts also had to adjust.

2.8. Conclusion

This paper discusses immigration restrictions and their influence on immigrant groups. The differential treatment of Filipinos, who were US nationals in the early twentieth century, allows me to apply a difference-in-differences approach to estimate the policy's effect on their relative labor market outcomes.

Using repeated cross-sectional census data between 1910–1940, I find support for the hypothesis that immigration restrictions did in fact affect Filipinos' labor market outcomes. In comparison to other immigrants, Filipinos' log occupation scores declined by about 0.2 log points (or 22.1 percent) after restrictions were in place. My estimates for employment status and labor force participation suggest that both increased among Filipinos by around 10 percentage points after the passage of the Johnson-Reed Act.

One possible explanation is that as fewer immigrants entered the US, it became easier for Filipinos to fill job vacancies and join the labor force. The estimated effects are similar for both recent and previous cohorts. According to the results, previous Filipino cohorts were slightly more affected, which may imply that they assimilated more slowly. These estimates are only suggestive because greater changes for Filipinos may also reflect the lower initial values in the early twentieth century. The estimates for recent cohorts could also be mitigated by subsequent restrictive policies on Filipinos, which is consistent with further estimations. Negative selection seems unlikely for both reasons.

Panel data analysis suggests that those Filipino immigrants who can be tracked across census years before and after the passage of the Johnson-Reed Act are unaffected. As in the cross-sectional analysis, the effect on Filipinos' relative log occupation scores is negative; however, the sign of the estimates on labor force participation is unclear. Generally, the estimates are small and statistically insignificant. Caution is advised because the smaller sample size or significant differences due to the matching method may render these estimates imprecise. In combination with the results from the cross-sectional analysis, this finding may suggest that the brunt is largely borne by less established Filipino immigrants.

The policy effects are particularly strong in California, the traditional destination of Filipinos within the US, and even more so in counties with a high share of Filipinos in 1920, the most recent year before the policy change. In general, these findings are consistent with estimates on the full sample.

The results in this paper highlight the importance of immigration restrictions on the economic outcomes of existing and potential migrants. Furthermore, my findings suggest imperfect substitutability of workers from different source countries, providing evidence on its importance in immigration policy analysis. This suggests the needs of public policy to address groups who might be disproportionally affected by restrictive policies. Further research is needed to investigate the mechanisms.

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Appendix

A. Appendix to Chapter 1

A.1. List of Campuses Merged

1. Federal Statistical Office
    - BTU Cottbus-Senftenberg
    - Filmuniversität Babelsberg Konrad Wolf
    - Hochschule Lausitz
2. States' statistical offices
    a) Baden-Württemberg
        - Allensbach Hochschule
        - Duale Hochschule Baden-Württemberg Mosbach, Heilbronn
        - Evangelische Hochschule Ludwigsburg, Reutlingen
        - German Graduate School of Management and Law Heilbronn
        - Hochschule Fresenius Heidelberg
        - Hochschule Macromedia, Freiburg
        - Internationale Hochschule Calw
        - Karlshochschule International University
        - Karlsruher Institut für Technologie
        - SRH Hochschule Heidelberg
        - Technische Hochschule Ulm

b) Bavaria

- Hochschule für angewandte Wissenschaften Coburg

c) Berlin

- Hochschule für Medien, Kommunikation und Wirtschaft
- SRH Hochschule der populären Künste

d) Hamburg

- Berliner Technische Kunsthochschule, Campus Hamburg

e) Hesse

- Hochschule Darmstadt

f) North-Rhine Westphalia

- Europäische Fachhochschule
- Fachhochschule Südwestfalen
- Praxis-Hochschule

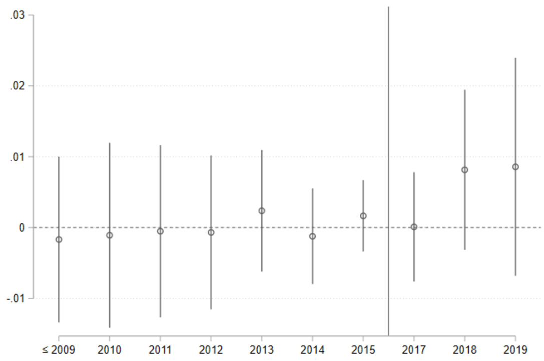
g) Schleswig-Holstein

- Technische Hochschule Lübeck

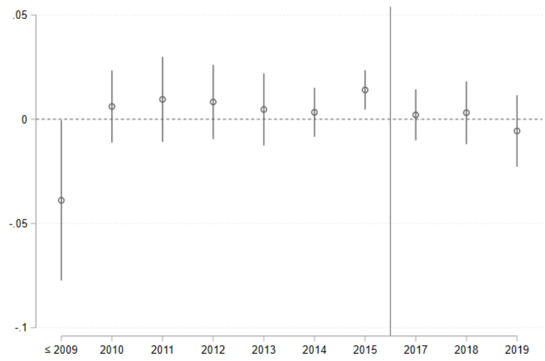
h) Thuringia

- Adam-Ries-Fachhochschule

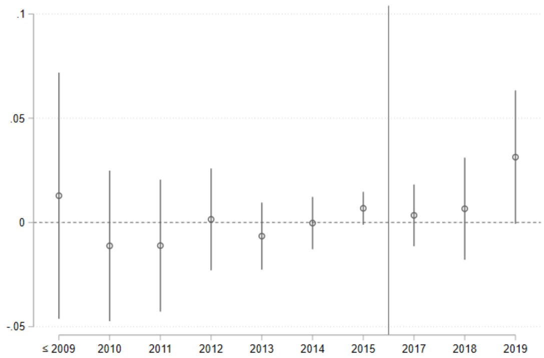
A.2. Placebo Tests for Foreign Share in Other States



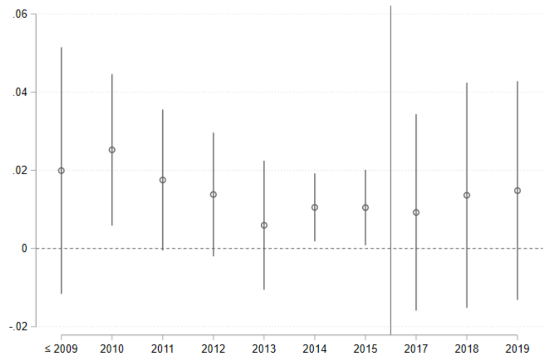
(a) Bavaria



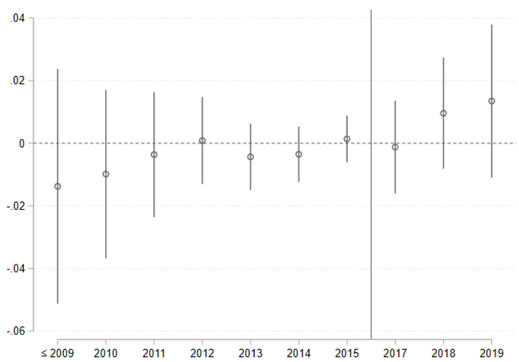
(b) Berlin



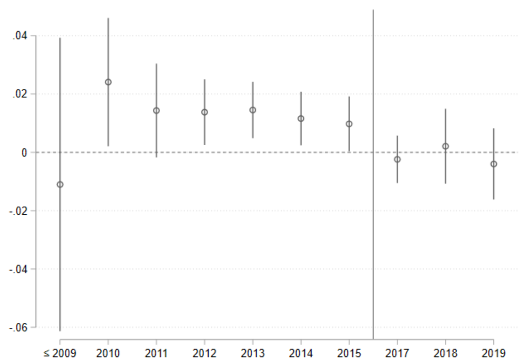
(c) Brandenburg



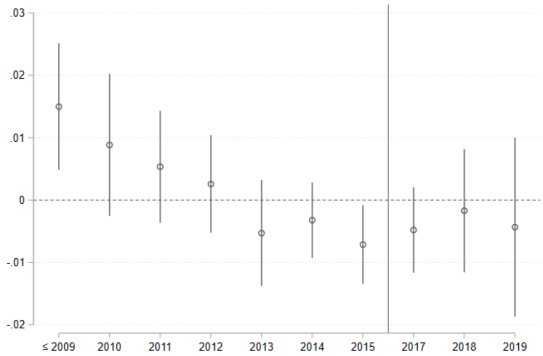
(d) Bremen



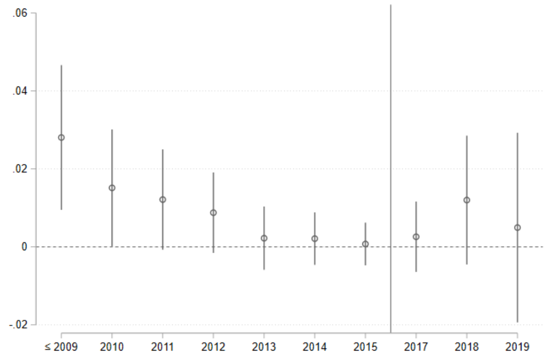
(e) Hamburg



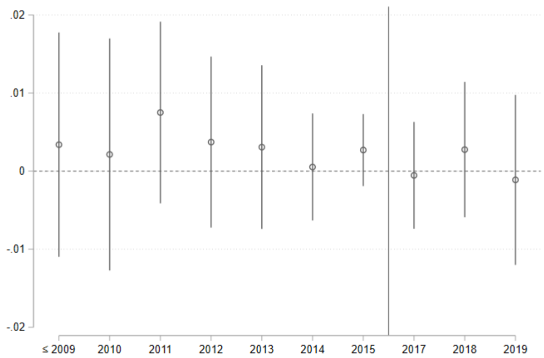
(f) Hesse



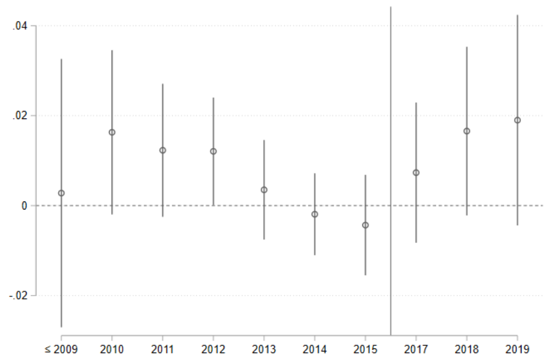
(g) Lower Saxony



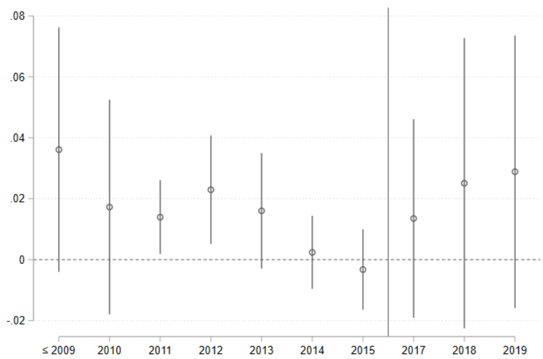
(h) Mecklenburg-Western Pomerania



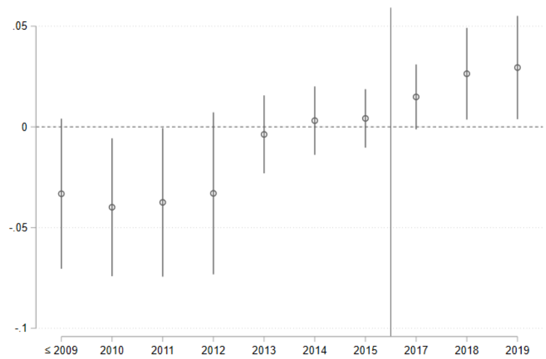
(i) North-Rhine Westphalia



(j) Rhineland-Palatinate

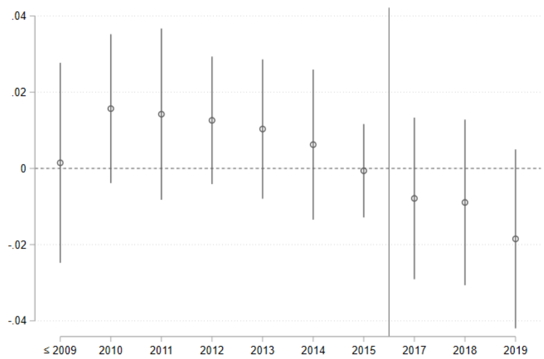


(k) Saarland

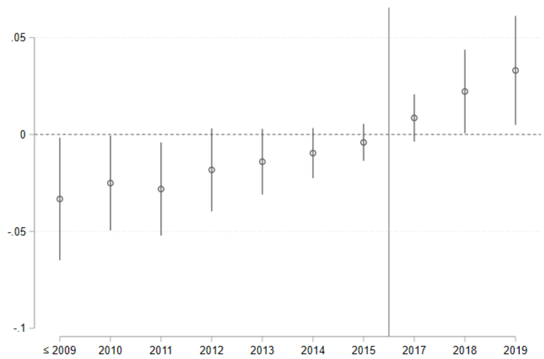


(l) Saxony

(m) Saxony-Anhalt

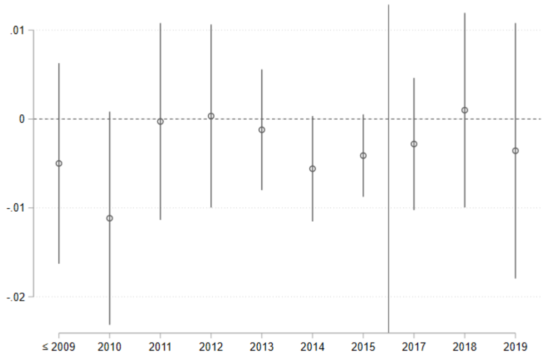


(n) Schleswig-Holstein

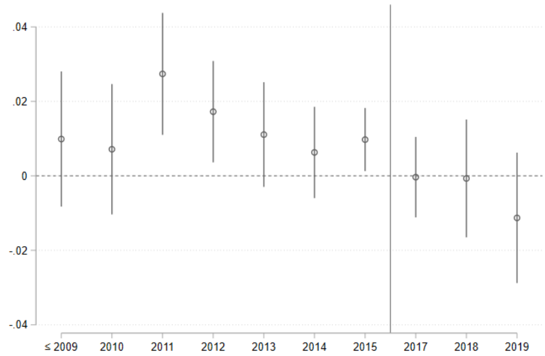


(o) Thuringia

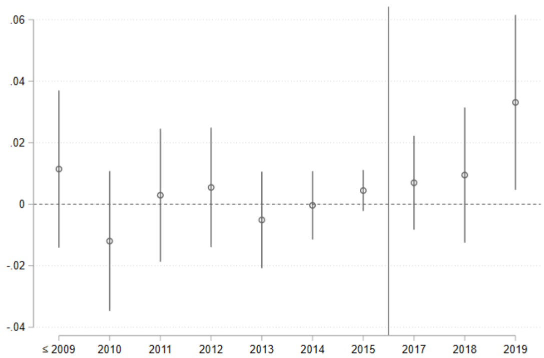
Figure A.2.1.: **Foreign Enrollment Rates by States, Placebo Policy in 2017.** The placebo policy is introduced at all public institutions within the state, while Baden-Württemberg is excluded from the samples. All estimations include covariates.
Data source: Statistisches Bundesamt (Destatis), 2020b.



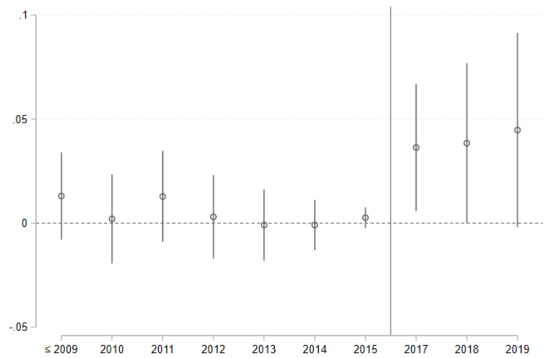
(a) Bavaria



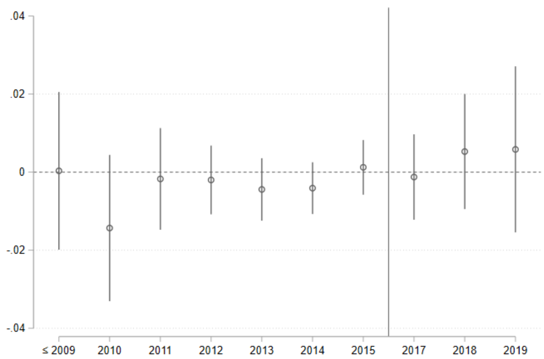
(b) Berlin



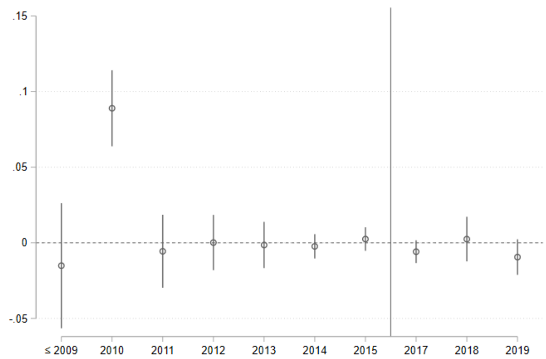
(c) Brandenburg



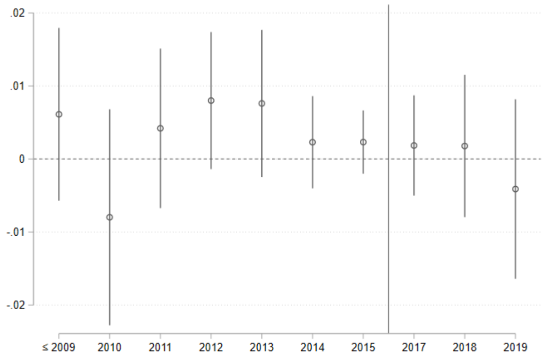
(d) Bremen



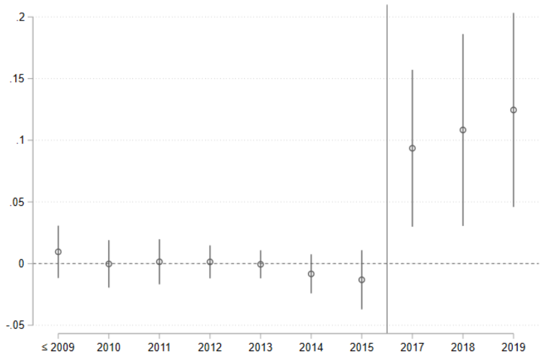
(e) Hamburg



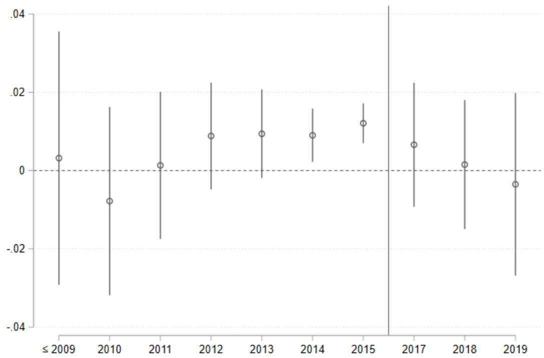
(f) Hesse



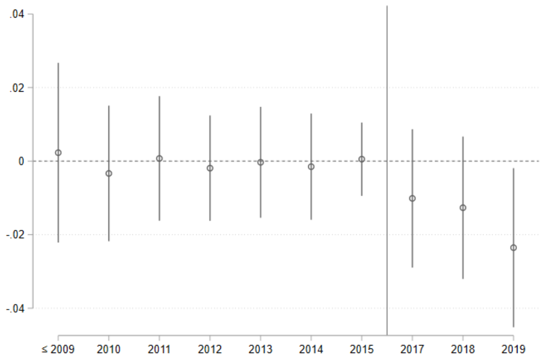
(g) North-Rhine Westphalia



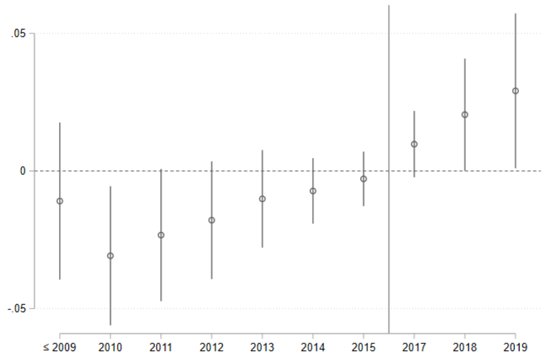
(h) Saarland



(i) Saxony-Anhalt



(j) Schleswig-Holstein



(k) Thuringia

Figure A.2.2.: **International Enrollment Rates by States, Placebo Policy in 2017.** The placebo policy is introduced at all public institutions within the state, while Baden-Württemberg is excluded from the samples. All estimations include covariates.
Data source: States' statistical offices, 2020.

B. Appendix to Chapter 2

B.1. Variables

Variable	IPUMS Variable	Notes
Male	SEX	I assigned a binary variable to men.
Age	AGE	No changes made.
Married	MARST	I assigned a binary variable to married people. For the full census data, persons under age 12 are assigned single status by IPUMS.
Year of immigration	YRIMMIG	Year of immigration in the 1910–1920 censuses reports the year of first arrival in the US. The 1900 census did not account for back-and-forth migration. I recoded zero values as missing.
Years in the US	YRSUSA1	Constructed by IPUMS from YRIMMIG.
In labor force	LABFORCE	For 1910, employment status refers to the day of the census, April 15. For 1930, the previous regular working day is considered; in 1940, March 24–30 serves as the relevant reference week. I construct a binary variable, which includes all employed and unemployed people.
Employed	EMPSTAT	See notes on *In labor force* on employment status. For 1940, one hour of paid work, 15 hours of unpaid family work or temporary absence from a job designate employment. I construct a binary variable, which represents whether a person was employed. Exclusion of institutional inmates makes data comparable across the censuses.
Log occupational score	OCCSCORE	Constructed by IPUMS. The occupation score represents the media total income in hundreds of 1950 dollars. Throughout the years included in this study, the 1950 occupational income data are considered. I construct the log occupation score by taking the natural logarithm of the provided data.

Table B.1.1.: **Variables Used in Analyses.**

B.2. Descriptive Statistics

Census year	1910		1920		1930		1940	
	(1) Filipinos mean/sd	(2) Others mean/sd	(3) Filipinos mean/sd	(4) Others mean/sd	(5) Filipinos mean/sd	(6) Others mean/sd	(7) Filipinos mean/sd	(8) Others mean/sd
Male	0.76	0.57	0.92	0.55	0.94	0.54	0.89	0.53
	0.43	0.50	0.27	0.50	0.24	0.50	0.31	0.50
Age	23.76	38.52	26.04	41.06	26.39	43.95	34.00	50.07
	16.94	17.05	8.88	16.65	7.77	16.20	9.68	15.12
Married	0.16	0.54	0.12	0.61	0.09	0.64	0.22	0.66
	0.36	0.50	0.32	0.49	0.28	0.48	0.41	0.47
Year of immigration	1900.80	1891.50	1911.04	1898.62	1921.66	1905.35	1935.00	1935.00
	11.12	15.21	7.73	14.58	7.41	15.11	0.00	0.00
Years in the US	9.20	18.50	8.96	21.38	8.34	24.65	.	.
	11.12	15.21	7.73	14.58	7.41	15.11	.	.
In labor force	0.61	0.59	0.81	0.56	0.89	0.55	0.85	0.51
	0.49	0.49	0.40	0.50	0.32	0.50	0.36	0.50
Employed	0.47	0.47	.	.	0.81	0.49	0.77	0.46
	0.50	0.50	.	.	0.39	0.50	0.42	0.50
Log occupation score	2.56	2.98	2.70	3.03	2.45	3.05	2.52	3.10
	0.62	0.52	0.50	0.47	0.44	0.48	0.48	0.47
N	1,629	13,678,160	5,841	14,040,717	41,933	14,319,128	44,645	11,705,726

Table B.2.1.: **Summary Statistics of Key Variables for Foreign-born Population by Census Year.** This table considers the full foreign-born population.

Year of immigration	Before 1900		1900–1910		1910–1920		1920–1930		1930–1940	
	(1) Filipinos mean/sd	(2) Others mean/sd	(3) Filipinos mean/sd	(4) Others mean/sd	(5) Filipinos mean/sd	(6) Others mean/sd	(7) Filipinos mean/sd	(8) Others mean/sd	(9) Filipinos mean/sd	(10) Others mean/sd
Male	0.87	0.54	0.90	0.58	0.93	0.55	0.94	0.51	0.64	0.48
	0.34	0.50	0.31	0.49	0.25	0.50	0.23	0.50	0.48	0.50
Age	42.22	51.20	27.96	34.01	27.01	31.91	24.61	28.42	25.27	35.98
	15.81	14.88	10.32	13.31	7.33	12.85	6.57	13.17	12.26	17.53
Married	0.28	0.66	0.15	0.57	0.12	0.55	0.05	0.45	0.19	0.47
	0.45	0.48	0.36	0.50	0.33	0.50	0.22	0.50	0.40	0.50
Year of immigration	1891.24	1884.60	1906.83	1905.90	1916.29	1914.32	1926.08	1924.61	1935.00	1935.00
	10.11	11.90	2.67	2.72	2.97	2.77	2.55	2.66	0.00	0.00
Years in the US	29.09	33.39	16.87	13.18	11.16	10.57	3.92	5.39	.	.
	11.19	13.25	7.80	8.45	4.98	5.35	2.55	2.66	.	.
In labor force	0.82	0.53	0.82	0.62	0.86	0.57	0.89	0.55	0.43	0.49
	0.38	0.50	0.38	0.49	0.35	0.49	0.32	0.50	0.50	0.50
Employed	0.70	0.43	0.71	0.53	0.80	0.53	0.81	0.50	0.39	0.44
	0.46	0.50	0.45	0.50	0.40	0.50	0.39	0.50	0.49	0.50
Log occupation score	2.70	3.04	2.59	3.02	2.53	3.01	2.42	2.94	2.68	3.03
	0.53	0.51	0.53	0.46	0.48	0.46	0.41	0.54	0.60	0.60
N	1,469	19,261,564	6,460	13,698,419	14,196	6,475,335	27,278	2,602,687	838	258,426

Table B.2.2.: **Summary Statistics of Key Variables for Foreign-born Population by Year of Immigration.** This table considers men between 15 and 65 years of age. Data for the 1940 census only consider those who migrated five years prior to the census.

Census year	1920		1930		1940	
	(1) Filipinos mean/sd	(2) Others mean/sd	(3) Filipinos mean/sd	(4) Others mean/sd	(5) Filipinos mean/sd	(6) Others mean/sd
Male	1.00	1.00	1.00	1.00	1.00	1.00
	0.00	0.00	0.00	0.00	0.00	0.00
Age	28.66	40.52	27.47	46.67	37.27	48.85
	7.17	11.36	10.96	11.07	9.85	10.45
Married	0.61	0.84	0.32	0.83	0.57	0.85
	0.49	0.37	0.47	0.37	0.50	0.36
Year of immigration	1908.43	1898.18	1915.60	1900.92	1935.00	1935.00
	7.45	11.79	7.88	10.98	0.00	0.00
Years in the US	11.57	21.82	14.40	29.08	.	.
	7.45	11.79	7.88	10.98	.	.
In labor force	0.91	0.96	0.61	0.95	0.88	0.91
	0.29	0.20	0.49	0.22	0.32	0.29
Employed	.	.	0.54	0.85	0.81	0.83
	.	.	0.50	0.36	0.39	0.37
Log occupation score	2.85	3.14	2.98	3.17	2.90	3.20
	0.59	0.39	0.60	0.39	0.56	0.39
Score falls	0.00	0.00	0.12	0.26	0.03	0.15
	0.00	0.00	0.32	0.44	0.16	0.36
N	121	2,476,211	294	2,317,218	1,056	2,294,010

Table B.2.3.: **Summary Statistics of Key Variables for Foreign-born Population by Year of Immigration (Panel 1920–1940).** This table considers men between 15 and 65 years of age.

B.3. Alternative regressions

B.3.1. Excluding 1920 from the Base Model

	(1)	(2)	(3)	(4)	(5)	(6)
			Labor force participation			
Johnson-Reed Act	-0.0295***	-0.0518***	-0.0479***	-0.148***	-0.00378***	-0.00514***
	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)
Filipino	-0.0697***	-0.0629***	-0.113***	-0.0973***	-0.0378***	-0.0303**
	(0.012)	(0.011)	(0.020)	(0.019)	(0.014)	(0.012)
Johnson-Reed Act × Filipino	0.0780***	0.0756***	0.123***	0.0950***	0.0257*	0.0135
	(0.012)	(0.011)	(0.021)	(0.019)	(0.014)	(0.013)
Age		-0.00210***		0.00178***		-0.00184***
		(0.000)		(0.000)		(0.000)
Married		0.0726***		0.0278***		0.0602***
		(0.000)		(0.000)		(0.000)
Constant	0.959***	1.015***	0.964***	0.917***	0.957***	1.009***
	(0.000)	(0.002)	(0.000)	(0.005)	(0.000)	(0.002)
State controls						
N	15,327,632	15,327,632	1,869,082	1,869,082	9,218,632	9,218,632

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.3.1.: **Panel Regression Results on Labor Market Outcomes, Excluding 1920.** The estimates are for men of prime-working age (15–65 years). These estimations exclude the year 1920 to provide a robustness check for the results in Table 2.5.

B.4. Robustness Checks on Other Countries

	(1)	(2)	(3)	(4)	(5)	(6)
	Log occupation score		Employed		In labor force	
Johnson-Reed Act	0.0821***	0.101***	0.0765***	0.0909***	-0.0231***	-0.0558***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Chinese	-0.247***	-0.110***	-0.0243***	0.0207***	-0.0127***	0.0233***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Johnson-Reed Act × Chinese	0.00236	-0.0259***	0.00839***	-0.0405***	-0.0439***	-0.0605***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)
Age		-0.000801***		-0.00331***		-0.00172***
		(0.000)		(0.000)		(0.000)
Married		0.118***		0.0919***		0.0687***
		(0.000)		(0.000)		(0.000)
Constant	3.086***	3.088***	0.754***	0.896***	0.953***	1.002***
	(0.000)	(0.003)	(0.000)	(0.003)	(0.000)	(0.001)
N	20,371,829	20,371,829	15,281,035	15,281,035	20,950,262	20,950,262

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.4.1.: **Cross-sectional Estimates for Chinese Immigrants.** Filipinos and American immigrants are dropped from the sample.

	(1)	(2)	(3)	(4)	(5)	(6)
	Log occupation score		Employed		In labor force	
Johnson-Reed Act	0.0826***	0.101***	0.0761***	0.0902***	-0.0236***	-0.0565***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Japanese	-0.459***	-0.342***	0.0326***	0.0243***	-0.00562***	-0.000483
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Johnson-Reed Act × Japanese	0.0948***	0.0886***	0.0680***	0.0506***	0.0312***	0.0247***
	(0.003)	(0.003)	(0.002)	(0.002)	(0.001)	(0.001)
Age		-0.000803***		-0.00330***		-0.00172***
		(0.000)		(0.000)		(0.000)
Married		0.117***		0.0918***		0.0687***
		(0.000)		(0.000)		(0.000)
Constant	3.087***	3.088***	0.753***	0.896***	0.953***	1.002***
	(0.000)	(0.003)	(0.000)	(0.003)	(0.000)	(0.001)
N	20,371,829		15,281,035		20,950,262	

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.4.2.: **Cross-sectional Estimates for Japanese Immigrants.** Filipinos and American immigrants are dropped from the sample.

	(1)	(2)	(3)	(4)	(5)	(6)
	Log occupation score		Employed		In labor force	
Johnson-Reed Act	0.0683***	0.0776***	0.0765***	0.0855***	-0.0234***	-0.0589***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Canadian	0.0389***	0.0781***	-0.0111***	-0.00527***	-0.00610***	-0.0103***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.000)	(0.001)
Johnson-Reed Act × Canadian	-0.00257***	-0.000908	0.00456***	-0.00697***	-0.0134***	-0.0125***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)
Age		-0.000783***		-0.00290***		-0.00144***
		(0.000)		(0.000)		(0.000)
Married		0.121***		0.0963***		0.0718***
		(0.000)		(0.000)		(0.000)
Constant	3.099***	3.122***	0.753***	0.835***	0.953***	0.975***
	(0.000)	(0.003)	(0.000)	(0.003)	(0.000)	(0.001)
State controls	No	Yes	No	Yes	No	Yes
N	18,462,468	18,462,468	16,681,168	16,681,168	22,801,676	22,801,676

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.4.3.: **Cross-sectional Estimates for Canadian Immigrants.** Filipinos and other American immigrants are dropped from the sample.

	(1)	(2)	(3)	(4)	(5)	(6)
	Log occupation score		Employed		In labor force	
Johnson-Reed Act	0.0798***	0.100***	0.0767***	0.0885***	-0.0233***	-0.0575***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Mexican	-0.304***	-0.197***	0.0259***	-0.00979***	-0.0290***	-0.0245***
	(0.001)	(0.002)	(0.001)	(0.002)	(0.000)	(0.001)
Johnson-Reed Act × Mexican	-0.0785***	-0.0704***	-0.0512***	-0.0600***	0.0215***	0.0136***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age		-0.000772***		-0.00321***		-0.00161***
		(0.000)		(0.000)		(0.000)
Married		0.116***		0.0921***		0.0697***
		(0.000)		(0.000)		(0.000)
Constant	3.084***	3.107***	0.753***	0.851***	0.953***	0.983***
	(0.000)	(0.003)	(0.000)	(0.003)	(0.000)	(0.001)
State controls	No	Yes	No	Yes	No	Yes
N	21,101,327	21,101,327	15,897,402	15,897,402	21,764,216	21,764,216

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.4.4.: **Cross-sectional Estimates for Mexican Immigrants.** Filipinos and other American immigrants are dropped from the sample.

B.5. Restricted and Unrestricted Immigrants

	(1)	(2)	(3)	(4)	(5)	(6)
	Log occupation score		Employed		In labor force	
Johnson-Reed Act	0.0589***	-0.0451***	0.0228***	-0.0107***	-0.0495***	-0.0154***
	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)	(0.000)
Filipino	-0.529***	-0.445***	0.0453***	-0.0283***	-0.0298***	-0.0281***
	(0.006)	(0.005)	(0.004)	(0.004)	(0.002)	(0.002)
Johnson-Reed Act × Filipino	-0.111***	-0.0579***	-0.0126***	0.0256***	0.0628***	0.0572***
	(0.006)	(0.006)	(0.004)	(0.004)	(0.003)	(0.003)
Age		-0.000942***		-0.00332***		-0.00174***
		(0.000)		(0.000)		(0.000)
Married		0.119***		0.0914***		0.0683***
		(0.000)		(0.000)		(0.000)
Constant	3.117***	3.118***	0.795***	0.896***	0.953***	1.003***
	(0.000)	(0.003)	(0.000)	(0.003)	(0.000)	(0.001)
State controls	No	Yes	No	Yes	No	Yes
N	17,045,421	17,045,421	15,327,632	15,327,632	20,999,413	20,999,413

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.5.1.: **Repeated Cross-sectional Regression Results (Migrated after Johnson-Reed Act).** Here, the post-period only applies for immigrants after 1924.

VITA

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