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I am submitting herewith a dissertation written by Justin R. Roush entitled "Essays in Environmental Economics and the Economics of Higher Education." I have examined the final electronic copy of this dissertation for form and content and recommend that it be accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy, with a major in Economics.

Matthew N. Murray, Major Professor

We have read this dissertation and recommend its acceptance:

Celeste K. Carruthers, J. Scott Holladay, T. Russell Crook

Accepted for the Council:
Dixie L. Thompson

Vice Provost and Dean of the Graduate School

(Original signatures are on file with official student records.)

Essays in Environmental Economics and the Economics of Higher Education

A Dissertation Presented for the
Doctor of Philosophy
Degree
The University of Tennessee, Knoxville

Justin R. Roush

May 2015

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This dissertation is dedicated to my beautiful fiancée, Kim, and our future children. I pray that all of the effort I've put into graduate school and this dissertation serves to make our lives nothing less than extraordinary.

Life is either a daring adventure, or nothing.

-Helen Keller

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Abstract

Maximizing welfare in the economy requires effective policies that mitigate negative externalities and incentivize positive externalities. My first paper concerns emissions at manufacturing plants in the US, a major source of negative externalities. Globalization has created concerns that pollution can move across country boarders. Therefore, I study the environmental performance of foreign manufacturing establishments in the US to test if foreign establishments in pollution intensive industries are heavy emitters. I find that foreign firms are not dirtier than domestic establishments on average. However, they are significantly cleaner in industries with high fixed costs and dirtier in industries with low fixed costs. This is because only the most productive establishments find it profitable to produce abroad in high fixed cost industries and high productivity establishments are generally cleaner. The next two papers focus on higher education, which bears large positive externalities. In the second paper in this dissertation I study the response of student charges to the Post-9/11 GI Bill at 2- and 4-Year public, private, and for-profit institutions. If institutions of higher education increase their student charges in response to increases in student aid, the goals of student aid policies are undermined. I test for supply side responses to the Post-9/11 GI Bill and find statistically significant increases in fees and tuition rates at 4-year public and 2-year private colleges. These increases reduce the effective benefit that students receive which harms their ability to access higher education. The third paper recognizes that the US currently has a shortage of STEM majors and tests whether major contingent loans can incentivize students to select into these fields. That is, if we only offer low interest loans to students studying majors that society values most, do students respond by choosing those majors? Using a natural experiment created by the National Defense Student Loan Program in 1959, which had a provision that teaching, foreign language, science, technology, and math fields receive priority in loan allocation, I find little evidence that students select qualifying majors in response to low-interest loans.

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Chapter 1

Introduction

This is an empirical study of the behaviors of economic agents that interact in socially inefficient markets. Economic theory establishes that the presence of negative externalities in a market for a good or service implies that society overproduces that good or service. Similarly, if a good or service possesses a positive externality society will underproduce it. Understanding the behavior of the economic entities who compose the supply and demand sides of inefficient markets is paramount to designing policies to correct for externalities and achieve social efficiency.

Two examples of markets that are socially inefficient are markets for manufactured goods and higher education markets. Manufactured goods markets tend to have negative externalities associated with production due to pollution released during the manufacturing process. The first paper in this dissertation, *The Environmental Performance of Foreign Owned Manufacturing Establishments in the United States*, focuses on documenting the behavior of foreign manufacturers. With regards to pollution externalities, it us unknown whether foreign establishments are dirtier or cleaner. If foreign manufacturers are cleaner, then blanket environmental policies aimed at reducing emissions at all establishments are not efficient because they allocate costly resources to mitigating pollution at relatively clean foreign establishments. In this paper I ask, "What is the environmental performance of foreign owned manufacturing establishments in the United States?"

Higher education markets are also inefficient. When an individual invests in their own education it is mostly a private investment. They choose to forgo opportunities while in school in order to secure greater future opportunities after graduation. However, individuals who accumulate high levels of human capital often generate positive benefits for society. For example, a student may be motivated to become an engineer because they enjoy the subject matter and the potential future income, both of which are private incentives. However, they cannot prevent society as a whole from benefiting from the knowledge they create as an engineer, like an innovative new bridge design that is safer to drive on and saves lives. People can drive on the bridge, but they do not have to accumulate the human capital in order to design the bridge. Individuals do not account for the positive benefits their own education can have on society when making human capital investment decisions. As such, acting only on their private incentives they underinvest in higher education and the market is socially inefficient.

The second and third chapters in this dissertation study the behaviors of consumers and suppliers of post-secondary education. The second paper entitled *The Incidence of the Post-9/11 GI Bill Subsidy at Institutions of Higher Education: A Study of the Response of In-State Tuition and Fees to Veteran Education Benefits* investigates the response of institutions of higher education (i.e., the supply side) to federal tuition and fee subsidies. If colleges increase tuition and fee rates in response to increased student aid, then the effectiveness of student financial aid is undermined. Given that students already underinvest in education on average, this behavior increases social inefficiency as aid-bearing students are disincentivized to attend college. In this paper I ask, "Do colleges increase their tuition and fees in response to federal financial aid subsidies?"

The final paper addresses a concern expressed by the federal government that the United States is lacking manpower in strategic areas, specifically Science, Technology, Engineering, and Mathematics (STEM). *Major-Contingent Loans and College Major Choice* proposes and tests a unique policy tool aimed at increasing major counts in strategic areas. Specifically, I ask, "Can conditioning federal student lending terms (e.g., interest rates) on student's choice of major increase the number of STEM and other desired degrees?" If so, the federal government can reduce social inefficiency in higher education markets by incentivizing students to pursue certain majors through their vast student loan framework. This policy increases social efficiency in two ways: first, offering low interest student loans has a primary mitigating effect on social inefficiency by increasing student access to college. Secondly, however, since social benefits vary across majors this policy tool has the secondary effect of nudging students towards majors with the largest positive externalities.

The three chapters that follow present the methods and results for each aforementioned paper. In the last chapter I synthesize my findings across the three papers to draw conclusions regarding the behaviors of economic agents in inefficient markets.

Chapter 2

The Environmental Performance of Foreign Owned Manufacturing Establishments in the United States

Abstract

This paper focuses on the relationship between foreign ownership in a multinational corporation and environmental performance at the establishment level. If foreign manufacturers are cleaner than their domestic peers, popular concerns over the migration of pollution across borders may be misplaced. On average, we do not find that foreign establishments are significantly cleaner across all industries. However, we document significant across-industry variation in foreign owned establishment environmental performance. Borrowing from findings in the international trade literature on firm heterogeneity, only the most productive firms find it profitable to become a multinational due to the high fixed costs of locating abroad. Productivity has been associated with better environmental performance in the trade and environment literature. Therefore, we attempt to explain the variation in environmental performance with across-industry variation in fixed costs and show that fixed costs can explain some of the variation in environmental performance. A 1% increase in fixed costs leads to a 3.64% improvement in environmental performance for the average foreign establishment.

2.1 Introduction

The impact of globalization on the environment has long been a central concern of environmentalists and policymakers alike. Trade liberalization and the rapid growth of foreign direct investment (FDI) since the 1980's generated fears that pollution may travel across country borders. Despite the considerable rhetoric around the issue, the environmental performance of multinationals has not been widely studied. If foreign manufactures are cleaner than their domestic peers, popular concerns over the migration of dirty manufacturing may be misplaced.

This paper focuses on the relationship between foreign ownership and environmental performance at the establishment level. We estimate the environmental performance of foreign owned manufacturing establishments in the United States (US) across industries. To do this, we construct a unique dataset of matched establishment characteristics and pollution emissions for 236,585 foreign and domestically owned manufacturing establishments in the US from 1990-2006. With this data we are the first to document the environmental performance of foreign establishments in the US by using plant level emissions.

The international trade literature has documented a variety of ways in which multinational firms differ from non-multinationals. Helpman et al. (2004) show that multinationals serving foreign markets through the establishment of subsidiaries abroad (FDI) are more productive than those that serve those markets through exports, which are in turn more productive than their solely domestic competitors. The fixed costs of setting up facilities in foreign countries are larger than the fixed costs of exporting to those markets. These fixed costs dictate that only the most productive firms in a particular industry can afford to become multinationals. Moreover, Doms and Jensen (1998) and Girma et al. (2002) document an empirical pattern that foreign owned affiliates are more productive than domestically owned producers in the country of the affiliate, which suggests that a multinational's high productivity is transferred to its affiliate. To that end, we hypothesize that foreign owned establishments are more productive than their domestic peers in high fixed cost cost industries.

Fixed costs of FDI may also play an important role in a multinational's reaction to environmental regulation. On average, establishments do not increase FDI abroad in response to environmental regulation. Ederington et al. (2005) document that studying the average impact of environmental regulatory costs hides the significant heterogeneity of the impact of regulation across industries. Firms in some industries are geographically immobile due to high relocation costs and therefore do not respond to regulation by increasing FDI abroad.

This suggests that countries may retain their dirty manufacturers despite environmental regulation if these establishments are in industries with high fixed costs.[1]

However, we observe emission reporting foreign establishments in the US in both high and low fixed cost industries. Given that polluting establishments from generally immobile industries have relocated, are they cleaner than their domestic peers? By addressing this question we contribute to an emerging literature that identifies a relationship between manufacturing facility productivity and environmental performance in the context of international trade. Holladay (ming) shows that exporters pollute less than non-exporters after controlling for output levels and that import competition is associated with the exit of the most pollution intensive firms. Cui et al. (ming) develop a model in which plant productivity is negatively correlated with pollution intensity. This literature has not yet addressed the environmental performance of firms that choose to serve foreign markets through FDI rather than exports.

The productivity advantages enjoyed by multinationals and the emerging consensus on the relationship between productivity and environmental performance suggest that multinationals' environmental performance may vary from that of their domestic competitors. On the other hand, there is evidence that environmental regulation affects the FDI decisions of foreign firms investing in the United Sates. Keller and Levinson (2002) finds that average pollution abatement costs in a state are related to the level of inward FDI that state receives. Millimet and Roy (ming) after controlling for the spatial determinants of FDI and endogeneity of environmental regulation, find that regulatory stringency is negatively correlated with inward FDI and larger than previous estimates. In one of the few papers to study environmental regulation's impact on outbound FDI, Hanna (2010) finds evidence that regulation increases foreign assets abroad by 5.3% for firms in the most pollution intense industries.The sensitivity of foreign-owned firms to environmental regulation is consonant with the argument that multinationals have relatively poor environmental performance.

We seek to clarify these potentially contradictory strands of research by evaluating the performance of multinationals directly. This paper adds to the literature in several ways. Primarily, we are the first to conduct a widespread study of the environmental performance of foreign establishments using establishment level emissions data. In the absence of detailed establishment level data, prior work has focused on the correlation between aggregate FDI

[1]Even still, US plant emissions have fallen over the time period studied. Levinson (2014) demonstrates that the reduction in US plant emissions from 1990-2008 is attributable to changes in "technique," meaning industry input use (i.e., cleaner fuels), cleaner processes, end-of-pipe controls, and so on. He concludes that the fall in emissions is not due to changes in the composition of goods produced (i.e., the flight of dirty production abroad). This implies that foreign firms in the US have not changed the composition of manufacturing in the US. Moreover, our study adds to this by investigating wether or not foreign firms added to or worked agains the fall in emissions.

intensities and total pollution levels in an area. For studies using microdata, researchers proxy for establishment emissions using plant fuel consumption. Eskeland and Harrison (2003) and Cole et al. (2008) find that foreign plants are more energy efficient and use cleaner types of fuels. On the contrary, King and Shaver (2001) study emissions levels for foreign versus US establishments in the chemical and petroleum sectors only. They find that foreign establishments generate more waste than US owned establishments, but also manage more waste internally. Secondly, our study employs a rich dataset of establishment characteristics, including sales, employment, credit rating and ownership status from the National Establishment Time Series (NETS) Database. We match this data to establishment level hazard scores from the Environmental Protection Agency's (EPA) Risk-Screening Environmental Indicators (RSEI). By this, we recognize an inherent flaw in using pounds emitted as a measure of environmental performance; large amounts of some chemicals may be relatively benign while small amounts of others can be very harmful. Lastly, we add to the literature by documenting the great heterogeneity in environmental performance of foreign establishments across industries and offering a candidate explanation for this observed heterogeneity.

In summary, we do not find that foreign establishments are significantly cleaner than domestic establishments on average. In fact, our baseline estimates suggest the opposite; foreign owned establishments, on average, are more hazardous than domestic establishments. However, average environmental performance hides significant heterogeneity in the hazard levels of foreign owned establishments across industries. Breaking up our baseline estimates by industry we document significant across-industry variation in foreign owned establishment environmental performance. For example, foreign establishments in two digit SIC industry code 27, Publishing and Printing, are 657% cleaner than domestic establishments. On the other hand, foreign establishments in two digit SIC industry 29, Petroleum and Cole, are 287% dirtier. This suggests that some characteristics that vary across industries could explain the heterogeneity in foreign establishment environmental performance, which we explore empirically. Pairing results from two literatures (i.e. international trade and trade and the environment), we hypothesize that industries with high fixed costs have more productive foreign establishments and that more productive establishments have better environmental performance. Our results are consistent with this hypothesis. Specifically, a 1% increase in fixed costs is associated with a 3.64% improvement in foreign establishment environmental performance.

The next section describes the data. Sections III and IV present the empirical results from the within industry and across-industry analyses respectfully. Lastly, we provide concluding remarks.

2.2 Data

Estimating the environmental performance of establishments requires detailed data on plant characteristics and pollution emissions. We construct a panel at the establishment-year level by matching establishment characteristics from the National Establishment Time Series (NETS) with pollution emissions from the Risk Screening Environmental Indicators (RSEI) data set. Our NETS data consists of 236,585 manufacturing establishments (SIC 20-39), approximately a ten percent extract from the full NETS manufacturing database which is marketed as the universe of manufacturing establishments. Due to entry and exit the panel is unbalanced and consists of 2,548,028 establishment-year observations covering 1990-2006. Just under 70,000 establishments survive across the seventeen years we observe and around 50,000 are observed for fewer than five years.

The NETS data includes a host of manufacturing facility characteristics including annual observations on sales, employment, credit rating, industry and location. It also captures static indicators on legal status (proprietorship, corporation, etc.), exporter status, CEO gender, ownership structure (including the ultimate parent firm), the number of related establishments and the number of establishments that report this facility as their parent. Importantly for this analysis, the data also includes a static indicator for whether the facility is foreign owned in the last year it was observed. The NETS has been used in a variety of empirical economic studies and Neumark et al. (2011) found that it was comparable in data quality to other public and proprietary establishment level data sources.[2]

The RSEI data is compiled from Toxic Release Inventory (TRI) data collected by the Environmental Protection Agency (EPA) under the Comprehensive Environmental Response, Compensation, and Liability Act (commonly known as the Superfund Act). The TRI consists of self-reported emissions of hundreds of toxic chemicals regulated under the Act. EPA collects this data and checks for internal consistency of submissions. There are significant legal penalties for intentionally misreporting toxic emissions, but EPA rarely enforces these rules. There is some evidence of underreporting of emissions, but there does not seem to be any evidence of systematic misreporting by foreign owned firms within particular industries.[3] The RSEI takes reported TRI emissions and weights them by the toxicity of the chemical emitted. This facilitates comparison across establishments that emit different types

[2] In additional to Neumark et al. (2011), see Levine et al. (2012) and Neumark and Kolko (2010) for examples of empirical studies using the NETS.

[3] Marchi and Hamilton (2006) find that for some chemicals the reported emissions are not consistent with Benford's Law and Koehler and Spengler (2007) find evidence of underreporting for polycyclic aromatic hydrocarbons in the aluminum industry. While neither of these studies look at foreign ownership directly the misreporting they discover does not appear to be correlated with productivity, size or other characteristics that are in turn correlated with foreign ownership.

of chemicals by measuring emissions as a function of their potential harm. The RSEI has been used in several studies to proxy for environmental performance of establishments, firms and communities.[4]

The NETS and RSEI data were matched using DUNS numbers (a unique plant identifier created by Dun&Bradstreet which also provides source data for the NETS) where possible. DUNS numbers is an optional field in the TRI and many DUNS are missing or invalid. For establishments that do not report a DUNS, NETS and RSEI data are matched using address and industry information employing a fuzzy matching procedure. The resulting data set of matched establishment characteristics and pollution emission data for just under seventy-five percent of TRI reporting facilities. The matched facilities were slightly larger in terms of emissions and hazard score than the unmatched, but the ratio of the hazard to emissions were not statistically significantly different.

Table 2.1 describes the TRI reporters in the matched data set. The first column reports means and standard deviations (in parentheses) for the full sample, Column 2 reports the averages for domestically owned establishments only, Column 3 describes foreign owned facilities. Foreign owned establishments are larger across every dimension. They have approximately 50% more sales, slightly more employees, and report TRI emissions just over twice as often as domestically owned establishments. Foreign owned establishments have lower hazard scores, but are more hazardous per unit of output than domestically owned establishments. Foreign ownership is also extremely rare; 2.7% of emissions reporting establishments in the data are foreign owned. This is consistent with Doms and Jensen (1998) who find that 1.9% of the establishments in their dataset are foreign owned.

Not all establishments are required to report their toxic chemical emissions to the TRI. Only establishments in selected industries (including all manufacturing industries) with more than ten employees that use more than 10,000 pounds of any single TRI listed chemical in a year must report. Firms that exceed the 10,000 pound threshold for any chemical must report their use for all TRI chemicals. Approximately 2.7% of manufacturing establishments in our matched establishment characteristic and environmental performance data report TRI emissions. More than 6% of foreign owned establishments in the data are TRI reporters potentially because their larger size makes them more likely to exceed the employment and chemical use thresholds.

[4]See Holladay (ming) for an establishment level example from the trade and environment literature and Banzhaf and Walsh (2008) for analysis on the community level for examples.

2.3 Environmental Performance of Foreign Owned Establishments

In this section we document the environmental performance of foreign owned establishments in the US manufacturing sector. We estimate pollution production functions comparing the environmental performance of domestic and foreign owned establishments conditional on a variety of co-variates. We also control for selection into TRI reporting status. In a pooled sample foreign owned manufacturing establishments pollution levels appears roughly comparable to domestically owned establishments. Average results hide significant variation across and within manufacturing industries. In particular, the results suggest that in some industries foreign owned establishments are significantly cleaner while in others they are much dirtier. These results are robust to a variety of different estimation schemes and sets of co-variates. Even in closely related manufacturing industries environmental performance of foreign owned establishments can vary drastically.

2.3.1 Empirical Approach

The existing literature suggests that multinational firms will be larger and more productive than domestic competitors in the same industry. We develop a straightforward empirical approach to analyze how these differences will translate into environmental performance of multinational firms. We begin by specifying a production function that includes pollution emissions as a joint input to production. We follow Cui et al. (ming) in assuming that within an industry all firms employ the same technology, though they differ in productivity. The production function for facility i in industry j in time t is:

$$q_{ijt} = exp(A_j + \phi_{ij} + e_{ijt}) * h_j(L_{ijt}, E_{ijt}, \mathbf{x}_{ijt}), \tag{2.1}$$

where q_{ijt} is the quantity produced, A_j is a time invariant industry level scaling parameter, ϕ_{ij} is facility productivity, and e_{ijt} is a random error term with zero mean. The production function, h_j is homogeneous of degree κ, which includes facility labor (L_{ijt}), emissions (E_{ijt}) and other input ($\mathbf{x}_{ijt}$) demands.

Assuming that the production function is HDκ allows us to separate out the observable labor and emissions inputs from the unobserved factor shares:

$$q_{ijt} = exp(A_j + \phi_{ij} + e_{ijt}) * L_{ijt} * E_{ijt} * h_j^{k_j} h(1, 1, \frac{\mathbf{x}_{ijt}}{L_{ijt} * E_{ijt}}), \tag{2.2}$$

Taking logs the production function can be re-written as:

$$ln(q_{ijt}) = A_j + \phi_{ij} + ln(L_{ijt}) + ln(E_{ijt}) + ln[h_j^{k_j} h(1, 1, \frac{\mathbf{x}_{ijt}}{L_{ijt} * E_{ijt}})] + e_{ijt}, \tag{2.3}$$

Assuming that firms in a given industry face the same input prices and have a common homogeneous production function implies that all firms in an industry employ the same input ratios $\mathrm{x}_{ijt}/\mathrm{L}_{ijt}\mathrm{E}_{ijt}$ through cost minimization. This implies that the input ratios, which are not reported in our data, are common within an industry and can be controlled for by a set of time varying industry fixed effects.

Our data does not include information on facility productivity. We do know, from the existing literature, that foreign owned plants in a particular industry are more productive than their domestically owned counterparts.[5] Rather than attempt to estimate productivity directly we use foreign ownership as a proxy for high productivity establishments. To the extent that this mis-characterizes low productivity foreign owned facilities as high productivity, it will tend to bias the estimated impact of productivity on pollution emissions towards zero.

We then estimate a regression based on equation 3. We employ a set of industry fixed effects to control for the industry scaling parameter as well as the unobserved factor shares. Specifically we estimate:

$$ln(E_{ijt}) = \gamma_1 ln(L_{ijt}) + \gamma_2 ln(q_{ijt}) + \gamma_3 ForeignOwned_{ij} + \delta_j t + e_{ijt}, \tag{2.4}$$

where $ForeignOwned_{ij}$ is an indicator for a foreign-owned manufacturing facility and $\delta_j t$ is a set of industry-by-year fixed effects. The parameter of interest is γ_3 which estimates the impact on emissions of being foreign owned compared to other firms in the same industry (holding output constant). This approach allows us to separate the impact of multinational size from the impact of multinational productivity.

Because selection into pollution reporter status may be non-random we also employ a panel selection model based on Wooldridge (1995).[6] We estimate a series of probits with a dependent variable equal to one if the establishment is a TRI reporter, for each year:

$$\begin{gathered} Probit[Pr(Reporter_{ijt} = 1)] = \\ \gamma_1 ln(L_{ijt}) + \gamma_2 ln(q_{ijt}) + \gamma_3 ForeignOwned_{ij} + \delta_j t + e_{ijt}, \\ \text{for each t=1990 to 2005} \end{gathered} \tag{2.5}$$

[5]See Doms and Jensen (1998), Girma et al. (2002) and Helpman et al. (2004) among many others.

[6]The other widely accepted approaches for estimating panel selection models (Kyriazidou (1997) and Rochina-Barrachina (1999)) rely on differencing the data. Because the foreign owned indicator does not vary across time in the NETS they are not appropriate for our setting. See Dustmann and Rochina-Barrachina (2007) for a review of panel selection models and an overview of their strengths and weaknesses.

We compute inverse Mill's ratios using estimates from each year's probit ($Rep\hat{o}rter_{ijt}$) and introduce them in a fixed effect OLS regression interacted with a set of year dummies (ν_t):

$$ln(E_{ijt}) = \gamma_1 ln(L_{ijt}) + \gamma_2 ln(q_{ijt}) + \gamma_3 ForeignOwned_{ij} + \widehat{Reporter}_{ijt} * \nu_t + \delta_j t + e_{ijt}, \quad (2.6)$$

The γ_3 in this equation represents the environmental performance of foreign owned establishments in a particular four digit industry and year relative to domestic owned establishments in the same industry and year. This estimate will be purged of any bias associated with non-random selection into TRI reporter status.

Finally to evaluate the heterogeneity in foreign owned establishment environmental performance, we estimate a Heckman selection model on a sample restricted to a single four digit SIC industry and year for each industry and year in the data set. We collect the coefficient on the foreign owned indicator variable and merge that data with industry characteristics to explore the sources of heterogeneity in environmental performance of foreign owned establishments.[7]

$$\begin{aligned} ln(E_{ijt}) =& \gamma_1 ln(L_{ijt}) + \gamma_2 ln(q_{ijt}) + \gamma_3 ForeignOwned_{ij} \widehat{Reporter}_{ijt} + e_{ijt}, \\ & \text{for each j=2000 to 3999 and t=1990 to 2005} \end{aligned} \quad (2.7)$$

2.3.2 Empirical Results

We begin by estimating the environmental performance of foreign owned establishments pooled across our entire sample. Table 2.2 reports the results of a series of regressions to evaluate the environmental performance of foreign owned establishments. Column 1 reports the unconditional difference in hazard scores between domestic and foreign owned establishments. Foreign owned establishments have a 91% higher hazard score than domestic owned establishments. Following the emerging heterogeneous-firm trade and the environment literature, Column 2 adds a set of industry fixed effects to report the within industry differences in environmental performance of foreign owned establishments. The results suggest that foreign owned establishments have approximately 45% higher hazard score than domestic owned establishments suggesting foreign owned establishments are concentrated in more pollution intensive industries. The coefficient is no longer statistically significant.

[7] Another option would be to use the panel selection model to estimate foreign owned environmental performance by industry and year through a triple interaction term. This would be more efficient than estimating separate equations for each industry and year, but computation limits make the pooled panel with a triple interaction difficult to estimate.

Column 3 adds log employment to control for size differences between domestic and foreign owned establishments.[8] Larger establishments emit more although the results suggest that increases in employment are associated with less than proportional increases in emissions. Despite being nearly twice as large on average, controlling for the level of employment has little impact on the foreign owned coefficient. Column 4 reports the results of a linear probability model where the dependent variable is an indicator equal to one if the industry reports their pollution emissions to the EPA. Foreign owned establishments are no more likely than competitors in the same industry to report emissions. Larger establishments, as proxied by employment, are significantly more likely to report emissions. If foreign owned establishments are more or less likely to report emissions to the TRI the estimated environmental performance of foreign owned polluters may be biased. One way to correct this bias is through a Heckman selection model.

Column 5 reports the results of a two-step Heckman selection model where the selection equation evaluates the probability of being a TRI reporting establishment based on equation 2.6. The exclusion restrictions are a set of six digit SIC industry dummies that control for product specific differences in the propensity to be required to report TRI emissions. Foreign owned establishments environmental performance is estimated to be slightly worse than domestic owned establishments, but the results are imprecisely estimated.

We now turn to evaluating the intra-industry heterogeneity in foreign owned environmental performance. We estimate equation 2.7 for each year and industry in the data set. This approach generates 1,058 estimates of the foreign owned establishments environmental performance relative to competing firms in the same four digit SIC industry and year. The estimates correct for potential selection into TRI reporting status. There is significant heterogeneity in environmental performance of foreign owned establishments across even closely related manufacturing industries.

Figure 2.1 displays estimates of the environmental performance of foreign owned establishments relative to domestic owned establishments in the same industry (γ_3). The bottom row summarizes the distribution of within two digit SIC by year estimates of foreign owned environmental performance. There is little variation in these estimates, although it is evident that in some two digit industries foreign establishments are relatively cleaner and in others they are not. However, the estimates in the bottom row hide a great amount of

[8] We do not use logged sales as a proxy for output in our estimations. Reporting of sales is rare in the Dun&Bradstreet data that supplies the NETS database. NETS reports imputed values of sales based on employment (which is more robustly reported). By dropping sales we are assuming that within industry the technology that transforms units of labor into output is the same for all firms. This implies that output is a function of labor and firm productivity only and that increases in employment map into increases in output. We show in the appendix that the omission of sales does not qualitatively change our results.

heterogeneity displayed in the rows above. Each row summarizes the distribution of four-digit industry estimates within a two-digit industry. Negative coefficient estimates suggest that foreign owned establishments are cleaner than their competitors. The box represents the intra-quartile range and the bar in the middle of the box represents the median. The lines represent the span of γ_3 coefficients for that two digit industry.[9]

In the majority of two-digit industries the median four-digit industry has foreign owned establishments with better environmental performance than domestic owned firms. There is also significant variation within two-digit industries. In only two two-digit industries are all foreign owned establishments in each four-digit industry estimated to be cleaner than their domestic owned competitors. In both the furniture and fixture industry (SIC 25) and the miscellaneous manufacturing industry (SIC 39) all four digit industries have relative cleaner foreign owned establishments. In the instruments and related industry (SIC 38) the median industry's foreign owned firms are slightly dirtier than domestic owned establishments. Almost every two digit industry contains four digit industries with significantly cleaner foreign owned establishments as well as industries with significantly dirtier foreign owned industries.

Table 2.3 provides another way to summarize the heterogeneity in foreign owned establishment's environmental performance. It reports summary statistics for the estimated environmental performance of four-digit industries within a two-digit industry. The standard deviation and range of γ_3 estimates are very large. The distributions are skewed with a small number of four digit industries with extreme differences in environmental performance of foreign owned establishments.[10] In the Custom Compound Purchased Resins industry (SIC 3087) foreign owned establishments are estimated to be over a thousand times worse than their domestic owned competitors.

We run several robustness checks to evaluate the sensitivity of these estimates to a number of control variables. We re-estimate the industry-by-year Heckman selection model including controls for the year the establishment entered our data set.[11] We run another robustness check that utilizes as much of the establishment specific data in the NETS as possible. This includes the number of related establishments, the gender of the company CEO, whether the company is public or private and a set of industry-by-county fixed effects. In both cases the resulting coefficients are highly correlated with the baseline specification (simple correlation

[9] Outliers, defined as data points exceeding 1.5 times the 75th percentile or lower than 1.5 times the 25th percentile of data, are suppressed in Figure 2.1.

[10] In the appendix we report the industries with the best and worst performing foreign owned establishments.

[11] We have two variables that provide proxies for establishment age. The year the company started and the year the establishment first entered the NETS data. Many companies in the dataset report they started over one-hundred years ago suggesting it is not an appropriate proxy for the age of the capital in the facility. The first database entry year in the database is 1989 for establishments that were open when data collection began, limiting identification of this age proxy variable to establishments opened since 1990.

coefficients of 0.89 and 0.61 respectively.) In both cases the significant heterogeneity in environmental performance of foreign owned establishments remains.

2.4 Explaining Foreign Owned Establishment's Environmental Performance

In the previous section we documented the environmental performance of foreign owned establishments in the US manufacturing sector. The results demonstrate that in some industries foreign owned establishments are significantly cleaner while in other industries they are much dirtier. In this section we seek to explain the cross-industry variation in foreign owned establishment environmental performance as it relates to industry characteristics, particularly fixed costs. Our approach is first, to gather point estimates of the coefficients on the foreign owned indicator (γ_3) from the within four digit industry estimations for years 1990 to 2006. These estimates measure the relative environmental performance of foreign owned establishments to domestic US establishments. Then, we pair these observations with annual industry-level data from the NBER-CES Manufacturing Industry Database. From this data we compute a proxy for industry-level fixed costs. Lastly, we estimate a measure of average foreign establishment productivity by industry and year. Altogether this data allows us to study the relationship between fixed costs, foreign establishment productivity, and environmental performance. We hypothesize that foreign establishments are more productive in industries with higher fixed costs and, coincidentally, have better environmental performance.

This hypothesis makes sense given recent findings in international trade regarding firm heterogeneity. Helpman et al. (2004) build a theoretical model expanding on Melitz (2003) relating the firm's decision to service a foreign market, by exporting or by horizontal FDI, as a function of the costs of each activity. In this study we focus on the role fixed costs play in determining the type of firms that can produce abroad.

In their model firms draw a productivity parameter a from a distribution $G(a)$ where profits are increasing in a. Upon observing this draw firms decide whether to produce or exit the market. If they produce, they pay fixed operating overhead costs f_D. If the firm chooses to export they incur additional fixed costs f_X per foreign market, which includes f_D and the costs of setting up a distribution network abroad. To serve a market by horizontal FDI the firm must pay f_I, which represents the fixed costs of setting up a distribution network as well as those from establishing a subsidiary abroad (i.e., duplicate overhead operation costs f_D). Therefore, $f_D < f_X < f_I$. This establishes a "pecking order" of productivity for establishments based on how they service foreign markets. All else equal, only the most productive firms find it profitable to participate in foreign markets with high fixed costs, so solely domestic firms are the least productive firms in a country. Additionally, of the firms that serve foreign markets, only the most productive engage in FDI; a firm must productive enough to remain profitable after setting up a distribution network *and* paying duplicate operating overhead. As a result, within a country multinationals are more productive than domestic exporters, who are in turn more productive than solely domestic establishments.

The theoretical predictions presented in the prior paragraph apply to multinationals, exporters, and domestic establishments of the same country. That is, a German multinational will be more productive than a German exporter, and the German exporter will be more productive than a solely domestic German firm. However, our dataset includes US establishments and foreign establishments. In our establishment level specification we assume that foreign establishments are more productive than US domestic establishments. This means, for example, that we are assuming the German subsidiary in the US inherits productivity from their multinational parent in Germany and will be more productive than US domestics and exporters in the same industry. This assumption is consistent with findings in the literature. Doms and Jensen (1998) and Girma et al. (2002) document an empirical pattern that foreign-owned affiliates are more productive than the domestic competitors in the country of the affiliate.

Cui et al. (ming) builds upon the model presented in Helpman et al. (2004) to link establishment productivity to environmental performance. They allow emissions to be an input to the production function (as in Copeland and Taylor (1994)) which require the purchase of a permit from the government per unit of emission. As a result, firms have the incentive to invest in clean technologies to lower their marginal costs of production. Investing in different technologies invokes different fixed costs. Firms desiring to produce in a market with basic "dirty" capital must pay a fixed cost f_{dirty}, which determines the technology of production. However, firms can also invest in a clean technology which requires fixed cost $f_{clean} > f_{dirty}$. Investing in a clean technology upgrade, in return, lowers the firm's marginal cost of production. Only the most productive firms find the clean investment profitable due to high fixed costs. Since firms that can service foreign markets are more productive than domestic establishments, we expect them to have better environmental performance.

In summary, only the most productive firms can produce abroad in high fixed cost industries, only the most productive establishments find it profitable to invest in green technologies, therefore foreign establishments abroad are relatively cleaner than the domestic peers in their home country. By assuming that foreign affiliates in the US adopt the high productivity of their parent, we can test if productivity advantages translate into better environmental performance. Again, the foreign owned indicator in the prior section proxies for high productivity establishments, but we observe that foreign establishments are only cleaner in some industries. This suggests that foreign establishments are only highly productive in some industries, and our candidate explanation is that this is due to fixed costs faced by the multinational.

2.4.1 Empirical Approach

The goal of this section is twofold. First, we test if the fixed costs and foreign establishment productivity can explain the observed variation in foreign establishment environmental performance from the prior section. Secondly, we wish to demonstrate that the observed variation in fixed costs across industries influence the productivity of foreign owned plants relative to their domestic peers. The estimation equation of interest is as follows:

$$\gamma_{3,jt} = \beta_0 + \beta_1 * prodgap_{jt} + \beta_2 * log(nonprod\_workers)_{jt} + \bar{X}_{jt} * \Gamma + u_{jt} \tag{2.8}$$

Recall that $\gamma_{3,jt}$ is the coefficient on the foreign owned indicator variable from within industry-year estimation of equation 2.7. We gather the coefficients from the Heckman selection model.[12] To measure the productivity advantage of foreign owned establishments relative to domestic establishments within an industry-year ($prodgap_{jt}$) we employ a common method of estimating total factor productivity found in the literature. Specifically, we estimate the Solow residual of the establishment's production function, which is the variation in output not explained by industry specific inputs.

$$log(sales_{ijt}) = \delta_j + \delta_j * log(emp_{ijt}) + \tau + \epsilon_{ijt} \tag{2.9}$$

This is an OLS regression of establishment output ($log(sales_{ijt})$) on industry dummies (δ_j) and labor-by-industry interactions to capture the effect of *industry* specific input productivity. What is left in the error term is an *establishment* level productivity parameter not explained by industry characteristics. We average these residuals within an industry-year for foreign and domestic firms separately and difference the averages to generate $prodgap_{jt}$. In the end, $prodgap_{jt}$ is the average productivity advantage of foreign owned establishments in industry j in year t:

[12] As demonstrated in the Appendix, these coefficients are robust to a variety of specifications.

$$prodgap_{jt} = \mu_{jt}(\hat{\epsilon}_{ijt}^{fo}) - \mu_{jt}(\hat{\epsilon}_{ijt}^{dom}) \tag{2.10}$$

The variable $log(nonprod\_workers)_{jt}$ is our proxy for fixed costs and is the logged value of the total number of non-production employees in an industry-year. Multinationals that set up production abroad incur fixed costs when setting up a subsidiary. The number of workers hired that do not contribute to production represent the costs a firm would face if they shut down in the short run (at which time the number of production workers would be zero but counts of non-production workers would remain unchanged). The vector $\bar{X}_{jt}$ contains industry-by-year control variables which include the total number of production hours worked, total real capital stock, total material costs, and total value of shipments.

Summary statistics, found below in Table 2.4, provide interesting insights into the insignificance of the foreign owned indicator in the previous analysis. While it is clear that on average foreign establishments are more productive than domestic establishments in the same industry-year, the standard deviation is large. In some industries, foreign establishments are much less productive than domestic establishments as indicated by a minimum difference in the average residuals of -1.02. The variation in productivity is met by high variation in foreign establishment environmental performance (γ_3) and fixed costs ($log(nonprod\_workers)$). In some industry-years, foreign establishments are much cleaner than domestic establishments (at minimum,γ_3= -234.32) while in others they are much dirtier (at maximum, γ_3=628.89). The expectation in the estimations that follow is that $prodgap_{jt}$ and $log(nonprod\_workers)_{jt}$ are collinear because foreign firms in the US should have higher productivity advantages as a result of higher fixed costs. We test this directly with an OLS regression of fixed costs on productivity.[13]

2.4.2 Empirical Results

Table 2.5 on the next page shows the results of the estimation of equation 2.8. Each column represents the addition of new controls. In all specifications, except Column (7), industry fixed costs are negatively correlated with γ_3 and significant with at least an α=0.10. This

[13] γ_3 was not estimable for all industry years; industry-years in which γ_3 has no standard error are dropped. Additionally, one industry, 3087: Custom Compounding of Purchased Plastics Resins, is dropped from the data. When included this industry held both the maximum and minimum values of γ_3 in the data, thus making a large swing in foreign establishment environmental performance over time. While this is interesting, it is an outlier in two respects: its erratic changes in environmental performance across time is orthogonal to the movements of the rest of the industries in the data and its maximum and minimum data points far exceed the next nearest maximum or minimum in the data. The max and min γ_3 for 3087 were 1111.02 and -648.99, respectively. See the Appendix for more details.

robust result means that foreign establishments in industries with higher fixed costs have lower hazard scores than foreign establishments in industries with lower fixed costs. To test for the relationship between fixed costs and foreign establishment productivity we include $prodgap_{jt}$ in Column (7). When foreign establishments have larger productivity advantages over US establishments they have better environmental performance, as is evident by the negative and statistically significant coefficient on $prodgap_{jt}$. Moreover, including productivity reduces the coefficient on our fixed cost proxy by 10% and renders it statistically insignificant. This reduction is small, suggesting that other industry characteristics remain that are correlated with fixed costs and foreign establishment environmental performance.

Though the collinearity between fixed costs and productivity is small, the general takeaways from Table 2.5 are: 1) foreign establishments are cleaner in industries with hight fixed costs, 2) in industries where foreign establishments are more productive than domestic establishments, foreign establishments are cleaner, and 3) some of the productivity advantage enjoyed by foreign establishments is related to fixed costs within an industry. In Column (8) we see that productivity appears to be time invariant with the inclusion of industry fixed effects. This suggests that productivity is primarily determined by cross sectional variation in industry attributes.

To determine the magnitude of the impact of fixed costs on environmental performance we must first solve for the impact of fixed costs on γ_3, then interpret the influence of the change in γ_3 on hazard scores from equation 2.7. The specifications in Table 2.5 are of level-log form, so a 1% increase in non-production workers results in a $\frac{\beta_2}{100}$ unit change in γ_3. In Column (6), this means a 1% increase in fixed costs is related to a 0.0364 reduction in the coefficient on the foreign owned indicator. Equation 2.7 is in log-level form, so the coefficient on our foreign own indicator represents the percent difference between foreign owned and domestic establishment hazard scores. As a result, β_2's impact on hazard scores can be interpreted as a reduction in the percent impact $(100 * (\gamma_3 - \beta_2)\%)$. Therefore, 1% higher fixed costs is associated with a $100 * (0.0364) = 3.64\%$ lower hazard score for foreign establishments in that industry.

The sample average of the total number of non-production workers is 17.18 thousand workers, so a 1% increase in fixed costs for the average industry is equivalent to 172 non-production workers. A standard deviation for non-production workers is approximately 20 thousand workers, or a 118% increase for the average industry with 17.18 thousand workers. This increase would result in a 430% improvement in hazard scores for foreign firms within that industry. As an example, four digit SIC industry 3491 (Industrial Valves) has an average total non-production worker count of 17.38 across years, near the sample average, and an average γ_3 of 1.32. A one standard deviation increase in non-production workers for industry

3491 would result in a γ_3 of -2.98, meaning foreign establishments would out-perform domestic establishments *ceteris paribus*.

In Column (7) of Table 2.5 we learned that $prodgap_{jt}$ and $log(nonprod\_workers)_{jt}$ have some collinearity. For completeness, we formally test for the robustness of the relationship between $prodgap_{jt}$ and fixed costs with a simple OLS regression of industry fixed costs on the foreign productivity gap in that industry. The estimates are provided in Table 2.6 below. Across all specifications, higher fixed costs are associated with larger productivity gaps. This result is robust to many other control variables. Column (7) shows us that, after holding constant time and industry invariant productivity determinants, a 1% increase in fixed costs results in a .0016 unit increase in the gap between foreign and domestic establishment performance (i.e., about 5% of a standard deviation increase).

In total, we combined estimates of four digit SIC industry-by-year foreign establishment environmental performance with industry level data to test the hypothesis that fixed costs within an industry explain some of the variation in foreign owned establishment environmental performance. Our results suggest that this hypothesis holds. However, it is interesting to note that after including fixed costs and productivity in Column (7) of Table 2.5, $prodgap_{jt}$ still has significant explanatory power. This suggests that fixed costs are not the sole determinant of the productivity advantage enjoyed by foreign establishments in the US. Fixed costs and productivity are our candidate explanation in this study; there remains other determinants of foreign establishment environmental performance that are related to fixed costs and productivity. We leave the exploration of alternative explanations to further study.

2.5 Conclusion

In this paper we compile a rich dataset of establishment level characteristics and emissions to test for differences in emissions intensities between foreign and domestic manufacturing establishments in the US. Our results suggest that there is significant heterogeneity in the environmental performance of foreign owned manufacturers both within and across industries. Our paper is the first to document the variation in environmental performance among foreign owned establishments. We then seek to explain this variation using industry level characteristics. Specifically, we study the relationship between cross-industry variation in fixed costs and establishment environmental performance. Citing recent findings in the international trade literature, we posit that the high fixed costs of FDI allow only the most productive foreign manufactures to earn profits abroad. This result, tethered with the a similar prediction that only the most productive firms invest in green capital, implies that foreign owned establishments are cleaner than their domestic competitors.

Our analysis supports this conclusion. We show that foreign establishments are cleaner in industries with hight fixed costs. Specifically, our conservative estimates suggest that foreign owned establishments in industries with 1% higher fixed costs have environmental performance 3.64% better than the average foreign establishment. Additionally, in industries where foreign establishments are more productive than domestic establishments, foreign establishments have better environmental performance. Lastly, some of the productivity advantage enjoyed by foreign establishments is related to fixed costs within an industry, but the relationship is not perfect. There remains other determinants of foreign establishment environmental performance correlated with fixed costs.

Our results are important in the context of concerns about dirty production processes migrating across country borders. If only the cleanest establishments find it profitable to locate production abroad, these concerns may be misplaced. Given that fixed costs tend to render establishments immobile, we ask whether firms that can overcome those fixed costs to engage in FDI pollute less intensely than firms in lower cost industries. Our results suggest that they are, in fact, cleaner. In designing policy to prevent the migration of pollution into a country from inward FDI, resources would be wasted if policymakers focus regulatory oversight on foreign establishments in pollution intensive industries with high fixed costs.

2.6 Appendix

2.6.1 Main Tables and Figures

Table 2.1: Summary Statistics for TRI Reporters (N=64,973)

Variable	Pooled	Domestic Owned	Foreign Owned
Sales (million $)	26.54	26.18	39.01
	(69.28)	(69.60)	(56.06)
Employees	266.349	265.93	280.71
	(543.30)	(548.06)	(342.52)
Relatives	122.551	125.95	6.22
	(388.56)	(393.61)	(44.25)
Kids	1.19	1.16	2.07
	(6.96)	(7.01)	(5.14)
Credit Rating (D&B Paydex Min)	67.53	67.50	68.76
	(9.59)	(9.62)	(8.47)
Hazard (millions)	2275.54	2299.59	1452.22
	(26407.97)	(26,762.29)	(7205.33)
Hazard/Sales	873.59	863.11	1231.98
	(61846.22)	(62,693.54)	(14,600.96)
Foreign Owned	0.028	0	1
	(0.166)	.	.
TRI Reporter*	0.027	0.026	0.064
	(0.161)	(0.160)	(0.245)

**All variables are conditional on the establishment reporting the TRI except TRI Reporter, which is summarized over the universe of firms in the NETS dataset (N=2,423,435). It demonstrates the percent of each group that reports to the TRI. Hazard scores are only available for establishments that report to the TRI. Relatives represents the count of firms belonging to an establishment's family tree. Kids represents the count of firms reporting the establishment as a parent. Credit rating is an index indicating an establishments history of debt repayment.*

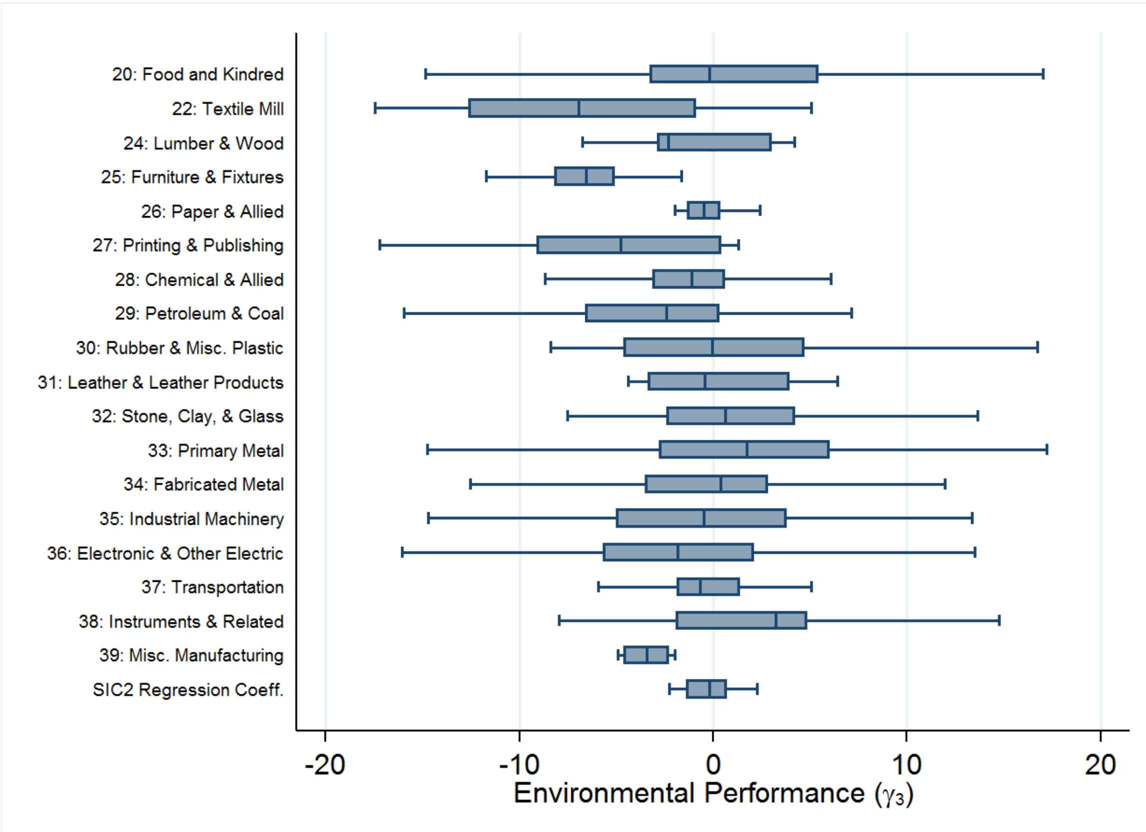

Figure 2.1: Estimated Foreign Owned Environmental Performance Relative to Domestic Owned Establishments in the Same Industry

Note: This figure reports the estimated environmental performance of foreign owned establishments relative to domestic owned establishments in the same industry. Each line represents the distribution of point estimates for the coefficient on an indicator variable for foreign owned establishments from a selection model estimated on a four digit SIC industry and year (γ_3) within a given two-digit industry (the last row is two digit SIC by year estimates). The box represents the intra-quartile range, the bar in the middle of the box represents the median. The line segments represent the tails of the distribution. showing an upper bound of 1.5 times above the 75th percentile or 1.5 times below the 25th percentile of data. Negative numbers indicate industries in which foreign owned establishments are estimated to be cleaner than domestic owned establishments.

Table 2.2: Environmental Performance of Foreign Owned Manufacturing Establishments

	(1) Log(Hazard)	(2) Log(Hazard)	(3) Log(Hazard)	(4) TRI Reporter	(5) Log(Hazard)
Foreign Owned	0.9116***	0.4475	0.3778	0.0015	0.1762
	(0.2996)	(0.2930)	(0.2830)	(0.0047)	(0.2820)
Log(Emp)			0.9018***	0.0290***	0.8557***
			(0.0386)	(0.0004)	(0.0994)
Industry x Year FE	N	Y	Y	Y	Y
Selection Model	N	N	N	N	Y
R^2	0.001	0.364	0.407	0.130	0.441
N	64820	64820	64820	2,322,598	60830

Note: Columns 1-3 report regression results for the level of pollution emissions measured by EPA's hazard score conditional on reporting emissions to EPA. Column 4 reports the results of a linear probability model where the dependent variable is equal to 1 if the establishment reports emissions to EPA. Column 5 reports a selection model where the first stage controls for selection into TRI reporter status and the second stage estimates the environmental performance of foreign owned establishments controlling for selection into TRI reporting. The results provide little evidence that foreign owned establishments pollute more than domestic owned competitors in the same industry. All standard errors clustered at the establishment level and reported in parentheses.

Table 2.3: Within Industry Variation in Foreign Owned Environmental Performance

SIC	Mean	Std. Dev	Min	Max	N	Description
20	-6.15	40.92	-234.32	50.63	40	Food & Kindred
22	-1.17	22.74	-39.82	59.86	20	Textile Mills
24	-0.61	3.66	-6.73	4.17	12	Lumber & Wood
25	-6.04	3.13	-11.73	0.09	13	Furniture & Fixtures
26	2.29	15.25	-1.96	89.71	35	Paper & Allied
27	-5.45	6.49	-17.23	1.32	7	Printing & Publishing
28	-1.33	3.96	-23.83	8.48	157	Chemical & Allied
29	-3.76	6.33	-15.95	7.13	17	Petroleum & Coal
30	11.08	160.03	-648.99	1111.02	91	Rubber & Misc. Plastic
31	0.29	4.73	-4.36	6.39	4	Leather & Leather Products
32	0.71	4.85	-16.91	13.67	59	Stone, Clay, & Glass
33	1.1	9.3	-43.18	34.95	144	Primary Metal
34	-1.82	11.12	-108.72	20.88	146	Fabricated Metal
35	-0.43	10.29	-50.34	45.77	86	Industrial Machinery
36	5.36	55.99	-45.22	628.89	156	Electronic & Other Electric
37	8.19	31.59	-5.94	139.49	26	Transportation
38	0.99	15.1	-80.4	33.04	41	Instruments & Related
39	-3.44	1.36	-4.92	-1.97	4	Misc. Manufacturing
Total	1.32	53.02	-648.99	1111.02	1,058	

Note: Each row represents summary statistics on the environmental performance of foreign owned establishments estimated at the four digit SIC-by-year level and summarized at the two digit SIC industry level. Results limited to four digit industries and years with sufficient observations to estimate the Heckman selection model on the environmental performance of foreign owned establishments.

Table 2.4: Summary Statistics (Industry Level)

Variable	Mean	Std. Dev.	Min.	Max.
FO Coefficient (γ_3)	0.4	25.257	-234.324	628.889
Productivity Gap	0.249	0.3	-1.02	1.472
log(Non-production Workers)	2.32	1.026	-0.105	4.745
log(Production Worker Hours)	3.94	0.971	1.668	6.754
log(Real Capital Stock)	8.258	1.111	5.268	11.291
log(Material Costs)	8.212	1.14	4.785	11.274
log(Value of Shipments)	8.885	1.172	5.814	14.809

Note: Non-Production Workers are in 1000s and Production Worker Hours are in millions. Real Capital Stock, Material Costs, and Value of Shipments are in millions of real dollars. Material costs include energy costs. All dollar values are inflation adjusted. This table demonstrates the considerable heterogeneity across industries for each establishment characteristic. N=1046.

Table 2.5: Foreign Owned Environmental Performance, Fixed Costs, and Productivity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
log(Non-production Workers)	-1.495**	-3.337**	-3.153*	-3.988**	-3.854**	-3.643*	-3.264	-16.09*
	(0.050)	(0.036)	(0.074)	(0.033)	(0.047)	(0.066)	(0.101)	(0.065)
log(Production Worker Hours)		2.216	2.279	2.136	2.135	2.113	2.085	-20.41**
		(0.189)	(0.182)	(0.211)	(0.212)	(0.224)	(0.230)	(0.046)
log(Real Capital Stock)			-0.279	-1.796	-1.592	-1.580	-1.671	20.85**
			(0.808)	(0.256)	(0.367)	(0.376)	(0.349)	(0.041)
log(Material Costs)				2.417	2.753	2.679	2.526	-2.811
				(0.165)	(0.204)	(0.218)	(0.246)	(0.726)
log(Value of Shipments)					-0.632	-0.756	-0.692	16.36
					(0.795)	(0.758)	(0.778)	(0.153)
Productivity Gap							-4.573*	-0.103
							(0.085)	(0.982)
Constant	3.868**	-0.587	1.038	-3.778	-2.913	-0.829	0.936	-167.5*
	(0.045)	(0.880)	(0.893)	(0.656)	(0.749)	(0.932)	(0.923)	(0.053)
Fixed Effects							Year	Year, SIC4
Adjusted R^2	0.0027	0.0034	0.0025	0.0034	0.0025	-0.0009	0.0010	0.0696

N=1046, p-values in parentheses

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

Table 2.6: Foreign Owned Productivity and Fixed Costs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
log(Non-production Workers)	0.0442***	0.0586***	0.0785***	0.0875***	0.0849***	0.0829***	0.156**
	(0.000)	(0.002)	(0.000)	(0.000)	(0.000)	(0.000)	(0.015)
log(Production Worker Hours)		-0.0173	-0.0105	-0.00891	-0.00888	-0.00606	-0.0168
		(0.382)	(0.602)	(0.657)	(0.658)	(0.767)	(0.824)
log(Real Capital Stock)			-0.0301**	-0.0138	-0.0178	-0.0198	0.144*
			(0.025)	(0.459)	(0.391)	(0.345)	(0.055)
log(Material Costs)				-0.0260	-0.0327	-0.0335	0.113*
				(0.203)	(0.200)	(0.192)	(0.056)
log(Value of Shipments)					0.0124	0.0141	-0.267***
					(0.663)	(0.627)	(0.002)
Constant	0.146***	0.181***	0.357***	0.409***	0.392***	0.386***	0.251
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.694)
Fixed Effects						Year	Year, SIC4
Adjusted R^2	0.0219	0.0216	0.0254	0.0260	0.0252	-0.0174	0.6406

N=1046, p-values in parentheses

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

2.6.2 Descriptive Statistics

Table 2.7: Clean and Dirty Foreign Owned Firms by Industry

	(Cleanest Foreign Owned Firms)
2022	Cheese; Natural and Processed
2011	Meat Packing Plants
2759	Commercial Printing, Nec
3312	Blast Furnaces and Steel Mills
	(Dirtiest Foreign Owned Firms)
3463	Nonferrous Forgings
3317	Steel Pipe and Tubes
2046	Wet Corn Milling
3087	Custom Compound Purchased Resins

2.6.3 Foreign Owned Environmental Performance Sample Robustness

Table 2.8: Baseline Estimates for Various Samples

	(1) Full Sample	(2) Omitted SIC4: 3087
log(Non-production Workers)	-4.030	-3.643*
	(0.332)	(0.066)
log(Production Worker Hours)	1.358	2.113
	(0.709)	(0.224)
log(Real Capital Stock)	-2.022	-1.580
	(0.585)	(0.376)
log(Material Costs)	4.207	2.679
	(0.355)	(0.218)
log(Value of Shipments)	-1.226	-0.756
	(0.812)	(0.758)
Constant	-1.482	-0.829
	(0.942)	(0.932)
Observations	1058	1046

p-values in parentheses

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

Note: This table shows the baseline estimation results for a regression of productivity on fixed costs when we remove industry 3087 outliers.

In Table 2.8 above we show the estimates of Column (6) from Table 2.5 for samples with and without four digit SIC 3087. The industry had data for 12 years but only 1 foreign establishment reported emissions for each year from 1995 to 2006.This establishment was much cleaner than domestic establishments at the beginning of the panel and much dirtier toward the end. With its omission, the effect of fixed costs is consistent across the two samples but becomes significant once the heavy outlier is removed. Even with it's inclusion the point estimate on fixed costs is the correct sign.

2.6.4 Foreign Owned Environmental Performance Robustness Over Estimations of γ_3

Table 2.9: Foreign Owned Environmental Performance Robustness Checks

	(1)	(2)	(3)	(4)
log(Non-production Workers)	-3.264	-0.471	-6.010*	-6.234**
	(0.101)	(0.141)	(0.072)	(0.029)
log(Production Worker Hours)	2.085	-0.0630	2.903	2.872
	(0.230)	(0.821)	(0.322)	(0.247)
log(Real Capital Stock)	-1.671	0.155	-3.877	-0.900
	(0.349)	(0.589)	(0.188)	(0.718)
log(Material Costs)	2.526	0.354	2.784	1.298
	(0.246)	(0.312)	(0.441)	(0.668)
log(Value of Shipments)	-0.692	-0.760*	2.028	0.740
	(0.778)	(0.054)	(0.618)	(0.829)
Productivity Gap	-4.573*	-0.875**	-6.381	-9.043**
	(0.085)	(0.040)	(0.147)	(0.016)
Constant	0.936	4.361***	-3.058	-1.251
	(0.923)	(0.005)	(0.849)	(0.927)
Observations	1046	1046	1022	991

p-values in parentheses

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

Note: The table above repeats the estimation of Column (7) from Table 5 using estimated γ_3's from OLS specifications with various controls. Results appear generally robust to a variety of controls.

Table 2.9 includes four estimations of the specification presented in Column (7) from Table 2.5. In each column, the dependent variable (γ_3) is collected from a different within four digit SIC industry regression. Each specification that generates γ_3 has industry and year fixed effects as well as a control for logged employment. In Column 1, γ_3 is the estimated results from Column (7) in Table 2.5. Column (2) presents results using γ_3 from an OLS regression without controlling for selection and controlling for logged sales. In Column (3) γ_3

is gathered from an estimation that controls for selection and the age of the establishment. While we do not observe the age of the establishment directly, we do observe the first year the firm entered the Dun&Bradstreet database. Since Dun&Bradstreet is a comprehensive source of all establishments, we trust this captures with marginal error the birth date of the firm. Column (4) adds logged sales as an additional control to Column (3). Across all specifications the results seem generally robust. The coefficients on log(Non-production Workers) and Productivity Gap remain negative and only vary slightly in significance.

2.6.5 Foreign Owned Environmental Performance Estimations using γ_3 from Equation (2.7)

Table 2.10 shows the results from replicating Table 2.5 while controlling for logged sales in the generation of γ_3 for each industry year. We do not include sales as a control in the specification used in our results section because reporting of sales in Dun&Bradstreet data is rare. When unavailable, NETS uses an imputed sales value based off of employment (which is more robustly reported). Coincidentally, employment in our model captures the output of the establishment. In other words, we are assuming output at a firm is increasing in employees. See the online appendix for Holladay (ming) for more information on sales imputation.

The results in Table 2.10 appear qualitatively similar. The column of interest is Column (7) in which productivity is added to the estimation. In both cases, the magnitude of log(Non-production Workers) is reduced by 10%. The only marked difference is that the estimation of the coefficient on log(Non-production Workers) is larger and more precise. This could be due to the loss of industry-years between specifications.

Table 2.10: Foreign Owned Environmental Performance using γ_3 estimated from Equation (2.7)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
log(Non-production Workers)	-3.096***	-5.384***	-5.427**	-6.651***	-6.658***	-6.295**	-5.773**	-34.77***
	(0.001)	(0.008)	(0.016)	(0.005)	(0.007)	(0.012)	(0.022)	(0.002)
log(Production Worker Hours)		2.752	2.737	2.528	2.528	2.180	2.141	-18.44
		(0.198)	(0.206)	(0.244)	(0.244)	(0.323)	(0.331)	(0.161)
log(Real Capital Stock)			0.0644	-2.163	-2.172	-1.957	-2.081	21.67*
			(0.965)	(0.282)	(0.333)	(0.388)	(0.358)	(0.098)
log(Material Costs)				3.547	3.531	3.336	3.125	-5.149
				(0.108)	(0.199)	(0.227)	(0.258)	(0.618)
log(Value of Shipments)					0.0293	-0.0360	0.0526	24.06
					(0.992)	(0.991)	(0.987)	(0.102)
Productivity Gap							-6.297*	-1.532
							(0.061)	(0.790)
Constant	8.407***	2.874	2.498	-4.570	-4.610	-1.887	0.544	-183.5*
	(0.001)	(0.562)	(0.799)	(0.671)	(0.690)	(0.878)	(0.965)	(0.100)
Observations	1046	1046	1046	1046	1046	1046	1046	1046

p-values in parentheses

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

Chapter 3

The Incidence of the Post-9/11 GI Bill Subsidy at Institutions of Higher Education: A Study of the Response of In-State Tuition and Fees to Veteran Education Benefits

Abstract

Student charges are growing rapidly at colleges and universities in the United States. Policymakers try to keep pace with the growing cost of education by offering generous financial aid packages to students in-need. In theory, giving aid to needy students relaxes their budget constraint by lowering the relative price of attending college. Assuming away a large income effect from the price subsidy, this increases their investment in education. However, this theoretical prediction relies on the established charges at colleges remaining constant. If institutions of higher education increase their student charges in response to increases in student aid, the goals of financial aid policies are undermined. In this paper I test for supply side responses using the Post-9/11 GI Bill which became available in academic year 2009. I exploit variation in veteran student enrollment and veteran benefits across states, pre and post policy, and find significant increases in fees and tuition rates at 4-year public and 2-year for-profit colleges in response to the Post-9/11 GI Bill. Specifically, 4-year public schools with 1 percentage point larger veteran student populations have in-state tuition rates that are 4.05% higher on average in 2009. Additionally, every 1% increase in veteran tuition benefits is accompanied by a 0.1% increase in tuition rates at 2-year for-profit colleges.

3.1 Introduction

Student charges are growing rapidly at colleges and universities in the United States (US). In the 2014-2015 academic year, public 4-year college tuition and fee rates averaged \$9,139 (up 17% from the 2009-10 academic year) while rates at 4-year private nonprofit schools averaged \$31,231 (up 10% from the 2009-10 academic year).[1] Policymakers try to keep pace with the growing cost of education by offering generous financial aid packages to students in-need. In theory, giving aid to needy students relaxes their budget constraint by lowering the relative price of attending college. Assuming away a large income effect from the price subsidy, this increases their investment in education. However, this theoretical prediction relies on the established charges at colleges remaining constant. If institutions of higher education increase their student charges in response to increases in student aid, the goals of financial aid policies are undermined.[2]

In this paper I directly test for supply side responses to federal financial aid at 4-year and 2-year public, private nonprofit (hereafter "private"), and for-profit institutions. Specifically, I study the decisions of institutions over their in-state tuition and fee rates in response to the Post-9/11 GI Bill. I contribute to the literature in two ways. First, this paper presents an empirical test of subsidy incidence at institutions of higher education. If tuition rates and fees increase in response to education subsidies then the benefits of the subsidy are split between students and higher education institutions. Secondly, I am the first to consider the supply side's response to GI Bill educational benefits across all school sectors. Prior studies of past GI Bills study the enrollment, enlistment, and reenlistment effects of the benefit. No attention has been given to how student charges at institutions respond to changes in veteran benefit packages.

The majority of existing supply side research has centered on federal financial aid, with particular interest to the Pell grant.[3] Findings at public institutions have ranged from negligible impacts to \$50 increases in tuition for every \$100 increase in Pell aid (Rizzo and Ehrenberg (2004)). Frederick et al. (2012) test for the response of student charges at community colleges to financial aid, as well as other forms of federal support like federal operating funds, and find no support for increases in tuition rates to federal benefits. However, schools in different sectors may repsond differently to tuition and fee subsidies. For example,

[1] See The College Board's "Annual Survey of Colleges".

[2] This pricing behavior has been popularly named the "Bennett Hypothesis" after former Secretary of Education William Bennett expressed concern about the supply side responses of higher education institutes in a 1987 *New York Times* editorial.

[3] The Pell grant is a federal need-based subsidy given to students with low incomes. For literature on the supply side response to state aid see Long (2004a). For research regarding student charges and tax based educational benefits see Turner (2012) and Long (2004b).

private schools determine student charges internally while public schools most often must negotiate with state legislatures to set tuition and fees. Coincidentally, public schools may have "stickier" rates. To that end, the literature has found some traction at private schools and along other margins at public schools. Singell and Stone (2007) show that federal Pell grants are crowded out one-for-one by tuition increases at private institutions with similar responses for out-of-state tuition rates at public schools. Turner (2014) shows that decreases in institutional aid at selective private schools crowds out nearly two-thirds of Pell grant aid.

There are only two papers studying the response of tuition rates to financial aid at for-profit schools. Cellini and Goldin (2012) study tuition responses to Title IV financial aid at for-profit institutions. Rather than exploiting variation in the amount of Title IV funds paid to for-profits, they study tuition rates at Title IV eligible and ineligible institutions. They find that eligible institutions set tuition 78% higher than comparable, non-Title IV eligible institutions. However, one confound in their paper is that quality differences likely exist between ineligible and eligible schools that correlate with tuition rates. Turner (2014) also considers pricing responses by for profit students. The author finds that 6% of each student's Pell Grant funds at for-profit institutions are captured by reduced institutional aid.

Studying subsidy incidence at institutions of higher education is difficult because financial aid is often endogenous. For example, federal financial aid is based on student need which is determined, in part, by tuition rates and student charges. As a result, student charges are larger at institutions where students possess more need based aid, but need based aid is also larger at institutions with higher costs. However, the Post-9/11 GI Bill benefits are exogenous to student charges and provide a nearly ideal random experiment to test for supply side responses. First of all, the Post-9/11 GI Bill became available for all accredited US degree granting institutions in August of 2009. Therefore, in a sample of accredited 4-year and 2-year institutions, all schools are eligible in 2009. Secondly, at the time of the policy only veterans could use their benefits for education. As a result, veteran-sparse schools with a low percentage of GI Bill users per enrolled student serve as a comparison group for relatively veteran-dense schools. I do not observe enrollment composition at the institutional level, but I do observe GI Bill use at the state level and proxy for treatment with the percent of GI Bill users by state and year. That is, schools in states with a larger proportion of veteran students are more likely to enroll veterans and receive GI Bill funds. The differential in veteran density across states should have no impact on tuition rates prior to the Post-9/11 GI Bill because education subsidies for veterans in this time period were a flat nominal rate nationwide.

Lastly, veterans who served 3 years or more on active duty after September 10th, 2001 were eligible to receive maximum education benefits equaling the highest full cost of any public college in an individual's state of enrollment. Since the benefits are based on the charges at *one* public school in a state, all other public schools and every private and for-profit within that state receive an exogenous increase in veteran ability to pay that is uncorrelated with their existing charges. As a result, veterans in some states realized large increases in nominal benefit levels while others saw little change at all over the existing policy. For treated institutions (i.e., in states with large veteran densities), the variation in benefit levels aligns schools along a continuum of treatment dosage where schools in low benefit states receive the smallest dosage of treatment and those in high benefit states received the largest dosage. Whereas an institution's student charges is a large determinant of the amount of Title IV aid a student receives, the amount of GI Bill a veteran receives is exogenous to the price of the institution. The Post-9/11 GI Bill provides a much cleaner test of the supply side responses of higher education institutions.

Interestingly, the response of institutions of higher education to GI Bill benefits has not yet been studied, likely because GI Bills prior to 2009 were modest in size. For example, the 2008 Montgomery GI Bill (MGIB) that preceded the Post-9/11 GI Bill offered veterans a maximum of $1,321 per month for 36 months to use for tuition, fees, books, and other educational related expenses. In 2009, however, the median tuition benefit level offered to Post-9/11 GI Bill eligible veterans was $343 per credit-hour, or $457.33 a month for tuition alone.[4] In Texas veterans were given $1,471 per credit-hour and $12,130 for fees which totals to $47,434 annually (compared to $12,000 annually for the MGIB in 2008). In addition to large tuition and fee reimbursements, veterans also receive funds for housing and a book stipend. Unlike prior GI Bills, Post-9/11 GI Bill tuition and fee benefits are paid directly to the school. The large increase in education subsidies for veterans is accompanied by a larger number of veterans using the GI Bill, increasing from 502,489 benefit users in fiscal year 2009 to 784,473 at the end of fiscal year 2010.[5] Since benefits are large and paid directly to schools, and since veterans have a greater presence in the college classroom, the Post-9/11 GI Bill stands to measurably impact decision making at institutions of higher education.

The data I use for this study is an institutional-level panel dataset from academic year 2006 to 2011 compiled from the Integrated Postsecondary Education Data System (IPEDS) of the National Center for Education Statistics (NCES).[6] My empirical strategy harnesses the unique variation in veteran student densities and benefit levels across time and space: 2009 separates institutions into ineligible (pre-2009) and eligible (post-2009) academic years while state-by-state variation in the percent of the student population using the GI Bill captures variation in treatment at schools across states. I test for the significance of treatment

[4] Calculation based on full time enrollment at 12 credit hours divided by 9 months of schooling.

[5] See the Veteran Benefit Administration's Annual Benefits Reports for years 2000-2012.

[6] I balance the panel by eliminating all schools that were not active from 2006 to 2011. This ensures that schools do not enter into states with high veteran density to capture benefit levels thereby biasing results.

interacted with all years pre- and post-policy, hypothesizing that student charges should only be differentially impacted in treatment years (i.e., 2009, 2010, and 2011 academic years). Additionally, I exploit the exogenous variation in dosage levels by interacting each year-by-treatment pair with the logged level of benefit in that state, varying the intensity of treatment. Although my treatment and dosage measures are continuous, these methods approximate difference-in-differences and triple difference approaches.

My results suggest the response to the Post-9/11 GI Bill subsidies is quite heterogeneous across charges and sectors. For public institutions I find evidence that tuition and fees increase for 4-year schools. Specifically, a state with 1 percentage point higher proportions of veteran students have tuition rates that are 4.05% higher. Moreover, every 1% increase in student fee reimbursement is accompanied by a 0.08% increase in fee charges. Private not-for-profit 2- and 4-year institutions show only a weak response to the Post-9/11 GI Bill by increasing tuition and fees, although this is generally insignificant. I find tuition and fee rates are lower at 4-year for-profits. A 4-year for-profit in a state with 1 percentage point larger percentage of students that are veterans has 2.25% lower tuition rates and 10.2% lower fees. However, I find 2-year for-profits increase tuition. Every 1% increase in veteran tuition benefits is accompanied by a 0.1% increase in tuition rates at 2-year for-profit colleges. One potential explanation for the downward effect on 4-year for-profit is the documented shift in veteran enrollment as a result of the Post-9/11 GI Bill found in Barr (2015). He shows enrollment is moving away from the more expensive 4-year private and for-profit options.

The positive and significant result for 2-year for-profits has greater policy implications given recent interest in the capture of Post-9/11 GI Bill funds at for-profit colleges. The rapid growth of the for-profit college sector over the last decade has policymakers concerned that for-profits are sustaining themselves with tax funded education benefits. These concerns may not be misplaced; despite enrolling 12% of all postsecondary students, for-profits collect 24% of Pell Grant disbursements and 26% of federal student loan disbursements (Deming et al. (2012)). One reason this regulatory capture occurs is that for-profit colleges typically attract lower income students that qualify for large amounts of financial aid (e.g., see Cellini (2005), Chung (2009)). However, it is also estimated that for-profits received nearly half of all GI Bill funds despite enrolling only 23% of veterans (Harkin (2010)). My results suggest that prices may be driven upwards at 2-year for-profit colleges in response to the Post-9/11 GI Bill.[7] I find evidence for tuition increases only in states with high treatment dosages.

In the section that follows I describe in greater detail the provisions of the Post-9/11 GI Bill and describe theoretical predictions for supply side responses to financial aid. In Section

[7]For a descriptive analysis of the early uptake of the Post-9/11 GI Bill see Michael Sewall's article for *The Chronicle of Higher Education* entitled "Veterans Use New GI Bill Largely at For-profit and 2-year Colleges."

4.3 I describe my data and estimation strategy and results. I make concluding remarks in Section 4.4.

3.2 Veteran Education Subsidies and Tuition Rates

3.2.1 The Post-9/11 GI Bill Benefits

The Montgomery GI Bill (MGIB) was the prevailing education benefit source for veterans of active duty service in the US military between 1985 and 2009. To be eligible, soldiers must agree to participate in the program upon enlistment into the military and have their pay reduced by $100 for the first 12 months of service. Additionally, the serviceman must complete a minimum agreed upon contract of service which was generally 3 years.[8] Importantly, the subsidy was an equal flat-rate amount paid to all participating military personnel regardless of state of residence and cost of institution. Periodically, the subsidy was increased but the nominal value of the benefit remained the same for all veterans in the US. In October of 2009, a full time student completing 3 years of active duty service was eligible for 12 months of Montgomery GI Bill benefits valued at $16,416 for the year ($1,368 per month). Funds were paid directly to the student for use on housing and schooling expenditures and required periodic proof of enrollment to maintain benefits.[9]

The passage of the Veterans Education Assistance Act of 2008 dramatically improved education benefits for the active duty military. The act, eventually deemed the Post-9/11 GI Bill, retroactively covered veterans serving at least 90 days on active duty following September 10th, 2001.[10] Service members no longer needed to enroll in the program upon enlistment or make contributions from their paycheck. Importantly, tuition and fee benefits were large, equaling up to the highest tuition level and fee level at any public institution in a veteran's state of residence. In order to receive maximum benefits, veterans must have served 36 months on active duty and received honorable discharge.[11] Additionally, veterans received $1,000 book stipend and a housing stipend based on the price of homes in the veterans living area. In total, maximum benefits for one semester of study in some states were larger than

[8] Military personnel who had received a certain amount of scholarship from ROTC participation or were officers commissioned from a military academy were ineligible.

[9] Funds could be used for a variety of educational opportunities. In addition to degree granting institutions, veterans could use benefits to study at vocational schools, for apprenticeships and flight classes, and even for tests and fees.

[10] Funds could only be used for future schooling, not past tuition expenditures

[11] Benefit levels were tiered based on length of active duty service, with 90 days allowing for 40% of maximum benefits. While 3 years of service earned maximum benefits, 2 years earned 80%. The overwhelming majority of enlistment contracts exceed 2 years Barr (2015). Commissioned officers and ROTC scholarship recipients are eligible provided a commitment of additional years of service.

the total of 36 months of MGIB benefits. As a comparison, the monthly MGIB amount in 2008 was $1,321 while students in Texas received $1,471 *per credit hour*.

The importance of the variation in maximum benefit levels under the Post-9/11 GI Bill is that they create an exogenous increase in educational benefits. Although MGIB benefits are equal nationwide, their purchasing power is determined in part by the average tuition prices in that state. Veterans in states with low-cost colleges received the largest real subsidy under the MGIB policy and those from states with most expensive colleges received a smaller real subsidy. Since the Post-9/11 GI Bill pins benefit levels to the tuition rates and fees of the most expensive public degree granting institution in the veteran's state of enrollment, one might think that students who benefit the most are now those from high-cost states. In many states, this is true. However, the Veteran Affairs website discredits this comparison on the basis of the state approving agencies' ad hoc adjustments of the stated tuition rates at each institution.[12] The agency considered all undergraduate program costs, including high cost programs such as flight courses or undergraduate pharmacy, nursing, or engineering charges. For example, program specific charges, like engineering, that are well above all other public schools stated tuition rates in a state would be considered the public maximum tuition and fee rate. This further randomizes the benefit increases across states.

This is the first study to investigate the supply side response to the GI Bill benefits. Prior research by Simon et al. (2010), Seftor and Turner (2002), Angrist and Chen (2011), etc. are primarily concerned with various GI Bill's impact on enrollment and military enlistment. However, this is not the first study to exploit the unique variation in Post-9/11 GI Bill benefits. Barr (2015) studies the Post-9/11 GI Bill's impact on enrollment and persistence. Comparing veterans to non-veterans, both of non-traditional college age, across time and states he finds that an additional $1,000 in maximum GI Bill reimbursement rates increased enrollment rates by 1 percentage point on average, but this impact was larger for states with larger increases in benefits. He also finds veterans tend to shift enrollment to four-year public institutions, not to for-profit institutions. Specifically, 75% of the 1 percentage point increase in enrollment was to public institutions. The author posits that an explanation for the small one percentage point increase in enrollment to the GI Bill subsidy could be because many veterans also qualify for large amounts of existing financial aid. However, an alternative explanation could be the supply side response studied in this paper.

[12]Each state has a State Approving Agency that oversees education and training programs for veterans. In order for an institution to receive funds, they must be approved by the State Approving Agency. The State Approving Agency also reports to the National Association of State Approving Agencies the maximum tuition and fee rate for that year.

3.2.2 The Supply Side Response

The supply side literature in the economics of higher education has not yet settled on a formal model of the objectives of colleges. It is still unknown whether colleges compete with one another and how they compete (i.e., enrollment or prices).[13] In the absence of a formal model of the college's optimization decision, past research has assumed that schools seek to maximize quality in order to increase prestige (Long (2004a)). McPherson and Schapiro (1991) outline several specific objectives: maintain and improve educational quality, expand the applicant pool, and improve prestige.

In order to meet these goals colleges require additional resources. For example, enhancing quality or enrolling more students increases costs, so revenue must be raised to cover these costs. Colleges can increase revenues from tuition and fees by enrolling more students or increasing student charges. The decision to increase revenues through increasing student charges, though, faces downward pressure from student budget constraints. Subsidies act to relax students' budget constraints for higher education, increasing their ability to pay. Since students are now able to pay more for college, colleges are incentivized to gather additional revenues through increased tuition rates. Coincidentally, students do not realize the full benefit of the subsidy (assuming supply is not highly inelastic). The potential for subsidy incidence in higher education markets was first noted in 1987 by former Secretary of Education William Bennett. In a *New York Times* editorial article he posited that because government aid enables students to pay more for education, colleges would respond by raising their tuition rates and holding enrollment constant (the "Bennet Hypothesis"). This paper is an empirical test for subsidy incidence in the market for higher education.

Admittedly, by studying only the response of tuition and fees this study misses several margins upon which institutions of higher education can adjust to appropriate financial aid funds. In addition to tuition and fees, schools can reduce institutional financial aid thereby using full tuition veterans to subsidize other student enrollment which frees up funds internally for other quality enhancing expenditures. This practice may be strongest at private institutions who already have large sticker prices, far above the maximum GI Bill benefits, but who offer large amounts of institutional financial aid. It may also be a practice of public institutions who negotiate student charges with state legislatures. Policymakers may find it unappetizing to levy increased student charges on voting constituents. Therefore, they too may reduce institutional aid or reduce state support for the college to appropriate

[13] Singell and Stone (2007) suggest that imperfect competition could arise since colleges are highly differentiated: public versus private, liberal arts versus comprehensive, selective versus less-selective, etc. Cellini (2009) is a notable empirical investigation of competition in the higher education market. She documents empirical evidence that 2-year for-profits and public institutions compete over quantities of students.

funds. Additionally, many institutions secure a large amount of revenues from out-of-state students (Rizzo and Ehrenberg (2004), Singell and Stone (2007)). Since veterans essentially increase the number of full tuition students, this eases a college's budget constraints and allows them to decrease the price of out-of-state tuition (or, enroll less out-of-state students). Movements on these unobserved margins only serve to bias my results towards zero and make any significant result even stronger.

3.3 Empirical Approach, Data, and Results

3.3.1 Empirical Model

I exploit the exogenous variation in veteran student densities and subsidy levels across states created by the Post-9/11 GI Bill to study the supply side response of 4- and 2-year public, private, and for-profit institutions for years 2006-2011. A difference-in-differences estimator would compare student charges between treated and untreated institutions, before and after the first year of funding (i.e., academic year 2009). The policy does not allow for an untreated group, however, since all schools were eligible to receive funds so long as they enrolled veterans. This implies that schools who do not enroll veterans would represent a treatment group. I do not observe veteran enrollment counts at each institution in my data, but I do observe the number of GI Bill users by state. I use the across state variation in veteran student enrollment by measuring the number of veterans students divided by total enrollment. By this, institutions in states with a high proportion of students that are veterans have higher likelihood of treatment than institutions in states with lower proportions of veteran students. Therefore, I approximate a difference-in-differences estimator by comparing states with a relatively larger proportion of GI Bill users (treatment) to states with relatively lower proportion of GI Bill users (control) both before and after the policy.

Across states there is also variation in the amount of funds each veteran receives for tuition and fees. This aligns "treated" institutions along a continuum of treatment intensities, where institutions enrolling veterans in low benefit states receive the smallest dosage of treatment and those in high benefit states receive the largest dosage. Thus, I can collect the differences between treatment and control and compare them for years before and after the policy, then compare that difference for high and low benefit states. This is an approximation to the so-called triple difference estimator. Both specifications are shown in (3.1) and (3.2) below:

$$log(T_{ist}) = \beta_0 + \beta_1 * \bar{X}_{st} + \beta_2 enroll_{ist} + \phi_i + D^2(\tau_t, vetpct_{st}) + \epsilon_{ist} \tag{3.1}$$

$$log(T_{ist}) = \beta_0 + \beta_1 * \bar{X}_{st} + \beta_2 enroll_{ist} + \phi_i + D^3(\tau_t, vetpct_{st}, log(vetben09_s)) + \epsilon_{ist} \tag{3.2}$$

where T_{ist} is the log of annual student tuition or log of student fees (depending on the specification) at institution i, in state s, during academic year t.

$D(\bullet)$ is an operator that maps inputs into an exhaustive linear summation of interactions as if in a difference-in-difference (D^2) or triple difference (D^3) framework. The proportion of veteran student enrollment by state and year ($vetpct_{st}$) is computed from GI Bill use counts found in the Veteran Benefit Administration's Annual Benefits Reports and IPEDS institutional enrollment data for all sectors.[14] Benefit levels ($vetben09_s$) in this study are the posted per-credit hour tuition reimbursement rates or fee rates for academic year 2009, depending on the dependent variable.[15] Rates can be gathered from the Department of Veteran Affairs website.[16]

The interactions of interest in equation (3.1) is $\tau_t * vetpct_{st}$ and $\tau_t * vetben09_s * vetpct_{st}$ in equation (3.2), where $t = (2006, ..., 2011)$. By this approach, the interactions of treatment and dosage with years prior to 2009 embed a falsification test into the study; veteran densities and benefit levels should have no differential impact across states prior to the academic year 2009. Coefficients on $\tau_t * vetpct_{st}$ and $\tau_t * vetben09_s * vetpct_{st}$ for treatment years are interpreted as increases in tuition rates above the omitted academic year 2006. This is appropriate since the uninteracted academic year fixed effects control for upward trends in tuition rates after 2006. Therefore, interacting $vetpct_{st}$ and $vetben09_s$ with academic year fixed effects will demonstrate if tuition rates systematically deviated from trend in states with larger proportions of veterans and benefit levels. In sum, academic year 2009 separates individuals into eligible and ineligible groups while $vetpct_{st}$ approximates a binary variable that separates states into treated and untreated. Then, the interaction of $vetben09_s$ captures the impact of changes in elasticities across all eligible institutions receiving treatment. The third interaction, therefore, captures the additional impact of high dosage relative to a low dosage state.

One potential concern given the timing of the Post-9/11 GI Bill policy is the simultaneity of a recession in 2009. First, recessions increase enrollment in higher education as the opportunity cost of forgone earnings in the workforce are lowered. Increases in enrollment could force tuition rates upwards as schools seek to expand capacity to meet enrollment demand. Thus, it could be argued that tuition increases in academic years after 2009 are *not* attributable to the Post-9/11 GI Bill. Additionally, colleges faced the potential for severe reductions in state financial support. To allow states to continue funding their programs, the federal government launched the American Reinvestment and Recovery Act (ARRA) in

[14] Total enrollment includes less than 2-year institutions since the GI Bill could technically be used a these schools as well. See http://www.benefits.va.gov/reports/annual_performance_reports.asp.

[15] Subsidy levels did not change much within a state between 2009 and 2010.

[16] http://benefits.va.gov/gibill/resources/benefits_resources/rate_tables.asp

February of 2009. As states substituted federal support for state support at institutions of higher education, one concern may be that schools feared that state funding levels may not return to their pre-recession levels. To plan for the future reduction in state support, colleges may increase tuition and fees.

My specification accounts for this confound in two ways. First, the covariate matrix $\bar{X}_{st}$ includes state level unemployment rates and average per-capita incomes. States impacted most by the recession will have higher unemployment and lower per-capita income. Therefore, these controls should capture the impact of recession induced enrollment changes. Secondly, if the impacts of the recession are similar across for schools along the continuum of $vetpct_{st}$, then these impacts are fully controlled for by the difference-in-difference style estimator. For example, suppose schools in low $vetpct_{st}$ states increased tuition rates due to recession induced enrollments. Also, schools in high $vetpct_{st}$ states increased tuition rates due to recession induced enrollments. However, schools in high $vetpct_{st}$ states are also more likely to be treated. Comparing schools in high and low treatment states nets out the impact of recession induced enrollment, leaving only the higher likelihood of treatment to explain any differences in tuition. Therefore, assuming the impact of ARRA and recession enrollment impacts did not disproportionately impact high-treatment states, the impact of the recession causes a common difference in tuition rates between high and low-treatment institutions and does not bias the impact of treatment.

Finally, $enroll_{ist}$ is total enrollment for institution i and captures scale effects. To gather additional revenues, smaller tuition increases may be needed at institutions with large student bodies. Institutional fixed effects ϕ_i capture time invariant determinants of costs and student charges that are school specific, like selectivity, mission statements, or other unobserved idiosyncratic characteristics.[17]

3.3.2 Data

Tuition and fee data is gathered from the Integrated Postsecondary Education Data System (IPEDS) of the National Center for Education Statistics (NCES). I restrict the data to only include schools who report a non-zero tuition rate and that were active from 2006 through the 2012 academic year. Although I study years 2006-2011, this eliminates struggling schools who were on the path to closing circa the policy break from my data. It also eliminates the impact of schools selecting into the sample in response to the large GI Bill funds. Lastly, I eliminate schools in California because public sector institutions offered free tuition for

[17] For example, Granite State College in New Hampshire focuses on adult education while some institutions only teach the arts.

in-state students until 2009. In total, my dataset includes institutions for 49 states plus Washington DC.

In this dataset I observe the sector of the institution: 2-year and 4-year public, private, and for-profit. Importantly, I also observe the location of the institution allowing me to attach treatment and dosage variables to each institution. Summary statistics for tuition rates and fees by year are found in Table 3.1 with policy-active years shaded in gray. What is evident from the sample means aggregated by sector is that there appears to be no visible jump in tuition or fees across the policy break for any sector. Instead, tuition rates increase at a near constant rate for most sectors. There is a leveling of off tuition rates and fees at 4-year for-profit colleges which may be attributable to the GI Bill for reasons mentioned in Section 3.2.2.

Also noticeable from Table 3.1 is the difference in student charges across sectors. Comparing tuition rates at 4-year institutions within each year, private institutions have the largest in-state list tuition rates while public schools have the lowest. On the other hand, public 4-year institutions have the largest fees while for-profits have the lowest. Comparing 2-year institutions within years, public schools are still the cheapest on average, but for-profits have the highest listed in-state tuition rates among 2-year options. In fact, private and for-profit college average tuition rates exceed the cost of the average 4- and 2-year public schools. Average fees are comparable between 2-year and 4-year private and for-profit institutions of the same sector, but far lower for 2-year public schools relative to 4-year public schools.

The relative prices of institutions across sectors has important implications for supply side responses. Since tuition and fee benefits are tied to public school charges, expensive private and for-profit colleges may be untreated. In essence, if a school's tuition is above the state maximum reimbursement rate, veterans cannot attend for free. The limiting case for schools with charges above reimbursable maximums is that they are completely untreated because veteran demand is nonexistent. Therefore, private and for-profit schools may have downward pressure on tuition rates. However, average fees at 4-year public schools far exceed all other sectors' average fees. Coincidentally, the strongest pressure to increase student charges may be placed on fee rates at non-public universities. By design of the policy, tuition reimbursement rates and fee reimbursement rates will necessarily be higher than a schools charges for all but (potentially) 2 universities.[18] Virtually all public schools in states with significant veteran presence will realize an increase in demand.

The dosage variables *vetpct* and *vetben*09 (for fees and tuition) are summarized in Figures 3.1, 3.2, and 3.3 for the academic year 2009. Comparing Figure 3.1 with Figure 3.2 reveals that

[18] That is, one school's tuition rate may constitute the maximum for that state while another's fees may be the largest. It is possible that tuition and fees are largest at the same school.

student veterans tend to have a greater presence in states with lower tuition reimbursement rates. Although, there is variation in the overlap of veteran tution benefits and veteran student population. There exists high veteran student populations in low benefit states (e.g., Washington and Montana), high benefit states and low veteran student populations (e.g., Minnesota, Wisconsin, Michigan, and the northeast), high benefit states with high veteran populations (e.g., Arizona and Colorado), and low benefit states with little veteran presence (e.g., Utah and Tennessee). This implies that veteran students do not appear to choose to enroll in states with higher tuition. Barr (2015) tests across state migration of students in response to Post-9/11 GI Bill benefit levels and does not find that selection occurs.

Interestingly, comparing Figures 3.1 and 3.3, states with high veteran presence tend to have higher fees. This may be harmful to the specification in equation (3.2); if *vetpct* and *vetben*09 are collinear then there may be an absence of low fee states to compare high fee states with. Still, there are some states with high veteran density but low fee benefits to use as comparison. Namely, Montana, Nebraska, and to a lesser degree South Carolina.

These maps display the rich variation in benefit dosage in this dataset. First, states differ greatly in the percent of their student population that is using the GI Bill. This ranges from 1.7% of students in Massachusetts to 8% of students in Alaska. As such, students in some states will realize a larger income boost than students in others. Moreover, benefits vary randomly across states with respect to institutional charges (as discussed earlier) and generally appear uncorrelated with GI Bill use.

3.3.3 Results

Tables 3.2 through 3.7 show the estimation results for 2 different treatment dosages across 2 types of student charges. The first two columns of results show the coefficients on the difference-in-difference and triple difference interactions with $log\_tuition$ as the dependent variable. The second two columns of coefficients correspond to log_fees. Each table shows results for a different sector. Included in every specification are institutional fixed effects, state-by-year controls for unemployment and per-capita income, and institution-by-year total enrollment. Robust standard errors are clustered at the state level. Coefficients on dosage interactions with years prior to the policy period are displayed as a falsification test; dosages should have no significant impact on tuition rates before the policy is active.

Interpreting the coefficients is not straightforward. Since 2006 is the omitted time dummy, all treatment effects are interpreted relative to 2006. The coefficient on the 2009 dummy variable describes the difference in tuition and fee rates between 2009 and 2006, so the coefficient displayed in column 1 for 2009 describes how this difference changes in response to the variation in veteran enrollment percentages. Since the dependent variable is in logs, the coefficient describes the percent change in tuition rates in states with 1 percentage point higher veteran student enrollment. If veteran benefits have no impact on student charges then veteran presence should show no significant impact on the tuition rate trends captured by the time dummies. The coefficients in column 2 describe how the marginal effect of $vetpct_{st}$ changes in response to variation in benefit levels. Since the independent variable $vetben09_s$ is logged and the marginal effect of $vetpct_{st}$ is in levels, a 1% increase in veteran benefits corresponds to a $\frac{\delta}{100}$ unit change in the marginal effect of $vetpct_{st}$ where δ is the coefficient represented in column 2. Since the marginal effect of $vetpct_{st}$ represents the change in logged tuition, this implies that 1% increase in veteran benefits corresponds to a $\frac{\delta}{100} * 100\%$ change in tuition. Or, simply put, a 1% change in $vetben09_s$ corresponds to a $\delta\%$ change in tuition.

Table 3.2 shows the results at 4-year public schools. It is shown that states with higher *vet_pct* have significantly higher tuition rates than states with less veterans after the policy. The change in tuition between 2006 and 2009 is 4.05% higher in states with 1 percentage point higher $vetpct_{st}$, ceteris paribus. As indicated by the second column of results, this impact did not vary with tuition reimbursement rates. The Post-9/11 GI Bill had a similar impact on fees at 4-year public schools. There is evidence that fees were higher post-policy in states with more veterans and this impact was amplified in states with larger fee reimbursements. This is expected since fee reimbursement rates were far in excess of institutional fees in many states. Also, insofar as tuition increases may be unappetizing for local legislatures to enact, fees may arguably be a less salient means of regulatory capture. The change in fee rates between 2006 and 2010 is 6.6% higher in states with 1 percentage point higher $vetpct_{st}$ and this impact is 0.11 percentage points higher in states with 1% greater fee reimbursement rates.

Table 3.3 shows the results at 4-year private schools. There appears to be no policy impact on the margins studied here. The insignificant result on tuition rates is expected since 4-year private schools have sticker prices well above maximum tuition reimbursement rates in many states. Additionally, prior studies of private institutions suggest that these schools price discriminate by changing net tuition and not list tuition and fees (see Long (2004a) and Turner (2014)). That is, private sector colleges reduce institutional financial aid to increase net student charges. Institutional financial aid is unobserved in this study.

Table 3.4 depicts the results at 4-year for-profit institutions. Large changes in the post-policy coefficients provide weak evidence that 4-year for-profits faced downward pressure on charges in response to the policy, although most of these estimates are insignificant. The strongest evidence of downward price pressure is in the year 2010 for the *pct_vet* interactions. Tuition rates between 2006 and 2010 were 2.25% lower and fee rates were 10.2% lower in states with 1 percentage point more veteran student on average, ceteris paribus. Although the Post-9/11 GI Bill increased veteran ability to pay for all colleges, only colleges that enroll veterans receive the subsidy. If veteran tastes have changed such that veterans no longer prefer 4-year for-profits, demand could actually decrease at these colleges. Prior research on the Post-9/11 GI Bill by Barr (2015) finds evidence that enrollment is indeed shifting away from for-profits and towards public options.

Table 3.5 and Table 3.6 illustrate the coefficients from regressions for 2-year public and private schools. There appears to be no strong evidence for changes in student charges at these institutions. However, Table 3.7 shows that 2-year for profits increased student charges in response to the policy. Tuition rate changes between 2006 and all policy years did not significantly differ with veteran student enrollment on average. But, the difference was .10% larger for every 1% increase in tuition benefits. This makes sense since tuition reimbursements are tied to the maximum public school tuition in any given state. On average, Table 3.1 shows that even 4-year public school tuition rates are below average 2-year for-profit tuition rates. Therefore, tuition subsidies may only be large enough to cover 2-year for-profit tuition charges in the highest benefit states. However, average fee rates at 4-year public schools are much larger than 2-year fee rates on average. Here, we see that states with more veteran students have higher fee rates. The magnitude of the coefficient jumps in the first year of the policy but the interactions with pre-policy years are significant as well. This suggests that fee rates may have already been trending this way in states with and higher percentage of veteran students and this result should be interpreted with caution.

3.4 Conclusion

The large increases in tuition rates at institutions of higher education over the last several decades has policymakers concerned about young peoples' access to continued education after high school. The federal government has loosened its purse strings in a variety of ways to subsidize the cost of higher education, offering affordable student loans, need based grant aid, merit aid, and tax credits for education expenditures. The Post-9/11 GI Bill is yet another example of the federal governments desire to improve access to college. This targeted policy hopes to improve the lives of men and women in the armed services who typically hail from backgrounds that would forgo a college education due to costs. However, as presented in this paper, the economic behavior of institutions of higher education can undermine the effectiveness of this policy. Chiefly, if colleges increase their tuition rates in response to increases in federal aid, then the total benefits are reduced.

This paper finds evidence that higher education institutions do increase their charges in response to federal aid dollars. Specifically, public 4-year institutions seem to appropriate GI Bill funds through increasing tuition and fee rates. This result could explain the modest enrollment effect as a result of the Post-9/11 GI Bill cited in Barr (2015). Even though veterans are shifting enrollment towards 4-year public institutions, these schools may be restricting enrollment expansion while increasing tuition rates. For 2-year for-profits, fees increase in response to the Post-9/11 GI Bill. The fact that other sectors do not appear to respond to the Post-9/11 policy is not surprising. Descriptive evidence suggest that 2-year for-profit colleges are a popular landing pad for veterans returning from active duty while the Post-9/11 GI Bill explicitly sought to increase veteran attainment of a 4-year degree. Additionally, private institutions generally have greater flexibility over institutional financial aid which is unobserved in this study. Although the Post-9/11 GI Bill did not impact in-state list prices at these institutions, these colleges (as well as public and for profits) could theoretically use full tuition veterans to subsidize increases in institutional financial aid or decreases in out-of-state tuition rates. Or, it could be the case that schools substitute non-veterans for veterans while keeping enrollment constant in order to subsidize other quality enhancing activities. While these extensions are interesting, I leave these to future research.

This paper also speaks to the growing concern of regulatory capture at for-profit colleges. As mentioned prior, it is estimated that for-profits received nearly half of all GI Bill funds despite enrolling only 23% of veterans. This could occur with GI Bill funds as small, strategically placed for-profit colleges typically attract veterans and lower income students that qualify for large amounts of financial aid (e.g., see Cellini (2005), Chung (2009)). However, my results suggest that regulatory capture may be occurring at 2-year for-profit colleges. Evidence suggest increases in tuition charges in response to the Post-9/11 GI Bill of around .10% for every 1% increase in benefits. This study falls short of estimating the incidence of these funds at for-profits, and other sectors as well, due to data availability. Future research should look at other margins like instructor salary, quality enhancing expenditures, and for 2-year for-profits in particular, profit margins.

3.5 Appendix

3.5.1 Main Tables and Figures

Academic Year	Dep. Var.	Sector of Institution					
		4-Year			2-Year		
		Public	Private	For-Profit	Public	Private	For-Profit
2006	Tuition	4,473.70	17,462.07	13,596.46	2,576.95	7,776.86	10,988.48
		(2,239.52)	(7,905.62)	(4,085.38)	(1,431.56)	(3,959.08)	(4,601.60)
	Fees	1,106.64	487.33	418.29	362.60	548.50	412.20
		(1,130.03)	(486.69)	(513.63)	(428.99)	(558.35)	(442.92)
2007	Tuition	4,686.18	18,542.50	14,335.86	2,681.11	8,322.60	11,479.45
		(2,251.85)	(8,362.18)	(4,302.70)	(1,490.86)	(4,203.82)	(4,436.42)
	Fees	1,186.79	518.69	430.21	379.95	578.98	441.35
		(1,187.96)	(521.66)	(513.12)	(447.04)	(588.50)	(458.83)
2008	Tuition	4,968.84	19,655.66	14,783.74	2,737.83	8,667.98	12,018.56
		(2,393.16)	(8,860.79)	(4,016.03)	(1,525.51)	(4,373.28)	(4,490.36)
	Fees	1,274.26	554.26	439.56	401.28	601.35	487.88
		(1,256.30)	(552.71)	(522.99)	(474.13)	(589.64)	(547.60)
2009	Tuition	5,285.75	20,535.91	14,798.89	2,858.87	9,141.00	12,726.63
		(2,472.24)	(9,223.36)	(4,715.97)	(1,547.17)	(4,810.94)	(4,906.81)
	Fees	1,344.99	592.99	402.79	426.45	614.68	538.59
		(1,362.38)	(587.72)	(520.30)	(498.64)	(608.74)	(611.15)
2010	Tuition	5,592.98	21,451.45	14,587.49	3,021.04	9,521.60	12,756.13
		(2,565.96)	(9,606.91)	(3,626.93)	(1,567.46)	(5,015.36)	(3,564.55)
	Fees	1,409.87	627.07	410.15	454.92	627.89	526.27
		(1,410.09)	(625.07)	(668.42)	(531.58)	(586.69)	(560.01)
2011	Tuition	5,972.78	22,416.73	14,634.15	3,210.60	9,995.68	12,980.74
		(2,690.06)	(10,014.46)	(3,693.09)	(1,576.05)	(5,157.57)	(3,569.36)
	Fees	1,484.42	667.47	407.20	496.73	698.50	559.87
		(1,474.85)	(696.26)	(476.74)	(592.50)	(648.02)	(665.41)
All Years	Tuition	5,163.37	20,010.72	14,456.10	2,847.73	8,904.29	12,158.33
		(2,493.01)	(9,175.67)	(4,105.74)	(1,538.11)	(4,648.78)	(4,346.29)
	Fees	1,301.16	574.64	418.03	420.32	611.65	494.36
		(1,314.68)	(585.49)	(538.84)	(500.30)	(596.74)	(554.76)
Number of Instutions		577	1152	333	817	106	225

Table 3.1: Summary Statistics

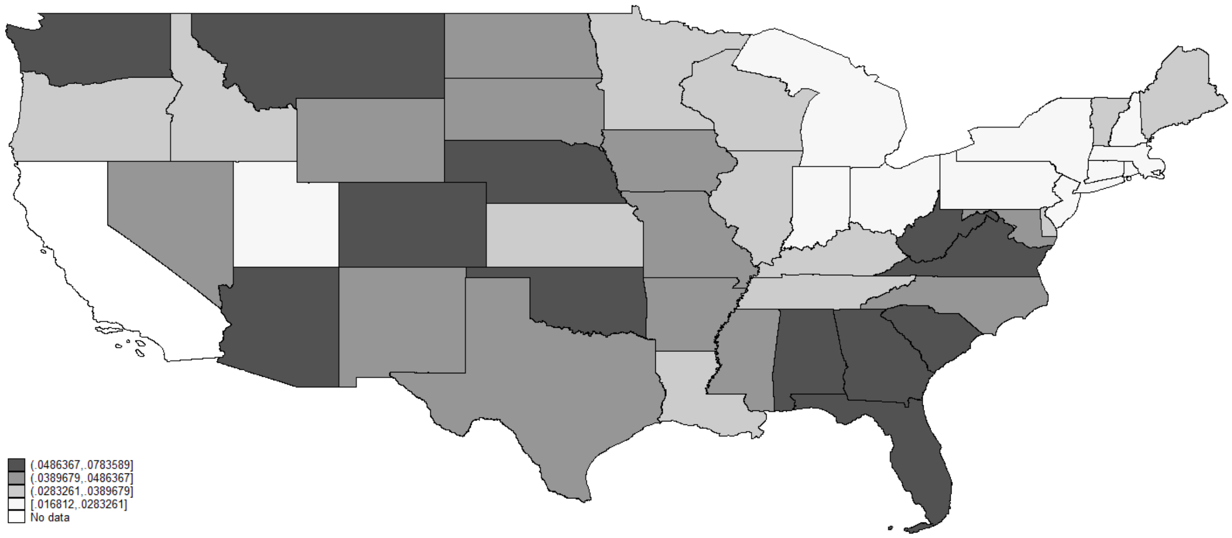

Figure 3.1: Variation in veterans as a percent of total student enrollment ($vetpct_{st}$) across states in academic year 2009. Recall that California is not in the sample. Hawaii(7.2%) and Alaska (8%) have been omitted from the graphic but are in the data.

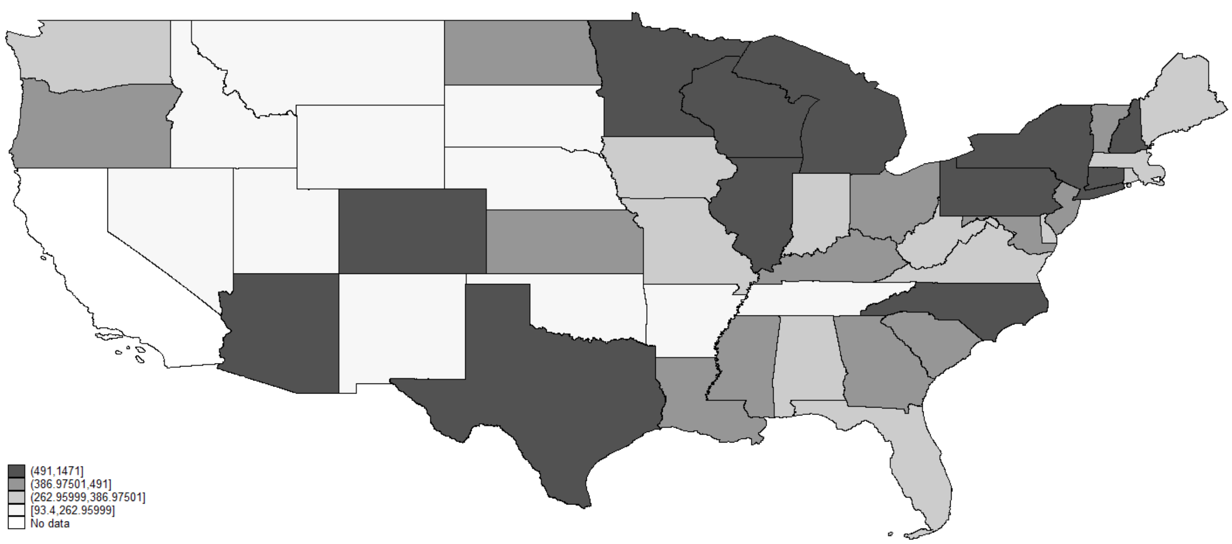

Figure 3.2: Variation in maximum tuition reimbursement levels ($vetben09$) across states in academic year 2009. Recall that California is not in the sample. Hawaii($282 per credit hour) and Alaska ($159 per credit hour) have been omitted from the graphic but are in the data.

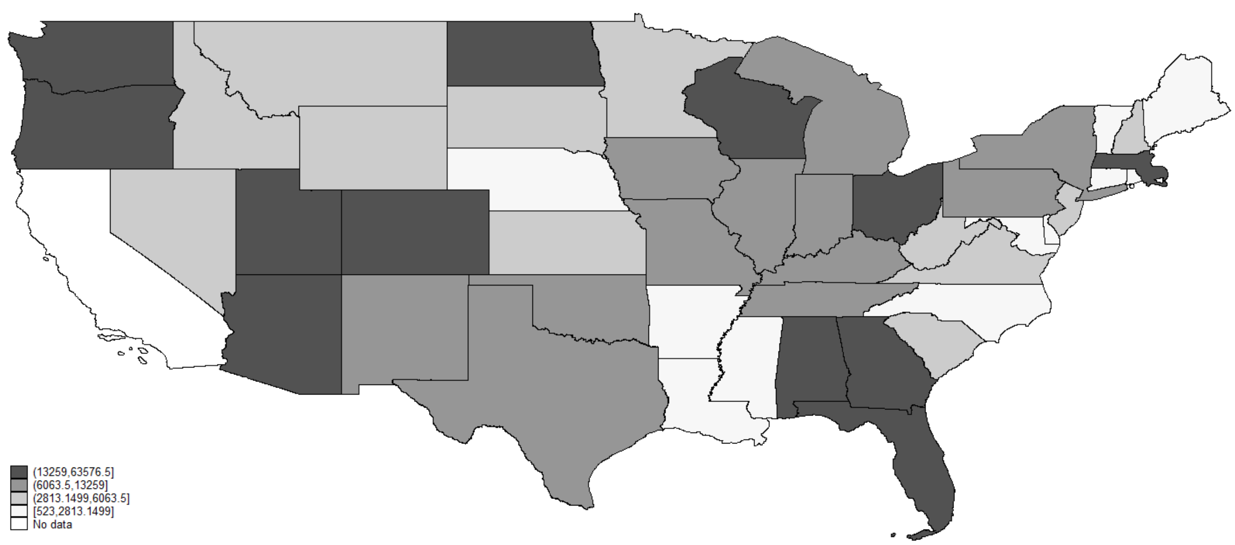

Figure 3.3: Variation in maximum fee reimbursement levels (*vetben*09) across states in academic year 2009. Recall that California is not in the sample. Hawaii($1,149.40) and Alaska ($13,429) have been omitted from the graphic but are in the data.

Table 3.2: Public 4-Year Results

Dep. Var.	Log_Tuition		Log_Fees	
Dosage:	pct_vet	pct_vetXtuit_ben	pct_vet	pct_vetXfee_ben
2007	0.5290	0.9600	0.0931	1.8486
	(0.6076)	(0.5548)	(0.9190)	(0.1465)
2008	2.3221	1.8407	2.4568	4.5005
	(0.1367)	(0.5139)	(0.1545)	(0.1794)
2009	4.0514**	-1.6971	5.7618	8.9082**
	(0.0440)	(0.4133)	(0.1092)	(0.0382)
2010	5.0972***	0.1276	6.6057*	11.0068**
	(0.0045)	(0.9521)	(0.0979)	(0.0175)
2011	5.8620***	-0.6941	7.8950*	11.3964**
	(0.0017)	(0.7436)	(0.0594)	(0.0181)
R^2	0.964	0.965	0.942	0.943
N	3163	3163	3227	3227

Note: Each cell represents the coefficient on the interaction of the dosage(s) denoted at the top of each column with the academic year specified in the row. Each column is a separate specification of either a modified difference-in-differences estimator or a modified triple difference estimator depending on the level of interaction. Each specification includes institutional fixed effects, state-by-year controls for unemployment and per-capita income, and institution-by-year total enrollment. Robust standard errors are clustered at the state level. P-values are in parenthesis with significance is indicated as follows: ***1%,**5%,*10%. For public school estimations only the schools with the maximum fee charges and tuition charges were removed from the data to avoid endogeneity.

Table 3.3: Private 4-Year Results

Dep. Var.	Log_Tuition		Log_Fees	
Dosage:	pct_vet	pct_vetXtuit_ben	pct_vet	pct_vetXfee_ben
2007	0.2069	1.1045**	-1.7034	1.7801
	(0.4028)	(0.0463)	(0.3121)	(0.2980)
2008	0.3308	1.0566*	-1.0200	-0.1216
	(0.3842)	(0.0979)	(0.5188)	(0.9500)
2009	0.4212	0.4671	0.9180	1.3174
	(0.3985)	(0.5705)	(0.1047)	(0.6552)
2010	0.3613	0.6123	0.8836	2.1580
	(0.5621)	(0.5568)	(0.7655)	(0.4932)
2011	0.4301	0.5503	0.7468	2.0062
	(0.5014)	(0.6044)	(0.8037)	(0.5208)
R^2	0.989	0.989	0.931	0.932
N	6912	6912	5777	5777

Note: Each cell represents the coefficient on the interaction of the dosage(s) denoted at the top of each column with the academic year specified in the row. Each column is a separate specification of either a modified difference-in-differences estimator or a modified triple difference estimator depending on the level of interaction. Each specification includes institutional fixed effects, state-by-year controls for unemployment and per-capita income, and institution-by-year total enrollment. Robust standard errors are clustered at the state level. P-values are in parenthesis with significance is indicated as follows: ***1%,**5%,*10%.

Table 3.4: For-Profit 4-Year Results

Dep. Var.	Log_Tuition		Log_Fees	
Dosage:	pct_vet	pct_vetXtuit_ben	pct_vet	pct_vetXfee_ben
2007	0.4451	-0.4916	0.6950	-0.4445
	(0.2884)	(0.6268)	(0.8577)	(0.8393)
2008	-0.0437	1.5181	-0.7185	-0.7908
	(0.9547)	(0.4194)	(0.8405)	(0.7941)
2009	-0.8498	0.2271	-3.4201	-1.9050
	(0.4645)	(0.9244)	(0.4122)	(0.6129)
2010	-2.2474*	-0.2993	-10.1777**	-2.0540
	(0.0834)	(0.9078)	(0.0426)	(0.6583)
2011	-2.1842	-1.5646	-6.7548	2.1434
	(0.1079)	(0.5723)	(0.1612)	(0.7168)
R^2	0.845	0.848	0.847	0.848
N	1998	1998	1733	1733

Note: Each cell represents the coefficient on the interaction of the dosage(s) denoted at the top of each column with the academic year specified in the row. Each column is a separate specification of either a modified difference-in-differences estimator or a modified triple difference estimator depending on the level of interaction. Each specification includes institutional fixed effects, state-by-year controls for unemployment and per-capita income, and institution-by-year total enrollment. Robust standard errors are clustered at the state level. P-values are in parenthesis with significance is indicated as follows: ***1%,**5%,*10%.

Table 3.5: Public 2-Year Results

Dep. Var.	Log_Tuition		Log_Fees	
Dosage:	pct_vet	pct_vetXtuit_ben	pct_vet	pct_vetXfee_ben
2007	1.1032	0.3068	-0.5679	-2.6883*
	(0.2393)	(0.6642)	(0.6034)	(0.0560)
2008	1.7794*	-0.0385	-0.8310	-3.8740*
	(0.0971)	(0.9812)	(0.6375)	(0.0663)
2009	1.6370	1.2685	-1.2522	-6.1427**
	(0.3605)	(0.5879)	(0.6756)	(0.0277)
2010	2.2657	0.4616	-1.7086	-6.3435**
	(0.1952)	(0.8552)	(0.5986)	(0.0380)
2011	3.3565**	0.5492	-1.3183	-5.7003*
	(0.0356)	(0.8278)	(0.6298)	(0.0591)
R^2	0.973	0.974	0.957	0.957
N	4862	4862	4480	4480

Note: Each cell represents the coefficient on the interaction of the dosage(s) denoted at the top of each column with the academic year specified in the row. Each column is a separate specification of either a modified difference-in-differences estimator or a modified triple difference estimator depending on the level of interaction. Each specification includes institutional fixed effects, state-by-year controls for unemployment and per-capita income, and institution-by-year total enrollment. Robust standard errors are clustered at the state level. P-values are in parenthesis with significance is indicated as follows: ***1%,**5%,*10%. For public school estimations only the schools with the maximum fee charges and tuition charges were removed from the data to avoid endogeneity.

Table 3.6: Private 2-Year Results

Dep. Var.	Log_Tuition		Log_Fees	
Dosage:	pct_vet	pct_vetXtuit_ben	pct_vet	pct_vetXfee_ben
2007	-0.7274	2.1947	-9.9486	-3.2183
	(0.5089)	(0.2612)	(0.2032)	(0.5761)
2008	1.8692	2.9259	-6.3764	-4.9670
	(0.3319)	(0.2828)	(0.4193)	(0.4677)
2009	3.7013	6.8503*	-3.8395	-13.0071
	(0.1874)	(0.0885)	(0.6365)	(0.1358)
2010	3.7013	4.8286	-5.9538	-14.5014
	(0.2635)	(0.2943)	(0.4477)	(0.1127)
2011	3.5010	5.3731	-5.8765	-12.2829
	(0.2805)	(0.2339)	(0.4346)	(0.1658)
R^2	0.972	0.972	0.897	0.899
N	636	636	549	549

Note: Each cell represents the coefficient on the interaction of the dosage(s) denoted at the top of each column with the academic year specified in the row. Each column is a separate specification of either a modified difference-in-differences estimator or a modified triple difference estimator depending on the level of interaction. Each specification includes institutional fixed effects, state-by-year controls for unemployment and per-capita income, and institution-by-year total enrollment. Robust standard errors are clustered at the state level. P-values are in parenthesis with significance is indicated as follows: ***1%,**5%,*10%.

Table 3.7: For-Profit 2-Year Results

Dep. Var.	Log_Tuition		Log_Fees	
Dosage:	pct_vet	pct_vetXtuit_ben	pct_vet	pct_vetXfee_ben
2007	1.1911	3.8156	13.7189**	-9.7835
	(0.4015)	(0.2805)	(0.0262)	(0.2149)
2008	0.9242	6.1437	13.1870***	-5.1636
	(0.5271)	(0.1053)	(0.0083)	(0.4616)
2009	0.5634	10.2071*	23.7105***	-5.1636
	(0.7532)	(0.0605)	(0.0001)	(0.8752)
2010	-0.9337	11.8521**	25.9330***	-2.1087
	(0.6433)	(0.0446)	(0.0002)	(0.8254)
2011	-1.4830	11.9555**	26.3298***	-1.0808
	(0.4540)	(0.0412)	(0.0001)	(0.9049)
R^2	0.807	0.809	0.861	0.865
N	1350	1350	1145	1145

Note: Each cell represents the coefficient on the interaction of the dosage(s) denoted at the top of each column with the academic year specified in the row. Each column is a separate specification of either a modified difference-in-differences estimator or a modified triple difference estimator depending on the level of interaction. Each specification includes institutional fixed effects, state-by-year controls for unemployment and per-capita income, and institution-by-year total enrollment. Robust standard errors are clustered at the state level. P-values are in parenthesis with significance is indicated as follows: ***1%,**5%,*10%.

Chapter 4

Major-Contingent Loans and College Major Choice

Abstract

This paper studies the impact on major choice of major-contingent federal student loans. Title II of the National Defense Student Loan Act of 1958 provides a natural experiment to study the impact of conditional lending on college major choice due to a stipulation that low interest loans be prioritized to students pursuing Science, Math, Engineering, Teaching, and Modern Foreign Language degrees. Using microdata from the Wisconsin Longitudinal Study, I estimate models of major choice. I find that the aims of the National Defense Student Loan Program to increase manpower in strategic areas largely failed. A simple reduced form simulation of the human capital investment decision with conditional borrowing suggests that the terms of the National Defense Student Loan Program only resulted in modest increases in student incentives to study qualifying majors. Empirical results show only 1 out of 4 majors studied in this paper showed weak signs of increases in response to the policy and 1 major shows decreases. Specifically, I find eligible students were 9 percentage point more likely to major in education in 1959 than in 1957, but this estimate is statistically insignificant. I also find students were 11 percentage points less likely to choose an engineering degree in the first year of the policy relative to 1957.

4.1 Introduction

Higher education policy over the last several decades reflects the desire of policymakers to increase the study of certain college majors, such as math and science. This paper studies whether a lending program that conditions lending terms, particularly interest rates, on college major choice can significantly increase study in qualifying majors. I exploit the sharp timing of loan funds from the National Defense Student Loan Program in 1958 to test whether offering cheaper borrowing rates to students aimed at studying certain majors significantly increased the study of those majors. Other papers have studied the impact of student debt as a whole on major choice, while some have considered income contingent loans' impact on debt repayment, but this is the first to look at major-contingent loans and major choice.

Policies incentivizing majors have mostly taken place at the state level in the form of grants, but many states are implementing a variety of other approaches. For example, Florida Governor Rick Scott proposed fixing tuition rates in "strategic areas" like engineering and biotechnology to encourage more majors in these areas. North Dakota, realizing a shortage of majors in STEM and teaching fields, offers loan forgiveness options while North Carolina enacted the Health, Science, and Mathematics Student Loan Program, allowing students to trade off future work in North Carolina for additional student loans. Michigan and Ohio recently began making appropriations to public universities conditional on institution performance, which included the production of degrees in preferred areas. This trend is motivated by the notion that technical human capital is important for future economic growth.

At the same time, the federal government also has an interest in the national supply of STEM degrees, targeting 1 million more STEM degrees by 2022 (President's Council of Advisors on Science and Technology (2012)). Like state governments, federal policymakers continue to issue grants targeting certain majors to increase student enrollment in college while improving the nation's manpower shortages. For example, Science and Mathematics Access to Retain Talent (SMART) grants are awarded to third-, fourth-, and fifth-year undergraduates who are majoring in a technical field or critical foreign language. Could the federal government also alter the existing federal student lending framework to achieve the same goal? For example, can conditioning federal student lending terms (e.g., interest rates) on students' choice of major increase the number of STEM or other desired degrees?

Student debt in general can influence college major decisions primarily through forgone future earnings. In a standard life-cycle model, student debt influences college decisions because borrowing allows students to trade off future consumption for current human capital investment (Ben-Porath (1967), Becker (1962)). As a result, borrowing to fund college costs the individual future consumption (in addition to forgone wages from not entering the workforce after high school). Given the heterogeneous wage returns to college majors (see Stinebrickner and Stinebrickner (2014) and Arcidiacono (2004)), college majors with higher returns leave borrowers less constrained after college. As a result, individuals with debt may select majors with higher earnings. Rothstein and Rouse (2011) study career and major choice in response to the exogenous removal of debt for a group of students at a highly selective university in the United States (US). They find weak evidence that students with higher debt select majors with higher earnings potential.

However, individuals also endure effort costs from college study and majors with higher earnings often require higher effort costs. For example, science and math are difficult majors. As demonstrated in Babcock and Marks (2011), they require approximately 5 more hours of study per week on average than the next most rigorous major (social sciences). Individuals must weigh the benefits from higher future earnings as a scientist against forgone future wages from borrowing and the effort costs of studying a science.[1] Insofar as effort costs deter students from needed technical studies in the US, policies that impact the future income stream can be used to offset the high effort costs and incentivize those majors. The policy studied in this paper increases the present value of future consumption of select majors by reducing the debt burden of borrowing for school via the interest rate.

The policy experiment used to study the impact of major-contingent loans (MCL) on college major choice is the National Defense Student Loan Program (NDSLP) of 1958. Created as a part of the National Defense Education Act, NDSLP was the first widely available federal student loan program. The first release of funds occurred in February of 1959. The act provided participating colleges the funds to issue low interest loans to in-need students. NDSLP promised students a 3% interest rate, 2 percentage points lower than the prevailing interest rate provided by the private market (Ulrich (1958)), as well as a deferment of interest until after graduation. Additionally, colleges were required to prioritize loans to students demonstrating a desire to study science, engineering, math, modern foreign language, and primary or secondary education. As an added benefit, teachers who served 5 years or more in a public primary or secondary school were offered loan forgiveness. Importantly, students were not required to stay in the major they indicated on their loan application after

[1] Babcock and Marks (2011) report that for the 1981 survey of the National Longitudinal Survey of Youth (1979), on average, the hours studied per week across majors were as follows: 22.2 for science and math (physical sciences, engineering, and biology), 17.15 for social sciences, 16.25 for arts, 14.6 for business, and 13.02 for education. Other surveys yielded slightly different results but in all surveys sciences required the most study time while business or education required the least. At the same time, Julian (2012) shows that the expected lifetime earnings by major are as follows (in millions): $2.82 for science and math, 2.6 for business, 2.25 for social sciences, 2.1 for arts, and 1.8 for education. With the exception of business, time spent studying is correlated with earnings.

enrolling. Coincidentally, the MCL policy studied in this paper approximates an experiment in which eligible students first try out a qualifying major and then decide whether or not to switch. Given the costs of switching majors, however, the NDSLP's pre-college nudge towards qualifying fields represents the first step of a fully contingent MCL policy.

I exploit this natural policy experiment to execute a difference-in-differences analysis. I gather student level data from the Wisconsin Longitudinal Study, a collection of surveys over time administered to a random sample of high school graduates in the spring of 1957 in Wisconsin. I also observe survey data from their siblings. In both surveys I observe the year the student first attended college. While I do not observe if the student received a loan, I do know their family's socioeconomic status as of 1957. NDSLP loans were need-based, therefore, I categorize students in the lowest quartile of socioeconomic status as in-need as a proxy for the likelihood of that individual qualifying for a loan. The average family income for in-need students in my data, gathered from a subset with reported household average incomes, roughly matches reported family incomes from surveys of NDSLP recipients (Science Technology Policy Institute (2006)). I then compare in-need students to not-in-need students with reported first years of college attendance before and after 1959 to test for changes in the propensity to choose a qualifying college major using a probit estimator.

My results provide some evidence that the distribution of majors chosen was impacted by NDSLP loans. The treatment effect is entirely concentrated in the group entering college in the academic year of 1959, which is the first full academic year of funding. The only major to demonstrate an increase is primary and secondary education. In-need students in 1959 were 9 percentage points more likely to choose education as a major than in-need students in 1957, ceteris paribus. This estimate is statistically insignificant, but the 95% confidence interval demonstrates a marked shift into the positive domain. Counteracting the NDSLP's goals, I find statistically significant evidence that in-need students reduced their likelihood of studying engineering. The result persists even after controlling for student ability. Specifically, I find in-need students were 11 percentage points less likely to choose an engineering major in 1959 than in 1957. The increase in education majors and decrease in engineers suggests that students responded more to the cheaper loans in general and not the conditional aspects of the policy. Specifically, major contingent loans should weakly increase enrollment in *all* majors, which is not what I find. Lowering a student's debt burden in general, however, incentivizes students to study less financially lucrative majors because they need less future income to repay loans. My findings are consistent with the latter and thus lend support the findings of Rothstein and Rouse (2011).

In the next section I discuss the nuances of the National Defense Education Act and provide some background regarding the policy period. In Section 4.3 I create a simple model of college major choice based on the Random Utility Model motivating the probit framework. Also in Section 4.3 I formalize the empirical specification and discuss the data. In Section 4.4 I discuss estimation results. Lastly, I make conclusive remarks in section 4.5.

4.2 Policy Background

The Soviet Union's successful launch of "Sputnik", the world's first man made satellite, put into motion an education revolution in the US. Policymakers feared that the US. was falling behind other nations in matters of security and that the cure was more education, stating that the "... present emergency demands that additional and more adequate educational opportunities be made available."[2] Their solution was to pass the National Defense Education Act of 1958, Title II of which was deemed the National Defense Student Loan Program (NDSLP). The NDSLP made funds available for institutions of higher education to lend to full-time students at low interest rates (3% annually) and accumulation of interest was postponed until after college completion.[3] As indicated in a letter from L. W. Ulrich to President Thomas A. Spragens of Centre College in 1958, existing loans to in-need students in the private market charged a 5% rate of interest.

This paper highlights the impact of an interesting facet of Title II requirements: institutes of higher education were to prioritize students of particular study. Specifically, in-need students that demonstrate a desire for teaching primary or secondary education or studying science, mathematics, engineering, or a modern foreign language were to be given special consideration in the lending process. Since students needed only to demonstrate preparation in and the desire to study a major, the policy is an approximation to an experiment in which loans are contingent upon students studying a qualifying major first. Said another way, switching majors after entering college with a loan was permitted. However, switching majors is not costless (e.g., credit hours accrued in one field of study may not transfer to another field, relationships with other students will change, new books and materials, and others).[4] Since students may be incentivized to first study a qualifying major and face switching costs, this study provides lower-bound inference on what result a fully major-contingent policy might generate.

[2]For a qualitative introduction to the attitudes that launched the STEM movement, see The National Defense Education Act (1958), Title 1: Findings and Declaration of Policy.

[3]Prior, direct government assistance went almost exclusively to graduate level research and research programs; only 8-10% of NDSLP funds were used by graduate students. Additionally, funds could be secured by existing students, although the majority of borrowers were first-time college students (Science Technology Policy Institute (2006)).

[4]Psychological effects may also be present that nudge a student to continue with their first choice of majors.

Prior to 1959, student loans were not a large funding source for students. Of the 1,227 institutions participating in the first year of the NDSLP program, over 560 had never lent to students before. Schools with existing programs rarely used them. However, by 1960 lending amounts were 4 times their 1955 levels from all institutional sources (Derthick (1960)). Additionally, evidence suggests these loans were reaching the in-need population. A 1961 survey of NDSLP recipients shows that 5 out of 7 borrowers came from households with average annual income less that $6,000 dollars (or approximately $47,578 in 2015 dollars). To put this in context, the average annual cost of attending college in 1956 dollars was $1260 for public institutions and $1820 for private institutions (Garsaud Jr. (1967)). These figures are $10,873.15 and $15,705.66 in 2015 dollars, respectfully. That is, college charges amounted for 20 to 30 percent of a students total family income in the absence of financial aid.

Evidence suggests that the field of study stipulation was important and sufficiently employed in loan allocation since demand of funds exceeded supply. Federal funds were not allocated to states according to the amount of loan funds requested but was based on the state's share of total national full time enrollment. If a state enrolled 5% of the national total of full time students, they were allocated to receive 5% of total appropriated funds. Additionally, institutions were required to contribute $1 for every $9 contributed by the federal government.[5] On February 3rd, 1959 the U.S. Department of Health, Education, and Welfare's Office of Education circulated a communication to all participating schools documenting the status of the initial loan disbursements. Title II of the NDEA authorized $47.5 million for the first fiscal year of the policy. However, for that year total loan requests were $62 million from all schools and the federal government appropriated only $30 million (approximately $33 million after school matching). The Office of Education states that every school requesting funds received some funding (1,227 in total) but few schools received their total requested funds (U.S. Department of Health and Welfare (1959a)).

Wisconsin was authorized to receive $1.07 million in the initial disbursement but was appropriated $135,462 (U.S. Department of Health and Welfare (1959b)). The University of Wisconsin-Madison received the largest appropriation of $37,697 with Wisconsin State College in Eau Claire receiving the second most at $14,404 (U.S. Department of Health and Welfare (1959a)).[6] Constraints persisted for the largest schools through 1963 with UW-Madison requesting $1.067 million, well above the federal government's cap of $800,00 per school (Garsaud Jr. (1967)).

[5] Aid was offered to colleges that could not match funds. Colleges must set up in-house administration of the funds to determine which students received loans and were also in charge of collection. They could use funds for legal enforcement of repayment.

[6] Later disbursements were made from a $24 million appropriation from the president's budget.

The senate recognized the funding shortcomings of the policy in a 1964 Senate Committee Report accompanying the 1964 amendments to the National Defense Education Act, stating "The denial of these funds has had the effect of denying loans of nearly 4,000 needy and capable students..." Facing the decision to allocate funds between two students with equal need, institutions relied on the choice of major provision in allocating funds (Garsaud Jr. (1967)). Miller (1969) outlines the official loan policy at Kansas State University. In their policy, KSU stipulates that students must demonstrate need or the application is disapproved. Moreover, their policy also states that KSU will prioritize the recommended fields of study.

As evidence that conditional lending practices were broadly applied, 55% of borrowers in 1959 were prospective teachers and another 20% were shown to have background preparation in science, engineering, mathematics, and foreign languages after the first year of the program. Through 1963, those numbers were 46% and 21% respectfully (U.S. Department of Health, Education, and Welfare, Office of Education, 1964).[7] Lastly, when the NDEA was amended in 1964, the requirement that student loans be prioritized for certain studies was removed, suggesting this aspect of the policy was being sufficiently employed. Garsaud Jr. (1967) notes a passage in a Senate report from the second session of the 88th Congress that generated the amendment: "This broadening of the act is in consonance with changes proposed in Titles III (which, under this amendment, now gives financial assistance for strengthening instruction in history, civics, geography, english, and remedial reading) ...of the National Defense Education Act and reflects the belief that the Nation should offer a greater opportunity for the development of the potential abilities of its talented human resources."

The scope of the NDSLP was geographically quite large. Pamphlets were distributed nationwide to colleges and high schools describing the loans and listing all of the qualifying institutions by state.[8] By June 1960, at the end of the program's first full year, nearly 70% of institutions of higher education in the US participated in the program. In total, 1,368 out of approximately 2,004 institutions of higher education (2-year and 4-year) in the US participated (Derthick (1960) and NCES Digest of Education Statistics (1995)). Total federal and institutional allocations exceeded $50 million and funded approximately 115,500 students (approximately 5% of full-time students). Loan use grew every year thereafter, reaching approximately 216,930 students, by the end of fiscal year 1962 (California Postsecondary

[7] Teachers were forgiven a portion of their loans upon entering primary or secondary school. Jordan (1982) finds no evidence that the loan forgiveness increased the number of public school teachers nor the quality of teachers.

[8] The pamphlets are entitled, "The National Defense Student Loan Program (Including Participating Institutions)" and exist from 1959-60 to 1964-65.

Education Commission (1984)). By 1963, participating schools enrolled nearly 90% of full-time college students in the US (United States Department of Health, Education, and Welfare, Office of Education, 1964).

In the next section I build a simple model of major choice and use it to generate predictions for the effect of major-contingent loans on major choice. Additionally, I discuss how the data fits the model in the reduced form probit estimator.

4.3 The Random Utility Model and Data

4.3.1 A Simple Model of Major Choice

The Random Utility Model is used when individual choices are observed, in this case college major choice. If an individual i chose major j over major k we know that major j provides the agent more utility than major k, or $U_{ij} > U_{ik}$. To the econometrician, this decision is the function of observable characteristics (V_{ij}) as well as unobservable inputs to utility (ϵ_{ij}). Let $U_{ij} = V_{ij} + \epsilon_{ij}$ be the utility of choosing major j for individual i. It follows that the probability of an individual choosing major j over all other majors is:

$$Pr(y_i = j) = Pr(U_{ij} \geq U_{ik}) \quad \forall k \tag{4.1}$$

$$= Pr(\epsilon_{ik} - \epsilon_{ij} \leq V_{ij} - V_{ik}) \quad \forall k \tag{4.2}$$

$$where\ V_{ij} = u_{ij}(Y_{i0}, E_{ij}) + \beta_i u_{ij}(\delta_j f_i(h^*) - (1+r)h^*) \tag{4.3}$$

In (4.3) I assume a two period utility function (increasing and concave in consumption) in which the individual borrows to finance all of their college education in the first period and earns income while repaying debt in the second period (β_i is the discount factor). Let h^* represent the optimal human capital investment, which in this case is postsecondary education. The first period utility depends on an individual's initial endowment (Y_{i0}), which determines consumption since I assume an individual borrows to fund all of their human capital investment h^*. That is, I assume individuals can fund none of their education with endowment and consume it (for some low income students, $Y_{i0} = 0$). First period utility also depends on the individual specific effort costs of major j, E_{ij}. E_{ij} is decreasing both in individual ability and individual preferences for major j. Second period utility is based on an individual's earnings net of debt repayment. Earnings are individual specific as well but depend on college major choice since different majors have different returns, $\delta_j > 0$. This is essentially assuming that an individual earns a fixed wage for completing college, h^*, across all majors. However, some majors earn more than others apart from individual inputs (Stinebrickner and Stinebrickner (2014), Arcidiacono (2004)), and this is reflected in the multiplier δ_j. Students borrow to finance college at an interest rate r. Notice that if the repayment amount, $(1+r)h^*$, decreases then individuals are willing to endure higher effort costs in the first period.

The probability of choosing major j increases as $V_{ij} - V_{ik}$ increases for all k, meaning the net discounted value of utility for major j becomes larger than for major k. Let $\Delta V = V_{ij} - V_{ik}$. Plugging (4.3) into this expression results in:

$$\begin{gathered}\Delta V = u_{ij}(Y_{i0}, E_{ij}) - u_{ik}(Y_{i0}, E_{ik}) + \\ \beta_i[u_{ij}(\delta_j f_i(h^*) - (1+r_j)h^*) - u_{ik}(\delta_k f_i(h^*) - (1+r_k)h^*)]\end{gathered} \tag{4.4}$$

I use equation (4.4) to generate predictions for the empirical estimation in this paper. Note that I have subscripted the interest rates r_j and r_k to denote major specific interest rates. By letting $r_j > r_k$ if major j is a qualifying major under the NDSLP, I implicitly assume that students eligible for NDSLP loans would need loans from the private market to study major k. As discussed in Ulrich (1958), private market interest rates in 1958 were 5% while the NDSLP rates were 3%. Therefore, an in-need student who needed loans is choosing between non-qualifying majors with higher interest rates and qualifying majors with lower interest rates. Deferred interest means that the effective interest rate on qualifying majors is even lower than 3%.

Whenever ΔV increases, the likelihood the individual chooses j over k increases. For example, suppose that δ_j, the returns to studying major j, increased ceteris paribus. As a result, the discounted future earnings stream for major j increases, so ΔV increases and the probability that individuals choose major j increases.

The impact of changing interest rates is similar. If the borrowing rate is the same for both majors ($r = r_j = r_k$) and it is reduced, then the effect on major choice depends on the difference in the earnings premium. It can be shown that:

$$\frac{\partial \Delta V}{\partial r} = -U'_{ij}(\delta_j f_i(h^*) - (1+r)h^*)h^* + U'_{ik}(\delta_k f_i(h^*) - (1+r)h^*)h^* > 0 \text{ if } \delta_j > \delta_k \tag{4.5}$$

This means that if the borrowing rate falls, although the present value of future earnings for both majors increases, the gap in the values between k and j decrease, ceteris paribus. Conditional on differences in effort costs, an individual may switch to a lower paying but less taxing major (i.e., lower effort costs). The comparative static presented in (4.5) demonstrates the change in incentives for a low-income student with only qualifying majors in their choice set, like science and primary education. It also models the case where all majors are eligible for the low interest rate, which is technically the case of the NDSLP. Moreover, if colleges do not sufficiently employ the choice of major as a heuristic for allocating loan funds, loan recipients receive low interest rates for any major. Coincidentally, the reduction in the interest rate is the same for all majors and their decision would be driven by differences in effort costs and projected future earnings only. If this is the case, I expect students to weakly choose less science, math, and engineering majors and more teaching majors.

However, if the interest rate is fully conditioned on major choice, say r for major j is decreases while r remains unchanged for k, then:

$$\frac{\partial \Delta V}{\partial r_j} = -U_{ij}^{'}(\delta_j f_i(h^*) - (1 + r_j)h^*)h^* < 0 \tag{4.6}$$

As a result, if the interest rate for major j falls, ceteris paribus, the likelihood that an individual chooses major j increases. This comparative static demonstrates the change in incentives for a student choosing between, for example, chemistry and business.

A simple back-of-the-envelope calculation yields insights into the magnitude of the interest rate reduction on future earnings and, coincidentally, its impact on major choice. Specifically, if interest rates drop from 5% to 3% for major j students benefit from having higher future consumption after repaying loans. I compute the magnitude of this future benefit to answer, "What is the maximum difference in future earnings between major j and major k such that the policy incentivizes the student to choose major j"? For illustrative purposes, I make several simplifying assumptions while quantifying ΔV. I assume a risk-neutral utility function so that future earnings impact major choice by their relative dollar amounts only. Additionally, let effort costs be the same for both majors j and k to focus on how MCL loans impact major choice through future earnings. Also, assume a level stream of income for each major that does not grow over time. Lastly, let the discount rate be 5%, which is the private market borrowing rate in 1959.

The NDSLP borrowing terms were explicitly stated in the National Defense Education Act. Students could borrow up to $1,000 per year for up to 5 years; I assume they borrow the average amount ($434) for 4 years only. Students first payment is due a year after exiting school; I assume they begin earning income immediately and payments are deducted from their income. Lastly, students had 10 years to repay their loan with 10 annual payments that I assume are equal. To further simplify this calculation I assume no payments are made nor interest earned on private loans while the student is in school. I discuss the implications of relaxing some of these assumptions below. In total, the student decides which major to choose based on the present value of consumption from the 10 year repayment period added to the present value of the level income perpetuity beginning at the end of repayment. In the calculation, all else is equal for both major j and major k except the level income payments (which are a variable in the calculation) and the differential interest rates ($r_k = 5\%$ and $r_j = 3\%$).

Given this setup, I calculate the maximum difference in earnings such that the student is incentivized by the policy to choose major j in two scenarios: 1) the student only considers the 10 year repayment period in making their college major decision (hereafter the "myopic case") and 2) the student considers the 10 years of repayment as well as their income stream into perpetuity after 10 years (hereafter the "long-term case"). For all majors except teachers, future earnings for major j can be \$3.90 less than major k for the long-term case and \$21.20 for the myopic case. In 2015 dollars (hereafter in parenthesis following the 1959 dollar amounts), this amounts to \$31.46 and \$171.00 respectfully. To interpret this result, even if the per period future earnings $\delta_j f(h^*)$ is \$3.90 less than major k, the student will choose j because the policy lowers lost consumption in repayment. If earnings are, say, \$4 less for major j, the MCL will not offer enough future benefits to incentivize the student to choose major j over major$k$. If I incorporate the 50% loan forgiveness afforded to teachers, their future earnings must be within \$22.61 (\$182.00) for the long-term case and \$122.75 (\$990.11) for the myopic case. If I assume the student borrows the maximum annual amount of \$1000 for four years, the maximum difference for switching to occur is \$9.04 (\$72.92) in the long-term case and \$49.00 (\$395.24) in the myopic case.

This exercise yields important insights regarding the magnitude of the comparative statics discussed earlier. First, the effect of the interest rate reduction is generally small unless students are highly myopic. This can be seen by comparing the long-term case to the myopic case in each example. However, the impact of borrowing at lower rates is likely to be stronger for majors with lower incomes *if* the assumption of risk-neutral utility is relaxed. For example, teachers in 1959 earned \$4,995 on average whereas engineers earned roughly \$28,920.[9] Any increase in future consumption for teachers will have larger impacts on the propensity to choose a teaching degree since the marginal utility of consumption is largest when income is small. Therefore, the NDSLP has potential to have larger impacts on students choosing between less financially lucrative majors.

Secondly, if major j is a high earnings major with high effort costs, then the small future benefits calculated here are weighed against potentially large losses in utility from effort during the schooling period. It is unlikely that the policy will incentivize the student to switch majors in this case. Therefore, given that lower income majors have higher marginal

[9]See the National Center for Education Statistics' Digest of Education Statistics: *Estimated average annual salary of teachers in public elementary and secondary schools: Selected years, 1959-60 through 2005-06* for data regarding teacher salaries and visit www.postsecondary.org for *Starting Salaries of College Graduates: 1947 to 1995* where engineering earnings are documents.

utilities of income and lower income majors often require less effort cost during the schooling period, this policy may have the greatest impact on teaching and modern foreign language.[10]

Lastly, I did not explicitly model the impact of in-school deferment in my theoretical model nor in this simulation, but it is likely to have large effects. NDSLP loan interest and payments were deferred until after graduation. Private loans, however, likely require the student to make payments while attending school. Therefore, the NDSLP loans increase current period consumption as well. Since current period consumption is worth more in present value than future period consumption, the estimates in the back-of-the-envelope calculations above are unduly low.

To this point I have not mentioned enrollment, but the model can be used to generate predictions for the choice to attend college as well. Let k be the choice to "not attend' college. An individual will choose to attend college if $U_{ij} > U_{ik}$ for at least one major j. This time, the student trades off future income net of debt from college study with immediate, lower earnings with no debt. Since there are no funds borrowed for human capital accumulation in k, a lower interest rate r increases future consumption after college only, making college attendance more attractive. In the context of major-contingent loans, enrollment should also weakly increase as students on the margin of choosing qualifying major j instead of working after school are nudged to attend college.

The decision to enroll has important implications for the interpretation of the major choice results in this study. Specifically, a low-income individual may be dissuaded from enrolling in college because the effort costs (E_{ij}) are too large. Recall that E_{ij} is decreasing in an individual's ability, meaning that higher ability individuals are more likely to study difficult majors. When the interest rate falls, the benefits of enrolling are increased and students become more willing to endure the effort costs of study. As a result, NDSLP loans may change the distribution of abilities in the student population. For example, I may find that loan use decreases science majors but increases teaching majors, not because of substitution between majors, but simply because of selection of low ability types into treatment. To control for ability selection, I account for student ability in my estimations with the best available ability proxy in the WLS database: IQ scores. In the next section I discuss the data and assumptions used to test the predictions of this simple model.

[10] See Babcock and Marks (2011) for documentation on hours studied per week by major and Julian (2012) for calculations of lifetime earnings by major. In most cases, majors that require more hours of study have higher lifetime earnings.

4.3.2 Empirical Model and Data

The model of the impact of interest rates on major choice connects to the data through the timing of the policy and the division of students into in-need and not-in-need subgroups. As a result, I estimate a reduced form version of the Random Utility Model of major choice. In-need individuals who attended college before 1958 would only be eligible for borrowing at the private interest rate, which hovered around 4.5-5% in 1959-1961 and began accumulating immediately (Government Printing Office, Economic Report to the President (2008), Ulrich (1958)). Beginning in 1959, in-need students could qualify for the low interest (3%) federal loans but needed to demonstrate a desire to study a qualifying major. By this, r is lower for in-need individuals after 1959 only if they indicated the desire to study a qualifying major, making the interest rate major-contingent in this period. This setup facilitates a difference-in-differences estimation strategy in which I compare in-need students to students ineligible for funds pre- and post-1959. The groups of students I consider entered college from 1957 to 1962.

Altogether, the estimation of the probability that an individual i chooses major j who entered college in year t is specified as:

$$Pr_{it}(Choice_i = j) = f(\beta_0 + \beta_2 Ability_{it} + \beta_3 1(InNeed)_{it} + \sum_{t=58}^{62} \tau_t + \sum_{t=58}^{62} \delta_t 1(InNeed)_{it}\tau_t) \quad (4.7)$$

where $Ability_{it}$ is a students IQ score and τ_t is a college entry year fixed effect. $1(InNeed)_{it}$ is a dummy equal to 1 if student's household was in the lower quartile of socioeconomic status among college-attending students in the sample. A 1961 survey of NDSLP borrowers showd that approximately 71.4% of borrowers came from families with average household income below \$6,000 (Science Technology Policy Institute (2006)). Therefore, $1(InNeed)_{it}$ is calibrated such that approximately 75% of in-need students from my data come from families with average family incomes at or below \$5,900.[11]

The coefficient of interest is δ_t for $t = (1959, 1960, 1961)$. It is the so-called difference-in-differences estimator comparing in-need students to not-in-need students before after after the policy. If NDSLP incentivized students to study qualifying majors, $\delta_t > 0$ for major j in post policy periods. I interact the treatment dummy, $1(InNeed)_{it}$, with τ_t for 1957 and 1958 as a falsification test; there should be no differential impact of treatment on major choice in these years.

[11] I use socioeconomic status instead of household income because the socioeconomic status reflected other variables that might determine need, such at how many children were in the household, if both parents worked, and others. See the appendix for robustness checks over the definition of $1(InNeed)_{it}$ as well as the household family income distributions that correspond to each definition of $1(InNeed)_{it}$.

To estimate equation (4.7), I combine data from three surveys conducted as part of the Wisconsin Longitudinal Study (Wisconsin Longitudinal Study (2012)). First, a random sample of graduating high school seniors was surveyed at Wisconsin high schools in 1957. The survey collected a myriad of student specific information, like the student's expected future plans, scores from IQ exams, and parental statistics. Students are later matched with their parent's tax records to provide socioeconomic and income measures. In 1975, the same students were surveyed a second time and asked about their higher educational attainment, including the year they first attended college and the major they were studying at their highest level of college achieved. Additionally, a randomly selected sibling was surveyed in 1977 and their higher educational attainment was documented as well as their IQ scores. The WLS is broadly representative of white, non-Hispanic men and women who have completed at least a high school education. Since this is publicly available microdata I do not observe many identifying characteristics of the student, like race, gender, location, etc. [12]

In total, the data include 3,446 students who attended a 2- or 4-year college and reported a major for their highest level of attainment. Table 4.1 demonstrates the distribution of the sample across first year of college attendance. Column 1 includes counts of observations by college entry year and shows the largest portion of the high school graduate sample attended college immediately after high school (in 1957). A steady decline in the number of persons enrolling in college thereafter raises concerns about differences in the samples across time. One concern is that the ability of students who delay entry might be lower than those who attend immediately and that lower ability students would have a higher propensity to choose majors with a lower effort cost. For example, high ability students may receive scholarships while lower ability students must work first and save money to attend college later. I measure ability using IQ scores elicited by tests administered to the students while they were in high school. The range of IQs begins at 61 with a max of 145 and a sample mean of 108.8. Indeed it appears that the average ability of each cohort falls after 1957 for the first two years of the policy and then increases again in 1961 and 1962. I include this measure in my estimations to capture differences in major choice post-policy that relate to the lower ability of later cohorts.

The WLS survey reports over 100 unique codes for college major. I aggregate them into science, engineering, math, modern foreign language, primary and secondary teaching, and non-NDSLP. Some majors were straightforward to aggregate, such as education and math. Others required some judgment by the author. Any erroneous placement of majors into the non-NDSLP designation only serves to bias treatment effects towards zero.

Figure 4.1 summarizes the major choices of Wisconsin high school graduates who entered college before the policy (1957 and 1958) and after the policy (1959 and 1960) for the entire

[12] A comprehensive summary of the data can be found in Herd et al. (2014).

sample. This aggregate view of major choice patterns between pre-and post-policy years does not provide visual evidence of a large scale treatment effect. It appears post policy years saw no change in the proportion of math majors, but decreases in engineering and education. However, there is a spike in the proportion of students receiving science degrees in 1960. Of course, these comparisons aggregate across in-need and not-in-need students as well as all ability types. Note that the sample contains too few modern foreign language majors in the treatment group (In-Need) to estimate the probit model so analysis focuses on the remaining majors. In the section that follows I present the results from the estimation of equation (4.7)

4.4 Estimation Results

Table 4.2 reports the marginal effects estimates of the difference-in-differences estimator in equation (4.7) for 5 different probit estimations. Each panel of results corresponds to a probit estimation with the binary dependent variable equal to 1 if individual i chose that major after enrolling in year t. NDSLP is an aggregate measure equal to 1 if individual i chose any NDSLP qualifying major and zero otherwise. Science, math, engineering, and education are binary dependent variables equal to 1 if the individual i chose that major and zero otherwise. Treatment years are shaded gray in the table. The probit coefficient estimates that are used to generate the marginal effects can be found in Table 4.3 in the appendix.[13]

The first panel labeled NDSLP indicates the impact of the policy on in-need students on their choice of any NDSLP. That is, NDSLP equals 1 for individual i in year t if they chose either science, math, engineering, modern foreign language, or an education major. Marginal effects are insignificant for all years except 1962. Students entering in 1962 were 27 percentage points less likely to choose a qualifying major. This could be attributable to the impact of the HEA of 1964 and the increase in financial aid provisions to in-need students regardless of major choice. However, looking at the average impact across all majors hides the potential heterogeneity of the impact across qualifying majors. The remaining panels show results for probit specifications estimating the likelihood of choosing a single qualifying NDSLP major over all other majors.

The policy had little impact on in-need student's propensity to choose science and math. All p-values are large for treatment years. However, in the first year of the policy students are less likely to choose engineering. Specifically, an in-need student is 11 percentage points less likely to choose engineering in 1959 than 1957, which is significant at $\alpha = 10\%$. Coefficients on $Year * 1(In - Need)$ interactions are highly insignificant for years farther from the policy

[13]Additionally, I estimate a Linear Probability Model and present coefficients in Table 4.4 in the appendix. Results are qualitatively similar across specifications.

start date, suggesting this finding is robust. Recall in the simple Random Utility Model presented in Section 4.3 that students can increase future consumption by studying majors with higher future earnings, but if a student's choice set only includes qualifying majors, the reduction in interest rates only serves to push marginal students to lower effort cost majors. Moreover, this movement away from engineering is not due to a lower ability cohort because ability is controlled for in the specification. Table 4.4 in the appendix shows that ability is positive and statistically significant in determining the propensity to choose engineering as a major, suggesting higher ability students are more likely to choose engineering.

Lastly, Table 4.2 shows weak evidence that in-need students chose education majors more often as a result of the NDSLP policy. Education majors were trending downward over the period of study. However, in-need students were 9 percentage points more likely to choose an education major in 1959 than in 1957. This result is insignificant but highly suggestive of an increase in education majors due to the large shift in the confidence interval towards the positive domain for this year. It is not surprising that education majors show some response to the policy given the additional loan cancellation benefit afforded to them in addition to lower borrowing rates and deferred interest. By working 5 years in public primary and secondary education, education majors were eligible to have 50% of their loans forgiven. This represents and even more drastic increase in future consumption for those choosing an education major than simply lowering the interest rate on borrowing.

Reflecting on the goals of the policy, this study suggests that the desire to increase manpower in strategic areas with NDSLP largely failed. Only 1 out of 4 estimable majors showed signs of increases in response to the policy. It is possible that measurement error harms the results in this study since 1) I do not observe which students received a loan and 2) I have to assume priority lending to students intending to study qualifying majors was unanimously applied at colleges across Wisconsin. If I miscategorize loan recipients with my in-need treatment proxy, I bias estimation of the difference-in-difference estimator towards zero. Moreover, if schools did not sufficiently employ the requirement that students study a particular major (despite descriptive evidence), then even students who receive loans are untreated.

It is also possible that switching costs are too low to prevent students from reverting to non-NDSLP majors after enrollment. Students only need to document continued enrollment with satisfactory academic achievement to retain loans. This means loan recipients could switch majors after enrollment and maintain increased future consumption from low interest borrowing.

However, given the disconnect of the NDSLP and a fully major-contingent loan policy it is even more interesting that engineering and education majors appear to have differing responses to major "nudges" created by conditioning successful loan receipt on desired major choice; in-need students moved away from engineering and towards education. This pattern suggests that students did not view these loans as major-contingent and the reduced interest rate for qualifying majors was merely a reduction in interest rates for all majors. As a result, the relevant comparative static tested is that of equation (4.5) in which $r_j = r_k$. These results support the theoretical prediction that a reduction in interest rates for all majors incentivizes students to choose less financially lucrative majors conditional on effort costs. In this regard, my findings parallel those of Rothstein and Rouse (2011).

4.5 Conclusion

This paper studies whether a lending program that conditions interest rates on college major choice can significantly increase study in qualifying majors. I exploit the timing of loan funds from the National Defense Student Loan Program in 1958 to test whether offering cheaper borrowing rates to in-need students for the study of certain majors significantly increased the study of those majors. Other papers have studied the impact of student debt as a whole on major choice, but this is the first to look at major-contingent loans and choice.

Conditioning interest rates on college major choice would be a useful policy for legislatures to consider. Policymakers can align the goals of low interest loans with the need to correct manpower imbalances with the existing student loan system. Major-contingent loans will continue to ensure access to education for low-income students by providing cheap loans. Meanwhile, students are ushered into studies desired by policymakers for economic growth.

However, this paper highlights that a binding major-contingent loan policy that penalizes students for switching majors after enrollment may be necessary to ensure increases in desired majors. My results suggest first that the aims of the National Defense Student Loan Program to increase manpower in strategic areas largely failed. Only 1 out of 4 majors studied in this paper showed signs of increases in response to the policy and the 1 shows decreases. Specifically, I find in-need students were 11 percentage points less likely to choose an engineering degree in the first year of the policy.

It is possible that measurement error harms the results in this study since 1) I do not observe which students received a loan and 2) I have to assume priority lending to students intending to study qualifying majors was unanimously applied at colleges across Wisconsin. If I miscategorize loan recipients with my in-need treatment proxy, I bias estimation of the difference-in-difference estimator towards zero. Moreover, if schools did not sufficiently employ the requirement that students study a particular major (despite descriptive evidence), then even students who receive loans are untreated. Better data is needed to identify loan recipients at institutions that effectively employed the priority lending protocol.

I do show suggestive evidence for a 9 percentage point increase in education majors, but this estimate is statistically insignificant. This behavior, coupled with the significant reduction in engineering majors, suggests that students did not treat the NDSLP as a binding major-contingent loan policy. That is, they switched majors after enrollment, which was allowed under the policy with no reduction in funding and no change in interest rates. As such, the pattern of major choice in response to the policy supports previous work demonstrating that student debt reduction reduces the future impact on consumption of loan repayment, thus incentivizing the study of less financially lucrative majors.

4.6 Appendix

4.6.1 Tables and Figures

Table 4.1: Sample Distribution Across Time

	Survey Sample		Ability
	Graduate	*Sibling*	*IQ*
1957	2,546	36	110.1
1958	307	29	105.2
1959	125	39	104.2
1960	81	37	103.0
1961	76	44	107.9
1962	55	39	105.1
Total	3,190	224	*Sample Mean*:108.8

Note: Excluded from the sample are students who attained higher education after high school at a technical school or who did not report their highest major.

Figure 4.1: Distribution of Major Choice for Pre- and Post- Treatment Years

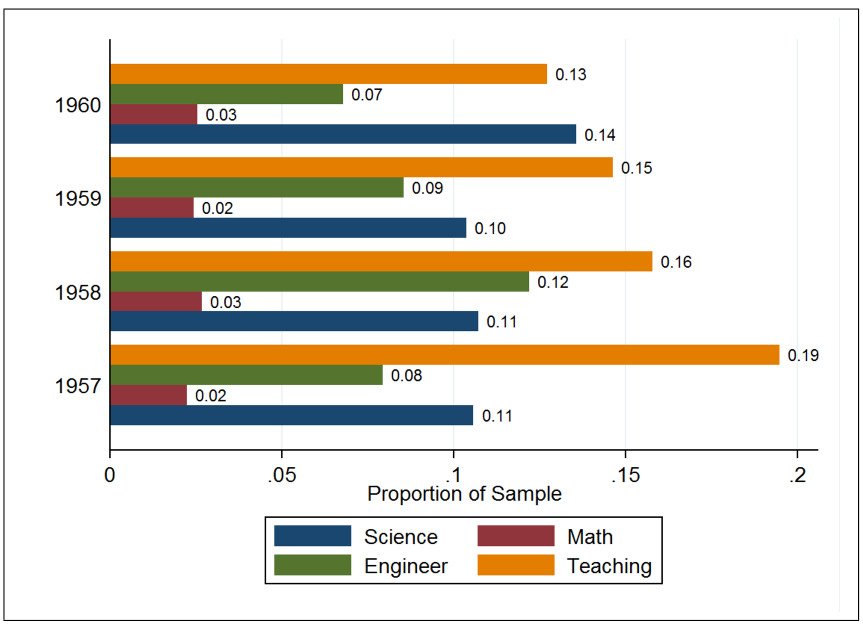

Table 4.2: Marginal Effects

Diff-in-Diff (1=In-Need)		Std. Err.	z	P>(z)	95% Conf. Interval	
NDSLP						
(58 vs 57) (1 vs 0)	-0.009	0.064	-0.140	0.890	-0.135	0.117
(59 vs 57) (1 vs 0)	-0.108	0.083	-1.300	0.195	-0.270	0.055
(60 vs 57) (1 vs 0)	-0.053	0.097	-0.550	0.584	-0.244	0.137
(61 vs 57) (1 vs 0)	-0.073	0.108	-0.670	0.501	-0.284	0.139
(62 vs 57) (1 vs 0)	-0.273	0.104	-2.640	0.008	-0.477	-0.070
Science						
(58 vs 57) (1 vs 0)	-0.010	0.039	-0.260	0.797	-0.086	0.066
(59 vs 57) (1 vs 0)	-0.062	0.048	-1.290	0.196	-0.156	0.032
(60 vs 57) (1 vs 0)	0.040	0.071	0.570	0.569	-0.099	0.180
(61 vs 57) (1 vs 0)	0.105	0.086	1.230	0.218	-0.062	0.273
(62 vs 57) (1 vs 0)	-0.118	0.056	-2.100	0.036	-0.228	-0.008
Math						
(58 vs 57) (1 vs 0)	0.026	0.025	1.020	0.306	-0.024	0.076
(59 vs 57) (1 vs 0)	-0.020	0.026	-0.780	0.437	-0.070	0.030
(60 vs 57) (1 vs 0)	-0.010	0.031	-0.320	0.748	-0.071	0.051
(61 vs 57) (1 vs 0)	0.015	0.037	0.400	0.691	-0.058	0.088
Engineering						
(58 vs 57) (1 vs 0)	0.009	0.042	0.220	0.823	-0.074	0.093
(59 vs 57) (1 vs 0)	-0.110	0.038	-2.910	0.004	-0.184	-0.036
(60 vs 57) (1 vs 0)	-0.032	0.049	-0.650	0.516	-0.129	0.065
(61 vs 57) (1 vs 0)	0.003	0.068	0.050	0.962	-0.131	0.137
(62 vs 57) (1 vs 0)	0.038	0.074	0.510	0.607	-0.107	0.184
Education						
(58 vs 57) (1 vs 0)	-0.034	0.049	-0.710	0.480	-0.130	0.061
(59 vs 57) (1 vs 0)	0.090	0.068	1.330	0.184	-0.043	0.224
(60 vs 57) (1 vs 0)	-0.049	0.069	-0.710	0.475	-0.185	0.086
(61 vs 57) (1 vs 0)	-0.172	0.052	-3.330	0.001	-0.273	-0.071
(62 vs 57) (1 vs 0)	-0.175	0.081	-2.160	0.030	-0.334	-0.017

Note: Treatment years shaded in gray. Standard errors computed using the Delta Method.

4.6.2 Probit Estimates

Table 4.3: Probit Model

	(1) NDSLP	(2) Science	(3) Math	(4) Engineer	(5) Education
Ability(IQ)	-0.0004	0.0071***	0.0097***	0.0121***	-0.0151***
	(0.7953)	(0.0011)	(0.0099)	(0.0000)	(0.0000)
In-Need	0.2435***	-0.0384	0.1763	0.0756	0.2634***
	(0.0000)	(0.6396)	(0.1581)	(0.3881)	(0.0001)
1958	0.0291	0.0596	-0.0375	0.2943**	-0.2023*
	(0.7388)	(0.6010)	(0.8590)	(0.0106)	(0.0620)
1959	-0.0603	0.1429	0.2023	0.3114*	-0.5025***
	(0.6315)	(0.3663)	(0.4277)	(0.0562)	(0.0036)
1960	-0.1194	0.1417	0.1620	0.0770	-0.3635*
	(0.4215)	(0.4436)	(0.5994)	(0.7215)	(0.0570)
1961	-0.0430	0.0747	-0.2236	0.1541	-0.2342
	(0.7528)	(0.6678)	(0.5634)	(0.4072)	(0.1665)
1962	0.2461	0.2541	0.0000	0.2106	0.1261
	(0.1413)	(0.2128)	(.)	(0.3575)	(0.5083)
1958*1(In-Need)	-0.0234	-0.0563	0.3418	0.0263	-0.0977
	(0.8857)	(0.8008)	(0.2824)	(0.9010)	(0.6057)
1959*1(In-Need)	-0.2757	-0.3935	-0.3535	-0.9851**	0.4294*
	(0.2137)	(0.2360)	(0.4706)	(0.0254)	(0.0998)
1960*1(In-Need)	-0.1309	0.1890	-0.1718	-0.2419	-0.1414
	(0.6110)	(0.5481)	(0.7443)	(0.5542)	(0.6565)
1961*1(In-Need)	-0.1832	0.4402	0.3189	0.0064	-0.9839**
	(0.5146)	(0.1751)	(0.5925)	(0.9864)	(0.0420)
1962*1(In-Need)	-0.7105**	-0.8532*	0.0000	0.1699	-0.6585**
	(0.0113)	(0.0735)	(.)	(0.6374)	(0.0461)
N	3414	3414	3320	3414	3414

p-values in parentheses

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

Note: Coefficients represent the percentage point change in likelihood of an individual choosing a given major (indicated in the column header). P-values are reported for robust standard errors.

4.6.3 Linear Probability Estimates

Table 4.4: Linear Probability Model

	(1) NDSLP	(2) Science	(3) Math	(4) Engineering	(5) Education
Ability(IQ)	-0.0002	0.0013***	0.0005**	0.0018***	-0.0039***
	(0.7948)	(0.0011)	(0.0134)	(0.0000)	(0.0000)
In-Need	0.0955***	-0.0067	0.0094	0.0123	0.0795***
	(0.0001)	(0.6336)	(0.2087)	(0.3463)	(0.0001)
1958	0.0112	0.0113	-0.0014	0.0484**	-0.0513**
	(0.7410)	(0.6029)	(0.8731)	(0.0261)	(0.0361)
1959	-0.0229	0.0270	0.0097	0.0513	-0.1068***
	(0.6291)	(0.4087)	(0.5396)	(0.1024)	(0.0002)
1960	-0.0448	0.0268	0.0079	0.0097	-0.0851**
	(0.4128)	(0.4833)	(0.6613)	(0.7509)	(0.0211)
1961	-0.0163	0.0144	-0.0087	0.0241	-0.0526
	(0.7518)	(0.6774)	(0.4423)	(0.4455)	(0.1369)
1962	0.0966	0.0524	-0.0186***	0.0331	0.0338
	(0.1478)	(0.2767)	(0.0000)	(0.4137)	(0.5462)
1958*1(In-Need)	-0.0089	-0.0105	0.0265	0.0087	-0.0346
	(0.8905)	(0.7851)	(0.2899)	(0.8366)	(0.4869)
1959*1(In-Need)	-0.1076	-0.0629	-0.0174	-0.1090***	0.0826
	(0.1954)	(0.1833)	(0.4894)	(0.0041)	(0.2302)
1960*1(In-Need)	-0.0534	0.0395	-0.0070	-0.0292	-0.0561
	(0.5839)	(0.5737)	(0.8257)	(0.5453)	(0.4286)
1961*1(In-Need)	-0.0727	0.1046	0.0166	0.0015	-0.1837***
	(0.5012)	(0.2199)	(0.6548)	(0.9821)	(0.0006)
1962*1(In-Need)	-0.2734***	-0.1155**	-0.0074	0.0304	-0.1800**
	(0.0085)	(0.0410)	(0.3313)	(0.6740)	(0.0290)
R^2	0.008	0.007	0.005	0.012	0.034
N	3414	3414	3414	3414	3414

p-values in parentheses

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

Note: Coefficients represent the percentage point change in likelihood of an individual choosing a given major (indicated in the column header). P-values are reported for robust standard errors.

4.6.4 Robustness Checks

Table 4.5 below shows the distributions of average household income across definitions of the In-Need proxy. According to a 1961 survey of NDSLP recipients, 5 out of 7 (approximately 71.4%) of students came from families with average incomes below $6,000.The family income at the 75th percentile of the data is $5,900 when In-Need captures students from families in the bottom quartile of socioeconomic status. When In-Need captures students from families below the median socieconomic status, the 75th percentile of average household incomes is $6,700. As demonstrated in Table 4.6, these two measures of student need have qualitatively and, with the exception of a few coefficients that are very imprecise, quantitatively similar results.

Table 4.5: Average Family Income Across Definitions of In-Need Proxy

Percentile	5% SES	10% SES	25% SES	50% SES
1st	$500	$500	$600	$700
5th	800	900	1,200	1,400
10th	1,000	1,400	1,800	2,000
25th	1,800	2,300	2,800	3,300
50th	2,800	3,500	4,300	5,000
75th	3,600	4,800	5,900	6,700
90th	4,700	6,000	7,100	8,300
95th	5,500	6,600	8,100	9,300
99th	6,600	8,800	10,100	11,600

According to a 1961 survey of NDSLP recipients, 5 out of 7 (approximately 71.4%) of students came from families with average incomes below $6,000. This table shows the distributions of average family incomes (fo years 1957-1960) for each definition of the In-Need proxy. The family income at the 75th percentile of the data is $5,900 when In-Need captures students from families in the bottom quartile of socioeconomic status.

Table 4.6: Robustness of Linear Probability Estimates to In-Need Dummy

	NDSLP	Science	Math	Engineering	Education
In-Need: Bottom 5% of SES Distribution					
1958*1(In-Need)	0.0797	-0.0141	0.0162	0.1122	-0.0297
	(0.4811)	(0.8257)	(0.7100)	(0.1971)	(0.7578)
1959*1(In-Need)	0.0968	-0.0265	-0.0258	0.0023	0.1419
	(0.4929)	(0.7315)	(0.1588)	(0.9739)	(0.2984)
1960*1(In-Need)	0.0005	-0.1293***	-0.0253	0.0742	0.0759
	(0.9975)	(0.0025)	(0.2086)	(0.4925)	(0.6243)
1961*1(In-Need)	-0.4811***	-0.1357***	-0.0174	-0.0852**	-0.2389***
	(0.0000)	(0.0014)	(0.3257)	(0.0221)	(0.0000)
1962*1(In-Need)	0.0032	-0.1074**	0.0012	0.0802	0.0243
	(0.9840)	(0.0151)	(0.9249)	(0.4886)	(0.8670)
In-Need: Bottom 10% of SES Distribution					
1958*1(In-Need)	0.0923	-0.0460	0.0209	0.1162	0.0063
	(0.3166)	(0.3143)	(0.5988)	(0.1055)	(0.9368)
1959*1(In-Need)	-0.0841	-0.0063	-0.0369**	-0.0356	-0.0103
	(0.4534)	(0.9246)	(0.0429)	(0.4743)	(0.9169)
1960*1(In-Need)	0.0567	-0.0339	-0.0341*	0.0421	0.0773
	(0.7148)	(0.7080)	(0.0850)	(0.6162)	(0.5775)
1961*1(In-Need)	-0.2930**	-0.0273	-0.0262	-0.0925***	-0.1430
	(0.0351)	(0.7928)	(0.1280)	(0.0083)	(0.1679)
1962*1(In-Need)	0.0417	-0.1068**	-0.0086	0.0572	0.0948
	(0.7845)	(0.0104)	(0.4547)	(0.5976)	(0.5081)
In-Need: Bottom 25% of SES Distribution					
1958*1(In-Need)	-0.0089	-0.0105	0.0265	0.0087	-0.0346
	(0.8905)	(0.7851)	(0.2899)	(0.8366)	(0.4869)
1959*1(In-Need)	-0.1076	-0.0629	-0.0174	-0.1090***	0.0826
	(0.1954)	(0.1833)	(0.4894)	(0.0041)	(0.2302)
1960*1(In-Need)	-0.0534	0.0395	-0.0070	-0.0292	-0.0561
	(0.5839)	(0.5737)	(0.8257)	(0.5453)	(0.4286)
1961*1(In-Need)	-0.0727	0.1046	0.0166	0.0015	-0.1837***
	(0.5012)	(0.2199)	(0.6548)	(0.9821)	(0.0006)
1962*1(In-Need)	-0.2734***	-0.1155**	-0.0074	0.0304	-0.1800**
	(0.0085)	(0.0410)	(0.3313)	(0.6740)	(0.0290)
In-Need: Bottom 50% of SES Distribution					
1958*1(In-Need)	-0.0017	-0.0712*	0.0090	0.0408	0.0151
	(0.9763)	(0.0539)	(0.6148)	(0.2590)	(0.7188)
1959*1(In-Need)	-0.1030	-0.0546	-0.0599*	-0.0950*	0.1058**
	(0.2068)	(0.3121)	(0.0624)	(0.0623)	(0.0396)
1960*1(In-Need)	-0.2010**	-0.0390	-0.0057	-0.0878	-0.0697
	(0.0312)	(0.5608)	(0.8475)	(0.1008)	(0.2856)
1961*1(In-Need)	-0.1370	0.0920	-0.0191	-0.0257	-0.1656***
	(0.1357)	(0.1335)	(0.4425)	(0.6482)	(0.0069)
1962*1(In-Need)	-0.1889*	0.0035	-0.0132**	0.0093	-0.1896**
	(0.0851)	(0.9581)	(0.0325)	(0.8907)	(0.0403)

Note: This table shows the linear probability estimates across several definitions of In-Need. Definitions of In-Need range from those students below in the bottom 5% of socioeconomic status (SES) in the first panel to those below the median SES in the last panel. Coefficients represent the percentage point change in likelihood of an individual choosing a given major (indicated in the column header). P-values are reported for robust standard errors. p-values in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Chapter 5

Conclusion

This dissertation is a study of the behaviors of economic agents that interact in socially inefficient markets. Understanding the behavior of the economic entities who compose the supply and demand sides of inefficient markets is paramount to designing policies to correct for externalities and achieve social efficiency. I studied two markets with social inefficiencies: the market for pollution intensive manufacturing goods and the market for higher education. My results support several interesting conclusions.

First, in *The Environmental Performance of Foreign Owned Manufacturing Establishments in the United States* I ask, 'What is the environmental performance of foreign owned manufacturing establishments in the United States?" I find that, on average, the environmental performance of foreign establishments does not differ from domestic establishments. However, there is significant heterogeneity in foreign establishment environmental performance across industries. In some industries, foreign establishments are much cleaner and in others they are much dirtier. A candidate explanation suggests that some of the heterogeneity is explained by fixed costs. That is, foreign establishments in high fixed cost industries are much cleaner. Therefore, blanket environmental policies aimed at reducing emissions are not efficient because they focus resources on mitigating pollution at relatively clean foreign establishments in high fixed cost industries. Policies should account for this heterogeneity.

Second, in *The Incidence of the Post-9/11 GI Bill Subsidy at Institutions of Higher Education: A Study of the Response of In-State Tuition and Fees to Veteran Education Benefits* I ask, "Do colleges increase their tuition and fees in response to federal financial aid subsidies?" I find evidence for tuition and fee increases across several types of higher education institutions. This behavior suggests that the effectiveness of student aid is undermined as students no longer receive the same benefit from their financial aid that they would have if student charges remained unchanged. In principle, the existence of this supply-side response implies that financial aid policies can never fully move higher education markets to their socially efficient equilibrium. The government would need to "overfund" students to account for the adverse response of schools which invoked opportunity costs in other markets (i.e., financial aid funds could be used to support other federal programs).

Lastly, in *Major-Contingent Loans and College Major Choice* I ask, "Can conditioning federal student lending terms (e.g., interest rates) on students' choice of major increase the number of STEM and other desired degrees?" The policy studied does not offer loans that are strictly major contingent, although students are nudged toward studying a qualifying major by first indicating a desire to study that major in the loan application process. I find that the behavioral response of students largely reflects the impact of the loan being lower interest and *not* major contingent. That is, students with less debt tend to select less financially lucrative majors, which is what my results suggest. For example, I find weak evidence for decreases in engineering majors and increases in teaching majors. In conclusion, a fully major contingent loan policy that penalizes students for switching majors may be what is necessary to incentivize students to switch to a qualifying major. A simple nudge in the application process is not sufficient to increase the study of needed majors.

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Vita

Justin R. Roush was born in Phoenix, Arizona to Judy M. and Dennis R. Roush. He has a very creative and artistic sister named Lindsay. Justin is engaged to marry Kimberly Huber, a Marketing major from Butler University who earned a Master of Business Administration degree from Duquesne University. Justin's passion for learning began at Foothills Elementary School in Glendale, Arizona. After relocating to Bardstown, Kentucky, Justin finished his primary education at Old Kentucky Home Middle School and attended high school at Nelson County High School. He then attended Centre College of Danville, Kentucky where he graduated *magna cum laude*, earning a Bachelors of Science in both Mathematics and Financial Economics. Thereafter, he accepted a graduate teaching assistantship in the Economics Department at the University of Tennessee, Knoxville where he studied Environmental Economics and Public Finance. Justin graduated with his Master of Arts degree in Economics in December of 2011 and will receive his Doctor of Philosophy degree in Economics in May of 2015. After Tennessee, Justin will be an Assistant Professor of Economics at Georgia College and State University in Milledgeville, Georgia.