THREE ESSAYS IN PUBLIC FINANCE AND LABOR ECONOMICS

by

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To Jack M. Pitlick (1919-2006): loving grandfather, friend and Wolverine. Your intellect, compassion, and joie de vivre continue to be an inspiration.

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CHAPTER I

Introduction

This dissertation explores three important empirical questions in public finance and labor economics. The dissertation includes three separate articles on broad topics in these fields. While it is difficult to find unifying topical themes, the papers all apply the theoretical and empirical tools from public finance and labor economics to achieve their aims.

The first paper in the dissertation, entitled "The Effect of Teachers' Unions on Education Production: Evidence from Union Election Certifications in Three Midwestern States" undertakes an empirical analysis of how teachers' unions influence the level and productivity of school district resources. The paper is motivated by the fact teachers' unions are a prevalent and stable fixture in the U.S. public education system. As of 2004, over 50% of public school teachers nationally, and over 80% of teachers in the Midwestern states studied in this paper, were covered by a collectively bargained contract.

Despite, or perhaps because of their prevalence, teachers' unions remain controversial. In this paper, I analyze the impact of teachers' unions on the level and distribution of school district resources and on the returns to those resources in the education production function. A major impediment to conducting this type of research is the lack of data on when teachers in each school district became unionized for the purpose of collective bargaining. To overcome this problem, I hand-collected data on the timing of teachers' union election certifications from public employment relations commissions in Iowa, Indiana, and Minnesota. These data are unique in their accurate description of union histories in each school district and have not been utilized before in any teachers' union impact study.

I combine the new union certification data with information on school district resources from the Census and Survey of Governments (1972-1991) and the ELSEGIS (1967-1979). To analyze the effect of unions on school district resource levels and on the distribution of district expenditures, I employ an event study methodology that allows me to estimate the time pattern of union effects without imposing any parametric restrictions on this pattern.

The results indicate unions have no impact on teacher pay, district expenditures or revenues per student, but they increase teacher employment by between 5 and 10 percent. This employment increase is offset by a relative increase in student enrollment in unionized districts, causing unions to have little effect on student-teacher ratios. I also find little evidence unions impact the intra-district distribution of expenditures. Further, I estimate education production functions using high school dropout rates calculated from the 1970-1990 U.S. Census. While there is little evidence of a net union effect on dropout rates, my results are consistent with unions causing an increase in the returns to lower class sizes and higher teacher pay. These findings are in conflict with much of the past literature on teachers' union impacts. I argue a major cause of this discrepancy is due to measurement error in the union measure constructed from survey responses in the Census of Governments. The results from this analysis highlight the importance of correctly measuring unionization status in union impact studies.

The second paper in this dissertation explores how cross-state price differentials impact the effectiveness of state-level cigarette excise taxes in reducing consumption and increasing tax revenue. Cigarette taxes have garnered increasing interest in the United States by both government and public health officials over the past 30 years. Due to the potential health and revenue gains from cigarette taxation, many states have increased their cigarette taxes markedly since the 1970s, causing large interstate price differences in many areas of the country. This cross-state price variation can confound many of the potential gains from cigarette taxation as higher taxes may cause individuals to purchase cigarettes across state lines.

This paper uses micro-data on cigarette consumption from the 1993, 1996, 1999, and 2001 CPS Tobacco Supplements to estimate cigarette demand models that incorporate the decision of whether to smuggle cigarettes across a state or Native American Reservation border. The central problem addressed by this study is the state of purchase is not identified in the data, which means the purchase price is unknown. My solution to this difficulty is to explicitly model the decision to smuggle and then include the parameters of this decision in the demand model. Specifically, I use the distance to a lower-price locality to proxy for unobserved heterogeneity in the response of demand to changes in the home state price.

My estimates imply increasing a state's cigarette tax has little impact on smoking behavior in that state; the elasticity with respect to the home state price is indistinguishable from zero. In contrast, I find the elasticity with respect to the full price is negative and of sizeable magnitude. My results also indicate between 13 and 25 percent of consumers purchase cigarettes in a lower-price state or Native American Reservation. These estimates represent a lower bound on the percentage of cigarettes purchased in border localities. The large magnitude of smuggling combined with the inelastic home state price elasticities suggest state-level cigarette taxation may be a poor policy instrument with which to decrease smoking and increase tax revenues. However, the fact that the full price elasticities are negative, statistically significant, and close to unity implies state-level cigarette excise taxes could be a useful tool to change smoking behavior and raise revenue if cross-border purchases were eradicated.

The final paper in this dissertation is entitled "Understanding the Increased Time to the Baccalaureate Degree." The central motivation of this study is time to completion of the baccalaureate degree has increased markedly among college graduates in the United States over the last four decades. Between the cohorts graduating from high school in 1972 and 1992, the percent receiving a degree within 4 years dropped from 56.8% to 43.6%, average time to degree increased from 4.7 to 4.9 years, and the eight-year college completion rate fell from 51.1% to 45.3%. Despite the documentation of this shift, little is understood about its causes and resulting welfare effects.

Among the reasons students may extend the collegiate experiences beyond the four-year norm include the need for academic remediation that lengthens the course of study, inability to finance full-time attendance requiring part-time enrollment and employment, or simply a desire to extend the consumption experience of collegiate life. The consequences of extended time-to-degree may include both individual loss of earnings and the social cost of potentially reduced economic growth as the supply of college-educated workers is limited by tradeoffs between school and work.

Using data from the NLS72 and NELS:88 longitudinal surveys, this paper shows the increase in time to degree is localized among those students who begin their postsecondary careers at public universities outside the top-ranked 15 and two year institutions. There is no evidence changes in student characteristics including pre-collegiate achievement or parental characteristics explain the observed increase in time to degree or drop in college completion rates. In contrast, the results indicate the increases in time to degree tend to be concentrated in states that experienced rapid growth in the size of their college-age population, which may adversely affect collegiate attainment through the dilution of resources at many public colleges. We also find evidence of increased hours of employment among students, which is consistent with students working more to meet rising college costs.

CHAPTER II

The Effect of Teachers' Unions on Education Production: Evidence from Union Election Certifications in Three Midwestern States

2.1 Introduction

In 1959, conservative state legislators in Wisconsin amended a bill establishing collective bargaining rights for public employees to include public school teachers. Their hope was such an amendment would cause moderates to vote against the legislation, as teacher organization was considered a radical idea at the time. Despite this effort, the bill passed, making Wisconsin the first state to establish the right of teachers to collectively bargain (Beilke, 2001).

Since its accidental beginnings, public school teacher collective bargaining has become a stable fixture in the American education system: by 1988, all but 7 states had passed a law either allowing for the right of teachers to bargain collectively or explicitly requiring districts to bargain with teachers' unions. Further, only four states had statutes prohibiting collective bargaining between public school districts and teachers (Freeman and Valletta, 1988). By 2004, 45.1% of public school teachers were members of a labor union that exists for the purpose of collective bargaining, and 50.8% were covered by a collective bargaining contract.[1]

Despite, or perhaps because of, the large rise in teacher organization over the past 40 years, teachers' unions remain controversial. Opponents of teachers' unions argue these

[1] Author's calculation from the May 2004 Current Population Survey.

organizations take reform power away from administrators and parents as well as drain district resources (Haar, 1996 and Moe, 2001). Advocates of teacher unionization believe empowering educators who are in the classroom bolsters student achievement by allowing for resources to be distributed in a more effective manner and to be used more efficiently (Retsinas, 1982 and Johnson, 2004).

This debate is particularly relevant today as many reformers push for more competition in primary and secondary schooling. Proponents of increased school competition suggest introducing more competition into the system will reduce the importance of teachers' unions and partially undo any deleterious impacts these unions may have on districts (Chubb and Moe, 1988 and Moe, 2001). However, this argument is invalid if teacher unionization has no negative effect on school districts or students.

The central questions I address in this study are whether teachers' unions distort the allocation of inputs to education or change the returns to those inputs. Unions can impact education production in two ways: they can constrain the ability of administrators to choose freely the level and mix of inputs to education production or they can alter the production function itself. These effects will likely be related due to diminishing marginal productivity and the fact unions may change teacher inputs that are either complements or substitutes to other inputs. To the extent affected resources impact educational attainment, unions can have a positive or negative effect on student achievement.

To investigate these issues, I undertake an empirical examination of the effect of teacher organization on the level and composition of school district resources and on the returns to those resources in the education production function. A major impediment to conducting this type of research is the lack of data on which districts have teachers' unions and when they first organized. To remedy this problem, I have hand-collected teacher union election certification data for all school districts in three Midwestern states: Iowa, Indiana and Minnesota. Because these data are available only in paper format at each state's Public Employment Relations Board office, this information has not been utilized before in any analysis of teacher unionization. These data allow me to construct a detailed panel of school districts that contains accurate union representation histories for every district in the sample.

I merge the certification data with three different data sets containing school district outcomes: the Census/Survey of Governments (COG/SOG) from 1972–1991, the Elementary and Secondary General Information System (ELSEGIS) from 1967–1979, and the school-district level summaries from the U.S. Census of Population and Housing from 1970–1990. As necessitated by the data, the level of analysis in this project is the school district, not the individual school.[2]

I employ an "event study" methodology that includes dummy variables for each relative year to unionization in order to analyze the impact of unions on district resource levels and expenditure allocations. Unlike previous teachers' union impact studies, this methodology allows me to estimate both the long and short term effect of unions on various resource levels by analyzing the change in district resources over time relative to union elections. By examining the pre-election trends, I also can determine whether there is any evidence that changes in educational inputs affect union election timing.

In contrast to the majority of other studies of the impact of teachers' unions, I find organization for the purpose of collective bargaining has little effect on educational inputs. Similar to studies such as Smith (1972), Balfour (1974), Zuelke and Frohreich (1977), and Kleiner and Petree (1988), my results indicate no increase in teacher pay, either in the short or long run, due to unionization.[3] While I find full-time teacher employment increases by between 5 and 10 percent, unions increase enrollment relative to non-union districts, which almost fully offsets any reductions in the student-teacher ratio due to the employment increase. Current operating expenditures and district revenues per student also respond negligibly to teacher unionization.

[2]This aggregation likely causes few problems because the unionization decision occurs at the district level. However, I will not be able to detect differential effects across school types within school districts insofar as they exist.

[3]In his comprehensive review of the literature, Freeman (1986) reports the majority of teachers' union impact studies find a positive effect of unionization on wages of between 3 and 21 percent. He also reports wage premia on the order of 5 to 10 percent for public sector protective services unions.

I analyze the effect of teachers' unions on the allocation of school district expenditures as well. The share of total expenditures that go to instruction (including teacher pay), administration, attendance and health, transportation, plant operation and maintenance, and fixed charges are largely unaffected by teacher unionization.

Finally, I estimate education production functions using the high school dropout rate as the measure of educational output. I find teachers' unions have no net effect on dropout rates, but there is evidence they increase the returns to educational resources such as teacher pay and student-teacher ratios. These results are consistent with unions increasing the efficiency of teacher-based inputs into education production.

My findings are provocative in that they conflict with much of the previous literature on teachers' unions.[4] Utilizing cross-sectional data on the existence of teacher collective bargaining contracts, Eberts and Stone (1986) estimate teachers' unions increase district costs by 15 percent, but they also increase educational productivity by 3 percent (1987). Baugh and Stone (1982) find unions increase teacher pay by between 4 and 12 percent in a study that utilizes teacher union membership data from the CPS. Using similar data, Moore and Raisian (1987) estimate a teacher union wage premium between 3 and 6 percent. In contrast, Kleiner and Petree (1988) find union membership and the percentage covered by contracts have a negligible effect on wages but have a positive and significant impact on SAT scores and non-wage expenditures per student at the state aggregate level.

In the most comprehensive study of teacher union impacts to date, Hoxby (1996) constructs a district-level panel from the 1972 through 1992 Census of Governments. This study is an advancement over previous cross-sectional work because it uses school district fixed effects to overcome the endogeneity of union status inherent in such estimates. She finds the presence of a teachers' union, as indicated by the existence of contracts combined with over 50 percent teacher union membership and the district reporting it engages in collective bargaining[5], increases average teacher pay by over five percent and current op-

[4]See Freeman (1986) for an overview of the literature.
[5]Hoxby classifies a school district as unionized only if all three of these conditions are met.

erating expenditures by almost three percent, while decreasing student-teacher ratios by 1.1. She also reports evidence unions increase high school dropout rates as well as reduce the returns to expenditures on teachers.

My results differ sharply from those reported in Hoxby (1996), despite the fact I am able to reproduce many of her results on my sample of three states using the same data. I show the differences between my results and Hoxby's are due to pervasive classification error of unionization status of upwards of 45 percent in the Census of Governments. While these results suggest the accepted notions that unions reduce teacher productivity and drain district resources may not be robust to more accurate unionization data, they are based only on data from three states. To the extent the political and educational environments in Iowa, Indiana, and Minnesota in the 1970s and 1980s differed from the rest of the country, my results will not be generalizable.[6] However, the comparison between my results and those found by Hoxby (1996) underscores the importance of correctly measuring union status in union impact analyses and suggests collecting certification data from more states will allow a more general and robust analysis of teachers' union impacts in future research.

The rest of this chapter is organized as follows: Section 2.2 provides a brief overview of theoretical predictions of union effects on school district resources and student achievement. Section 2.3 describes the data used in the analysis and discusses the election certification data in detail. Section 2.4 presents the empirical methodology and results for the analysis of union effects on educational resources, while Section 2.5 contains the methodology and results pertaining to the effect of unions on the education production function. In Section 2.6, I analyze the impact of measurement error on union impact estimates as a means to explain the differences between my results and previous findings. Section 2.7 concludes.

[6]There is reason to believe the union effects in the three states in this analysis would differ from the rest of the country. Iowa, Indiana, and Minnesota had high private-sector union representation due to the prevalence of manufacturing in these states in the 1970s. This pattern, combined with high public sector unionization rates in neighboring states, suggests public sector unions might not have been as controversial or undesirable to residents as in other areas of the country.

2.2 Theoretical Predictions

Because no comprehensive theoretical model of public sector union behavior exists, it is not clear *a priori* how unions will impact either district resources or student achievement. A central purpose of any labor union is to maximize the well-being of its members. In order to accomplish this goal, teachers' unions often advocate for higher wages, fewer hours and higher benefits for teachers. If these unions are successful in advocating for such changes, then districts might redistribute resources towards teacher pay and away from other areas of expenditure that may be more effective at increasing student achievement. As unions become more entrenched and gain more power over time, such effects could amplify as teachers extract more and more rents from districts. Further, because unions often make it more difficult for districts to fire teachers, and because union contracts typically do not involve performance–based compensation, any increase in teacher pay will not necessarily be correlated with an increase in teacher output. Thus, the marginal returns to teacher pay may fall due to teacher organization.[7]

Even a purely rent-seeking union may have a non-negative effect on student achievement. Because unions are often focused on improving working conditions as well as pay (Retsinas, 1982), teacher organization may lead to smaller class sizes and more satisfied teachers. The increase in workers' job satisfaction due to unionization is typically referred to as a "union voice" effect, and there is evidence in the private sector union literature that giving workers a voice with which to change their working environment increases productivity (see Gunderson (2005) for an overview). If teachers protect themselves from perceived or actual administrative abuses through the exercise of their union voice, unionization can have positive productivity effects. Additionally, any increase in wages or benefits could attract better teachers, thus increasing average teacher productivity.

In contrast to the rent–seeking model of union behavior, teachers' unions may seek

[7]This is typically called the "rent–seeking" model of union behavior, as unions seek to extract rents from the district without regard to their impact on students.

explicitly to maximize student achievement. If there is misallocation of district resources absent unionization,[8] teachers' unions can use their collective power and their first–hand experience in the classroom to help redistribute resources in a manner that is more effective for education. Similarly, unions may have a positive impact on districts if they divert more local government funds from other sources to schools. This would result in an increase in the level of funding for schools, but not necessarily a change in the distribution.

These predictions of the impact of unionization on school districts and students are not mutually exclusive. Unions might be advocating simultaneously for increases in teacher pay, better working conditions, and for resources that will more effectively serve students. To the extent these outcomes have differential effects on achievement, simple models of union behavior do not yield unique predictions about the impact of unionization. It therefore is necessary to analyze empirically the effect teachers' unions have on students and school districts in order to evaluate the claims made by both advocates and opponents of these unions.

2.3 Data

2.3.1 Measuring Teacher Unionization

2.3.1.1 Overview

Studies of the impact of teachers' unions have traditionally utilized two forms of unionization measures depending on the level of observation in the study. If the study is at the teacher level, the union measure is typically whether the teacher is a member of a union (Moore and Raisian, 1987 and Baugh and Stone, 1982).[9] There are, however, several problems with such measures that can bias impact estimates. The largest problem with union membership data is teachers can be employed in unionized districts without being members of the union. Further, being a union member does not necessarily mean the union engages in collective bargaining; many unions in the United States function merely as professional

[8] Such a misallocation could arise due to the politicization of funding decisions at the local level or from inefficient district management (see Chubb and Moe (1988) for a discussion of these issues).

[9] Examples of data sets that include teacher union membership are the Sustaining Effects Study, the Current Population Survey, High School and Beyond, and the Panel Study of Income Dynamics.

organizations.[10] The differences between union membership and the existence of a union for the purpose of collective bargaining in a given teacher's district will likely cause an attenuation bias in the estimates of union impacts.

Studies that take the school district as the level of observation tend to use the existence of a contract or collective bargaining agreement as the measure of teacher unionization (Eberts and Stone, 1986; Eberts and Stone, 1987; Woodbury, 1985; Kleiner and Petree, 1988; Hoxby, 1996).[11] Absent measurement error, a collective bargaining agreement will accurately measure the presence of a union as long as all unions obtain contracts.[12] According to the NEA and AFT, which represent the vast majority of teachers' unions in the United States, it is rare for a unionized district to never obtain a contract, although there can be a lag between union formation and the culmination of collective bargaining in the form of a contract.

No previous union effects study has been based on data that accurately describe both the timing of unionization and the existence of a teachers' union in a given district. In order to obtain an improved measure of teacher unionization, I hand-collected teacher union certification dates from union election certifications housed in the Public Employment Relations Board/Commission (PERC) office in Iowa, Indiana and Minnesota. When teachers in a district organize for the purpose of collective bargaining, the state PERC conducts an election. If over 50 percent of all school district teachers vote "yes," then the commission certifies the union as the sole bargaining representative of the teachers. The date of election certification is thus the official date of unionization in each district.

Because the unions in the three states in this analysis are all members of the National Education Association (NEA), groups of locals are aggregated into "UniServ" districts. The UniServ offices oversee the bargaining and governance of each of the union locals in their district. I validated the election certification data by contacting the UniServ districts

[10] Both the NEA and the AFT began this way before the official onset of collective bargaining for teachers.

[11] Information on the existence of collective bargaining agreements generally comes from either the Sustaining Effects Study or the Census of Governments.

[12] Being unionized is necessary for engaging in collective bargaining, but a union that negotiates with a school district is not guaranteed to obtain a contract.

and requesting the date of first contract and, if available, the date of first certification for each union local in their district. Many UniServ districts did not have this information, which highlights the difficulty in collecting accurate union data. I augmented the validation data by searching for case law on LexisNexis, the Indiana Education Employment Relations Board, and the Iowa State Teachers' Association that indicated when a district began collectively bargaining with teachers. The union election certification data accurately represented the timing of union formation. In the few cases in which there was a discrepancy, I used the date given by the UniServ office or listed in the legal decision rather than the date recorded from the PERC office.[13]

I chose Iowa, Indiana and Minnesota for this analysis because all three states passed "duty-to-bargain" laws in a time period covered by my outcome data. Prior to 1972, all states in my sample allowed collective bargaining between teachers and districts, but a school district did not have a duty to bargain with teachers if the administration did not choose to do so. As a result, there were few contracts in place prior to 1972.[14] Beginning in Minnesota in 1972 and followed by Indiana in 1973 and Iowa in 1975, these states passed "duty-to-bargain" laws, which mandated a school district administration is legally bound to bargain in good faith with employees if the employees desire such negotiation. These laws dramatically increased unionization rates among teachers in these states (see Figure 2.1).

Because there was little voluntary recognition of teachers' unions by school districts prior to the passage of the duty-to-bargain laws in these states,[15] the election certifications measure the time of first organization for the purpose of collective bargaining. The data show teachers' unions established a significant presence in the public education system over the time period of this analysis in Iowa, Indiana, and Minnesota; all three states had school district teacher unionization rates of over 75 percent by 1987.

[13] All of the discrepancies came either from early recognition or from the merger of two districts that necessitated a new union election. The certification date recorded from the PERC offices were incorrect for 5.8 percent of school districts.

[14] These contracts were all due to "voluntary recognition" of the union by the school district.

[15] When I exclude voluntarily recognized unions from the analysis, the results are unchanged.

The union certification data have several advantages over the measures used in earlier analyses. The first is instead of measuring whether teachers have a contract, which is the outcome of collective bargaining, I measure whether they have an agent certified by the state to engage in collective bargaining. If one is interested in the effect of teachers' unions on school districts rather than the effect of collectively bargained contracts on school districts, this measure is more appropriate than ones previously used. However, the validation study showed, in the vast majority of cases, unions negotiate a contract within one school year of certification. I found no districts in which the union did not achieve a contract. This result suggests, while the existence of a union and the existence of a negotiated contract are conceptually distinct, in practice they are similar. Analyzing the effect of winning a unionization election as opposed to negotiating a contract should yield comparable results.

Secondly, because the certification dates are obtained from official state documents, there will be less measurement error than in data based on survey responses. Finally, similar to the contract measure, the certification measure will not confound the existence of a union whose purpose is collective bargaining with a teachers' organization. The latter professional group will not engage in a unionization election.

Figure 2.1 presents the distribution of teachers' union certification years by state. The spikes in the distributions correspond to years in which a state passed a "duty-to-bargain" law. The small number of districts that obtained certification prior to the passage of the state law did so through voluntary recognition by the district administration. As is evident in Figure 2.1, passage of a law establishing teacher collective bargaining was a major determinant of winning a unionization election.[16] This trend is consistent with those reported in Saltzman (1985), who argues unionization laws were largely a cause and not an outcome of teacher collective bargaining.

[16] Unlike in the private sector, these elections are rarely unsuccessful. In fact, in my sample, there are no districts in which an election was lost.

2.3.1.2 A Comparison of Alternative Unionization Measures

How does the new election certification measure of teacher unionization compare to the only other district-level panel containing unionization information: the Census of Governments (COG) Labor Relations Survey used in Hoxby (1996)? The COG does not directly ask respondents about the existence of a teachers' union or a contract with that union. Instead, it contains three survey items related to labor relations that can be used to infer union status in a district:

1. Total number off full-time teachers who are members of an employee organization.
2. Does your agency engage in collective negotiations or meet and confer discussions with employee organizations for the purpose of reaching agreement on conditions of employment?
3. Total number of contractual agreements between your agency and employee organizations in effect as of October 15 of the survey year.

From these survey responses, one can construct a unionization measure using the following criteria: at least 50 percent of teachers are union members, the form of labor negotiations is collective bargaining, and the district has at least one contract or memorandum of understanding with *any* employee organization in effect as of October of the survey year. This is the union measure utilized by Hoxby (1996) in her analysis of teacher unionization and is appropriately designed to identify teacher contracts that are collectively bargained with a school district rather than a contract with other employee unions.

While the above measure is the most sensible alternative in the COG, it has several drawbacks. The first, as previously discussed, is it effectively measures whether a district has a collectively bargained contract with the teachers' union, not whether a teachers' union exists. Given the short lag between certification and negotiation of a first contract, however, this discrepancy is likely small.

The second, more serious, problem with the union measure in the COG is classification error. Although Hoxby's COG-based union measure is designed to reduce potential measurement error by making the definition of unionization relatively strict, there are significant differences between the COG and election certification measures of union status that suggest measurement error exists in the former data. Table 2.1 contains a comparison of district-level unionization rates from the Census of Governments and the election certifications for each state in the sample. Note the COG is conducted every 5 years and labor relations information was only included in the 1972, 1977, 1982 and 1987 surveys.

The table illustrates the substantial differences between the two union measures. In the table, each four-cell square sums to one, and each diagonal within a cell represents the observations for which the union measures agree. For example, in Iowa in 1977, the COG and election certification measures agree 49.89% of school districts were unionized and 26.61% were not. However, 9.31% of the school districts are classified as unionized by the COG measure but had not successfully completed a teachers' union election by that date. Conversely, 14.19% of districts had completed an election but were measured as not unionized by the constructed Census of Governments union measure.

I interpret the disagreement between the two data sources as measurement error, with true union status measured by the election certifications. Given there was little voluntary recognition occurring in these states in this period and the validation study made every attempt to find such districts, measurement error in the Census of Governments is a natural explanation for why there are districts that had not completed a unionization election yet were measured as unionized by the COG. Further, since most districts achieve a contract within a year of certification, the lag between certification and successfully negotiating a contract cannot explain why so many districts that had certified unions were not measured as unionized by the Census of Governments.

The accuracy of the COG unionization construct is also called into question by the differential time trends in union status within states across measures. Because there are no decertifications, unionization as measured by election certifications weakly increases over time. Thus, conditional on completing a successful election, a district will always be classified as unionized. In contrast, after 1977, unionization rates decline over time in the

Census of Governments.[17]

To investigate further the source of the discrepancy, I look at which of the three criteria used in the COG union measure "fail" when a district has completed a successful unionization election but is not classified as unionized in the COG. I find for such districts in all three states, the provision that the percentage of teachers who are union members must be greater than 50 fails at higher rates over time. Further, in Indiana, an increasing number of districts report having no negotiated contracts over time, despite the fact that, conditional on obtaining a first contract, it is rare the teachers are ever without a negotiated contract with the district.[18] This is suggestive evidence that the measure of the existence of contracts in the COG contains measurement error.

Taking the election certification data as the true measure of unionization status, Table 2.2 reports the misclassification rates by state and year in the Census of Governments. Aside from 1972, the average misclassification rate remains relatively constant at between 31 and 36 percent in the sample.[19] However, the misclassification rate is as high as 47.2% in Indiana in 1987. The high misclassification rates from the union measure constructed from the Census of Governments suggest this measure does not accurately characterize the history and state of collective bargaining in the school districts in the sample. I will turn to the impact of this measurement error on union impact estimates in Section 2.6.

2.3.2 Other Data Sources

I combine my more accurate teachers' union election certification data with three data sets that contain outcome variables of interest. The first data set I use is the Census and Survey of Governments (COG/SOG) Employment and Finance Surveys. I construct

[17]There is some disagreement over the COG-based unionization rates shown in Table 2.1. I constructed this measure by following the guidelines in Hoxby (1996). In an alternative COG measure sent to me by a referee, while the aggregate level of disagreement across the COG and election certification measures is lower, there are still significant differences that suggest measurement error exists in the former data. Further, the decline in unionization rates over time is still evident in the alternative COG union measure. Unfortunately, I am unable to replicate the alternative measure in order to understand the nature of the discrepancy.

[18]While there are no available credible aggregate statistics on this assertion, lawyers I have spoken to at both the AFT and NEA agree with this generalization. Also, note that even if a contract expires, teachers typically continue to work under that contract until a new one is negotiated with the district. One explanation for the decrease in unionization rates apparent in the COG is expired contracts are coded as "no contract."

[19]Saltzman (1986) provides some outside validation for these misclassification rates. He validates the 1977 Census of Governments union measure for 1000 districts in the U.S.. He finds a misclassification rate of 30% for the U.S., which is similar to the 31% misclassification rate I report for my sample of 3 states in that year.

measures of real monthly full-time teacher pay, full-time teacher employment, student-teacher ratios, real current operating expenditures (COE) and real total revenues for each district in the sample.[20] I have district-level observations for the years 1972-1991, excluding 1975 and 1986 due to data availability. Appendix A contains further details on the Census and Survey of Governments data.

In order to estimate the impact of teachers' unions on the intra-district allocation of expenditures, I use the Elementary and Secondary General Information System (ELSEGIS).[21] The ELSEGIS survey was conducted in 1967-1970, 1973-1974, 1976-1977, and 1979. Unfortunately, the survey was terminated in 1979 without a suitable replacement, but the years in which it was conducted correspond to the highest unionization activity in my sample (see Figure 2.1). ELSEGIS asks school districts for expenditures broken down into six mutually exclusive categories: administration, total instruction, attendance and health, transportation, plant operating and maintenance, and fixed charges. Aside from fixed charges, these categories constitute current operating expenditures in each school district. As with the COG/SOG data, all expenditures are inflated to real 2004 dollars using the CPI deflator.

I use high school dropout rates calculated from the 1970, 1980 and 1990 U.S. Census as my measure of educational attainment in order to estimate the impact of unions on the education production function.[22] I measure high school dropout rates using the following formula:[23]

$$\text{H.S. Dropout Rate} \quad = \quad \left(1 - \frac{\text{total high school enrollment}}{\text{total population 14-18 years}}\right) * 100. \qquad (2.1)$$

[20] Given the errors in the Census of Governments labor relations data described in the previous section, one must be skeptical of the accuracy of the financial and employment information in these surveys as well. However, since the survey is filled out by the central administrative offices that have access to payroll records and budgets, it is reasonable to expect such data will be supplied with greater accuracy than, for example, the number of teachers belonging to the union. The latter information is not likely to be kept on file by the district administrative offices.

[21] ELSEGIS is a precursor to the Common Core of Data and contains detailed revenue and expenditure data for a random sample of school districts in the United States.

[22] All 1990 Census estimates are from the *School District Data Book*. The 1980 census data are taken from the 1980 *Summary Tape File 3-F* (U.S. Department of Commerce, 1980), and the 1970 data are taken from the *1970 Census Fourth Count (Population)* (U.S. Department of Commerce, 1970) and the *Census of Population and Housing, 1970: Fifth Count Tallies: Sample Data for School Districts* (U.S. Department of Education, 1970).

[23] Hoxby (1996) utilizes a similar formula that includes those who are nineteen years old in the denominator. I exclude this group because the typical ages for high school attendance are 14 through 18. This change has no effect on the results.

I also calculate total population, percent urban, average real income, median real gross rent, percent of families in poverty, percent unemployed, percent black, percent Hispanic, percent with a high school diploma or some college, percent with at least a BA, percent enrolled in private school, and total public school enrollment for each district in my sample. These variables come directly from the Census files and are the same as those used in Hoxby (1996).[24]

2.4 The Effect of Teachers' Unions on the Inputs to Education Production

2.4.1 Empirical Methodology

To analyze the effect of teachers' unions on the level and allocation of school district resources, I utilize an empirical methodology derived from the event study literature. I estimate the following equation on the Census/Survey of Governments and ELSEGIS data sets described in the previous section:

$$Y_{ist} = \beta_0 + \sum_{j=-5}^{k} \gamma_j I(t - \text{year}_c = j) + \tau_i + \phi_{st} + \epsilon_{ist}, \qquad (2.2)$$

where Y_{ist} is the log of an outcome variable of interest, ϕ_{st} are year fixed effects that are separate for each state, τ_i are district fixed effects, and ϵ_{ist} is an error term. The term year_c refers to the calendar year in which district i became certified, and the expression $I(t - \text{year}_c = j)$ is an indicator variable that equals 1 if district i is j years from a unionization election in year t and zero otherwise. For example, if district i successfully completed a union election in 1975, $I(t - \text{year}_c = 5)$ would equal one in 1980 only and would equal zero in all other periods for that district. For districts that never complete a union election and for observations for which the relative time to unionization is outside the event window, these indicator variables are set to zero.

In the regressions using COG/SOG data, I set k equal to 10, meaning the event window spans from five years prior to certification to ten years after unionization. I choose this

[24] Hoxby (1996) also includes a variable that measures the percentage of K-12 enrollment attributed to African Americans. Because this is difficult to measure in the 1970 Census and because the states in my sample have small African American populations, I exclude this variable from my analysis.

event window because sample sizes drop outside of this range. When I estimate equation (2.2) using outcome variables from the ELSEGIS survey, I set k equal to 7 due to the same sample size considerations. All district-year observations for which the time since certification is greater than k years are dropped from the analysis. Although all the qualitative results and conclusions remain unchanged, the standard errors on the relative time dummies outside the event window became noticeably larger when these variables were included in the regressions.

Due to data limitations, previous studies have been constrained to model union effects by including a dummy variable for union status in their regressions. Equation (2.2) is more general than using a single union dummy because it semi-parametrically[25] estimates both short-term and long-term effects of unionization; the inclusion of dummy variables for each relative year to unionization imposes no structure on the pattern of time trends either pre- or post-treatment. This flexibility is important because unions may have non-linear impacts on districts over time that will be masked by imposing the parametric assumption that the effects are equal.[26] Thus, the full time pattern of union impacts over the event window allowed by the data will be estimated by equation (2.2), whereas standard models of union impacts are much more restrictive.

Another major advantage of equation (2.2) is that it includes district and time fixed effects in order to take advantage of the panel data. This contrasts with most of the previous work on union impacts, which has been cross sectional (Freeman, 1986). Such a design is often necessitated by the lack of time series data on teacher unionization, but if unionization depends on unobservable factors that are correlated with both the decision to unionize and district outcomes (such as a bad administration, for example), cross-sectional estimates will be biased. In contrast to a cross-sectional model that compares outcomes

[25]The specification is semi-parametric because I impose the parametric assumption that the relative time effects and the state-specific year effects are additively separable. This is a standard assumption built into linear regression models.

[26]One might expect the time pattern of union effects to differ over time for several reasons. If unions focus first on gaining a foothold in the district rather than on affecting change, the short-run and long-run union impacts will differ. Unions may also need time to learn how to successfully bargain with administrators. Lastly, unions can change the administration in the long-run by supporting pro-union candidates for school board and local office.

across different districts, the fixed effects model compares the same district at different times relative to the unionization year and controls for any unobservable (and unchanging) effects.[27]

The central identifying assumption of the model is

$$E(\epsilon_{ist}|I(t-\text{year}_c=j) \quad \forall j\in[-5,k],\tau_i,\phi_{st}) \quad =0. \tag{2.3}$$

Satisfying (2.3) necessitates that, conditional on the fixed effects, the timing of unionization is uncorrelated with the outcome variables. If there is selection into unionization based on pre-union wages, expenditures, or revenues, estimates of the γ_j parameters from equation (2.2) will be biased. For example, if a trend of decreasing salaries causes teachers to organize into a union, the estimated union wage effect will be biased towards zero. Because close to 85 percent of the school districts that unionize do so within one year of the passage of their state's duty-to-bargain law, such selection is not likely to be a confounding factor. In addition, if school boards anticipate unionization and enact policy to attempt to defeat the organization movement in the district, it will become apparent in the pre-election relative time to unionization estimates. I therefore estimate γs prior to the union election (j<0) in order to test for any selection on the outcome variable that may be a causal factor in the decision to hold an election. Note because the Census of Governments panel begins in 1972 and the collective bargaining laws were passed in 1972, 1973, and 1975 in Minnesota, Indiana, and Iowa, respectively, the relative time dummies with j<0 will be identified predominantly off of districts that unionize relatively later in the sample.[28] I find little evidence of selection or "anticipation" effects in the the results below.

The COG/SOG and ELSEGIS surveys contain no school district demographic informa-

[27]Because the outcome measures of interest are correlated across time within districts, traditional OLS standard error estimates will be biased. To correct for this bias, all standard errors are clustered at the school district level. It is also possible that outcomes are spatially correlated. I performed a diagnostic where I clustered at the county level; the standard errors were unchanged. I also directly calculated the spatial correlation of the errors (ϵ_{ist}) and found little evidence of such correlation, especially for school districts more than 10 miles apart. Spatial correlation graphs are available from the author upon request.

[28]Because the school district panel is unbalanced with respect to relative time to unionization, each γ_j is identified off of a potentially different set of school districts. This will cause the estimates to be biased if there are unobserved (or unmodeled) heterogeneous treatment effects. To test for this source of bias, I run equation (2.2) separately for those districts that unionize within one year of their state's passage of the duty-to-bargain law. Results are qualitatively and quantitatively similar to those presented below, which is not surprising given over 84 percent of treatment observations fall into this group. These results suggest the unbalanced panel used in this analysis does not cause a bias in the estimates due to heterogeneous treatment effects over time.

tion. Given this limitation, it is important to think about why school districts unionized when they did and what determined whether they certified directly after the duty-to-bargain law change or later. I investigate this question by comparing means of observable district demographic characteristics by district unionization status and timing using the 1980 U.S. Census data described in Section 2.3.2. Columns (i) and (ii) of Table 2.3 compare districts that never unionize to districts that do unionize as of 2004. The table indicates districts that never unionize have more high school graduates, fewer high school dropouts, are less urban, have a lower private school enrollment rate, are smaller, and have a higher poverty rate but a lower median rent than districts that unionize. Columns (iv) and (v) in Table 2.3 compare districts that unionized within a year of the passage of their state's duty-to-bargain law and those that unionized later. The comparison of means suggests districts that unionized immediately following passage of their state's duty-to-bargain law had a larger percentage of adults with a bachelor's degree, were larger, more urban, had higher median rent, unemployment rate and district enrollment, but had a lower poverty rate than those that unionized later. Overall, this exercise suggests districts in larger cities and suburbs organized earlier while the more rural districts unionized later or not at all.[29]

What effect can one expect these differences to have on the estimates from equation (2.2)[30] given the parameter of interest in this study is the average treatment effect on the treated (ATT)? Note selection into unionization based on perceived or actual gains from organizing will not bias identification of the ATT; such selection will only bias identification of the average treatment effect. Because the district fixed effects control for any time-invariant differences in outcome levels between the school districts, what is needed to identify the ATT is for the state-specific year effects to accurately reflect the counterfactual

[29]There are many explanations for this trend in the literature on the history of teachers' unions. The first is administrative abuses were most severe in the larger and more urban districts, therefore inducing a union vote. Secondly, the urban districts tended to be more industrialized and have a higher fraction of the populace with union membership. These populations may have been more favorable to teachers' unions, thereby increasing the returns to unionizing. Finally, there are historical reasons the NEA and AFT were focused on the cities: the NEA started project URBAN in 1968 to specifically target city school districts as a response to AFT successes there. See Murphy (1990) for a detailed history of teacher organization.

[30]Most of the differences between the districts that never unionize, the districts that unionize early, and the districts that unionize later are due to the urban/rural distinction. When I drop all districts that have census blocks in urbanized areas, the panel becomes much more balanced with respect to the observables in Table 2.3. However, the union impact estimates do not change appreciably nor do the substantive conclusion from those estimates change when this restriction is imposed.

trends in the dependent variables for the treated observations. Correctly identifying ϕ_{st} is therefore the main difficulty in estimating the treatment effect on the treated using equation (2.2).

The year effects are identified off year-specific variation in the dependent variable from treated observations and from the control group (i.e., non-treated observations). If the year coefficients were identified solely off of the control group observations, equation (2.2) would be identical to a traditional difference-in-difference estimator. While this restriction does not hold for equation (2.2),[31] the main source of variation off of which the state-year effects are identified is the control group districts.[32]

In the results presented in Section 2.4.2, I estimate equation (2.2) using two different samples, each of which implies a different control group. The first sample I utilize is all districts that never unionize combined with all district-year observations for which the relative time to union election is less than or equal to k. The control group in this sample is comprised of never-unionized districts and those district-year observations for which the relative time to unionization is less than -5. This sample is attractive because it utilizes all observations that are arguably unaffected by the treatment, which allows for the most power in identifying all parameters of equation (2.2). Results from estimation of equation (2.2) on this sample are reported in Panel A of each figure in Section 2.4.2.

Alternatively, in Panel B of each results figure, I restrict the estimation sample to include only never-unionized districts and the district-year observations for which the relative time to certification falls within the event window. The Panel B sample therefore excludes

[31] I perform a sensitivity analysis in which I impose this restriction. For each treated observation of district i in year t, I construct a state-specific year fixed effect constituting the state-specific demeaned average of the dependent variable from never-unionized districts. I difference out this fixed effect from the dependent variable for each treated observation. I then regress this difference on a set of relative time dummies, clustering the standard errors at the school district level. Estimates and 95 percent confidence intervals are calculated by bootstrapping this process. While this methodology does increase the noise in the estimates as well as the size of the 95 percent confidence intervals, it does not change the main substantive conclusions drawn from estimating equation (2.2). The exception is for teacher pay: the difference-in-difference estimates imply a reduction in real teacher pay of close to 7.5 percent due to unionization occurring 6 years after union election certification that is not present in the estimates of equation (2.2). Full results are available from the author upon request.

[32] To test this assertion, I split the sample of those who unionize into two groups and ran equation (2.2) with a group identification dummy interacted with relative time dummies using never-unionized districts as a control group. I then dropped one of the groups and re-ran equation (2.2). The estimates of the year fixed effects changed between the two regressions, which would not be true if the year effects were identified solely off of variation from the control group districts. However, the year effects did not change appreciably, which leads me to conclude variation from the control group districts constitute the main source of identification of ϕ_{st} in equation (2.2).

all district-year observations for which the relative time to unionization is less than -5 years.[33] The control group implied by this estimation sample is comprised of only the never-unionized districts and is more clearly defined relative to the Panel A control group because the proportion of districts that do unionize and never unionize is not changing over time. Further, if there are union effects on the dependent variable more than 5 years prior to unionization, the estimates reported in Panel A will be biased, but not those reported in Panel B.

As a complete set of relative time dummy variables always sums to one for a district that unionizes in the estimation sample used in Panel B, the relative time dummies and the district fixed effects will be collinear unless I drop one of the relative year dummy variables. While this procedure is not necessary for the Panel A sample, I drop the relative time indicator variable for j=-1 (the year prior to unionization) throughout this analysis for ease of comparison. The γ_j coefficients therefore identify treatment effects relative to the effect for the year prior to unionization, γ_{-1}.[34]

To assess the fragility of my results to the choice of estimation sample, I run sensitivity analyses of equation (2.2) using additional samples that each imply a different control group. In addition to the two samples listed above, I obtain estimates using only those district-year observations for which the relative time to unionization is less than or equal to k. This sample is the same as the one used in Panel A, but it excludes never unionized districts. The implied control group is thus the district-year observations for which the relative time to certification is less than -5. I also estimate equation (2.2) using all observations. This sample adds those observations for which the relative time to unionization is greater than k to the Panel A sample, and I include a relative time to union election dummy variable that equals 1 if a district has been unionized for more than k years to

[33]For example, in the 1975 COG survey, a district that unionized in 1982 will be part of the Panel A sample but not part of the Panel B sample.

[34]These coefficients will be identical to the non-relative treatment effects if γ_{-1} is zero. To test for this possibility, I include the j=-1 relative time dummy variable in the specifications reported in Panel A of the results. In no case were the coefficients on the j=-1 dummy statistically distinguishable from zero even at the ten percent level. These results suggest it is not incorrect to interpret the relative time coefficients as treatment effects.

equation (2.2). Estimates from these robustness checks are strikingly similar in both magnitude and quality to those presented in Section 2.4.2 and are available from the author upon request.

Unfortunately, lack of sufficient pre-treatment data precludes comparing pre-treatment trends in the dependent variables among treated and untreated districts to directly test the validity of the control groups. However, the robustness of my estimates and conclusions to the use of various estimation samples suggests lack of a control group similar to the treated group on the observables does not limit my ability to identify the ATT using equation (2.2). It is also important to stress that because the unionization decision is discrete and long-run trends are more gradual, the short-run union impact estimates will identify the ATT even without an adequate control for these long-run trends.

A further challenge to identifying the ATT using equation (2.2) is the potential for spillover effects of unionization on outcomes of non-union districts. For example, if non-unionized school districts raise wages in order to keep teachers from becoming unionized or in order to attract higher quality teachers in the presence of a positive union wage differential, the union impact estimates will be biased towards zero. While there is little discussion and evidence regarding spillover effects in the teacher unionization literature, studies focusing on private–sector unions have found unionization raises non-union wages (Kahn, 1980 and Neumark and Wachter, 1995), reduces non-union wage dispersion (Kahn and Curme, 1987), and increases non-union benefits (Freeman, 1981).

To determine the degree to which spillover effects bias the estimates reported below, I run "false" experiments using the never-unionized school districts. I set the "unionization date" to be the year a district's state passed its public sector duty-to-bargain law and then calculate relative time to unionization accordingly. I estimate equation (2.2) using only these districts and variable definitions. This false experiment yields insight into the effect of a rapid diffusion of teachers' unions on the outcomes of those districts that do not unionize. The results from these tests show little evidence of spillover effects. Nonetheless, results reported below should be interpreted with care due to the possibility that the untreated districts are affected by the treatment.

2.4.2 Results

2.4.2.1 Union Impact on Resource Levels

Figures 2.2-2.6 depict the estimates of γ_j from equation (2.2) for log real monthly full-time teacher pay, log full-time teacher employment, log student-teacher ratios, log real current operating expenditures per student, and log real revenues per student, respectively. In each figure, the solid line indicates the point estimates of the γ coefficients from each relative-year-to-union-election dummy variable, and the dotted lines represent the 95 percent confidence interval calculated from the standard errors that are clustered at the school district level. Full regression estimates for the results in Panels A and B are reported in Appendix B, Tables 2.B.1 and 2.B.2 respectively.

The results consistently indicate unions have little impact on school district resource levels. Focusing on Figure 2.2, there is no evidence teachers' unions increase teacher pay in either specification;[35] none of the point estimates is statistically distinguishable from zero at the 5 percent level, and most are less than 1% in both panels. Further, there are no evident pre-election trends or anticipation effects that suggest there is selection in union election timing based on teacher pay.

These results contradict the vast majority of teachers' union impact studies that find a positive union wage premium (See Freeman (1986) for an overview). Hoxby's (1996) estimate of 5.1% is also outside the 95% confidence interval estimated here for all but the last two years of the event window. Secondly, although there is evidence in the literature that the union wage premium increased substantially over the 1970's (Freeman, 1986 and Baugh and Stone, 1982), no such increase appears in Figure 2.2.[36] Over time, as the union position became more solidified in these school districts, there is no statistically significant

[35]It is important to note these are average wages. Unions may change the wage structure within districts without shifting the mean.

[36]The explanation commonly given for this increase is in the earlier years of the teacher unionization movement, unions were focused on gaining a foothold in the district rather than on wage gains. As unions became more accepted over the course of the 1970s, they turned their attention to obtaining wage increases for their constituents.

evidence they achieved wage gains for their members.

Results for full-time teacher employment are shown in Figure 2.3 and suggest employment increases immediately following unionization by close to 5 percent. The effect increases over time and ultimately reaches 10 percent. Further, the majority of these estimates are statistically distinguishable from zero at the 5 percent level and all are significant at the 10 percent level. These results are consistent with a model of union behavior in which teachers bargain over class size, causing more teachers to be hired.[37]

The employment results in Figure 2.3 suggest class sizes, as measured by student-teacher ratios, should decrease. Figure 2.4, however, illustrates winning a unionization election has little effect on this class size measure for either specification. While all point estimates for $j>0$ are negative, none is statistically significant.

Given the significant increase in full-time teacher employment, why is there no commensurate decrease in student-teacher ratios? There are two possible explanations for this result: full-time-equivalent teacher employment does not change while full time employment does, or enrollment increases. To explore the first possibility, I estimate equation (2.2) using log full-time-equivalent teacher employment rather than log full-time teacher employment as the dependent variable.[38] The results are similar to those reported in Figure 2.4, suggesting unionization causes the same response in the two types of employment measures.

To investigate the second explanation, I run equation (2.2) using log student enrollment as the dependent variable. Results are reported in Figure 2.7. They indicate log enrollment is unaffected in the first two years following unionization but then increases to between 5 and 8 percent over the next three years and remains in this range for the remainder of the event window.[39] Taken together, these estimates suggest teacher employment increases

[37]However, Figure 2.3 could also be evidence of a principal-agent model in which the union representatives seek to maximize union dues by forcing the district to hire more teachers.

[38]In addition to instructional employees, "teachers" in the Census and Survey of Governments include educational support staff and school-level administrators, such as principals. Both full time and full time equivalent teacher employment include the same categories of staff members. The major difference between them is the proportion of each staff type that is full time or part time.

[39]The enrollment increases due to unionization are an interesting result because no other study has found such effects. This result, however, could be due to relative increases in the population of unionizing areas that coincide with, but are

immediately upon unionization, but within four years after certification, enrollment expansion in treated relative to control districts undoes much of the impact on student-teacher ratios that occurs from increased teacher employment. That teachers' unions have little long-term effect on student-teacher ratios can be attributed largely to the relatively fast enrollment increases in newly unionized districts, which raises this class size measure to near its pre-union level.

Unlike private sector unions, public sector unions can try to influence the total amount of resources available as well as their share of resources (Freeman, 1986 and Courant, Gramlich and Rubinfeld, 1979); through political lobbying and public relations, teachers' unions can increase the provision of public education.[40] Figures 2.5 and 2.6 examine this possibility by analyzing the effect of teachers' unions on log real current operating expenditures per student and log real total revenues per student, respectively.

Because current operating expenditures represent the bulk of total expenditures, it is not surprising Figures 2.5 and 2.6 are similar. In both figures, there is considerable variation in the estimates, with an upward spike of 3.8 to 4.8 percent respectively in Figures 2.5 and 2.6 for the first year of unionization, though the estimate is not statistically significant at the 5 percent level in Figure 2.5. After the first year, these estimates become negative, remain close to zero in magnitude, and are not statistically significant for the remainder of the event window. Note these graphs represent changes in per-student expenditures and revenues. As enrollment is increasing by between 5 to 8 percent over this period in unionized districts relative to control districts, total expenditures and revenues do increase, though not enough to keep up with the enrollment increases. One interpretation of Figures 2.5 and 2.6 is teachers' unions successfully guard against per-student revenue and expenditure losses in the face of rising enrollment.[41] However, an equally plausible interpretation is unions have

unrelated to, unionization. While my results are robust to dropping urban districts from the analysis, they are only suggestive that unions cause enrollment increases. Collecting certification data from other states that passed duty-to-bargain laws at different times than in my sample will allow me to test the robustness of this finding in future research.

[40]Interestingly, this is one area where the administration and teachers' union might agree. One explanation for the acquiescence of school boards to teacher unionization might be that the administration hopes to increase provision of public education through the union's political actions.

[41]Note this enrollment increase is relative to districts in the control group. Total enrollment is decreasing for both treated and untreated districts, but is declining slower in the former than in the latter.

little effect, especially in the long-run, on these outcomes.

2.4.2.2 Union Impact on the Distribution of Expenditures

The results shown in Figure 2.5 suggest teacher unionization has, at most, a small effect on the total level of current operating expenditures per student. This finding does not necessarily imply unions have no impact on resource allocation, because unions may change the composition of expenditures without affecting the level.

Figure 2.8 presents estimates from equation (2.2), where the dependent variable is the log share of total expenditures going to instruction. The results indicate unionization has little effect on this share. None of the point estimates in either panel is statistically distinguishable from zero at the 5 percent level, and they range from -2.1 to 1.9 percent. It is interesting the share of expenditures on instruction is essentially unchanged by unionization, as Figure 2.3 suggests full-time employment is increasing. If the increase in the total teacher wage bill is not fully made up by revenue increases, unionization can cause reductions in other instructional expenditures. Such a redistribution can have either positive or negative effects on student achievement, depending on the efficiency of expenditures prior to unionization.

In addition to instruction, I estimate the effect of unions on the proportion of expenditures going to administration, attendance and health, transportation, plant operating and maintenance (O&M) and fixed charges. While I find little evidence of union effects on these proportions,[42] the confidence intervals are large and make it hard to draw strong conclusions. Full regression results can be found in Appendix B, Tables 2.B.3 and 2.B.4.

2.4.2.3 Discussion

Taken together, the results presented above suggest teachers' unions have little net effect on the level and allocation of expenditures. What theories of school district and union

[42]The exception is for transportation expenditures. However, this result is likely due to the fact the laws requiring school boards to negotiate with teachers' unions applied to all public sector employees, including bus drivers. The increase in the proportion of expenditures going to transportation is in all likelihood a result of transportation employees organizing, not teachers' unions.

behavior might be consistent with my findings? One model that fits into the context of the above results is Tiebout sorting (Tiebout, 1956). Tiebout sorting could occur due to the increases in teacher employment and current operating expenditures per student directly after unionization. To the extent parents value these increased resource levels, enrollment in unionized districts should increase, which is what the data show. This relative enrollment increase is the same magnitude as the teacher employment effect and negates any student-teacher ratio and per-student expenditure and revenue gains from rising employment, expenditure and revenue levels following unionization. This explanation assumes the enrollment increase is caused by the district-level unionization decision, which is consistent with the lagged nature of the response. Conversely, unions may be reacting to expected relative enrollment increases in their district and force the administration to keep student-teacher ratios and expenditures per student roughly constant.[43]

Finally, teachers' unions may simply be ineffective at influencing resource allocation. This could occur if teachers' unions face restrictive district budget constraints; if there are few rents to extract, the unions will not be able to affect school district budgets, regardless of their underlying goals. Further, union aggressiveness in extracting rents may be limited by a fear of taxpayer backlash at the local level. It remains an open question in the literature whether teacher unionization causes tax revolts, but teachers' unions may react to this possibility by reducing the degree to which they attempt to influence educational inputs. Teachers' unions also may achieve non-salary benefits for teachers, such as health care and pensions, rather than salary increases.[44]

A final explanation for union ineffectiveness is unions may be focusing their resources on negotiating over work rules and practices rather than wages. There is much anecdotal and historical evidence unions fundamentally change workplace practices (Moe, 2001; Murphy, 1990; Retsinas, 1982; Johnson, 2004). What is poorly understood, however, is how these

[43]To test this explanation, I use the age distribution in each school district from the 1980 U.S. Census to explore whether the 0-5 age population in any given year has any power in predicting the timing of the unionization election. I find no evidence of correlation between union vote timing and forecastable population in the school district.

[44]Freeman (1986) cites evidence that public sector unions raise non-wage benefits by more than they raise wages, though the evidence is scant for teachers' unions. Freeman (1981) finds the same effect for private sector unions.

changes influence teacher productivity and the returns to teacher based inputs into education production. I next turn to an empirical analysis of the education production function in order to test for such effects.

2.5 The Effect of Teachers' Unions on the Education Production Function

2.5.1 Empirical Methodology

The education production function describes how inputs are transformed into educational outputs. In order to test whether teachers' unions change this relationship, I estimate linear education production functions that include interaction terms of union status with educational inputs. I proxy the inputs to education production with log real average teacher pay and the student-teacher ratio. My measure of educational output is the high school dropout rate. This analysis will therefore be focused on those at the lower end of the educational attainment distribution.

Because the high school dropout rate is calculated from 1970, 1980 and 1990 school district-level U.S. Census data (see Section 2.3.2), I cannot employ the event study methodology given by equation (2.2) due to the fact there are relative years to union election with no observations. Thus, I will be unable to detect subtleties in the time pattern of union impacts on the education production function. Instead, I will estimate the average effect of unions on high school dropout rates and on the returns to education inputs over all relative time periods included in the analysis. I estimate linear education production functions of the form:

$$\begin{aligned}\text{Dropout Rate}_{ist} \;=\;& \beta_0 + \beta_1 \text{Union}_{ist} + \beta_2 \frac{\text{Student}}{\text{Teacher}}_{ist} + \beta_3 \frac{\text{Student}}{\text{Teacher}} * \text{Union}_{ist} \\ & + \beta_4 (\text{Log Teacher Pay})_{ist} + \beta_5 (\text{Log Teacher Pay}) * \text{Union}_{ist} \\ & + \delta X_{ist} + \tau_i + \phi_{st} + \epsilon_{ist}, \end{aligned} \tag{2.4}$$

where Union is a dummy variable equal to 1 if a district has successfully completed a teacher union election, X is a vector of demographic characteristics listed in Section 2.3.2, and all other variables are as previously defined.

I use two different samples to identify the parameters in equation (2.4). The first sample includes all observation from 1970 and 1980, excluding the 1990 observations. I exclude 1990 because I want to identify the state-specific year effects only off of school districts that are not unionized. The results from this specification are presented in Columns (i) and (ii) of Table 2.4. I also estimate equation (2.4) using all three years of data. The state-specific year effects therefore include variation from unionized districts in this specification, but the sample size increases substantially.

I estimate equation (2.4) both with and without the union interaction terms. In the specifications that leave out these interaction terms, β_1 gives the average effect of teachers' unions on high school dropout rates. In the specifications that include the union interaction terms, estimates of β_3 and β_5 test whether teachers' unions change the returns to expenditures on student-teacher ratios and teachers' salaries, respectively. A positive β_3 will imply unionized districts have a higher return (as measured by dropout rates) to lowering the student-teacher ratio. Similarly, $\beta_5 < 0$ means unionized districts have a higher return to increasing teacher pay than non-unionized districts. In other words, β_3 and β_5 measure the degree to which unionized districts differ in their ability to transform class sizes and teacher pay into reduced dropout rates.

2.5.2 Results

Results from estimation of equation (2.4) are presented in Table 2.4. The coefficient on the Union variable in columns (i) and (iii) gives the net effect of teachers' unions on high school dropout rates (in percent). I estimate unions decrease high school dropout rates by 0.140 percent using the 1970 and 1980 data and raise dropout rates by 0.202 percent using observations from all three years. Neither of these coefficients is significant at even the 10 percent level. These results are suggestive that, on average, teachers' unions do not influence high school dropout rates.

The Union interaction terms reported in columns (ii) and (iv) of Table 2.4 allow me to test for the effect of teachers' unions on the education production function. In both columns, the Union, Student-teacher Ratio interaction term is positive and statistically significant at the 5 percent level. Again, the magnitudes are small: in the specification using only 1970 and 1980 observations, a 1 unit decrease in the student-teacher ratio reduces the percent of high school dropouts by 0.44 in unionized districts relative to non-unionized districts. These estimates provide evidence lowering the student-teacher ratio is more effective at reducing dropout rates in unionized than non-unionized school districts. The coefficient on the interaction between Log Teacher Pay and Union is negative and of sizeable magnitude, though it is only significant at the 5 percent level in column (ii). The estimates suggest increasing teacher pay has higher returns to reducing dropout rates in unionized as opposed to non-unionized districts.

It is important to note the results from Table 2.4 measure the net change in the productivity of teacher-based inputs into education production. As discussed in Section 2.2, unions likely change many of the aspects of the teacher-administrator relationship, each of which has a different implication for teacher productivity. For example, by making it more difficult to fire teachers and by linking pay to experience and education level instead of to output, unions can reduce teacher productivity. However, unions can be productivity-enhancing by protecting teachers from bad administrative practices and giving them a voice with which to influence their workplace. Table 2.4 is suggestive that the positive productivity effects of unionization outweigh the negative effects, on average. This finding is consistent with studies such as Eberts and Stone (1987), who find unions increase education productivity. However, this productivity increase is achieved without the coincident increase in teacher pay that is typically associated with teacher unionization.

My education production function estimates contrast with those of Hoxby (1996), who utilizes the Census of Governments labor relations data to measure teacher unionization. In explaining dropout rates, Hoxby finds a negative coefficient on the Student-teacher Ratio, Union interaction term and a positive coefficient on the Log Teacher Pay, Union interaction term. She takes these results as evidence of teachers' union rent seeking behavior. The central difference between the two sets of estimates is due to the measurement error in the Census of Governments measure discussed in Section 2.3.1.2. In the proceeding section, I conduct a formal comparison of my results and those reported in Hoxby (1996) in order to understand the effect of measurement error on union impact estimates.

2.6 The Effect of Different Union Measures on Union Impact Estimates

In order to understand more fully the differences between my results from Sections 2.4 and 2.5 and those from the existing literature, it is instructive to undertake a comparison of union impact estimates using my new certification data and the previously used Census of Governments constructed union measure. Specifically, I replicate estimates from Hoxby (1996) using both union measures. I focus on this paper because it is the most comprehensive and empirically sophisticated study of teachers' unions in the literature and because our studies use similar data and time periods.

The empirical specifications in Hoxby (1996) are of the form:

$$Y_{it} = \beta_0 + \beta_1 U_{it} + \delta X_{it} + \tau_i + \phi_t + \psi_i * t + \epsilon_{it}, \tag{2.5}$$

where Y_{it} is an outcome variable of interest, U_{it} is an indicator variable equal to 1 if district i is unionized at time t, X_{it} is a vector of demographic characteristics listed in Section 2.3.2, τ_i are district fixed effects, ϕ_t are year fixed effects, $\psi_i * t$ are district-specific linear time trends, and ϵ_{it} is a normally distributed error term.

Tables 2.5-2.7 present the results of the regressions when Y_{it} is, alternatively, log real teacher pay, log real current operating expenditures per student, and student-teacher ratios, as these are the dependent variables analyzed by Hoxby. Each regression contains three years of data from the 1970, 1980 and 1990 U.S. Census school district files. Column (i) in Tables 2.5-2.7 presents the results taken directly from Hoxby (1996). Column (ii) contains estimates using Hoxby's methodology on my sample of three states and allows me to determine how much of the difference in our estimates is due to the fact I use only three states and she uses all districts in the U.S.. Column (iii) is identical to column (ii), except unionization is measured using the election certification data. This last column thus will yield insight into the effect of the measurement error on union impact estimates.[45]

Hoxby identifies union effects by using only within-state variation through the use of district fixed effects and by an IV strategy that uses only cross-state variation over time. The instruments she uses are passage of state-level public sector bargaining laws. As I only have data for three states, I am restricted to a within-state analysis. While this restriction is justified by my more accurate union data, I will compare my results only to the within-state estimates from Hoxby (1996) for consistency. However, her IV and fixed effects estimates are both qualitatively and quantitatively similar.

Changing the unionization measure has a large impact on estimates of union effects in all three tables. In Table 2.5, the union impact estimates on log real teacher pay using the COG union measure are similar in magnitude, sign and statistical significance for the national sample and the Midwest sample. However, when I employ the election certification definition of unionization, the coefficient on the union variable becomes negative, smaller in magnitude, and not statistically significant at even the 10 percent level. Note the standard error on the union coefficient increases by a factor of 3 between Columns (ii) and (iii) in Table 2.5. This increase occurs because there is variation in the Census of Governments measure that is due to measurement error and is correlated with the dependent variable (see Appendix C). Eliminating this variation increases the standard error estimate substantially. While the union estimate in Column (iii) does not allow one to rule out the verity of the union estimate in Column (i), it illustrates the fragility of the estimate to correcting for measurement error.

A similar pattern emerges in Table 2.6, which presents results for current operating expenditures per student. Switching from the national to the midwest sample reduces the magnitude of the union coefficient, but the signs are the same across columns (i) and

[45]Typically, one would run "horse race" regressions to compare the two measures, but since the measurement error is correlated with the regression errors (see Appendix C), such a methodology is not appropriate.

(ii). However, in column (iii), the union impact estimate becomes negative when I use the election certification measure, and the standard error increases by a factor of 3.8. Table 2.7 is more problematic because there is a marked difference between the estimates in the first two columns; the union impact on student-teacher ratios in the 3 midwestern states is of a different sign than for the nation as a whole. However, the difference in union coefficients and the increase in the size of the standard error of these coefficients between columns (ii) and (iii) in Table 2.7 is consistent with the sensitivity of the results reported in Hoxby (1996) to measurement error.[46]

What is most interesting about the form of the measurement error bias is it is not attenuating, which is the form of bias one would expect from classical measurement error. Classical measurement error occurs when the error is uncorrelated with the dependent variable, the independent variables, the regression error, and the true value of the variable. Despite the fact the measurement error must be correlated with the true measure of union status as union status is a binary variable, Bound, Brown and Mathiowetz (2001) show as long as the misclassification is what Carroll, Ruppert and Stefanski (1995) term "non-differential," the bias in the coefficient will still be attenuating as long as the rest of the classical measurement error assumptions hold.[47]

Appendix C contains a detailed discussion of measurement error issues and an analysis of the properties of this measurement error, treating the election certification data as the true measure of union status for each school district. I find the measurement error in the Census of Governments is differential; the classification error is correlated with the dependent variable in all regressions. Thus, the misclassification bias is not guaranteed to attenuate the coefficient estimates. I also find the classification error is correlated with the

[46]Because the Census of Governments union construct measures whether a district has a contract with a teachers' union and the election certification data measure whether a teachers' union exists for the purpose of collective bargaining, one could argue the differences between the estimates in columns (ii) and (iii) of Tables 2.5-2.7 are due to the difference between having a union and having a negotiated contract. As previously discussed, my validation study suggests most districts achieve a contract within one year of certification, and no district fails to achieve a contract conditional on certifying a union. While this difference may cause some attenuation in the results, it cannot account for the sign change in coefficient estimates and is likely to be small.

[47]Non-differential classification error occurs when, conditional on the true classification, reporting errors are independent of the dependent variable.

demographic characteristics included in equation (2.5). Finally, I perform Bound, Brown, Duncan and Rodgers (1994) decompositions of the measurement error. The BBDR decompositions decompose the measurement error into the part that is due to misclassification of union status and the part that is due to the correlation of this misclassification with the regression error. My results indicate both forms of bias are present and reinforce each other for teacher pay, current operating expenditures, and student-teacher ratios but work in opposite directions for high school dropout rates.[48]

The central conclusion from Tables 2.5-2.7 is the classification error reported in Tables 2.1 and 2.2 in the COG union measure is not innocuous. My results using the Midwest sample are similar to those in Hoxby (1996) for two of the three comparisons, but switching the union measure illustrates those results are not robust to correcting for measurement error. These comparisons underscore the importance of accurately measuring union status in an analysis of teachers' union impacts.

2.7 Conclusion

Using new hand-collected data on the timing of teachers' union election certifications in Iowa, Indiana and Minnesota combined with school district-level data from the Census/Survey of Governments and the Elementary and Secondary General Information System (ELSEGIS), I investigate the impact of teachers' unions on the level and allocation of school district educational resources. Contrary to many past studies on teachers' unions (Hoxby, 1996; Freeman, 1986; Moore and Raisian, 1987; Baugh and Stone, 1982), I find unions have no effect on teacher pay. I also present evidence teacher unionization causes an increase in full–time teacher employment of between 5 and 10 percent, a negligible decrease in student-teacher ratios, and has a short-run positive effect on current operating expenditures per student and total revenue per student, but this positive effect disappears

[48] While I am able to decompose the various sources of the misclassification bias that occurs from the Census of Governments union measure, I am unable to determine why the measurement error is occurring. There are no clear trends in the classification error to suggest certain types of districts are systematically misunderstanding survey questions or certain schools are filling out the forms incorrectly in a systematic manner. The reasons why the measurement error in the COG data documented here takes the form that it does remain an open question.

after the first year following certification. Further, one cannot reject the null hypothesis that teachers' unions have no influence on the allocation of expenditures with my results.

I also estimate the impact of unions on high school dropout rates and on the education production function using 1970-1990 U.S. Census school district summary data. I cannot reject the null hypothesis teachers' unions have no effect on high school dropout rates: the point estimates are small in both specifications, and in neither case are the coefficients statistically differentiable from zero. I find evidence consistent with unions increasing teacher productivity in the form of higher returns to lower class sizes and higher teacher pay. A topic for further research will be to determine whether such effects exist for other student achievement measures, especially those that include more students from higher portions of the ability distribution.

My findings contrast markedly with those of the literature, most notably Hoxby (1996). I argue the basic reason for the differences between my analysis and Hoxby's is the accuracy of the union certification data I use relative to the union measure constructed from the Census of Governments Labor Relations survey she uses. These differences highlight the importance of correctly measuring unionization status in union impact studies.

The results and conclusions of this analysis raise a puzzle: why do teachers bother to organize, especially at the high rates observed in the data, given the lack of wage and class size effects? One possible answer to this puzzle is teachers perceive organization increases their pay. Indeed, when talking to union members during this study, wage increases were the most commonly mentioned benefit of unionization, in contrast to what this analysis shows. Another important reason for unionizing is to give teachers a voice with which to improve their working conditions as well as to establish well defined rules governing hiring and firing, pay structure and promotion. There is anecdotal evidence teachers' unions provide these benefits (Woodbury, 1985), and the production function estimates reported in Section 2.5 are consistent with such effects. Finally, unionization may increase non-wage benefits such as pensions or health care that are valued by teachers. Freeman (1981) finds private sector unions increase non-wage benefits more than they increase wages, and Freeman (1986) reports many previous studies on public sector unions in general have found similar effects. It is a topic for further study whether teachers' unions in particular have such an impact on these benefits.

One must be careful in drawing too general a conclusion from the results presented above, as this study includes only three states concentrated in the Midwest. Further, because these states all passed duty-to-bargain laws in the early to mid-1970s, I am not able to disentangle confounding relative trends in unionized districts from actual treatment effects. It is possible such spurious trends are responsible for producing the enrollment increases and the lack of wage, expenditure, and class size effects reported in Section 2.4.2. Rather than interpreting my results as representative of union impacts for the United States as a whole, one can view this study as provocative in suggesting the commonly accepted effects of teachers' unions – raising wages and reducing teacher productivity – may not be robust to the use of more accurate union data. Collecting union election certification data from other states will allow a more general and robust analysis of teachers' unions than I am able to produce in this paper. The main implication of this study is more research using such data is necessary to understand more fully the nature and impact of collective bargaining in public education and to inform meaningful labor relations policy.

Table 2.1: A Comparison of Union Status from the Census of Governments and the Union Election Certifications by State and Year

	Census of Governments Union Measure					
	1972					
Election Certification	**Iowa**		**Indiana**		**Minnesota**	
Union Measure	Union	Non-Union	Union	Non-Union	Union	Non-Union
Union	0.00%	0.67%	3.63%	1.98%	53.58%	17.78%
Non-Union	5.99%	93.35%	10.23%	84.16%	19.40%	9.24%
	1977					
Election Certification	**Iowa**		**Indiana**		**Minnesota**	
Union Measure	Union	Non-Union	Union	Non-Union	Union	Non-Union
Union	49.89%	14.19%	57.43%	22.77%	55.89%	20.79%
Non-Union	9.31%	26.61%	11.55%	8.25%	16.17%	7.16%
	1982					
Election Certification	**Iowa**		**Indiana**		**Minnesota**	
Union Measure	Union	Non-Union	Union	Non-Union	Union	Non-Union
Union	51.22%	17.96%	55.12%	26.73%	58.20%	22.17%
Non-Union	8.43%	22.39%	8.58%	9.57%	13.16%	6.47%
	1987					
Election Certification	**Iowa**		**Indiana**		**Minnesota**	
Union Measure	Union	Non-Union	Union	Non-Union	Union	Non-Union
Union	46.78%	25.28%	42.24%	40.59%	64.67%	16.86%
Non-Union	9.76%	18.18%	6.60%	10.56%	13.63%	4.85%

Source: Authors' calculations from the 1972, 1977, 1982, and 1987 Census of Governments and the teachers' union election certification data described in the text.

Table 2.2: Misclassification Rates in the Census of Governments by State and Year, Treating the Election Certifications as the True Measure of Unionization Status

Year	Iowa	Indiana	Minnesota	Average
1972	6.65%	12.21%	37.18%	19.21%
1977	23.50%	34.32%	36.95%	31.17%
1982	26.39%	35.33%	35.33%	31.93%
1987	35.03%	47.19%	30.48%	36.48%

[1] Source: Authors' calculations from the 1972, 1977, 1982, and 1987 Census of Governments and the teachers' union election certification data described in the text.

[2] The misclassification rate is the sum of the total number of times the Census of Governments and the election certification union measures disagree for each state and year. Each state-level misclassification rate is calculated by taking the sum of the off-diagonal entries from the appropriate four-cell square in Table 2.1. The average misclassification rate is a weighted average of the state-level misclassification rates, where the weight is the number of school districts in each state.

Table 2.3: Comparison of School District-Level Means of Demographic Characteristics From the 1980 School District Census Data for Never Unionized vs. Ever Unionized Districts and for Districts that Unionize Within a Year of Their State's Passage of a "Duty-to-Bargain Law" vs. Districts that Unionize Later

Demographic Variable	(i) Never Unionized	(ii) Ever Unionized	(iii) Difference (i)-(ii)	(iv) Unionized At Law	(v) Unionized After Law	(vi) Difference (iv)-(v)
Percent Black	0.15	0.55	-0.40	0.60	0.35	0.25
	(1.00)	(3.12)	(0.27)	(3.40)	(1.49)	(0.25)
Percent Hispanic	0.10	0.16	-0.05	0.17	0.12	0.04
	(0.32)	(0.78)	(0.07)	(0.85)	(0.39)	(0.06)
Percent Some High School	14.83	16.15	-1.32**	16.22	15.89	0.33
	(3.57)	(4.98)	(0.44)	(5.01)	(4.86)	(0.40)
Percent High School Graduate	56.34	54.45	1.89**	54.27	55.19	-0.91*
	(6.07)	(6.24)	(0.57)	(5.01)	(4.88)	(0.50)
Percent Some College	16.95	16.52	0.44	16.45	16.78	-0.33
	(3.87)	(4.16)	(0.38)	(4.22)	(3.90)	(0.33)
Percent BA	11.88	12.88	-1.00*	13.06	12.14	0.91**
	(4.88)	(5.66)	(0.51)	(5.93)	(4.26)	(0.45)
Percent Urban	4.44	9.70	-5.27**	10.73	5.40	5.33**
	(19.10)	(27.53)	(2.43)	(22.82)	(20.87)	(2.20)
Percent Private Enrollment	5.86	7.66	-1.76**	7.66	7.49	0.17
	(6.74)	(7.83)	(0.70)	(7.39)	(9.49)	(0.63)
Log Average Income	9.59	9.59	0.01	9.59	9.57	0.02
	(0.17)	(0.17)	(0.02)	(0.18)	(0.17)	(0.01)
Log Median Rent	6.08	6.12	-0.04**	6.13	6.08	0.06**
	(0.19)	(0.19)	(0.02)	(0.19)	(0.18)	(0.02)
Percent Unemployed	2.63	2.86	-0.23*	2.90	2.66	0.24**
	(1.36)	(1.39)	(0.13)	(1.38)	(1.40)	(0.11)
Percent Below Poverty	5.28	4.81	0.48*	4.70	5.25	-0.54**
	(3.26)	(2.86)	(0.27)	(2.81)	(3.03)	(0.23)
Public School Enrollment/100	10.73	22.31	-11.58**	23.99	15.28	8.72**
	(32.63)	(42.95)	(3.81)	(45.07)	(31.74)	(3.42)
N	137	1006	.	812	194	.

Columns (i) and (ii) present means for all districts by whether a district unionized and Columns (iv) and (v) present means for districts that unionize by whether a district unionized within the same year as passage of a state "duty-to-bargain" law or after, respectively. All demographic characteristics are calculated from the 1980 Census as described in the text. Standard deviations are in parentheses in Columns (i), (ii), (iv), and (v). The difference between the two preceding columns are presented in Columns (iii) and (vi), and the standard error of this difference is in parentheses in these columns: ** indicates significance at the 5 percent level and * indicates significance at the 10 percent level.

Table 2.4: Education Production Function Estimates

Independent Variable	Dependent Variable: High School Dropout Rate in Percent			
	1970 and 1980		1970, 1980, and 1990	
	(i)	(ii)	(iii)	(iv)
Union	-0.140 (0.576)	48.613** (20.924)	0.202 (0.656)	26.262 (18.593)
$\frac{\text{Student}}{\text{Teacher}}$ Ratio	0.037** (0.016)	0.015 (0.015)	0.049** (0.022)	0.029 (0.021)
$\frac{\text{Student}}{\text{Teacher}}$*Union	. .	0.438** (0.116)	. .	0.227** (0.092)
Log Real Full Time Teacher Pay	1.604 (1.567)	4.026** (1.889)	-2.752** (1.392)	-1.125 (1.711)
(Log Real Full Time Teacher Pay)*Union	. .	-6.793** (2.584)	. .	-3.612 (2.317)
Log Population	27.790** (2.942)	27.913** (2.857)	31.475** (2.411)	31.009** (2.386)
Percent Urban	0.641 (0.487)	0.486 (0.487)	0.511 (0.590)	0.465 (0.588)
Log Average Income	-5.789* (2.978)	-5.259* (3.019)	-21.965** (3.179)	-21.005** (3.238)
Log Median Gross Rent	0.193 (1.130)	0.025 (1.271)	0.277 (1.516)	0.219 (1.498)
Percent Below Poverty	-0.082 (0.081)	-0.048 (0.088)	-0.303** (0.120)	-0.282** (0.123)
Percent Unemployed	0.365** (0.162)	0.371** (0.160)	0.022 (0.085)	0.041 (0.084)
Percent Black	-0.256** (0.090)	-0.204** (0.089)	0.204 (0.222)	0.208 (0.216)
Percent Hispanic	0.094 (0.393)	0.239 (0.377)	0.069 (0.179)	0.072 (0.167)
Percent 12–15 Years School	0.209** (0.052)	0.180** (0.052)	0.168** (0.061)	0.154** (0.062)
Percent 16+ Years School	0.033 (0.074)	0.019 (0.074)	0.450** (0.054)	0.441** (0.054)
Percent Private Enrollment	-0.258** (0.066)	-0.254** (0.063)	-0.369** (0.057)	-0.360** (0.058)
Log Public School Enrollment	-24.915** (2.530)	-26.239** (3.545)	-29.904** (1.993)	-29.919** (1.987)
Constant	-62.921** (21.159)	-74.439** (21.267)	13.733 (20.499)	4.550 (22.077)

[1] Source: Author's calculation as described in the text from the 1970, 1980, and 1990 U.S. Census School District files. Columns (i) and (ii) use only observations from 1970 and 1980, while columns (iii) and (iv) include observations from all three years.

[2] All models include year and school district fixed effects. Robust standard errors are in parentheses: ** indicates significance at the 5 percent level and * indicates significance at the 10 percent level.

Table 2.5: Comparison of the Effect of Different Union Measures and Estimation Samples on Estimates of the Union Impact on Teacher Pay

	Dependent Variable: Ln(Real Monthly Average Teacher Pay)		
	COG/Hoxby (1996)		Election Certification
	(i) U.S. Estimation Sample	(ii) IA,IN,MN Estimation Sample	(iii) IA,IN,MN Estimation Sample
Independent Variable			
Union	0.051** (0.008)	0.054** (0.024)	-0.019 (0.072)
Log Population	-0.015** (0.004)	0.029 (0.063)	0.029 (0.057)
Percent Urban	0.0005** (0.0002)	0.0004 (0.0007)	0.0003 (0.0007)
Log Average Income	0.199** (0.022)	-0.066 (0.186)	-0.049 (0.187)
Log Median Gross Rent	-0.021** (0.010)	0.064 (0.103)	0.078 (0.107)
Percent Below Poverty	-0.0001 (0.0006)	-0.009 (0.006)	-0.009 (0.007)
Percent Unemployed	-0.003** (0.001)	-0.009* (0.005)	-0.009* (0.005)
Percent Black	-0.004** (0.001)	0.0001 (0.010)	0.002 (0.009)
Percent Hispanic	-0.004** (0.001)	0.002 (0.010)	0.001 (0.011)
Percent 12–15 Years School	-0.002** (0.0003)	-0.002 (0.004)	-0.004 (0.004)
Percent 16+ Years School	0.004** (0.0004)	-0.005 (0.005)	-0.007 (0.005)
Percent Private Enrollment	0.001** (0.0002)	0.003 (0.004)	0.004 (0.004)
Log Public School Enrollment	0.041** (0.002)	-0.050 (0.057)	-0.053 (0.057)
R^2	NR	0.9366	0.9337

[1] Source: Estimates in column (i) come from Hoxby (1996) Table (IV) Column 6. Column (ii) contains estimates using the COG-based union measure on the IA, IN, and MN sample. Column (iii) presents estimates using the election certification union data on the IA, IN and MN sample.

[2] Hoxby (1996) utilizes median household income, whereas I utilize mean household income because median household income is not included in the 1970 Census school district summary files.

[3] All regressions include district and year fixed effects as well as district-specific linear time trends. Standard errors are clustered at the district level: ** indicates significance at the 5 percent level and * indicates significance at the 10 percent level.

Table 2.6: Comparison of the Effect of Different Union Measures and Estimation Samples on Estimates of the Union Impact on Current Operating Expenditures

	Dependent Variable: Ln(Real Current Operating Expenditure Per Student)		
	COG/Hoxby (1996)		Election Certification
	(i) U.S. Estimation Sample	(ii) IA,IN,MN Estimation Sample	(iii) IA,IN,MN Estimation Sample
Independent Variable			
Union	0.029** (0.007)	0.017 (0.017)	-0.010 (0.064)
Log Population	0.029** (0.004)	0.013 (0.068)	0.013 (0.068)
Percent Urban	-0.001** (0.0001)	-0.0004 (0.0007)	-0.0004 (0.0007)
Log Average Income	0.116** (0.019)	0.146 (0.149)	0.151 (0.150)
Log Median Gross Rent	0.232** (0.008)	-0.032 (0.105)	-0.027 (0.106)
Percent Below Poverty	-0.007 (0.001)	-0.009* (0.005)	-0.009* (0.005)
Percent Unemployed	-0.005** (0.001)	-0.006 (0.004)	-0.006 (0.005)
Percent Black	0.005** (0.001)	-0.005 (0.006)	-0.004 (0.005)
Percent Hispanic	0.003** (0.001)	-0.005 (0.008)	-0.005* (0.009)
Percent 12–15 Years School	0.005** (0.001)	-0.0001 (0.003)	-0.001 (0.003)
Percent 16+ Years School	0.004** (0.001)	-0.007 (0.005)	-0.008* (0.005)
Percent Private Enrollment	0.003** (0.001)	-0.001 (0.003)	-0.001 (0.003)
Log Public School Enrollment	-0.409** (0.011)	-0.024 (0.031)	-0.025 (0.030)
R^2	NR	0.9997	0.9997

[1] Source: Estimates in column (i) come from Hoxby (1996) Table (III) Column 6. Column (ii) contains estimates using the COG-based union measure on the IA, IN, and MN sample. Column (iii) presents estimates using the election certification union data on the IA, IN and MN sample.

[2] Hoxby (1996) utilizes median household income, whereas I utilize mean household income because median household income is not included in the 1970 Census school district summary files.

[3] All regressions include district and year fixed effects as well as district-specific linear time trends. Standard errors are clustered at the district level: ** indicates significance at the 5 percent level and * indicates significance at the 10 percent level.

Table 2.7: Comparison of the Effect of Different Union Measures and Estimation Samples on Estimates of the Union Impact on Student-Teacher Ratios

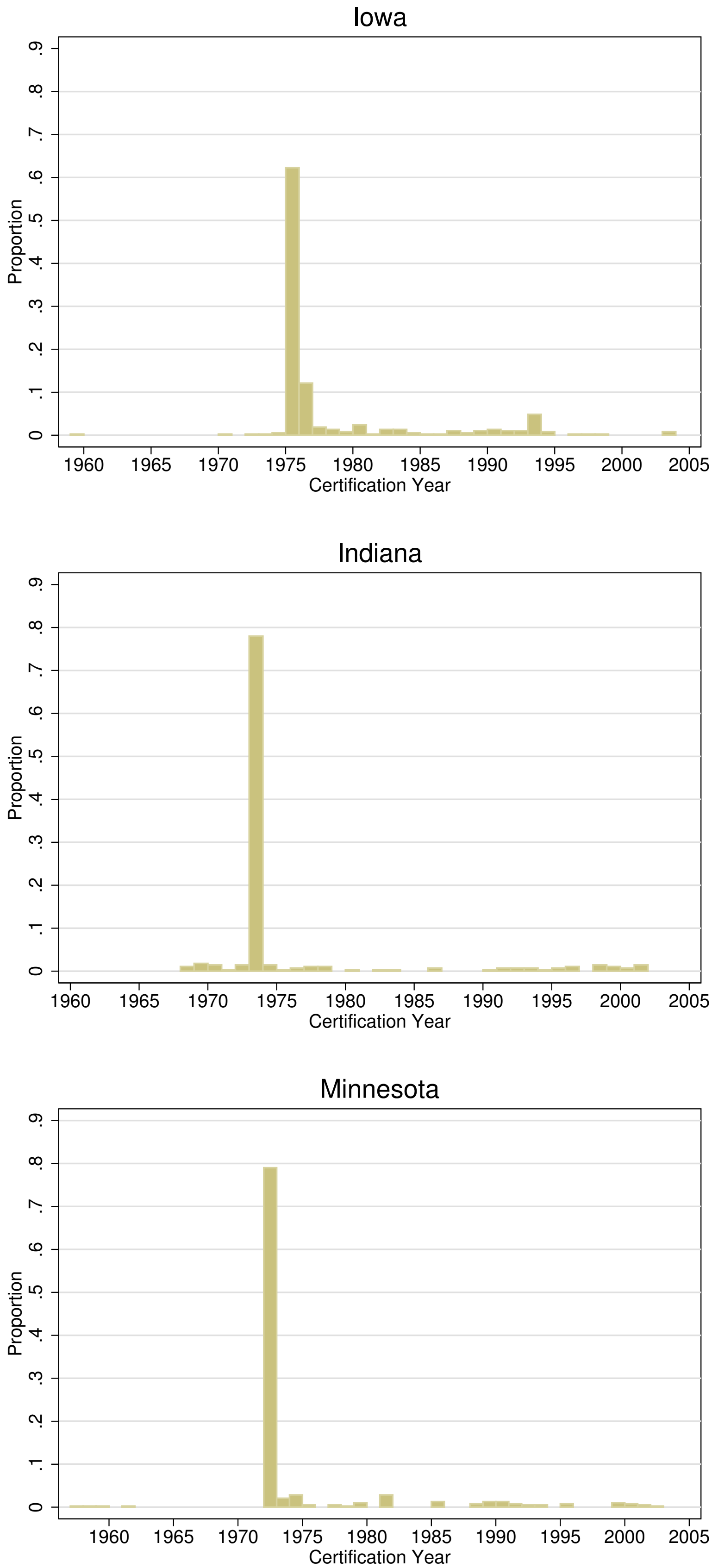
	Dependent Variable: Student-Teacher Ratio		
	COG/Hoxby (1996)		Election Certification
	(i) U.S. Estimation Sample	(ii) IA,IN,MN Estimation Sample	(iii) IA,IN,MN Estimation Sample
Independent Variable			
Union	-1.112**	0.117	-0.189
	(0.338)	(0.547)	(0.836)
Log Population	-0.841**	1.154	1.158
	(0.071)	(2.618)	(2.604)
Percent Urban	0.029**	0.020	0.020
	(0.003)	(0.015)	(0.014)
Log Average Income	-1.170**	-4.698	-4.660
	(0.367)	(4.100)	(4.150)
Log Median Gross Rent	-1.167**	0.024	0.056
	(0.161)	(2.519)	(2.483)
Percent Below Poverty	0.149	-0.038	-0.039
	(0.012)	(0.136)	(0.137)
Percent Unemployed	0.123**	-0.101	-0.102
	(0.015)	(0.103)	(0.103)
Percent Black	-0.143**	0.285	0.286
	(0.012)	(0.478)	(0.455)
Percent Hispanic	-0.065**	-0.162	-0.161
	(0.014)	(0.233)	(0.225)
Percent 12–15 Years School	-0.129**	0.071	0.067
	(0.011)	(0.098)	(0.105)
Percent 16+ Years School	-0.082**	0.165	0.162
	(0.015)	(0.154)	(0.161)
Percent Private Enrollment	-0.098**	-0.250	-0.249
	(0.009)	(0.344)	(0.345)
Log Public School Enrollment	7.334**	-2.990	-2.997
	(0.217)	(4.769)	(4.745)
R^2	NR	0.9612	0.9612

[1] Source: Estimates in column (i) come from Hoxby (1996) Table (V) Column 6. Column (ii) contains estimates using the COG-based union measure on the IA, IN, and MN sample. Column (iii) presents estimates using the election certification union data on the IA, IN and MN sample.

[2] Hoxby (1996) utilizes median household income, whereas I utilize mean household income because median household income is not included in the 1970 Census school district summary files.

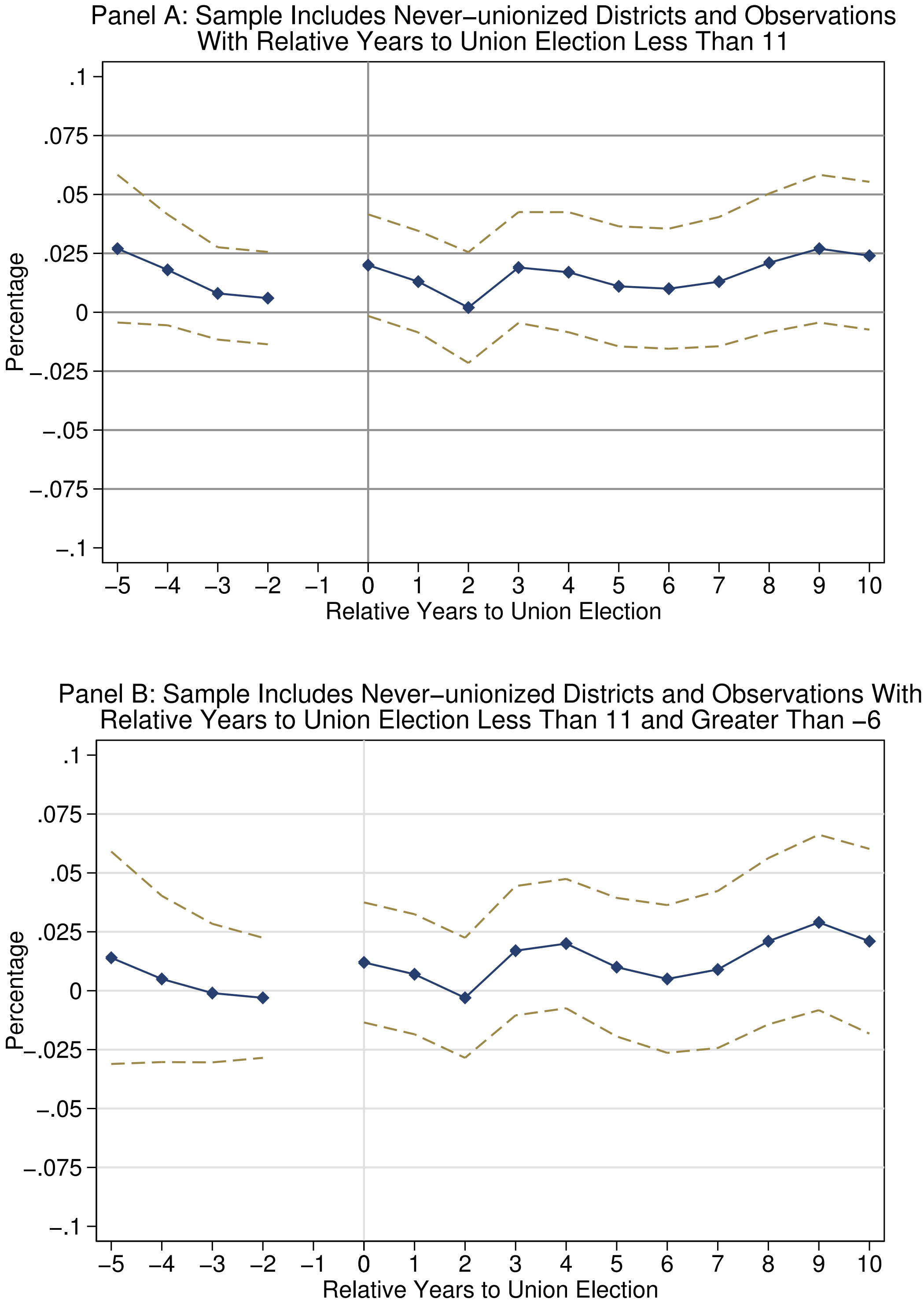
[3] All regressions include district and year fixed effects as well as district-specific linear time trends. Standard errors are clustered at the district level: ** indicates significance at the 5 percent level and * indicates significance at the 10 percent level.

Figure 2.1: Distribution of Teachers' Union Election Certifications by State



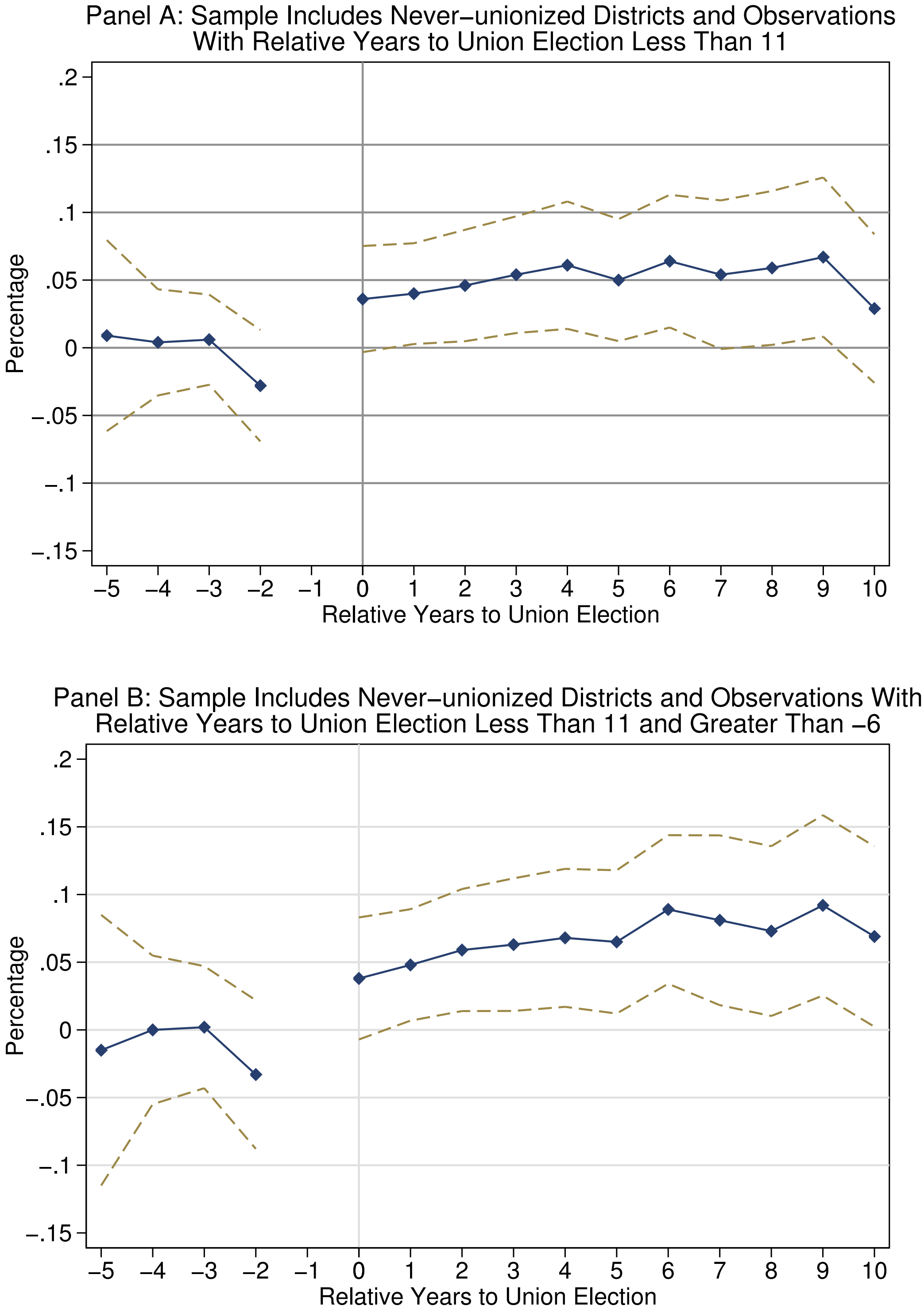
Source: Teachers' union election certification data described in the text.

Figure 2.2: The Effect of Teachers' Unions on Log Real Monthly Full Time Teacher Pay



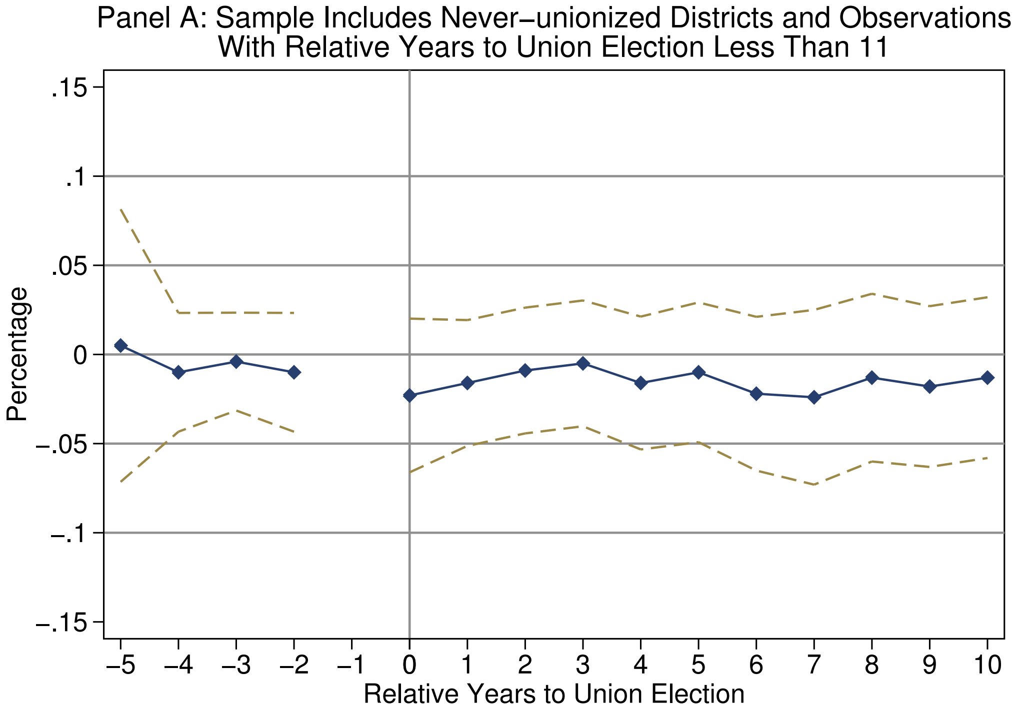
[1] Source: Author's calculations from the 1972–1991 Census/Survey of Governments as described in the text.
[2] Solid lines represent coefficient estimates from estimation of equation (2.2) in the text. Dotted lines are the bounds of the 95 percent confidence interval calculated from standard errors that are clustered at the district level.
[3] Relative year -1 is omitted in order to identify the model: a complete set of relative time dummy variables is collinear with the district fixed effects in equation (2.2).

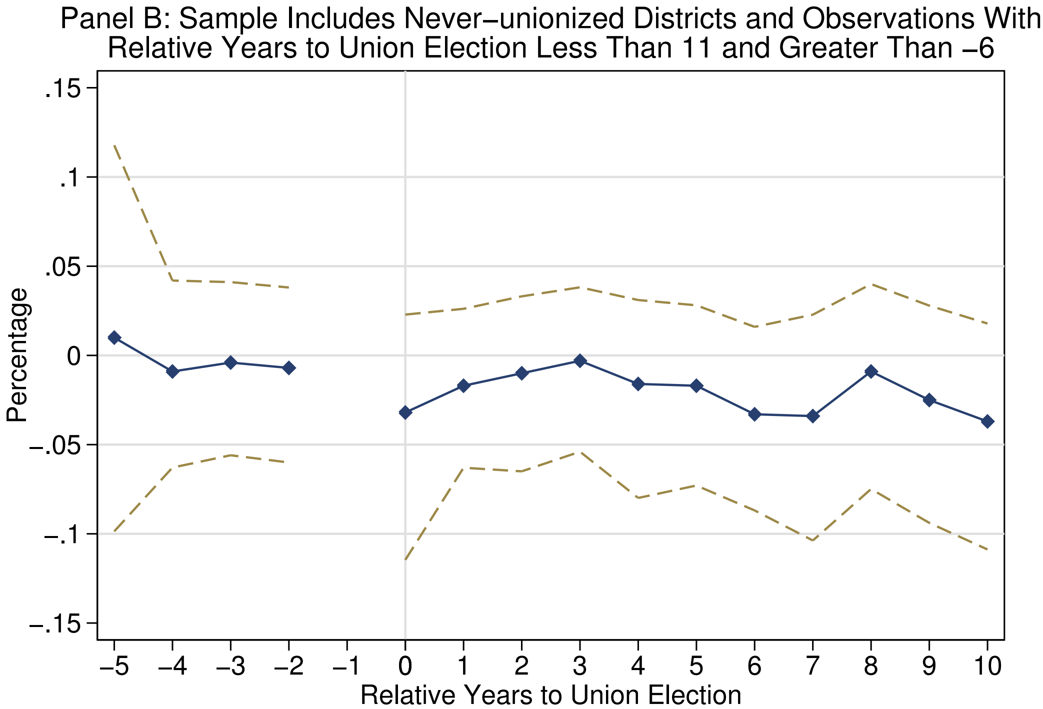
Figure 2.3: The Effect of Teachers' Unions on Log Full Time Teacher Employment



[1] Source: Author's calculations from the 1972–1991 Census/Survey of Governments as described in the text.
[2] Solid lines represent coefficient estimates from estimation of equation (2.2) in the text. Dotted lines are the bounds of the 95 percent confidence interval calculated from standard errors that are clustered at the district level.
[3] Relative year -1 is omitted in order to identify the model: a complete set of relative time dummy variables is collinear with the district fixed effects in equation (2.2).

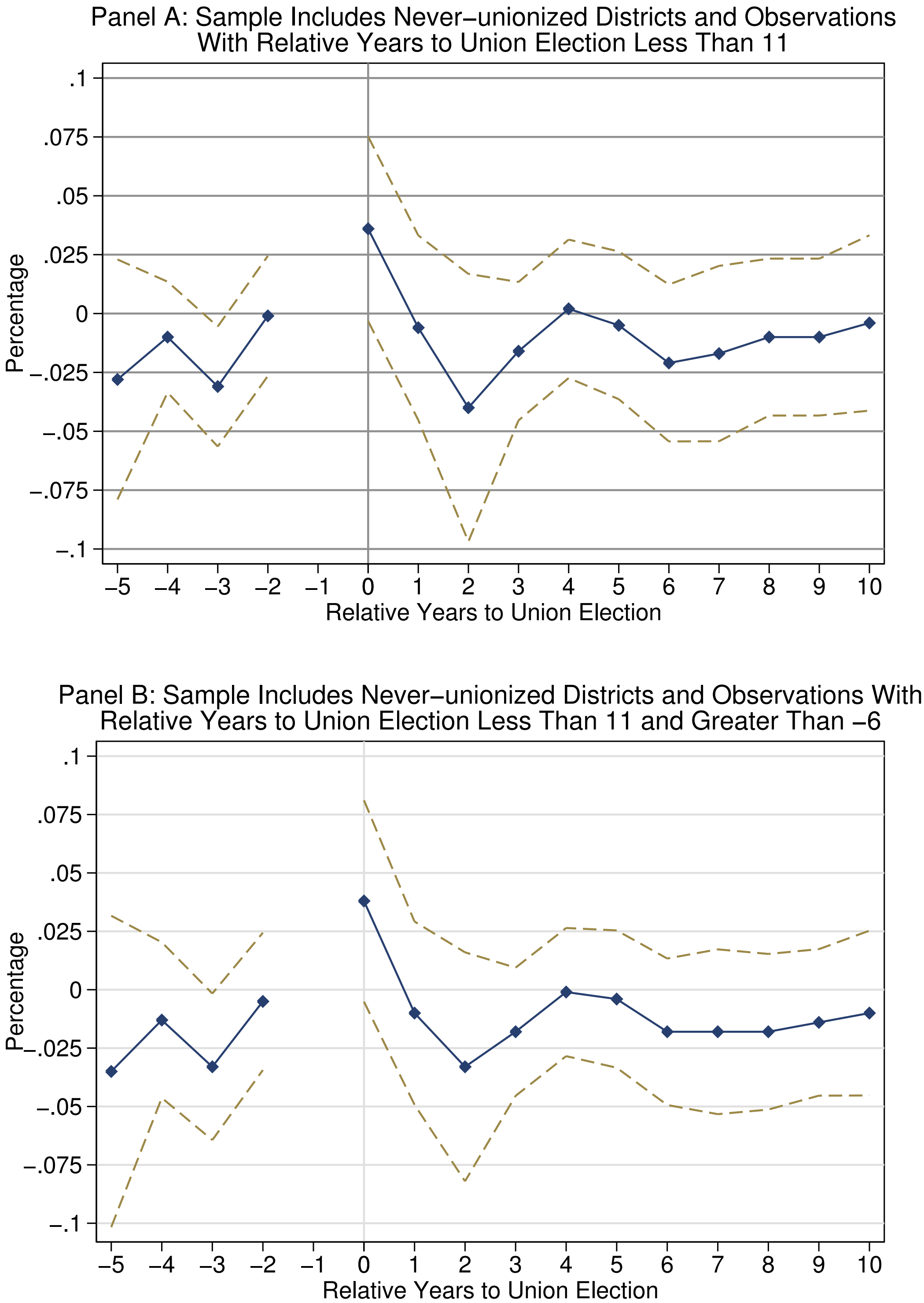
Figure 2.4: The Effect of Teachers' Unions on Log Student-Teacher Ratios





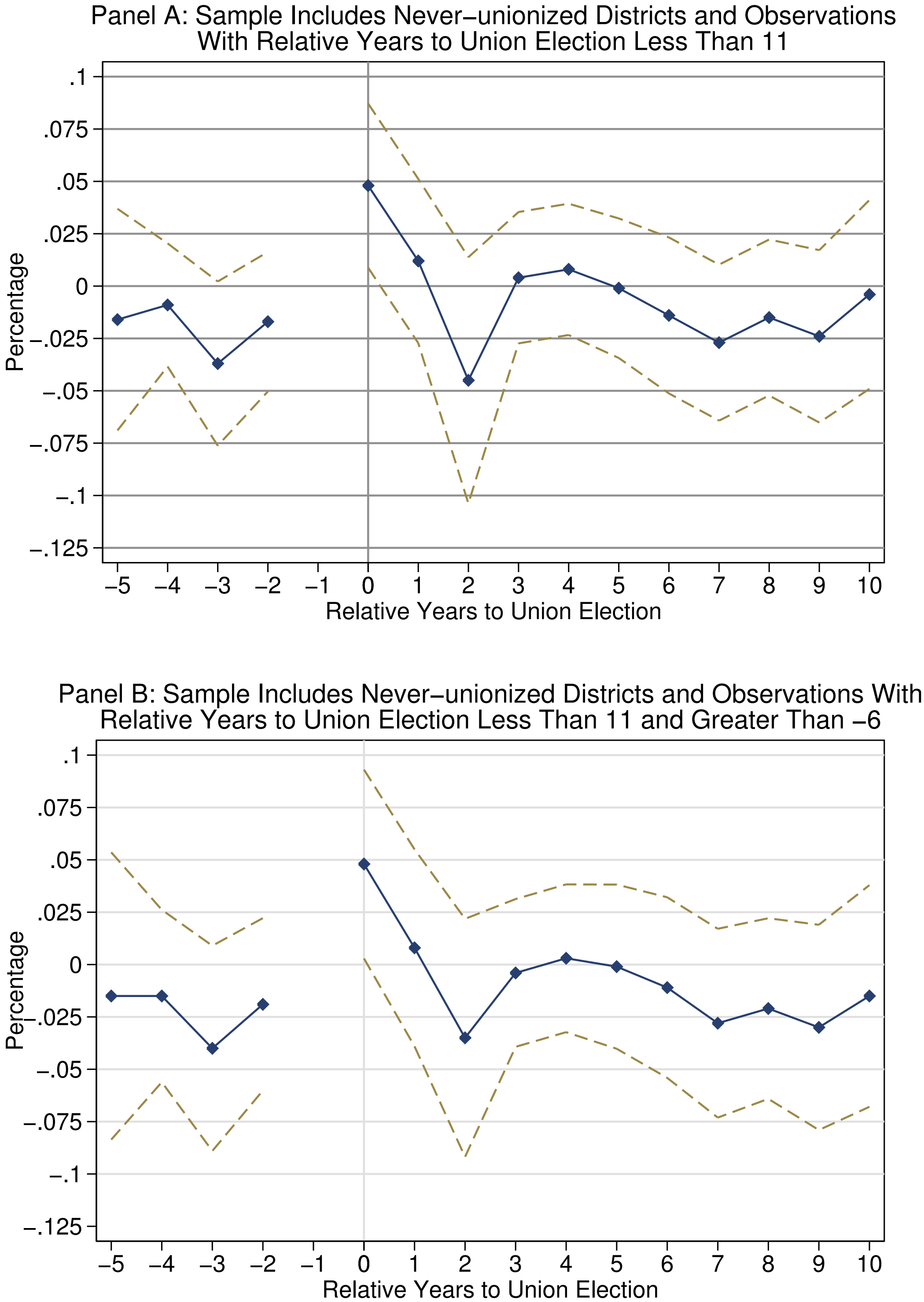
[1] Source: Author's calculations from the 1972–1991 Census/Survey of Governments as described in the text.
[2] Solid lines represent coefficient estimates from estimation of equation (2.2) in the text. Dotted lines are the bounds of the 95 percent confidence interval calculated from standard errors that are clustered at the district level.
[3] Relative year -1 is omitted in order to identify the model: a complete set of relative time dummy variables is collinear with the district fixed effects in equation (2.2).

Figure 2.5: The Effect of Teachers' Unions on Log Real Current Operating Expenditures Per Student



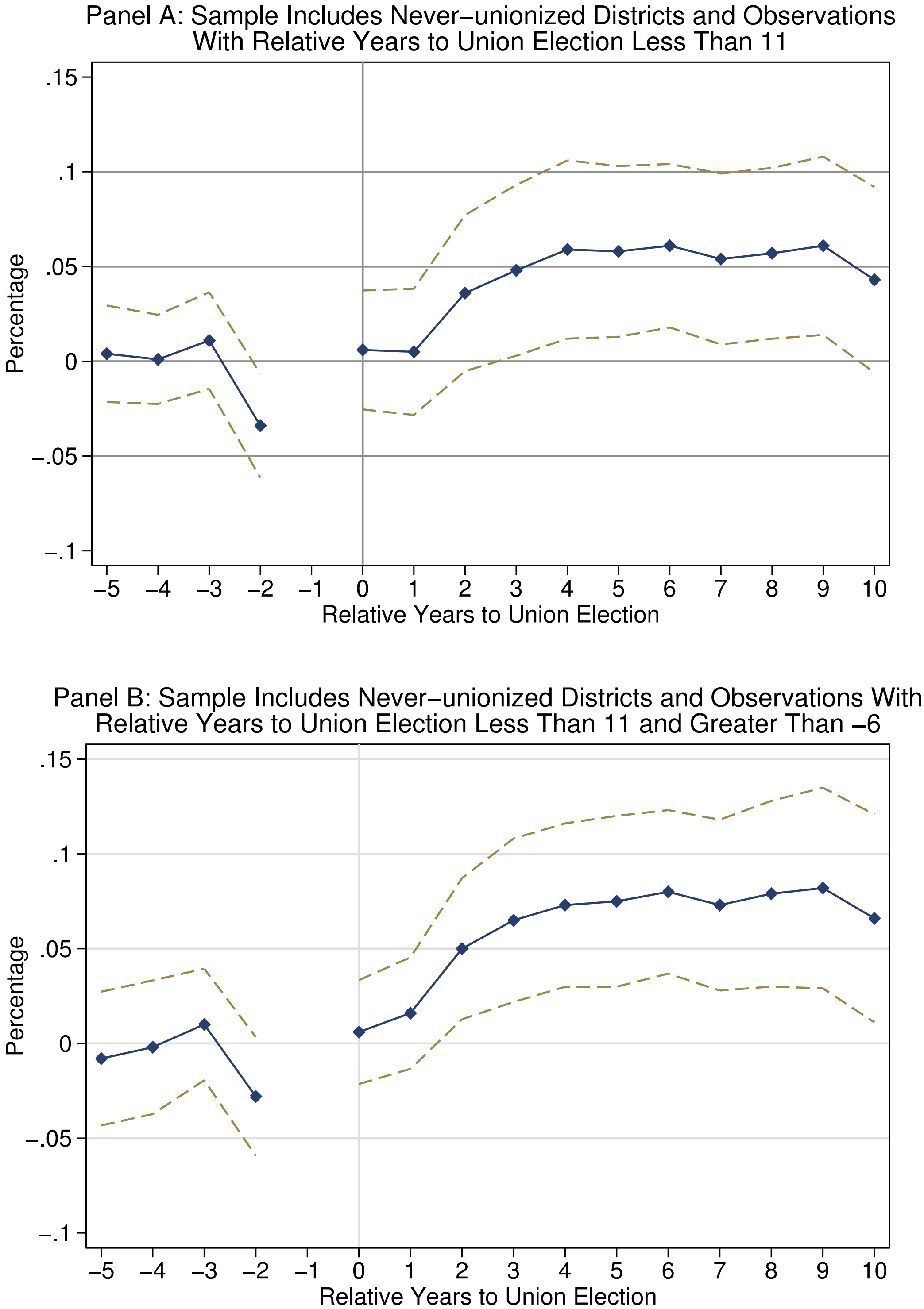
[1] Source: Author's calculations from the 1972–1991 Census/Survey of Governments as described in the text.
[2] Solid lines represent coefficient estimates from estimation of equation (2.2) in the text. Dotted lines are the bounds of the 95 percent confidence interval calculated from standard errors that are clustered at the district level.
[3] Relative year -1 is omitted in order to identify the model: a complete set of relative time dummy variables is collinear with the district fixed effects in equation (2.2).

Figure 2.6: The Effect of Teachers' Unions on Log Real Total Revenues Per Student



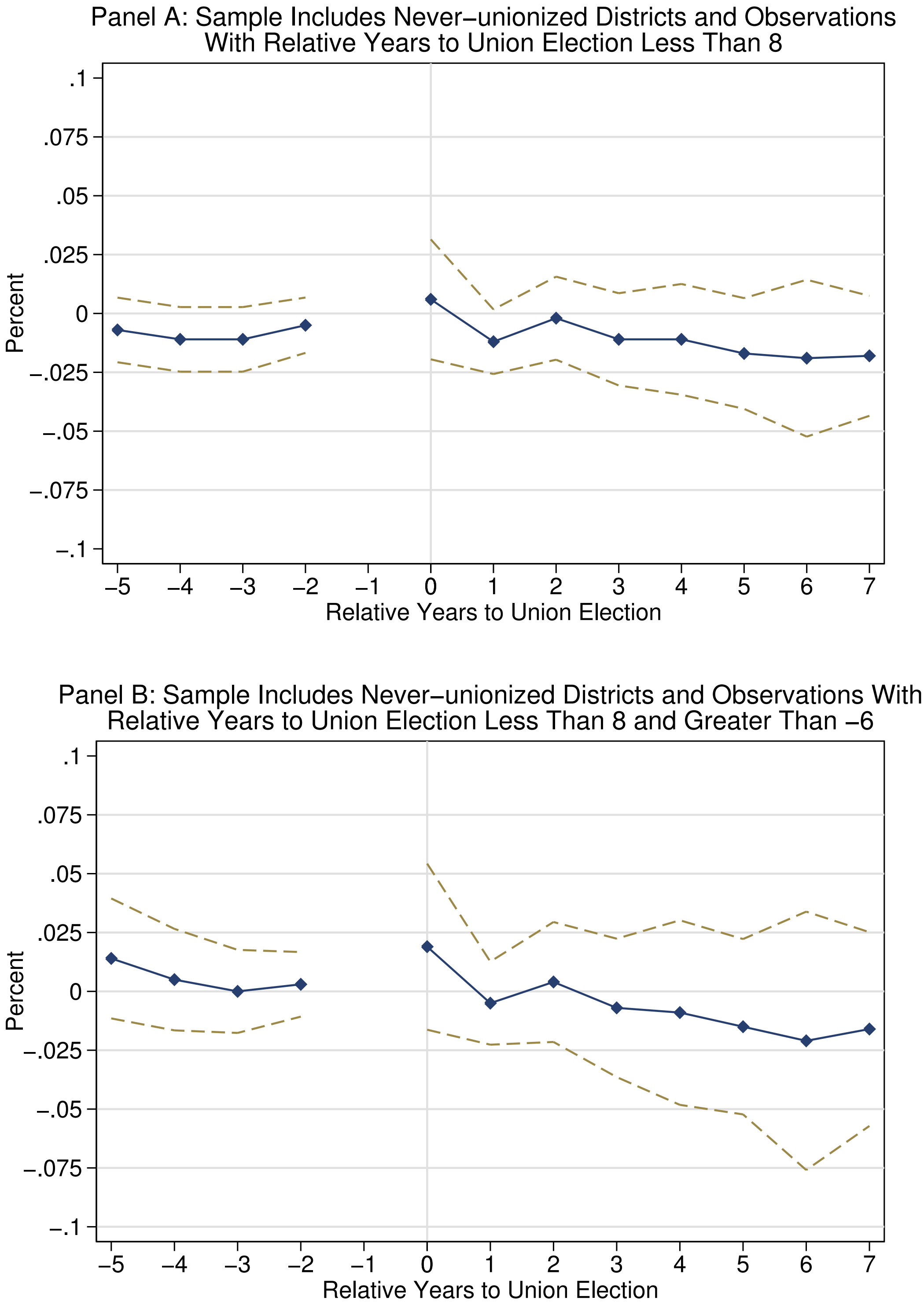
[1] Source: Author's calculations from the 1972–1991 Census/Survey of Governments as described in the text.
[2] Solid lines represent coefficient estimates from estimation of equation (2.2) in the text. Dotted lines are the bounds of the 95 percent confidence interval calculated from standard errors that are clustered at the district level.
[3] Relative year -1 is omitted in order to identify the model: a complete set of relative time dummy variables is collinear with the district fixed effects in equation (2.2).

Figure 2.7: The Effect of Teachers' Unions on Log Student Enrollment



[1] Source: Author's calculations from the 1972–1991 Census/Survey of Governments as described in the text.
[2] Solid lines represent coefficient estimates from estimation of equation (2.2) in the text. Dotted lines are the bounds of the 95 percent confidence interval calculated from standard errors that are clustered at the district level.
[3] Relative year -1 is omitted in order to identify the model: a complete set of relative time dummy variables is collinear with the district fixed effects in equation (2.2).

Figure 2.8: The Effect of Teachers' Unions on Log Proportion of Total Expenditures on Instruction



[1] Source: Author's calculations from the 1967–1970, 1973–1974, 1976–1977, and 1979 ELSEGIS as described in the text.
[2] Solid lines represent coefficient estimates from estimation of equation (2.2) in the text. Dotted lines are the bounds of the 95 percent confidence interval calculated from standard errors that are clustered at the district level.
[3] Relative year -1 is omitted in order to identify the model: a complete set of relative time dummy variables is collinear with the district fixed effects in equation (2.2).

Appendix A: Census and Survey of Governments

The Census of Governments is conducted every five years beginning in 1957, however data are only available electronically starting in 1972. The survey contains expenditure, revenue and employment data for every independent government in the United States, including independent school districts. Independent school districts are those deemed separate enough from other local governments that they are considered their own autonomous government. In states such as Maryland and Massachusetts, there are a large number of dependent school districts. In Iowa and Indiana, all school districts are independent. In Minnesota, however, about 7 percent of students are enrolled in dependent school districts. Thus, the universe of school districts in the COG/SOG is close to the full universe of school districts in the three states included in this analysis.

The Survey of Governments is conducted in each non-COG year beginning in 1973 and contains a random sample of local governments included in the previous census. In 1979, the Census Bureau began sampling every school district in certain states (including Iowa and Minnesota) for their *Annual Survey of Local Government Finances - School Systems (F-33)* survey. The employment survey, which is conducted separately, remained a random sample for all states.

Because the Census Bureau does not code school districts in a systematic manner, the only way to combine information across years is to merge files based on district name. However, in the 1975 finance and 1986 finance and employment files, these names are missing. Thus, I am forced to exclude data from these survey years from the analysis. I do use the 1975 employment data, however.

I construct measures of real monthly full time teacher pay, full time teacher employment, student-teacher ratios, current operating expenditures (COE) and total revenues for each district in the sample. All financial variables are inflated to real 2004 dollars. The definitions of most of these variables are straightforward and come directly from the

COG/SOG, with the exception of teacher pay and the student-teacher ratio.

I construct real monthly full-time teacher pay by dividing the gross monthly payroll for full time instructional staff by the number of full-time instructional staff. Note in the COG/SOG data, administrators such as principals and guidance counselors are included in the definition of full-time instructional staff. However, other administrators, such as the superintendent, are excluded from this category. Unfortunately, there are no district-level data from this period on teacher pay that will allow me to further separate this group. To the extent unions affect the mix of full-time teachers in the school district through changes in seniority rules and hiring practices, the impact on teacher pay will only be detected if these changes shift the mean salary of teachers.

The student-teacher ratio is my measure of class size (Woodbury (1985) and Hoxby (1996) also use this measure). While it does not measure the exact number of students included in each class, it is a reasonable and standard approximation of the human resources per student in each district. I calculate the student-teacher ratio by dividing total enrollment by the number of full time equivalent teachers in each school district.

Appendix B: Parameter Estimates from Estimation of Equation (2.2)

This appendix presents parameter estimates and clustered standard errors from estimation of equation (2.2) in the text. Tables 2.B.1 and 2.B.2 contain the point estimates and standard errors shown graphically in Panels A and B of Figures 2.2–2.6, respectively. Tables 2.B.3 and 2.B.4 contain point estimates and standard errors from estimates of equation (2.2) using the ELEGIS data to explore the effect of unions on the allocation of educational resources.

Table 2.B.1: Regression Results from Fixed Effects Estimates of Teachers' Union Impacts on Resource Levels from the Census/Survey of Governments Using the Sample Comprised of Never-Unionized Districts and Observations With Relative Years to Union Election Less Than 11

Relative Time	Dependent Variable: Log of Real Monthly Full Time Teacher Pay	Full Time Teacher Employment	Student-Teacher Ratio	Real COE Per Student	Real Revenue Per Student
-5 Years	0.027*	0.009	0.005	-0.028	-0.016
	(0.016)	(0.036)	(0.039)	(0.026)	(0.027)
-4 Years	0.018	0.004	-0.010	-0.010	-0.009
	(0.012)	(0.020)	(0.017)	(0.012)	(0.015)
-3 Years	0.008	0.006	-0.004	-0.031**	-0.037*
	(0.010)	(0.017)	(0.014)	(0.013)	(0.020)
-2 Years	0.006	-0.028	-0.010	-0.001	-0.017
	(0.010)	(0.021)	(0.017)	(0.013)	(0.017)
0 Years	0.020*	0.036*	-0.023	0.036*	0.048**
	(0.011)	(0.020)	(0.022)	(0.020)	(0.020)
1 Year	0.013	0.040**	-0.016	-0.006	0.012
	(0.011)	(0.019)	(0.018)	(0.020)	(0.020)
2 Years	0.002	0.046**	-0.009	-0.040	-0.045
	(0.012)	(0.021)	(0.018)	(0.029)	(0.030)
3 Years	0.019	0.054**	-0.005	-0.016	0.004
	(0.012)	(0.022)	(0.018)	(0.015)	(0.016)
4 Years	0.017	0.061**	-0.016	0.002	0.008
	(0.013)	(0.024)	(0.019)	(0.015)	(0.016)
5 Years	0.011	0.050**	-0.010	-0.005	-0.001
	(0.013)	(0.023)	(0.020)	(0.016)	(0.017)
6 Years	0.010	0.064**	-0.022	-0.021	-0.014
	(0.013)	(0.025)	(0.022)	(0.018)	(0.019)
7 Years	0.013	0.054*	-0.024	-0.017	-0.027
	(0.014)	(0.028)	(0.025)	(0.019)	(0.019)
8 Years	0.021	0.059**	-0.013	-0.010	-0.015
	(0.015)	(0.029)	(0.025)	(0.017)	(0.019)
9 Years	0.027*	0.067**	-0.018	-0.010	-0.024
	(0.016)	(0.030)	(0.023)	(0.017)	(0.021)
10 Years	0.024	0.029	-0.013	-0.004	-0.004
	(0.016)	(0.028)	(0.023)	(0.019)	(0.023)
Constant	8.095**	4.602**	2.463**	8.759**	8.833**
	(0.022)	(0.049)	(0.032)	(0.018)	(0.019)
N	7549	7549	6633	10822	10825
# Clusters	1112	1112	1104	1137	1137
R^2	0.753	0.979	0.630	0.923	0.756

[1] Source: Parameter estimates from estimation of equation (2.2) in the text.
[2] Regressions include school district and state-specific year fixed effects. All standard errors are clustered at the school district level: * indicates significance at the 10 percent level and ** indicates significance at the 5 percent level.
[3] A complete set of relative time dummy variables are collinear with the district fixed effects in equation (2.2) on the sample that excludes observations with relative time to union election less than -6. Relative year -1 is omitted to make the parameter estimates consistent with that model.

Table 2.B.2: Regression Results from Fixed Effects Estimates of Teachers' Union Impacts on Resource Levels from the Census/Survey of Governments Using the Sample Comprised of Never-Unionized Districts and Observations With Relative Years to Union Election Less Than 11 and Greater Than -6

	Dependent Variable: Log of				
Relative Time	Real Monthly Full Time Teacher Pay	Full Time Teacher Employment	Student-Teacher Ratio	Real COE Per Student	Real Revenue Per Student
-5 Years	0.015	-0.015	0.010	-0.035	-0.015
	(0.023)	(0.051)	(0.056)	(0.034)	(0.035)
-4 Years	0.005	0.000	-0.009	-0.013	-0.015
	(0.018)	(0.028)	(0.026)	(0.017)	(0.021)
-3 Years	-0.001	0.002	-0.004	-0.033**	-0.040*
	(0.015)	(0.023)	(0.023)	(0.016)	(0.025)
-2 Years	-0.003	-0.033	-0.007	-0.005	-0.019
	(0.013)	(0.028)	(0.023)	(0.015)	(0.021)
0 Years	0.012	0.039*	-0.032	0.038*	0.048**
	(0.013)	(0.023)	(0.028)	(0.022)	(0.023)
1 Year	0.007	0.048**	-0.017	0.010	0.008
	(0.013)	(0.021)	(0.022)	(0.020)	(0.024)
2 Years	-0.003	0.059**	-0.010	-0.033	-0.035
	(0.013)	(0.023)	(0.022)	(0.020)	(0.029)
3 Years	0.017	0.063**	-0.003	-0.018	-0.004
	(0.014)	(0.025)	(0.021)	(0.014)	(0.018)
4 Years	0.020	0.068**	-0.016	-0.001	0.008
	(0.014)	(0.026)	(0.024)	(0.014)	(0.018)
5 Years	0.010	0.065**	-0.017	-0.004	-0.001
	(0.015)	(0.028)	(0.023)	(0.015)	(0.020)
6 Years	0.005	0.089**	-0.033	-0.018	-0.011
	(0.016)	(0.028)	(0.025)	(0.016)	(0.022)
7 Years	0.009	0.081**	-0.034	-0.018	-0.028
	(0.017)	(0.032)	(0.029)	(0.018)	(0.023)
8 Years	0.021	0.073**	-0.009	-0.018	-0.021
	(0.018)	(0.032)	(0.025)	(0.017)	(0.022)
9 Years	0.029	0.092**	-0.025	-0.014	-0.030
	(0.019)	(0.034)	(0.027)	(0.016)	(0.025)
10 Years	0.021	0.069	-0.037	-0.010	-0.015
	(0.020)	(0.034)	(0.028)	(0.018)	(0.027)
Constant	8.070**	4.633**	2.447**	8.758**	8.828**
	(0.028)	(0.059)	(0.043)	(0.021)	(0.024)
N	8515	8515	7500	12225	12229
# Clusters	1165	1165	1157	1165	1165
R^2	0.735	0.977	0.622	0.923	0.754

[1] Source: Parameter estimates from estimation of equation (2.2) in the text.
[2] Regressions include school district and state-specific year fixed effects. All standard errors are clustered at the school district level: * indicates significance at the 10 percent level and ** indicates significance at the 5 percent level.
[3] Relative year -1 is omitted in order to identify the model: a complete set of relative time dummy variables are collinear with the district fixed effects in equation (2.2).

Table 2.B.3: **Regression Results from Fixed Effects Estimates of Teachers' Union Impacts on Expenditure Allocation from the ELSEGIS Survey Using the Sample Comprised of Never-Unionized Districts and Observations With Relative Years to Union Election Less Than 8**

	Dependent Variable: Log Proportion of Total Expenditures on:					
Relative Time	Administration	Instruction	Attendance & Health	Transportation	Plant O&M	Fixed Charges
-5 Years	-0.150**	0.014	0.059	-0.060	-0.013	-0.016
	(0.076)	(0.013)	(0.270)	(0.066)	(0.052)	(0.044)
-4 Years	-0.108*	0.005	0.215	-0.056	0.012	0.009
	(0.061)	(0.011)	(0.171)	(0.050)	(0.032)	(0.037)
-3 Years	-0.065	-0.0005	0.162	-0.037	0.019	-0.043
	(0.060)	(0.009)	(0.166)	(0.039)	(0.032)	(0.035)
-2 Years	-0.056	0.003	0.114	-0.008	-0.005	-0.035
	(0.044)	(0.007)	(0.133)	(0.028)	(0.024)	(0.024)
0 Years	-0.152*	0.019	0.099	0.013	0.016	-0.005
	(0.080)	(0.018)	(0.157)	(0.038)	(0.033)	(0.037)
1 Year	-0.054	-0.005	0.097	0.051	-0.002	-0.006
	(0.055)	(0.009)	(0.154)	(0.036)	(0.027)	(0.031)
2 Years	-0.074	0.004	-0.048	0.041	-0.068*	0.009
	(0.058)	(0.013)	(0.170)	(0.037)	(0.041)	(0.031)
3 Years	-0.089	-0.007	0.032	0.045	0.003	-0.010
	(0.068)	(0.015)	(0.179)	(0.039)	(0.047)	(0.033)
4 Years	-0.068	-0.009	-0.094	0.082**	-0.041	-0.042
	(0.068)	(0.020)	(0.179)	(0.041)	(0.042)	(0.031)
5 Years	-0.029	-0.015	-0.218	0.080*	-0.045	-0.018
	(0.078)	(0.019)	(0.201)	(0.045)	(0.039)	(0.040)
6 Years	-0.083	-0.021	-0.030	0.056	-0.079*	-0.022
	(0.098)	(0.028)	(0.242)	(0.054)	(0.044)	(0.046)
7 Years	-0.025	-0.016	-0.107	0.076	-0.047	-0.037
	(0.068)	(0.021)	(0.214)	(0.051)	(0.041)	(0.048)
Constant	1.404**	4.246**	-1.084**	1.806**	2.546**	1.209**
	(0.074)	(0.016)	(0.131)	(0.053)	(0.036)	(0.057)
N	4142	4185	3960	4135	4139	4155
# Clusters	1072	1074	1042	1073	1074	1075
R^2	0.756	0.655	0.846	0.919	0.715	0.904

[1] Source: Parameter estimates from estimation of equation (2.2) in the text.

[2] Regressions include school district and state-specific year fixed effects. All standard errors are clustered at the school district level: * indicates significance at the 10 percent level and ** indicates significance at the 5 percent level.

[3] A complete set of relative time dummy variables are collinear with the district fixed effects in equation (2.2) on the sample that excludes observations with relative time to union election less than -6. Relative year -1 is omitted to make the parameter estimates consistent with that model.

Table 2.B.4: Regression Results from Fixed Effects Estimates of Teachers' Union Impacts on Expenditure Allocation from the ELSEGIS Survey Using the Sample Comprised of Never-Unionized Districts and Observations With Relative Years to Union Election Less Than 8 and Greater Than -6

Relative Time	Administration	Instruction	Attendance & Health	Transportation	Plant O&M	Fixed Charges
-5 Years	-0.066*	-0.007	0.021	0.029	0.016	-0.011
	(0.039)	(0.007)	(0.184)	(0.053)	(0.031)	(0.033)
-4 Years	-0.054	-0.011	0.254*	0.029	0.036	0.007
	(0.041)	(0.007)	(0.150)	(0.047)	(0.020)	(0.029)
-3 Years	-0.036	-0.011	0.235	0.009	0.033	-0.038
	(0.041)	(0.007)	(0.135)	(0.034)	(0.023)	(0.028)
-2 Years	-0.026	-0.005	0.167	0.039	0.008	-0.035
	(0.036)	(0.006)	(0.113)	(0.027)	(0.023)	(0.022)
0 Years	-0.112*	0.006	0.220	0.061*	0.023	-0.004
	(0.060)	(0.013)	(0.159)	(0.037)	(0.024)	(0.029)
1 Year	-0.007	-0.012*	0.219	0.088**	0.011	0.004
	(0.039)	(0.007)	(0.156)	(0.033)	(0.021)	(0.024)
2 Years	-0.034	-0.002	0.113	0.075**	-0.048	0.020
	(0.043)	(0.009)	(0.167)	(0.033)	(0.038)	(0.026)
3 Years	-0.036	-0.011	0.212	0.084**	0.019	0.008
	(0.049)	(0.010)	(0.181)	(0.038)	(0.033)	(0.027)
4 Years	-0.022	-0.011	0.097	0.111**	-0.023	-0.017
	(0.049)	(0.012)	(0.178)	(0.035)	(0.030)	(0.026)
5 Years	0.002	-0.017	-0.007	0.120**	-0.016	-0.006
	(0.059)	(0.012)	(0.195)	(0.038)	(0.029)	(0.034)
6 Years	-0.093	-0.019	0.178	0.096**	-0.034	0.015
	(0.092)	(0.017)	(0.217)	(0.047)	(0.032)	(0.035)
7 Years	-0.004	-0.018	0.154	0.128**	-0.015	-0.021
	(0.058)	(0.013)	(0.220)	(0.042)	(0.032)	(0.040)
Constant	1.391**	4.276**	-1.154**	1.777**	2.434**	1.360**
	(0.033)	(0.005)	(0.106)	(0.035)	(0.016)	(0.024)
N	4796	4844	4570	4788	4792	4808
# Clusters	1190	1192	1157	1191	1192	1193
R^2	0.756	0.669	0.836	0.920	0.714	0.905

[1] Source: Parameter estimates from estimation of equation (2.2) in the text.
[2] Regressions include school district and state-specific year fixed effects. All standard errors are clustered at the school district level: * indicates significance at the 10 percent level and ** indicates significance at the 5 percent level.
[3] Relative year -1 is omitted in order to identify the model: a complete set of relative time dummy variables are collinear with the district fixed effects in equation (2.2).

Appendix C: Classification Error in the Constructed Census of Governments Teachers' Union Measure

Table 2.1 in the text presents the non-parametric identification of the measurement error in the Census of Governments union measure. This appendix investigates some properties of this classification error and includes a decomposition that breaks the bias due to the error into the part directly due to measurement error and the part due to the correlation of the measurement error with the regression error.

C.1 Non-Differential Classification Error

Let U be union status as measured by the Census of Governments variables and let U^* be true union status as indicated by the election certification data. If μ is the measurement error, then

$$U = U^* + \mu. \tag{2.C.1}$$

If one can only observe U instead of U^*, then instead of estimating the true model given by:

$$Y = \alpha + \beta U^* + \delta X + \epsilon, \tag{2.C.2}$$

one must estimate

$$Y = \tilde{\alpha} + \tilde{\beta} U + \tilde{\delta} X + \tilde{\epsilon}, \tag{2.C.3}$$

where ϵ is the regression error, X is a vector of demographic characteristics assumed to be measured without error, and Y is the outcome variable of interest that contains no measurement error. The standard result under the classical measurement error assumption in which μ is uncorrelated with U^*, X, Y, or ϵ is $\tilde{\beta}$ will be less than β in absolute value. In other words, classical measurement error will cause an attenuation bias. Note this result holds regardless of the number of independent variables measured with error as long as the classical measurement error assumptions hold.

When the mismeasured variable is binary, such as union status, the measurement error (i.e., the classification error) cannot be classical. This result is due to the fact U^* and μ will have to be negatively correlated. For example, if $U^* = 1$, $\mu \in -1, 0$, but if $U^* = 0$, $\mu \in 0, 1$. Thus, the typical attenuation result does not necessarily hold.

Bound, Brown and Mathiowetz (2001) show as long as the misclassification is non-differential and none of the other classical measurement error assumptions are violated, the bias in the coefficient will still be attenuating. Non-differential classification error occurs when reporting errors are independent of the dependent variable. More formally, this can be written:

$$\text{P(U=i|U*=i,Y)} = \text{P(U=i|U*=i)}, \tag{2.C.4}$$

where i $\in$ 0,1. I use a linear probability model to test for non-differential classification error for log real teacher pay, log real current operating expenditures per student, student-teacher ratios, and high school dropout rates. Specifically, I run models of the form:

$$U = \alpha_0 + \alpha_1 Y + \eta, \tag{2.C.5}$$

where U is an indicator variable that equals 1 if the school district is measured as unionized in the Census of Governments, Y are the dependent variables used in the analysis in the main text, and η is an error term. I perform this test separately for the probability of correctly classifying a district as unionized conditional on being unionized and for the probability of correctly classifying a district as non-unionized conditional on not being unionized. The estimates of α_1 test for the existence of differential classification error. These estimates are presented in Table 2.C.1.

Assuming the election certification data accurately represent true union status, the data strongly reject that the measurement error from the Census of Governments is non-differential. In each row of Table 2.C.1, the estimates of α_1 are statistically different from zero for at least one of the misclassification types. The implication of Table 2.C.1 is the misclassification of union status in the Census of Governments is correlated with the dependent variables of interest; the classification error is differential. The bias due to the error in variables is therefore not guaranteed to be attenuating. This result is consistent with the positive biases in absolute value reported in Tables 2.5–2.7 of union effects when the imperfectly measured union measure is utilized.

C.2 Misclassification as a Function of X

Thus far, I have established the intuition about the effect of measurement error on parameter estimates when the error in variables is classical does not hold because μ is correlated with Y (as the error is differential) and with U^* (as the variable is binary). It is also instructive to determine whether the assumption holds that the measurement error is uncorrelated with the observable Xs. To test the relationship between misclassification and the Xs, I estimate the probability a district is reported as unionized in the COG when it had successfully completed a union election and the probability a district is reported as non-union in the COG when no union election certification was on file, conditional on observables. More formally, I estimate the following models using a linear probability model:

$$\mathrm{P(U{=}1|U^{*}{=}1,X)} \tag{2.C.6}$$

$$\mathrm{P(U{=}0|U^{*}{=}0,X)} \tag{2.C.7}$$

Table 2.C.2 contains the results from these regressions from the pooled 1970, 1980 and 1990 U.S. Census and Census of Governments data described in the main text. Each cell in the table represents a separate regression. As Table 2.C.2 illustrates, the probability

of misclassifying a district's union status is correlated with the observable demographic characteristics of the district. Some general trends do emerge from Table 2.C.2: smaller, less urbanized districts with lower public school enrollment are less likely to be correctly classified as unionized, while those districts with lower average income, lower average rent, and a smaller proportion of BA recipients are more likely to be misclassified as unionized. School districts with a higher percentage of residents with 12 or more years of schooling are less likely to be classified as unionized regardless of true union status, and conversely, districts with a higher percentage of private enrollment have a higher probability of being classified as unionized regardless of true union status. Lastly, those districts with higher poverty and unemployment rates have a higher probability of being misclassified conditional on their true union status. The assumption necessary for classical measurement error that the error is independent of the correctly measured observables clearly does not hold in the data.

C.3 BBDR Decompositions

Since the misclassification error is correlated with both the dependent variables and the independent variables in the union impact regressions, it is interesting to determine the extent to which each of these correlations cause the observed differences in the estimated union effects. Bound, Brown, Duncan and Rodgers (1994) propose a decomposition of the difference between the biased coefficient and the unbiased coefficient into the difference directly due to measurement error and the difference due to the correlation of the measurement error with the regression error.[49] More formally, let

$$Z = [U|X]' \tag{2.C.8}$$

be a matrix of all the data. Then

[49] See Black, Sanders and Taylor (2003) for an implementation of the BBDR decomposition similar to the one presented here.

$$
\begin{aligned}
\tilde{\beta} &= (Z'Z)^{-1}Z'Y && (2.C.9)\\
&= (Z'Z)^{-1}Z'[Z^{*}\beta+\epsilon]\\
&= (Z'Z)^{-1}Z'[(Z-\mu)\beta+\epsilon]\\
&= (Z'Z)^{-1}Z'Z\beta+(Z'Z)^{-1}Z'[-\mu\beta+\epsilon]\\
&= \beta-(Z'Z)^{-1}Z'\mu\beta+(Z'Z)^{-1}Z'\epsilon\\
&\Longleftrightarrow\\
\tilde{\beta}-\beta &= -(Z'Z)^{-1}Z'\mu\beta+(Z'Z)^{-1}Z'\epsilon\\
&= -(\text{E}[\mu \mid \text{U=1, X}] \text{ - } \text{E}[\mu \mid \text{U=0, X}])\beta+(\text{E}[\epsilon \mid \text{U=1, X}] && (2.C.10)\\
&\quad \text{ - } \text{E}[\epsilon \mid \text{U=0, X}]),
\end{aligned}
$$

where the last line follows from the fact only union status is assumed to be measured with error in the data. The first term on the right-hand side of Equation 2.C.10 gives the part of the total difference that is due to measurement error, while the second term shows the part of the total difference that is due to the correlation between the measurement error and the regression error. I perform this decomposition separately for each of the four dependent variables used above in a model that includes district fixed effects, year fixed effects and district-specific linear time trends. The coefficient estimates are thus identical to those reported in Tables 2.5–2.7 in the main text.

Table 2.C.3 presents the results of the BBDR decompositions. As is evident from the table, both forms of bias are present. These biases reinforce each other for log real teacher pay, log real current operating expenditures per student, and student-teacher ratios in this sample. The bias due to measurement error implies the direct effect of wrongly classifying a district as unionized is to increase the estimated union effect on teacher pay, expenditures per student, and student-teacher ratios. This result occurs because non-unionized districts have higher pay, expenditures, and class sizes than unionized districts, so mis-classifying non-unionized districts as unionized will bias upward the estimated impact of teachers'

unions on all three measures. That the classification error is positively correlated with the regression error for the three inputs is due to the fact school districts incorrectly classified as unionized tend to have higher levels of teacher pay, expenditures, and student-teacher ratios than school districts for which union classification is correct. Thus, the misclassification of union status will serve to bias further upward the union impact estimates on these variables.

For the high school dropout rate decompositions, the biases offset each other somewhat, but the relatively large negative effect from measurement error dominates the positive correlation between the measurement error and the regression error. Non-union schools tend to have lower dropout rates than union schools, which is partially offset by the fact districts wrongly classified as unionized have higher dropout rates.

Table 2.C.1: Tests of Non-Differentiation in COG Misclassification

Independent Variable	P(U=1|U*=1,Y)	P(U=0|U*=0,Y)
Log Real Teacher Pay	0.356**	-0.076
	(0.051)	(0.064)
Log Real Expenditures per Student	-0.002	0.024**
	(0.015)	(0.004)
Student-Teacher Ratio	0.017**	0.007**
	(0.004)	(0.002)
High School Dropout Rate	-0.0003	0.004**
	(0.001)	(0.001)

[1] Source: Authors' calculations from the 1972, 1977, 1982, and 1987 Census of Governments and the teachers' union election certification data described in the text.
[2] Each cell above represents a separate pooled linear probability model regression. Standard errors are in parentheses: ** indicates significance at the 5 percent level.

Table 2.C.2: Relationship Between Misclassification and the Observables

Independent Variable	P(U=1|U*=1,X)	P(U=0|U*=0,X)
Log Population	0.035** (0.010)	0.009 (0.013)
Percent Urban	0.067** (0.036)	0.026 (0.041)
Log Average Income	-0.007 (0.022)	-0.042** (0.007)
Log Median Rent	-0.041 (0.036)	-0.126** (0.024)
Percent Poverty	-0.755** (0.268)	-0.570** (0.224)
Percent Unemployed	-0.002** (0.001)	-0.008** (0.001)
Percent Black	-0.001 (0.003)	0.022* (0.012)
Percent Hispanic	-0.07 (0.006)	0.026 (0.020)
Percent 12–15 Years School	-0.001 (0.001)	0.008** (0.002)
Percent 16+ Years School	0.001 (0.001)	-0.010** (0.002)
Percent Private Enrollment	0.004** (0.001)	-0.003** (0.001)
Log Public School Enrollment	0.039** (0.010)	0.010 (0.013)

[1] Source: Authors' calculations from the 1972, 1982, and 1987 Census of Governments, the 1970, 1980 and 1990 U.S. Census, and the teachers' union election certification data described in the text.
[2] Each cell above represents a separate pooled linear probability model regression. Standard errors are in parentheses: ** indicates significance at the 5 percent level and * indicates significance at the 10 percent level.

Table 2.C.3: Bound, Brown, Duncan and Rodgers (BBDR) Decompositions

Dependent Variable	COG Estimate	Union Election Estimate	Total Difference	Difference From Measurement Error	Difference From Correlation of Measurement Error and Regression Error
Log Real Teacher Pay	0.054	-0.019	0.073	0.018	0.054
Log Real COE per Student	0.017	-0.010	0.027	0.010	0.017
Student-Teacher Ratio	0.117	-0.189	0.306	0.183	0.124
High School Dropout Rate	0.589	1.385	-0.796	-1.332	0.536

[1] Source: Authors' calculations from the 1972, 1982, and 1987 Census of Governments, the 1970, 1980 and 1990 U.S. Census, and the teachers' union election certification data described in the text.
[2] Each regression includes district and year fixed effects as well as district-specific linear time trends.

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CHAPTER III

How Far to the Border?: The Extent and Impact of Cross-Border Casual Cigarette Smuggling, 1992-2002

3.1 Introduction

Cigarette taxes have garnered increasing interest in the United States by both government and public health officials over the past 30 years. The former are interested in using state-level excise taxes to increase government revenues, while the latter believe increased taxes could be used to reduce smoking behavior. The degree to which each of these goals can be met is a function of the demand elasticity of cigarettes. If cigarette demand is price elastic, then increasing taxes will reduce the amount of smoking but will be less effective in raising revenues. Conversely, if cigarette demand is price inelastic, then tax increases will succeed in raising revenues but not in reducing smoking behavior.

Due to the potential gains from cigarette taxation, many states have increased their cigarette taxes markedly since the 1970s (Orzechowski and Walker, 2006). The differential increase across states in the United States has caused large interstate price differences in many areas of the country. For example, as of November 2001, there was a $0.73 per pack tax difference between Washington, DC and Virginia, despite the fact the average consumer in Washington lived less than 3.5 miles from the Virginia border. Of the 5 states that had cigarette taxes over $1.00 per pack in 2001, there was an average tax difference of $0.83 between them and the closest lower-price border. The median consumer in these states was less than 38 miles from the nearest lower-priced jurisdiction.

This cross-state price variation can confound many of the potential gains from cigarette taxation as increased taxes may cause individuals to evade local taxes by purchasing cigarettes in a nearby lower-price locality. Such "casual smuggling" behavior can limit the effectiveness of state-level cigarette excise taxes in reducing smoking and in increasing state tax revenues.[1] This study seeks to estimate the extent of casual smuggling as well as its effect on cigarette demand elasticities in order to assess how this type of tax evasion impacts the revenue-generating potential and the smoking reduction benefits of cigarette taxes.

There is much evidence from previous literature regarding the existence of casual cigarette smuggling, though few studies have been able to estimate the extent of such behavior nor its effect on demand elasticities. Because smuggling causes a bias in sales as a measure of consumption, the majority of cigarette demand studies using taxed sales data control for smuggling incentives. Many studies have found a negative relationship between the average border state tax or price differentials weighted by border populations and taxed sales (Chaloupka and Saffer, 1992; Keeler et al., 2001; Coates, 1995; Yurekli and Zhang, 2000). Coates (1995) uses this specification to estimate sales elasticities with respect to both the home state price and all cigarette prices. He finds 80 percent of the sales elasticity is due to cross-border sales. Alternatively, Baltagi and Levin (1986, 1992) control for the minimum border state price and conclude an increase in this minimum price increases home state sales.

There are a small number of studies that utilize individual consumption data paired with sales data in order to identify the existence of cigarette smuggling. In their detailed study of smoking in Canada, Gruber, Sen and Stabile (2003) compare taxed sales elasticities from provinces in which smuggling is low to consumption elasticities from household expenditure data. Since prices do not vary appreciably across provinces, the authors argue these methods are effective in controlling for the biases associated with demand estimation when there is smuggling. They find ignoring smuggling causes them to overstate the price

[1] In most states, consumers can purchase a small quantity of cigarettes, usually no more than two or three cartons, legally from a lower-priced state. Purchasing more than that amount and avoiding home-state tax payments on the purchase is illegal.

elasticity of cigarettes in absolute value[2] and estimate smuggling-corrected elasticities between -0.45 and -0.47. Stehr (2005) uses a similar methodology in the U.S. to explain the per-capita differences in reported consumption and taxed sales as a function of border tax differences and state populations. He finds between 59 and 85 percent of the sales elasticity is due to changes in the locality of purchase and almost 13% of cigarettes in 2001 were purchased without payment of the home state tax. While he attributes only 0.7% of the smuggling behavior to casual smuggling,[3] he is unable to account for where consumers live in each state with respect to the lower-price borders, which limits his ability to identify casual smuggling behavior. Further, individuals may be traveling to nearby lower-price jurisdictions that are not a border state.

This paper uses micro-data on cigarette consumption from the 1992–1993, 1995–1996, 1998–1999, and 2001–2002 Current Population Survey (CPS) Tobacco Supplements combined with geographic information on the location of consumers with respect to lower-price jurisdictions to estimate cigarette demand models that incorporate the decision of whether to smuggle cigarettes across a state or Native American Reservation border. This is therefore the first paper to estimate the extent and impact of casual smuggling using only micro data on consumption. I also address a central empirical problem inherent in using such data: the state of cigarette purchase for each consumer is not identified. In the presence of casual smuggling, using the home state cigarette price as a proxy for the true cigarette price will produce a biased estimate of the effect of price changes on cigarette demand.[4] The bias stems from the fact the home state price is a biased estimator of the "true" price at which consumers purchase cigarettes, and this bias is systematically correlated with

[2]When taxed sales are used as the measure of consumption, smuggling will cause one to overstate the demand elasticity of cigarettes in absolute value as the change in sales will be a combination of a change in the quantity demanded and a change in the location of purchase. Conversely, when micro-level data on cigarette consumption are used as the measure of consumption, the bias in the elasticity due to smuggling will tend to understate the full price elasticity in absolute value as consumption will respond less to home state price changes in the presence of cross-border price differences than when prices are equalized.

[3]There are two types of smuggling commonly discussed in the literature: organized smuggling and casual smuggling. The former type of smuggling typically involves illegally transporting large quantities of cigarettes from one of the tobacco producing states (such as North Carolina, Virginia, and Kentucky) for illegal resale in another state. Organized smuggling became a federal crime in 1978 with the *Contraband Cigarette Act* and was followed by a marked decrease in interstate bootlegging (ACIR, 1985). Thursby and Thursby (2001) estimate between 3-7% of cigarette sales can be attributed to organized smuggling, which are lower than the estimates in Stehr (2005).

[4]I call this the "home state price bias"

smuggling incentives. I find strong empirical evidence of the existence of the home state price bias in my sample, which suggests ignoring smuggling will lead to biased demand model parameter estimates when using micro cigarette consumption data.

To correct for this problem, some authors assign the minimum price in a 20 mile band to each consumer (Lewit et al., 1981 and Lewit and Coate, 1982) or use an average of prices in a 25 mile band (Chaloupka, 1991). However, both the definitions of the regions and the assumptions about the fractions of cigarettes smuggled are arbitrary in these studies. In contrast, I explicitly model the decision to smuggle and then incorporate the parameters of this decision into the demand model. The distance to a lower-price locality is then used to proxy for unobserved heterogeneity in the response of demand to changes in the home state price that has been ignored by previous studies.

In the presence of smuggling, there are two elasticities of interest: the home state price elasticity and the full price elasticity. The home state price elasticity is the percentage change in demand when the home state price changes by 1 percent, and the full price elasticity is the percentage change in demand when all prices change by 1 percent such that smuggling incentives are unaffected. When cross-border sales are prevalent, the full price elasticity reveals the potential for cigarette prices to impact demand, and the home state price elasticity yields insight into how home state prices actually affect demand, holding constant the price of cigarettes in border localities.

From either a state tax or a public health policy perspective, both elasticities are of interest. However, there is little distinction in the literature between these parameters.[5] Most studies that attempt to correct for smuggling biases are implicitly attempting to estimate the full price elasticity as this is the elasticity in the absence of smuggling. However, the home state price elasticity is arguably of more value to state policy makers as they cannot control prices in border localities. My cigarette demand model allows me to dis-

[5]Coates (1995) is the only previous study to make this distinction. He undertakes his analysis using taxed sales rather than consumption data. Note that if one correctly controls for smuggling behavior, the taxable sales and full price consumption elasticities should be identical (see Gruber, Sen and Stabile, 2003). Coates finds full price elasticities similar in magnitude to the ones estimated in this study.

tinguish empirically between these different elasticities and thus provides a more detailed understanding of the relationship between cigarette prices and cigarette demand in the presence of cross-border purchases.

I find the home state price elasticity is indistinguishable from zero, due primarily to the close proximity of most individuals to the closest lower-price border. The full price elasticities tell a much different story, however, and are universally negative and non-negligible in magnitude.

The demand model I develop in this analysis also allows me to estimate the impact of smuggling on cigarette consumption and the percentage of consumers who casually smuggle.[6] I find cross-border sales cause a modest increase in consumption, and between 13 and 25 percent of consumers purchase cigarettes in border localities in the CPS sample. While these estimates are large relative to previous studies (Stehr, 2005), they are consistent with the significant savings potential from purchasing cross-borders and with the close proximity of many individuals to these borders. I use the smuggling estimates combined with information on the state to which individuals are most likely traveling to purchase cigarettes to calculate approximate revenue losses (or gains) from casual smuggling. My estimates indicate large differences across states in the effects of casual smuggling, with states such as New Hampshire, Kentucky, and Virginia gaining revenue and states such as New York, Kansas and Maryland losing significant revenue due to cross-border sales.

The remainder of this chapter is organized as follows: Section 3.2 provides a description of the data used throughout the analysis. Section 3.3 presents evidence on the home state price bias, and section 3.4 derives the demand model used throughout this study and discusses its implications. The estimation strategy is described in Section 3.5, and all results are presented in Section 3.6. Section 3.7 concludes.

[6]This study focuses on casual smuggling, as the distance to a lower-price border state will most influence this type of behavior. However, to the extent this measure is correlated with organized smuggling, bootlegging activity will be included in the study as well.

3.2 Data

The individual-level data in this analysis come from the Current Population Survey (CPS) Tobacco Supplements: September 1992, 1995, and 1998; January 1993, 1996, and 1999; March 1993, 1996, and 1999; June and November 2001; and February 2002. These surveys span nine years in four waves given approximately every two years. Because I am interested in combining these data with a measure of smuggling distance, I restrict the sample to those living in an identified MSA; this is the most specific level of geographic identification available in the CPS. As there are 11 MSAs that split state lines, each state-MSA combination is taken as a separate MSA. I will use state-MSA and MSA interchangeably below.

I combine these data with state average price and tax data from *The Tax Burden on Tobacco* compilation (Orzechowski and Walker, 2006). All prices are inflated to real 2004 dollars using the GDP implicit price deflator. Prices listed in this compilation are spot prices as of November of that year. One of the central problems with using spot price data is tax changes occurring between November and the CPS survey month are not reflected in the price. Much of the research in this area suggests tax increases are fully passed on to consumers in the form of higher prices (Keeler et al., 1996 and Coates, 1995). Thus, November spot prices are not an accurate proxy for non-November prices if there are tax increases.[7] To construct a more accurate price series, I subtract the November excise tax in each state from the listed price and smooth the pre-tax price changes evenly over the entire year. I then add in the appropriate excise and sales taxes for each state and month in the Tobacco Supplement.[8]

The central variable in the analysis is the distance to a lower-price locality. I use 2000 Census geographic data to estimate a weighted average distance from each state-MSA

[7]There are rarely tax decreases.

[8]There are a number of counties and cities that have local cigarette taxes. Unfortunately, no data exist on the history of these taxes back to 1992. I thus exclude these taxes from the analysis and only utilize state-level taxes. As a consequence, the cross-state price differences may be understated in some cases, causing an attenuation bias in the estimate of the effect of the price difference on cigarettes demanded.

combination to the closest lower-price border. This calculation is done by finding the "crow-flies" distance from each census block point in a state-MSA to each intersection between a state border and "major road."[9] Once I calculate the distance from each block point to each road crossing, I take the closest crossing from each block point to a given border state and calculate a population-weighted average across block points for each border state. By measuring distance from the population center rather than the geographic center of a given MSA, I am able to more accurately characterize the distance an average individual must travel to smuggle cigarettes. In the tables below, the distance measure is the distance to the closest lower-price border, which is often, but not always, a border state.[10]

In addition to neighboring states, many individuals can obtain lower-price cigarettes from Native American Reservations. Native American Reservations are considered separate legal entities from the United States and are thus not subject to sales and excise taxes. In 1976, the U.S. Supreme Court ruled in *Moe v. Confederated Salish and Kootenai* that states have the right to impose sales and excise taxes on cigarette sales occurring on reservations to non-tribal members. Although evidence suggests a substantial amount of sales occur on reservations to non-tribal members (ACIR, 1985 and FACT Alliance, 2005), only 12 states have passed legislation that allows taxation of these sales. Table 3.1 contains information on which states tax non-tribal reservation sales and the case law or regulation that legitimates these taxes. I collected these data using *Cigarette Tax Evasion: A Second Look* (ACIR, 1985), which documents much of the case law and state legislation through 1985 on Native American cigarette sales. I augmented and updated this information using state taxation statutes found through LexisNexis.

I assume a state taxes non-tribal sales if there is a statute in the state law that provides for the remittance of these taxes or if there is a state-specific case determining that the

[9]A major road is a census classification and contains most non-residential roads. The exclusion of residential roads is trivial as the vast majority of interstate travel does not occur on such roads.

[10]In many MSAs, there are farther lower-price jurisdictions with lower prices than the closest lower-price locality. Using the closest lower-price state will cause measurement error in the distance variable if people are willing to travel a little farther to obtain a slightly better price. The results from this paper suggest individuals are quite sensitive to the distance to a lower-price border but not the level of the price difference. Further, for most MSAs, the distance to a better price than the closest lower-price is quite substantial. Thus, the use of the closest lower-price border is consistent with the data and likely causes little measurement error.

application of these taxes to non-tribal reservation sales are legal. A few states, such as Michigan, Wisconsin and Minnesota handle their tribal tax agreements through individual tribal compacts. These compacts all provide for taxing non-tribal sales and I thus include these states in Table 3.1. It is important to note the method of enforcing the remittance of these taxes varies significantly across states, and the method of tax enforcement likely influences the effectiveness of the laws. A brief overview of the differences in enforcement regimes is given in Appendix A, but due to sample size considerations, this heterogeneity is ignored in the analysis and all reservations in states listed in Table 3.1 are excluded from the study.

Appendix A also contains a discussion of the data and methodology used to calculate price differences and distances to Native American Reservations. Due to potential measurement error in these variables, I conduct the analysis below both including and excluding reservation smuggling incentives.

Table 3.2 presents means of distance, price differences and tax differences by state. The table also lists the number of tax changes observed in the data as well as all of the closest lower-price localities for each state. Table 3.2 illustrates the heterogeneity across states in smuggling incentives. For example, consumers in Massachusetts, New York, Illinois, and Wisconsin live close to areas in which cigarettes are substantially less expensive. However, in states such as Delaware, Nevada, and Oregon, consumers likely live too far away from the lower-priced jurisdictions to realize the savings from purchasing cigarettes there.

3.3 Home State Price Bias

Most previous studies on cigarette demand estimate a linear demand function of the form:

$$E[Q|P,X] = \beta_0 + \beta_1 P + \gamma X, \tag{3.1}$$

where Q is cigarette consumption or sales, P is the home state cigarette price and X is a vector of individual preference parameters. Such models have traditionally been run in order to find the demand elasticity of cigarettes, which is equal to $\beta_1 * \frac{P}{Q}$. The parameter β_1 gives the average partial effect of price on quantity demanded for individuals in the sample.

When the opportunity to purchase cigarettes in lower-price localities exists, β_1 will not be a consistent estimator of the true average partial effect if there are unobserved differences in how individuals respond to home state price changes. The heterogeneity in demand response is a function of smuggling incentives that are typically not included in models of cigarette demand using micro-data. This problem essentially equates to an omitted variables bias as the propensity to smuggle is likely correlated with home state cigarette prices.[11] Thus, ignoring smuggling incentives in the estimation of cigarette demand can lead to a biased estimate of β_1. I term this source of bias the "home state price bias" as it stems from an inability of the home state price to correctly measure the true price paid by consumers.[12]

While many studies using individual cigarette data assert the existence of this bias (Lewit et al., 1981; Lewit and Coate, 1982; Chaloupka, 1991; Gruber, Sen and Stabile, 2003), there has been little documentation of how the responsiveness of demand to the home state price varies with smuggling incentives. Tables 3.3 and 3.4 present evidence of the relationship between distance to the closest lower-price border and cigarette demand using the CPS sample described in the previous section.

Table 3.3 contains mean residuals by distance quartile from a regression of log cigarette consumption on log home state cigarette price, demographic characteristics and MSA fixed effects.[13] The residuals from this regression represent the within-MSA variation in cigarette consumption that is unexplained by demographics and home state price. I calculate the

[11] The correlation could also be due to policy issues that arise at the state level. For example, states in which cigarette demand is highly price responsive may set lower taxes, which will in turn affect cigarette prices.

[12] See Gruber, Sen and Stabile (2003) for further discussion of the effect of this bias on elasticity estimates

[13] All data in these regressions are collapsed to MSA means as distance is measured at the MSA level. I therefore can utilize the log-log specification as there are no MSAs in which no one smokes. I employ the log-log specification throughout this analysis in order to simplify the elasticity calculations.

mean log cigarette residuals by quartile of distance to the nearest lower-price border state for three margins of demand: intensive, extensive and full. As Table 3.3 illustrates, the residuals are positive in MSAs that are closer to the border and negative for those farther away from the border. These signs are consistent with a home state price bias because consumers who live closer to the border smoke more than suggested by the home state price.

Table 3.4 compares responses to changes in home state and border state prices for those living on the high-price side and low-price side of the borders in the 11 MSAs that split state lines. Each cell in the table represents a regression of log cigarette consumption on the relevant price as well as on MSA demographic characteristics (see Section 3.5 for a list of these variables) and MSA fixed effects. In all regressions in Table 3.4, I instrument cigarette prices with cigarette taxes in order to avoid potential endogeneity problems with cigarette prices. See Section 3.5 for a more detailed discussion of this issue. The results presented in Table 3.4 are consistent with the existence of the home state price bias: those living on the high-price side of the border respond to changes in the border state price more than the home state price, and those living on the low-price side respond more to changes in the home state price than the border state price.

Taken together, Tables 3.3 and 3.4 suggest the home state price is indeed a biased estimator of the true price paid by consumers, and this bias is systematically correlated with the distance one lives to a lower-price border state. In order to obtain parameters of the cigarette demand function that are less prone to this source of bias, I explicitly model the heterogeneity in home state price effects due to varying smuggling incentives. In lieu of directly observing smuggling activity (which is unobservable in the data), I construct a model of cigarette demand that incorporates the decision of whether to smuggle based on observable consumer characteristics.

3.4 A Model of Cigarette Demand with Cross-Border Purchases

3.4.1 Cigarette Demand Model

Assume each consumer faces two prices: the price of cigarettes in the home state and the price of cigarettes in the closest lower-price locality. Additionally, assume the parameters of the demand function are the same regardless of the place of purchase. In other words, consumers differ solely by the price they pay for cigarettes. Let demand of consumer i be given by

$$E[ln(Q_i)|P_{h,b}, X_i] \quad = \quad \beta_0 + \beta_1 ln(P_{h,b}) + \gamma X_i. \tag{3.2}$$

where P_h and P_b are home state and border state prices, respectively, and X is a vector of individual characteristics. Demand can then be written:

$$\begin{aligned} E[ln(Q_i)|P_h, P_b, X_i] &= (\beta_0 + \beta_1 ln(P_h) + \gamma X_i)(1 - S_i) \\ &\quad +(\beta_0 + \beta_1 ln(P_b) + \gamma X_i)(S_i) \\ &= \beta_0 + \beta_1(ln(P_h) * (1 - S_i) + ln(P_b) * S_i) + \gamma X_i, \end{aligned} \tag{3.3}$$

where S_i is an indicator function that equals 1 if an individual smuggles and zero otherwise. One can see from equation (3.3) the biases associated with treating the home state cigarette price as the actual price paid by all consumers. The elasticity with respect to the home state price (hereafter the "home state price elasticity") is given by:

$$\epsilon_H \quad \equiv \quad \beta_1(1 - S_i) - \frac{\Delta S_i}{\Delta ln(P_h)} \beta_1 ln(\frac{P_h}{P_b}) \tag{3.4}$$

Note if $S_i = 0$, meaning consumer i does not smuggle, then the home state price elasticity will be weakly less than β_1 as the home state price is higher than the border price by construction. An increase in P_h can induce the consumer to smuggle, thereby decreasing her consumption response and the revenue-generating potential of the price increase.

The other elasticity of interest is the "full price elasticity," which yields the percent change in cigarette demand when the full price of cigarettes changes by 1 percent. In other words, the full price elasticity measures the responsiveness of demand when all prices change such that the smuggling decision is unaltered.[14] This elasticity is given by β_1 in equation (3.3).

The central difficulty in estimating the parameters of demand equation (3.3) is S_i is unobserved; location of purchase is not in the data. My solution to this problem is to parameterize the S function and then incorporate these parameters into equation (3.3) above. Instead of a deterministic indicator function governing the decision to smuggle, the parameterization yields the probability, conditional on the observables, that individual i purchases cigarettes in a border state. Specifically, I assume the probability an individual smuggles is decreasing in the cost of smuggling and increasing in the marginal gains from smuggling.

I model the smuggling cost of obtaining cigarettes in a lower-price locality as $\delta ln(D) - \phi$, where D is the distance to the closest lower-price border. The other cost parameter is ϕ, which indexes the fixed non-traveling cost individual i would incur by purchasing in the home state regardless of his location with respect to the lower-price border.

Note smuggling costs are modeled here as fixed costs: each time an individual crosses a border to obtain cheaper cigarettes, he incurs this cost.[15] For reasons of simplicity, I assume all smugglers make the same number of trips (i.e., the number of trips is not a function of the cost of smuggling or the amount consumed). If this were not the case, demand, conditional on the decision to smuggle, would be a function of distance as distance is the primary measure of smuggling costs. The data strongly reject the correlation between log distance and consumption (see Table 3.6), which is consistent with the manner in which the fixed costs are modeled here.

[14] Such a price change would occur if log home state and log border state prices changed by the same amount simultaneously. While such price changes are not prevalent in the data, I will not require this type of variation to identify the full price elasticity.

[15] Casual smuggling costs do vary by distance, but conditional on an individual's location, the cost of obtaining border cigarettes is fixed. In other words, the cost of smuggling does not depend on the quantity of cigarettes purchased.

I assume the savings from purchasing in a lower-price locality is proportional to the difference in log home and log border state prices. Assuming the probability one smuggles can be given by a linear probability model, the smuggling equation is

$$P(S_i = 1) \quad = \quad \phi + \alpha(ln(P_h) - ln(P_b)) - \delta ln(D_i) \equiv \rho. \tag{3.5}$$

Using the law of iterated expectations, equation (3.3) becomes

$$\begin{aligned} & \beta_0 + \beta_1(ln(P_h)(1 - P(S_i = 1)) + ln(P_b)P(S_i = 1)) + \gamma X_i \\ = \quad & \beta_0 + \beta_1 ln(P_h) - \beta_1(ln(P_h) - ln(P_b))\rho + \gamma X_i. \end{aligned} \tag{3.6}$$

Equation (3.6) represents a regression of log cigarette consumption on expected price given log distance, log price difference, and ϕ. If ρ equals zero such that the consumer purchases at home with certainty, then only the home price matters. Conversely, if ρ is 1 and the consumer smuggles with certainty, then only the border price matters. Thus, ρ will be identified by determining the degree to which each individual behaves as if they are purchasing cigarettes at the prevailing home or border-locality price.

In previous studies that assume full smuggling in a 20 mile band (Lewit et al., 1981 and Lewit and Coate, 1982), $\rho = 1$ if individuals live within 20 miles of the border and $\rho = 0$ if they do not. Similarly, by using an average price within 25 miles for all consumers, Chaloupka (1991) implicitly sets $\rho = \frac{1}{2}$ for those within 25 miles of a border and assumes $\rho = 0$ for the rest of the sample. My approach provides a less arbitrary and more reasonable account of casual smuggling than previous models as it allows the probability of smuggling (i.e., the weights on home and border state prices) to vary over the entire population based on differences in smuggling incentives.

Substituting equation (3.5) into equation (3.6) yields the reduced form demand equation used throughout this study:

$$\begin{aligned} & \beta_0 + \beta_1 ln(P_h) - \beta_1 \phi(ln(P_h) - ln(P_b)) - \beta_1 \alpha(ln(P_h) - ln(P_b))^2 \\ & +\beta_1 \delta ln(D_i)(ln(P_h) - (P_b)) + \gamma X_i \\ \equiv \quad & \Pi_0 + \Pi_1 ln(P_h) + \Pi_2(ln(P_h) - ln(P_b)) + \Pi_3(ln(P_h) - ln(P_b))^2 \\ & +\Pi_4 ln(D_i)(ln(P_h) - ln(P_b)) + \gamma X_i. \end{aligned} \tag{3.7}$$

Note equation (3.7) is similar to a somewhat simpler adaptation of traditional cigarette demand functions in which one controls for both the log of home and border state price and each log price interacted with log distance. Both the home and border state prices need to be interacted with the distance variable because the relative importance of the two prices changes as you move closer or further from the border, and equation (3.7) imposes the assumption that the coefficients on the interaction terms are equal and opposite in sign.[16] The motivation for the derivation of equation (3.7) is not to derive a demand function that is qualitatively distinct from the simpler adaptation, but to provide structure to the analysis in order to understand more fully why this is the correct model to run and how to interpret each coefficient. Further, the model is specific about how to separate the two elasticities of interest and the assumptions that underlie their calculation.

One concern with the reduced form demand function given by equation (3.7) is distance enters log linearly.[17] This is a potential problem because one might expect the impact of distance on demand to go to zero as distance approaches infinity. The log linear distance term implies as distance becomes arbitrarily large, log demand decreases to negative infinity. While such a critique could be levied against any log linear model, it is important to note the log linearity of distance is a simplifying assumption, and equation (3.7) represents a parametric approximation to the true demand function. To address this problem when

[16]Equation (3.7) also includes a squared difference in log price variable, but this variable is typically not statistically significant; my results are robust to excluding this variable.

[17]Another way to proceed would be to relax the constraints imposed by a log linear distance measure and use a polynomial in distance. This specification is attractive as it allows the relationship between demand and distance to be relatively flexible as distance changes. I estimate demand functions using such a specification, but the small sample sizes in the data do not allow meaningful statistical inferences to be drawn from the results. Taking the point estimates at face value yields results that are similar to the ones presented below.

calculating the home state price elasticities, I constrain the home state price elasticity to be weakly smaller in absolute value than the full price elasticity. In effect, this restricts cross-state purchases to be zero when the cross-border price differential is low and/or the consumer lives far from this border.[18]

3.4.2 Model Predictions

As the model is constructed, the expectation is that δ, ϕ and α are all positive because the probability of smuggling should be decreasing in distance from a lower-price border, increasing in price difference, and increasing in the fixed cost parameter. It is natural to expect β_1 to be negative, which implies $\Pi_1 < 0$, $\Pi_2 > 0$, $\Pi_3 > 0$, and $\Pi_4 < 0$.

Both the home state and full price elasticities can be calculated simply from equation (3.7):

$$\begin{aligned}
\text{Home State Price Elasticity} &= \frac{\partial ln(Q)}{\partial ln(P_h)} \qquad (3.8) \\
&= \Pi_1 + \Pi_2 + 2\Pi_3(ln(P_h) - ln(P_b)) + \Pi_4 ln(D) \\
\text{Full Price Elasticity} &= \frac{\partial ln(Q)}{\partial ln(P_h)}|_{dln(P_h)=dln(P_b)} = \Pi_1. \qquad (3.9)
\end{aligned}$$

The expected signs of Π_1 - Π_4 illustrate the predictions of the model for the home state price elasticity. Conditional on distance, an increase in the price difference should render the home state price elasticity closer to zero. Conversely, an increase in distance to a lower-price border should make demand more responsive to the home state price as the cost of obtaining a given amount of savings has risen.[19]

[18] When I relax this restriction, the home state price elasticities become slightly more negative, but the substantive conclusions and findings reported below do not change. In Appendix B, I perform sensitivity tests by restricting the effect of distance on demand to be zero for those living far away from borders or for whom the savings per mile from smuggling is low. I find these models yield similar results to the simple log linear model above. Distance is entered log linearly in all regression below for simplicity, but my results are robust to more complex relationships between smuggling and distance.

[19] The role of distance in the home state price elasticity illustrates the advantage of using log distance rather than distance. If distance is used in the regression, then the partial derivative of the home state demand elasticity with respect to distance is Π_4. This formula states as distance increases to infinity, the demand elasticity converges to negative infinity. In other words, the impact of distance on quantity demanded is constant regardless of how far one lives from the border. Using log distance, the partial derivative of the home state price elasticity with respect to distance becomes $\frac{\Pi_4}{D}$. With this specification, the impact of distance on consumption decreases with distance. Thus, a one mile increase in distance to a lower-price state will impact the home state demand elasticity more for a consumer living 5 miles from the border than for a consumer living 500 miles from the border.

3.5 Estimation Strategy

I estimate demand functions on the intensive margin (Q=number of cigarettes per day smoked by smokers), extensive margin (Q=smoking participation rate) and full margin (Q=number of cigarettes smoked per day, including non-smokers). I employ state-MSA fixed effects in all regressions, so only within-MSA across-time variation in prices, distance and price differences are used to identify the parameters of the demand function. It is important to use fixed effects in such regressions because individuals may differ across MSAs and across states in their preferences for smoking, conditional on price. For example, people might be less averse to smoking in a tobacco producing state such as Kentucky than in a high anti-smoking sentiment state like Massachusetts. The fact that Massachusetts is a high cigarette tax state and Kentucky is a low cigarette tax state is likely a function of these same preferences. Without fixed effects, demand regressions attribute some of the preference-related smoking differences across states or MSAs to price differences, causing an upwards omitted variables bias in the coefficient on price.[20]

Because I am interested in estimating demand functions, the price changes that occur in the data need to be independent of the unobservables in the quantity demanded equation, conditional on the observable variables included in the model. Keeler et al. (1996) present evidence that such independence may not hold; they find cigarette producers price discriminate by state based on numerous demographic and state legal factors. If prices are a function of the demographic composition of the state and if these demographic factors play a role in preferences for cigarettes, price changes will be endogenous to cigarette demand. It is unlikely I will be able to control for all factors that jointly affect demand and price discrimination. Thus, using state average prices in the demand regressions is likely to lead to biased parameter estimates on the price variables. In order to account for this

[20]One complication with using state or MSA fixed effects is multicollinearity with prices. I run auxiliary regression of home state price on a year trend and state fixed effects and find an R^2 of 0.82. The associated variance inflation factor ($R^2/(1\text{-}R^2)$) is 4.42. A VIF less than 10 is typically considered an acceptable amount of multicollinearity, so the fixed effects are not soaking up all of the variation in price in my regressions.

endogeneity, I instrument all price variables with tax variables.[21] Note taxes are only a valid instrument for prices if state excise taxes are not set in response to differing home state price elasticities.[22]

While most of the data are collected at the individual level, the constraints imposed by the geographic identifiers render the base unit of analysis the state-MSA. Thus, for each of the 12 tobacco supplements, I collapse the data into MSA-specific means using the non-response weights included in the survey data. This aggregation is justified by interpreting the consumer in the model presented in Section 3.4 as the representative or "average" consumer in a given MSA.[23] The aggregated data set contains 2,904 observations at the MSA level. I also weight all regressions by the number of observations that constitute each MSA mean and estimate heteroskedasticity-robust standard errors.[24]

The demographic variables used in the regressions that follow are the state-MSA mean values of age, sex, weekly wage, marital status, race (with white as the excluded category), education (with no high school diploma as the omitted category) and labor force status (with not in the labor force as the omitted category). Means of all variables by year are presented in Table 3.5.

As Table 3.5 illustrates, there is a large decrease in the amount smoked by smokers and a modest decrease in the percentage of smokers over the time span of this analysis. These trends could be due to the price increases that occur over this period, but there are undoubtedly also secular trends stemming from aggregate changes in views and pref-

[21] Using taxes to instrument for prices is also beneficial because the price variation due to cigarette tax changes more likely identifies the demand curve. Much of the evidence on cigarette taxes suggests these taxes are either fully or more than fully passed on to consumers. Coates (1995) regresses real state price on real state taxes for the period 1964 - 1986 and finds a coefficient on tax indistinguishable from unity. Keeler et al. (1996) find a $1 rise in state cigarette taxes leads to a price increase of $1.11. In their review of the literature on this subject, Chaloupka and Warner (2000) conclude such results are common. Using the price data described in the previous section, I regress real state price on real state taxes with state fixed effects and a year trend for 1992-2002. I estimate a coefficient of 1.28 on the tax variable with a standard error of 0.003. While this coefficient is larger than unity, it is consistent with the literature's findings that state taxes are more than fully passed on to consumers. Due to this evidence, I will assume throughout that supply is inelastic and that the parameters estimated in the demand function are not confounding supply and demand. This assumption is prevalent in the literature.

[22] The evidence on how states set cigarette excise taxes, while sparse, supports this assumption. The cross-state variation in excise taxes is driven largely by differences in attitudes towards smoking as well as by economic factors that may lead states to increase excise taxes as a way to raise revenue (ACIR, 1985).

[23] Aggregating the data also allows me to log the dependent variable, which enables simpler elasticity calculations that are not level dependent. However, results are similar when I use the individual responses and cluster the standard errors at the state-MSA level.

[24] Results are qualitatively similar when no weights are used.

erences with respect to smoking. Including a linear year trend in the demand models is thus appropriate. I present estimates both including and excluding the year trend for all specifications.[25]

It is important to note distance does not appear as a separate right hand side variable in equation (3.7). This exclusion comes from the assumption that the distance to a lower-price jurisdiction impacts smuggling but not quantity demanded, conditional on the decision to smuggle. In other words, the model predicts distance does not belong in X. In the regressions below, I include log distance in X as an over-identification test of the exclusion restriction.

Including log distance as a regressor, Equation (3.7) can be interpreted as a specific form of a more general log-linear second order demand function approximation. The second order approximation includes the $ln(P_h)$, $ln(P_h) - ln(P_b)$ and ln(D) terms as well as all squared terms and cross-products. Equation (3.7) is a form of this approximation in which the coefficients on $ln(P_h)^2$, $ln(P_h) * ln(D)$, $ln(P_h) * (ln(P_h) - ln(P_b))$, and $ln(D)^2$ are all zero. I use the demand model given by equation (3.7) as my preferred specification because the coefficients are easier to interpret than in the full second order approximation. However, while there are some quantitative differences, the elasticity estimates from the full second order log linear approximation are qualitatively similar to the ones presented below and are presented in Appendix C, Table 3.C.1.[26] Thus, while the demand model presented in Section 3.4 is useful in providing an interpretation of the regression coefficients, my results are robust to a more general demand function approximation that embodies fewer assumptions.

[25]The results and conclusions are unchanged when I use year fixed effects or survey date fixed effects instead of a linear year trend.

[26]Full regression results are available from the author upon request.

3.6 Results

3.6.1 Coefficient Estimates

Table 3.6 presents the results from estimation of demand function (3.7) above. Panels A–C contain estimates for the intensive, extensive and full demand models, respectively. All three panels contain six columns of results; I control for year trends in even numbered columns only. Columns (i) and (ii) present estimates from the demand model ignoring all smuggling incentives and geographic variability. Such a model is similar to what other researchers have used when studying cigarette demand using micro data and is useful in understanding the impact of accounting for smuggling behavior. Columns (iii) - (vi) contain estimates from the demand model outlined in the previous sections, with the final two columns including Native American Reservations in the price difference and distance variables.

In the specifications that account for smuggling, the coefficient on log real home state price is negative and significant at either the 5 or 10 percent level. As this coefficient also represents the full price elasticity, Table 3.6 illustrates, absent smuggling, there is a consistent negative relationship between price and consumption on the intensive, extensive and full margins.

The coefficient on the difference in log price, log distance interaction variable is a central parameter in this study because it describes how the responsiveness of demand to home state price changes varies with distance to a lower-price border. As equation (3.8) illustrates, the partial derivative of the home state price elasticity with respect to distance is $\frac{\Pi_4}{D}$, where Π_4 is the coefficient on the log distance, difference in log price interaction term. Thus, Π_4 is a major component of the volume and impact of cross-border sales.

In all relevant columns of Table 3.6 (columns (iii)-(vi)), this coefficient is negative and is significant at the 5 percent level in all but the final two columns of Panel B. I estimate this coefficient to be around -0.2 in the intensive and extensive demand models and in the range -0.58 and -0.42 for the full model. Because all variables are in logs, this coefficient represents the percentage change in the home state price elasticity when distance changes by one percent. For example, on the intensive and extensive margins, a one percent increase in distance corresponds to a fall in the home state price elasticity of about -0.2 percent. Thus, both quantity demanded and the home state price elasticity are quite sensitive to the distance to the closest lower-price border.[27]

The coefficient on the difference in log price variable is positive in all specifications but is often not significant at either the 5 or 10 percent level. The estimates range from 0.69 to 1.06 on the intensive and extensive margins and 2.17 to 2.55 on the full margin. Finally, across all specifications in Table 3.6, the coefficient on the difference in log price squared varies in sign but is not statistically significant.

As discussed in section 3.5, the log distance variable does not appear in equation (3.7) as a separate explanatory variable. The inclusion of this coefficient provides an over-identification test that excluding distance from the demand model is appropriate.[28] In all three panels, I find the coefficient on log distance to be small and not statistically significant at the 5 or 10 percent level. This is evidence that changes in distance do not affect consumption if the price difference is zero. In other words, distance is only important insofar as there are smuggling incentives; conditional on the decision to smuggle, distance has no impact on quantity demanded.

[27] One potential bias in identifying the parameter on the log distance, log price difference variable is the existence of internet smuggling. Goolsbee, Lovenheim and Slemrod (2007) find evidence using CPS internet data and taxed state sales of substantial internet smuggling, which would bias my estimates because one would expect as distance to a lower-price locality increases, the likelihood of smuggling over the internet would also increase, *ceteris paribus*. Excluding internet smuggling might cause an overstatement of the estimated impact of distance on demand. To check whether this is the case, I construct a series on internet connectivity by MSA using the October 1989, 1993 and 1997 CPS combined with the December 1998, August 2000, September 2001 and October 2003 CPS Internet Supplements. I use these surveys to construct state-MSA specific means and then smooth the differences evenly over the time between surveys. I then apply the internet connectivity mean for each MSA to the CPS Tobacco Supplement for the relevant month. To test whether ignoring internet connectivity biases my results, I run model (iv) from Table 3.6, Panel C but include internet connectivity and internet connectivity interacted with the price difference, log distance interaction. If the exclusion of the internet is a source of bias, the coefficient on the triple interaction term should be positive and significant. The point estimates on both internet terms are negative, small and not significant. Further, the other coefficients are quite similar to those in Table 3.6. Ignoring internet sales does not bias the results presented above. Results are available from the author upon request.

[28] Log distance is likely to be correlated with $(ln(P_h) - ln(P_b)) * ln(D)$. Thus, although the coefficient on ln(D) is not statistically differentiable from zero, its exclusion from the regression may affect the coefficients on other variables. I estimate the demand model both including and excluding log distance and find no difference in results.

3.6.2 Estimated Elasticities

Table 3.7 summarizes the quantitative results by presenting home state and full price elasticity estimates calculated from the coefficients in Table 3.6. All panels and columns correspond to the same specification from Table 3.6. In columns (i) and (ii), where geographic variability and smuggling incentives are ignored, the home state and full price elasticities are identical by definition. Thus, only the former statistic is shown. Robust standard errors are in parentheses.

The home state price elasticities range from -0.03 to 0.08 on the intensive margin, -0.06 to -0.02 on the extensive margin and -0.11 to 0.06 for the full margin. In no specification are these elasticities differentiable from zero at the 5 or 10 percent level. These numbers imply, on average, in the presence of cross-locality price differentials, home state price changes have a negligible effect on cigarette demand.

The home state price elasticities contrast markedly and statistically significantly with the full price elasticities, which range from -0.18 to -0.10 on the intensive margin, -0.30 to -0.23 on the extensive margin and -0.53 to -0.44 on the full margin. These elasticities are larger in absolute value than the home state price elasticities, and the full margin elasticities are consistent with much of the elasticity estimates from the taxable sales literature.[29] When one adequately controls for cross-border purchases, it is possible for the full price elasticities calculated using micro data to mirror the estimates from the taxable sales literature.

A specific example is illustrative of the difference between the home state and full price elasticities. In the last column of Panel C, the home state price elasticity is 0.025 while the full price elasticity is -0.527. This gap suggests while smoking is unresponsive to changes in the home state price on average in the presence of casual smuggling, if smuggling were eradicated, home state cigarette price elasticities could reduce cigarette consumption. Due to the inelastic nature of the full price elasticity, cigarette taxes could serve as an effective

[29] Chaloupka and Warner (2000) report these studies are consistent in estimating elasticities in a neighborhood of -0.4.

revenue generating mechanism for states as well.

The elasticities in the first two columns range from -0.21 to -0.06 on the intensive and extensive margins and -0.44 to -0.33 on the full margin. They are generally consistent in magnitude and sign with other studies using individual consumption data with fixed effects (Farrelly et al., 2001; Farrelly and Bray, 1998; Coleman and Remler, 2004). In all three panels of Table 3.7, a comparison of the first two columns with the last four columns illustrates ignoring geographic variability causes one to overstate the home state price elasticity and understate the full price elasticity in absolute value, though the "naive" elasticity estimates are often quite close to and are not statistically different from the full price elasticities. The implication of this finding is ignoring smuggling incentives when using micro-data will not produce large biases in estimates of the full price elasticity on average. However, one will also not be able to estimate the home state price elasticity, which is arguably the more important parameter from a state tax policy perspective as it yields the actual effect of a tax increase in a given state rather than the potential effect absent smuggling.

3.6.3 Smoking Increases, Casual Smuggling Percentages, and Net Revenue Losses

Because cross-state price differentials offer consumers access to lower-priced cigarettes, casual smuggling can increase cigarette consumption. I calculate smoking increases due to the effective price reduction from smuggling by comparing the predicted value from each regression to the predicted value from a counterfactual in which there is no casual smuggling. This counterfactual is constructed by setting the price difference equal to zero, as then there are no incentives for cross-border purchases.[30] More explicitly:

$$\text{Percent Change in } Q = \frac{E[Q|X, P_h = p_h, P_b = p_b, D = d] - E[Q|X, P_h = P_b, D = d]}{E[Q|X, P_h = p_h, P_b = p_b, D = d].} \tag{3.10}$$

Due to the functional form of the demand function, the above expression can be negative

[30] This part of the analysis assumes the eradication of smuggling incentives has no general equilibrium effect on cigarette prices.

for those who live very far from the border or for whom the price difference is quite small. To correct for this problem, I set the percent change equal to zero if it is negative. Note this adjustment produces similar results to constraining the home state price elasticity to be weakly greater than the full price elasticity: those who live far from lower-price borders are assumed to not smuggle. The third row of each panel in Table 3.7 contains estimates of the percent increase in smoking due to smuggling. Cross-border purchases increase consumption by between 1.5 and 2.5 percent on the intensive margin and between 4.0 and 8.2 percent for the full model. Further, the availability of cheaper cigarettes increases the smoking participation rate by 2.0-4.3 percent.

The demand model given by equation (3.7) also allows me to calculate the proportion of individuals who purchase cigarettes in border localities in a given MSA. I assume if everyone lived directly on the border, no one would purchase in the higher price state. Comparing consumption for such individuals with consumption for those who do not live close to the border yields the percentage of consumers who smuggle:

$$\text{Smuggling Percentage} = \frac{E[Q|X,P_h=p_h,P_b=p_b,ln(D)=ln(d)]-E[Q|X,P_h=P_b,ln(D)=ln(d)]}{E[Q|X,P_h=p_h,P_b=p_b,ln(D)=0]-E[Q|X,P_h=P_b,ln(D)=ln(d)].} \tag{3.11}$$

If everyone behaves as if they live on the border, so $E[Q|X, P_h = p_h, P_b = p_b, ln(D) = ln(d)] = E[Q|X, P_h = p_h, P_b = p_b, ln(D) = 0]$, then the above equation implies 100 percent smuggling. If, on the other hand, everyone behaves as if they purchase from their home state (meaning that the price difference is zero), then $E[Q|X, P_h = p_h, P_b = p_b, ln(D) = ln(d)] = E[Q|X, P_h = P_b, ln(D) = ln(d)]$, and there will be zero smuggling. The smuggling percentage is the ratio of these two quantities. I am essentially determining the extent to which individuals behave as if they live in the home state and face only the border price or live in the home state and face only the home state price. I perform this calculation only for the full demand model, as the statistic does not have the same interpretation if applied to the intensive or extensive margins. Results are presented in Panel C of Table 3.7.

I find evidence of large amounts of cross-border purchases. Depending on the specification, the above calculation implies between 13 and 25 percent of consumers in MSAs purchase cigarettes in a lower-price state or reservation. The estimates including Native American Reservations are much larger due to the reduction in traveling distance and price when these jurisdictions are included (see Table 3.5 and Figure 3.1). It is important to note these percentages can only be generalized to the United States as a whole if the distribution of distance with respect to lower-price borders for MSAs are representative of the distribution for non-MSAs. It is unclear whether the above estimates are smaller or larger than they would be for the United States as a whole, and the reader is urged to use caution when applying these estimates out of sample.

Under the assumption cross-state purchasers smoke the same amount as those who purchase cigarettes in their home state, the smuggling percentage also can be interpreted as the proportion of consumed cigarettes that are purchased in border localities. My estimates imply consumers who smuggle will smoke more than those who do not. Thus, the smuggling percentage represents a lower bound on the percentage of cigarettes that are casually smuggled. When interpreted in this manner, these estimates are strikingly large, particularly in light of previous estimates of casual smuggling under 1% (Stehr, 2005). It is therefore important to determine whether these estimates are plausible given the potential savings from smuggling and the distribution of consumers with respect to lower-price borders.

Figure 3.1 presents a histogram of the distance variable for the CPS MSA sample, both with and without Native American Reservations. The width of each bar is ten miles. These histograms illustrate the distribution of the population with respect to lower-price borders is heavily skewed towards zero. Although the mean of distance is 93 miles excluding Native American Reservations and 68 miles including Native American Reservations, the median of these variables is 65 and 45 miles, respectively.

There are some sources of validation for this finding in New York State. The Center for a Tobacco-Free New York conducted a survey and found 25 percent of New York State residents purchased cigarettes on a Native American Reservation (FACT Alliance, 2005). Further, the New York Association of Convenience Stores found Western New York cigarette sales dropped between 25 and 50 percent after the 2000 tax increase (FACT Alliance, 2005). There is further anecdotal evidence of high volumes of casual smuggling: when South Dakota increased its cigarette excise tax by $1.00 in January 2007, Larchwood Mini Mart in Iowa reported its January cigarette sales tripled their total sales for 2006. One consumer reported she makes the 20 mile trip from Sioux Falls once or twice a week (Efrati, 2007).

Together with the average price differences listed in Tables 3.2 and 3.5, the distance distributions in Figure 3.1 are consistent with the large predicted smuggling amounts, as there are substantial potential savings from purchasing cigarettes in the nearest lower-price locality. For example, in the 2001-2002 CPS supplements, the median person living in an MSA lived approximately 49 miles from a lower-price border state or reservation. The average per-pack price difference faced by consumers was $0.45 (a little over 12 percent of the average real home state price). As the average smoker smoked 15 cigarettes per day (0.75 of a pack), she would save $123.19 per year by purchasing all of her cigarettes in a border locality and not changing her smoking behavior. This is a fairly substantial amount of average savings given most individuals need only travel 50 miles or less 1 or 2 times a year to realize them.[31] The large amount of casual smuggling implied by the empirical estimates is consistent with many consumers taking advantage of the savings from purchasing in lower-priced jurisdictions.

Table 3.8 presents similar information to Table 3.7 broken down by state for the full model. The estimates are derived from column (iv) of Table 3.6, so they exclude Native American Reservations but include a year trend. Table 3.8 illustrates the large differences across states in the responsiveness of consumption to changes in the home state price

[31] This calculation is based on an average cigarette shelf life of 8 months (Wong, Ashcraft and Miller, 1991). They report the shelf life of "normal cigarettes."

as well as in the percent of consumers who engage in casual smuggling. These results underscore the importance of accurately accounting for smuggling incentives in cigarette demand models; the "naive" elasticity estimate of -0.326 in Column (ii), Panel C of Table 3.6 provides a poor estimate of the home state price elasticity in many states.

The casual smuggling estimates presented in Table 3.8 vary from a high of 63 percent in Washington, DC to a low of 0 percent in Delaware, Idaho, Kentucky, Missouri, New Hampshire and New Mexico. The large value for DC occurs because it is 3 miles from Virginia and there is an average difference of $0.80 per pack between the two locations. Given the location of their MSAs with respect to lower-price borders, at least 25 percent of consumers in Arkansas, Massachussetts, Maryland, New Jersey, Rhode Island, and West Virginia are estimated to engage in smuggling activity. The home state price elasticities reflect these differences, with the low-smuggling states being more home price elastic than the high smuggling states. Similar patterns emerge for the impact of smuggling on smoking.

Using the MSA-specific estimates of the percent of consumers that casually smuggle combined with information on the closest lower-price locality, I calculate the net percent change in sales for each state due to cross-border purchasing activity.[32] Results are reported in the final column of Table 3.8 and suggest there are clear winners and losers from the existence of interstate price differentials. At the extreme, New Hampshire sales double because they are the lowest-tax state in New England. Virginia, Indiana, Kentucky, and Delaware also gain substantial sales from cigarette tax evaders. Conversely, Maryland, Kansas, Massachusetts, and Illinois lose significant sales (and thus tax revenue) due to the availability of lower-price cigarettes in nearby jurisdictions. These results imply in the states with large quantities of smuggling and inelastic home state price elasticities, cigarette taxes are ineffective at both reducing smoking of residents and providing substantial tax revenue to the home state. Instead, these taxes often serve to export both consumers and

[32]For each MSA, I multiply the smuggling percentage by the number of cigarettes smoked. Summing this number within states gives the total number of consumed cigarettes purchased in another jurisdiction. I then attribute these purchases to the closest lower-price state for each MSA to find the sales increases due to smuggling in each state. The denominator in each calculation is total consumed cigarettes in each state.

tax revenues to nearby states.

Table 3.8 is useful from a policy perspective as it indicates the states in which cigarette taxes would be most effective at raising tax revenue and/or reducing smoking behavior. For example, in New Mexico, where there is virtually no smuggling, cigarette taxes will be a more effective policy instrument in reducing smoking than in New Jersey, where over 30 percent of consumers purchase cigarettes in border localities. Interestingly, the cigarette tax in New Mexico was \$0.59 per pack lower than in New Jersey as of 2002. It is a topic for further study whether this is indicative of sub-optimal cigarette taxation at the state level.

3.7 Conclusion

Using data from the Current Population Survey Tobacco Supplement for four years over the period 1992-2002, this paper has developed and estimated a cigarette demand model that explicitly accounts for cross-border purchases. Unlike previous studies using individual consumption data, I am able to distinguish between the elasticity with respect to the home state price and the elasticity with respect to the full price of cigarettes, both of which are important parameters in setting effective state cigarette tax policy. The evidence presented above suggests cross-border sales are significantly more prevalent than suggested by previous work (Stehr, 2005); across all specifications and margins of demand, I consistently find cigarette demand becomes more elastic with respect to the home state price the farther one lives from a lower-price border.

My estimates imply increasing state cigarette taxes has little impact on smoking behavior on average; the home state price elasticity of demand is modest in magnitude across the majority of specifications. In fact, in all specifications, the home state price elasticity is indistinguishable from zero. There is, however, a large amount of heterogeneity across states in the effect of tax increases on consumption that is based on the geographic distribution of the population. In contrast, my findings suggest the full price elasticities are negative and of sizeable magnitude, though also inelastic.

Using the parameters from my demand model, I am able to estimate directly the percent of consumers that purchase in a lower-price jurisdiction as well as the net change in sales due to such behavior. My results indicate between 13 and 25 percent of consumers purchase cigarettes in a lower-price state or Native American Reservation. These estimates represent a lower bound on the percentage of cigarettes purchased in border localities. Further, I find significant heterogeneity across states in the sales and revenue effects of casual smuggling.

The large magnitude of smuggling combined with the inelastic home state price elasticities suggest state-level cigarette taxation may be a poor policy instrument with which to decrease smoking and increase home state tax revenues in many states. However, that the full price elasticities are negative and significant across all specifications implies state-level cigarette excise taxes could be a useful tool to change smoking behavior and raise revenue if smuggling were eradicated. Slemrod (2007) finds reducing organized smuggling incentives through a cigarette stamping law in Michigan had just such an effect.

The central implication of this study is, while cigarette taxes are ineffective in many states at achieving the goals for which they were levied, there are significant potential gains from price increases that are confounded by cross-border sales. From a policy standpoint, states may be better served by expending resources to reduce casual smuggling or by lowering the excise tax to reduce the smuggling incentives supplied by a positive border price differential. In the absence of such policies, differential price increases across states will continue to be counterproductive for many states attempting to decrease smoking behavior and increase tax revenues.

Table 3.1: States that Tax Cigarette Sales to Non-Tribal Members on Native American Reservations

State	Statute/Case Name	Year
Arizona	A.R.S. 42-3302	1997
Kansas	State v. Oyler	1990
Michigan	MCLS 205.30c/Individual Tribal Compacts	1947
Minnesota	Minn. Statute 297F.07/Individual Tribal Compacts	1997/Pre-1992
Montana	Moe v. Confederated Salish and Kootenai	1976
Nebraska	Nebraska Department of Revenue (1996)	Pre-1992
Nevada	NRS 370.280	1947
Oklahoma	Okl. St. 349	Pre-1992
Oregon	ORS 323.401	1979
South Dakota	Individual Tribal Compacts	Pre-1992
Washington	Washington v. Confederated Colville Tribes	1980
Wisconsin	Wis. Stat. 139.323/Individual Tribal Compacts	1984

Source: ACIR (1985) updated using LexisNexis searches for state cigarette taxation laws.

Table 3.2: Tax Changes, Price Differentials, and Distance to a Lower-price Locality by State

Home State	Average Home State Tax	Tax Changes	Closest Lower Price Jurisdictions	Average Distance (miles)	Average Price Difference	Average Tax Difference
AL	0.30	0	GA,MS,TN	50.2	0.19	0.08
AR	0.45	3	MO,MS,OK	65.4	0.14	0.13
AZ	0.69	1	CA,NM,NV,NAR	85.5	0.50	0.47
CA	0.84	2	AZ,NV,NAR	72.8	0.78	0.78
CO	0.30	0	KS,NM,OK,WY,NAR	113.8	0.13	0.12
CT	0.72	1	MA,NH,NJ,NY PA,RI,VT,NAR	25.7	0.60	0.59
DC	0.86	1	VA	3.5	0.80	0.73
DE	0.27	0	NC,VA	118.4	0.10	0.13
FL	0.53	0	AL,GA,NAR	52.7	0.47	0.47
GA	0.20	0	NC,SC,NAR	91.7	0.08	0.04
IA	0.53	0	IL,MO,NE,NAR	52.9	0.46	0.46
ID	0.41	1	MT,NAR	101.7	0.41	0.41
IL	0.70	2	IA,IN,MO,WI	29.3	0.49	0.39
IN	0.29	0	KY	108.6	0.11	0.12
KS	0.39	0	KY,MO,NC,OK	124.3	0.13	0.12
KY	0.16	0	VA,WV,NAR	204.3	0.13	0.12
LA	0.32	1	AR,GA,MO,MS,NAR	64.2	0.25	0.23
MA	0.80	2	CT,NH,RI	11.9	0.53	0.37
MD	0.65	1	PA,VA,WV	20.42	0.36	0.31
ME	0.80	2	NH	32.4	0.41	0.39
MI	0.82	1	IN,OH	61.2	0.65	0.47
MN	0.72	0	IA,ND,WI	71.2	0.25	0.16
MO	0.27	1	KS,KY	204.4	0.13	0.10
MS	0.36	0	LA,TN,NAR	44.4	0.12	0.12
NC	0.14	0	KY,SC,VA,NAR	105.1	0.09	0.08
ND	0.63	1	SD,NAR	63.2	0.63	0.63
NE	0.48	1	IA,KS	45.0	0.06	0.03
NH	0.42	2	DE,VA,NAR	110.1	0.42	0.42
NJ	0.79	1	CT,DE,NY,PA	24.0	0.33	0.24
NM	0.34	1	CO,WY,NAR	36.4	0.34	0.34
NV	0.57	0	AZ,ID,OR,UT,NAR	188.8	0.50	0.50
NY	0.76	2	CT,NJ,PA,VT,NAR	26.0	0.50	0.44
OH	0.38	1	IN,KY,WV	78.3	0.11	0.12
OK	0.37	0	KS,MO	122.0	0.11	0.06
OR	0.57	2	CA,ID,NV	274.5	0.23	0.13
PA	0.49	0	DE,OH,WV	38.3	0.17	0.20
RI	0.90	3	CT,MA,NH,NAR	16.0	0.19	0.27
SC	0.19	0	GA,KY,NC,NAR	54.8	0.09	0.07
SD	0.43	1	IA,MO,ND,NAR	138.0	0.36	0.33
TN	0.33	0	GA,KY,MO,NC,VA,NAR	46.3	0.24	0.18
TX	0.63	0	LA,NM,OK,NAR	116.5	0.44	0.43
UT	0.56	1	WY,NAR	43.0	0.56	0.56
VA	0.13	0	KY,NC,WV,NAR	59.8	0.13	0.05
VT	0.58	1	NH	61.2	0.22	0.17
WA	1.00	3	ID,OR	118.6	0.64	0.44
WI	0.70	3	IA,IL,MI,MN,NAR	43.1	0.42	0.37
WV	0.33	0	KY,OH,VA	43.4	0.08	0.08

[1] Source: author's calculation as described in the text.
[2] Prices and taxes are in real 2001 dollars. Closest lower-price jurisdictions are all localities that have a lower-price than the home state at some time during the sample period: "NAR" refers to Native American Reservations.

Table 3.3: Mean Log Cigarette Residuals From Cigarette Demand Models Excluding Smuggling Variables, by Distance Quartile

Independent Variable	First Quartile	Second Quartile	Third Quartile	Fourth Quartile
Full Log Cig Residual	0.005	0.003	-0.011	-0.005
	(0.329)	(0.334)	(0.345)	(0.359)
Intensive Log Cig Residual	0.004	0.0002	-0.008	-0.010
	(0.176)	(0.185)	(0.191)	(0.215)
Extensive Log Cig Residual	0.0003	-0.0001	-0.0005	0.0007
	(0.026)	(0.035)	(0.031)	(0.032)

[1] The table shows mean residuals from a regression of log mean MSA cigarette consumption on log home state cigarette price and mean MSA demographic characteristics by quartile of distance to a lower-price locality. Standard deviations are in parentheses.

[2] All regressions include fixed effects for each unique state-MSA combination and are weighted by the number of observations that constitute each MSA-level mean. MSA means of the following variables are included in the regressions: age, percent male, percent married, weekly wage, percent Black, percent Native American, percent Hispanic, percent Asian, percent high school diploma, percent some college, percent associates degree, percent BA, percent graduate school, percent working, and percent unemployed as well as a linear year trend. Full regression estimates are available from the author upon request.

[3] Home state cigarette prices are instrumented with home state cigarette taxes in all regressions.

Table 3.4: IV Estimates of Full Cigarette Demand For Those Living in Border MSAs that Split State Lines

Independent Variable	High Side	Low Side
Log Home State Price	-0.173	-0.496**
	(0.172)	(0.211)
Log Border State Price	-0.350**	-0.213
	(0.131)	(0.255)

[1] The table shows estimates from a regression of log mean MSA cigarette consumption on log home state or log nearest lower-price border state cigarette prices and mean MSA demographic characteristics for MSAs that are split by a state border.

[2] All regressions include fixed effects for each unique state-MSA combination and are weighted by the number of observations that constitute each MSA-level mean. MSA means of the following variables are included in the regressions: age, percent male, percent married, weekly wage, percent Black, percent Native American, percent Hispanic, percent Asian, percent high school diploma, percent some college, percent associates degree, percent BA, percent graduate school, percent working, and percent unemployed as well as a linear year trend. Full regression estimates are available from the author upon request.

[3] Home state cigarette prices are instrumented with home state cigarette taxes.

[4] Robust standard errors are in parentheses: * indicates 10 percent significance and ** indicates 5 percent significance.

Table 3.5: Means of Selected CPS Variables by Year

Variable	1992-1993	1995-1996	1998-1999	2001-2002
Cigarettes per Day (all)	3.25 (1.00)	3.01 (1.07)	2.59 (0.94)	2.29 (1.00)
Cigarettes per Day (smokers)	16.91 (2.43)	16.60 (2.84)	15.77 (2.80)	14.83 (2.82)
Percent Smokers	0.23 (0.05)	0.22 (0.05)	0.20 (0.05)	0.19 (0.05)
Real Home State Price	2.27 (0.245)	2.26 (0.30)	2.83 (0.41)	3.67 (0.45)
Price Difference (without Native American Reservations)	0.21 (0.14)	0.26 (0.24)	0.31 (0.25)	0.35 (0.27)
Price Difference (with Native American Reservations)	0.29 (0.19)	0.35 (0.26)	0.43 (0.31)	0.45 (0.32)
Real Home State Tax	0.48 (0.16)	0.54 (0.23)	0.63 (0.29)	0.68 (0.32)
Tax Difference (without Native American Reservations)	0.14 (0.14)	0.21 (0.19)	0.26 (0.22)	0.28 (0.22)
Tax Difference (with Native American Reservations)	0.24 (0.21)	0.30 (0.23)	0.40 (0.30)	0.40 (0.31)
Distance (without Native American Reservations)	89.56 (86.11)	91.74 (87.43)	93.02 (85.68)	98.94 (96.92)
Distance (with Native American Reservations)	65.88 (66.44)	62.95 (63.07)	67.91 (65.49)	74.41 (80.95)
Percent Closest to Native American Reservations	0.29 (0.45)	0.29 (0.46)	0.36 (0.48)	0.32 (0.47)
Age	42.67 (2.37)	42.89 (2.40)	42.95 (2.51)	43.15 (2.54)
Percent Male	0.53 (0.03)	0.52 (0.03)	0.52 (0.03)	0.52 (0.03)
Percent Married	0.55 (0.06)	0.55 (0.07)	0.53 (0.06)	0.53 (0.06)
Weekly Wage	70.85 (43.80)	76.29 (22.52)	87.71 (26.74)	72.06 (51.520)
Percent Black	0.13 (0.11)	0.13 (0.12)	0.13 (0.11)	0.12 (0.12)
Percent Native American	0.003 (0.008)	0.005 (0.01)	0.006 (0.01)	0.006 (0.01)
Percent Hispanic	0.08 (0.11)	0.09 (0.12)	0.10 (0.13)	0.10 (0.12)
Percent Asian	0.02 (0.03)	0.03 (0.04)	0.03 (0.04)	0.03 (0.04)
Percent HS Diploma	0.32 (0.06)	0.31 (0.06)	0.30 (0.06)	0.29 (0.06)
Percent Some College	0.18 (0.05)	0.19 (0.04)	0.19 (0.04)	0.19 (0.05)
Percent Associates Degree	0.06 (0.02)	0.07 (0.03)	0.07 (0.03)	0.07 (0.03)
Percent BA	0.14 (0.04)	0.16 (0.05)	0.16 (0.05)	0.17 (0.05)
Percent Graduate School	0.07 (0.03)	0.08 (0.03)	0.08 (0.04)	0.09 (0.04)
Percent Work	0.61 (0.06)	0.63 (0.07)	0.65 (0.06)	0.65 (0.06)
Percent Unemployed	0.05 (0.02)	0.04 (0.02)	0.03 (0.02)	0.04 (0.02)

Source: Current Population Survey Tobacco Supplements. Means include individuals living in an identified MSA only. Standard deviations of each variable are in parentheses.

Table 3.6: IV Estimates of Cigarette Demand Models

Panel A: Intensive Margin
Dep. Var. = Log Mean Cigarette Consumption of Smokers

Independent Variable	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Log Real Home	-0.148**	-0.058	-0.175**	-0.115*	-0.150**	-0.098
State Price	(0.023)	(0.048)	(0.022)	(0.059)	(0.024)	(0.071)
Difference in	.	.	0.882*	0.767	0.748*	0.690
Log Price	.	.	(0.494)	(0.481)	(0.452)	(0.445)
Difference in Log	.	.	0.449	0.546	0.369	0.412
Price Squared	.	.	(0.818)	(0.808)	(0.786)	(0.772)
Log Distance	.	.	0.003	0.004	0.003	0.003
	.	.	(0.007)	(0.007)	(0.009)	(0.009)
Log Distance x Diff-	.	.	-0.226**	-0.213**	-0.180**	-0.177**
erence in Log Price	.	.	(0.080)	(0.079)	(0.091)	(0.090)
Year	.	-0.013**	.	-0.007	.	-0.006
	.	(0.004)	.	(0.005)	.	(0.006)

Panel B: Extensive Margin
Dep. Var. = Log Mean Smoking Participation Rate

Independent Variable	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Log Real Home	-0.210**	-0.176**	-0.231**	-0.227**	-0.229**	-0.295**
State Price	(0.030)	(0.061)	(0.029)	(0.084)	(0.034)	(0.104)
Difference in	.	.	1.059	1.051	0.836	0.913
Log Price	.	.	(0.673)	(0.662)	(0.633)	(0.644)
Difference in Log	.	.	-0.393	-0.388	-0.639	-0.128
Price Squared	.	.	(1.049)	(1.036)	(0.960)	(0.122)
Log Distance	.	.	0.004	0.004	-0.015	-0.016
	.	.	(0.008)	(0.008)	(0.011)	(0.011)
Log Distance x Diff-	.	.	-0.211**	-0.210**	-0.120	-0.208*
erence in Log Price	.	.	(0.108)	(0.106)	(0.121)	(0.128)
Year	.	-0.005	.	-0.0006	.	0.009
	.	(0.006)	.	(0.008)	.	(0.010)

Panel C: Full Margin
Dep. Var. = Log Mean Cigarette Consumption of All Individuals

Independent Variable	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Log Real Home	-0.437**	-0.326**	-0.489**	-0.457**	-0.444**	-0.527**
State Price	(0.046)	(0.093)	(0.045)	(0.125)	(0.052)	(0.154)
Difference in	.	.	2.547**	2.483**	2.171**	2.269**
Log Price	.	.	(1.064)	(1.044)	(0.892)	(0.894)
Difference in Log	.	.	0.114	0.151	-0.416	-0.435
Price Squared	.	.	(1.722)	(1.703)	(1.391)	(1.384)
Log Distance	.	.	0.011	0.011	-0.010	-0.011
	.	.	(0.011)	(0.012)	(0.016)	(0.017)
Log Distance x Diff-	.	.	-0.584**	-0.576**	-0.420**	-0.430**
erence in Log Price	.	.	(0.161)	(0.158)	(0.171)	(0.173)
Year	.	-0.017**	.	-0.004	.	0.011
	.	(0.009)	.	(0.012)	.	(0.015)

[1] Source: estimation of equation (3.7) using the 1992-2002 Current Population Survey Tobacco Supplements. Only those living in identified MSAs are included in the regressions

[2] All regressions include fixed effects for each unique state-MSA combination and are weighted by the number of observations that constitute each MSA-level mean. MSA means of the following variables are also included in the regressions: age, percent male, percent married, weekly wage, percent Black, percent Native American, percent Hispanic, percent Asian, percent high school diploma, percent some college, percent associates degree, percent BA, percent graduate school, percent working, and percent unemployed. Full regression estimates are available upon request.

[3] Price variables are instrumented with tax variables as described in the text.

[4] Robust standard errors are in parentheses: * indicates significance at the 10 percent level and ** indicates significance at the 5 percent level.

Table 3.7: Price Elasticities, Smoking Increases, and Smuggling Percentages Implied by Parameter Estimates in Table 3.6

Panel A: Intensive Margin						
Elasticity	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Mean Home State	-0.148**	-0.058	-0.029	0.004	0.047	0.076
Price Elasticity	(0.023)	(0.048)	(0.111)	(0.115)	(0.087)	(0.092)
Mean Full Price	.	.	-0.175**	-0.115*	-0.150**	-0.098
Elasticity	.	.	(0.022)	(0.059)	(0.024)	(0.071)
Percentage Increase in Smoking Due to Smuggling	.	.	1.516	1.211	2.543	2.164

Panel B: Extensive Margin						
Elasticity	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Mean Home State	-0.210**	-0.176**	-0.063	-0.061	-0.018	-0.045
Price Elasticity	(0.030)	(0.061)	(0.137)	(0.140)	(0.113)	(0.115)
Mean Full Price	.	.	-0.231**	-0.227**	-0.229**	-0.295**
Elasticity	.	.	(0.029)	(0.084)	(0.034)	(0.104)
Percentage Increase in Smoking Due to Smuggling	.	.	2.036	2.007	3.670	4.277

Panel C: Full Margin						
Elasticity	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Mean Home State	-0.437**	-0.326**	-0.105	-0.088	0.059	0.025
Price Elasticity	(0.046)	(0.093)	(0.224)	(0.229)	(0.170)	(0.175)
Mean Full Price	.	.	-0.489**	-0.457**	-0.444**	-0.527**
Elasticity	.	.	(0.045)	(0.125)	(0.052)	(0.154)
Percentage Increase in Smoking Due to Smuggling	.	.	4.154	3.972	7.520	8.172
Smuggling Percentage	.	.	13.405	13.068	24.048	25.071

[1] Source: elasticity estimates come from the Author's calculation of equations (3.8) and (3.9) in the text using parameter estimates from Table 3.6. Smoking increases are calculated from equation (3.10) in the text and smuggling percentages from equation (3.11) in the text using the parameter estimates from Table 3.6 as well.

[2] All means in the table are calculated over state-MSA and year and are weighted by the number of observations that constitutes each state-MSA observation.

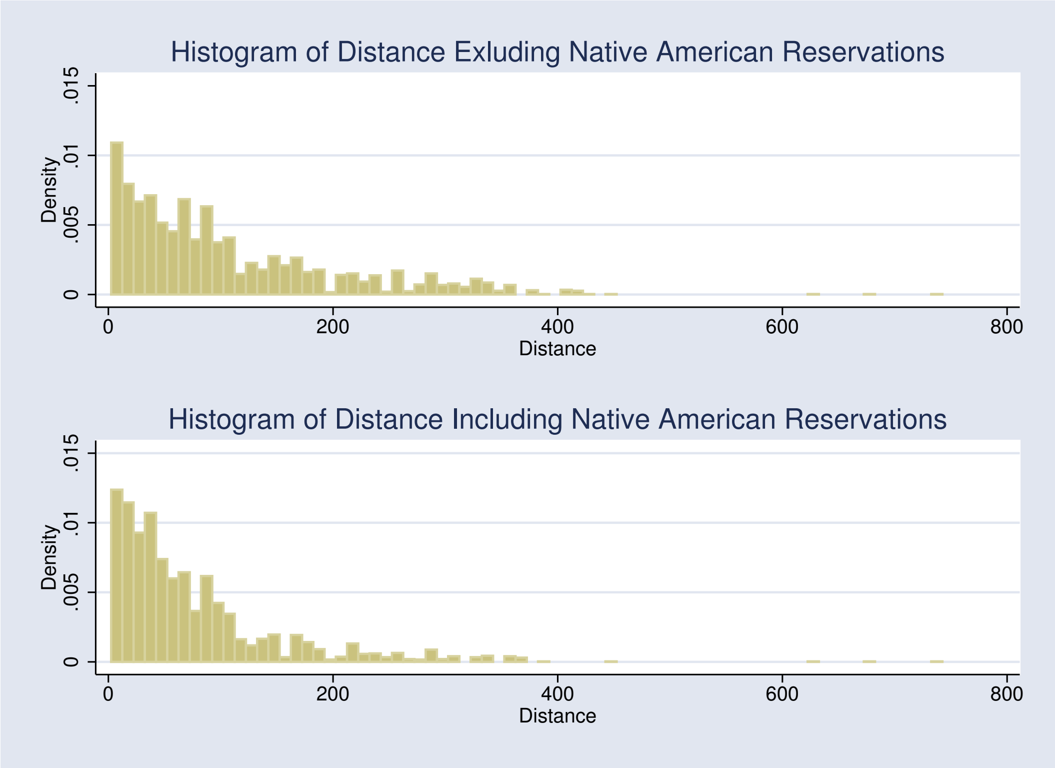
[3] Robust standard errors are in parentheses: * indicates significance at the 10 percent level and ** indicates significance at the 5 percent level.

Table 3.8: Full Price Elasticities, Smoking Increases and Smuggling Percentages by State

Home State	Home State Price Elasticity	Full Price Elasticity	Percent Increase in Smoking Due to Smuggling	Percent of Consumers Who Smuggle	Percent Change in Net Sales Due to Smuggling
Alabama	-0.071	-0.457	2.52	18.66	-7.44
Arkansas	-0.034	-0.457	3.51	24.85	-16.04
Arizona	-0.427	-0.457	0.79	6.53	5.71
California	-0.455	-0.457	0.01	0.01	0.36
Colorado	-0.414	-0.457	0.45	3.93	-1.37
Connecticut	-0.186	-0.457	2.06	12.68	42.47
Washington DC	1.398	-0.457	41.80	63.48	-63.48
Delaware	-0.457	-0.457	0.00	0.00	52.32
Florida	-0.357	-0.457	1.66	6.49	-4.34
Georgia	-0.367	-0.457	0.79	11.54	10.68
Iowa	-0.283	-0.457	0.88	7.26	49.70
Idaho	-0.457	-0.457	0.00	0.00	8.76
Illinois	0.210	-0.457	6.09	16.31	-15.83
Indiana	-0.240	-0.457	2.17	16.03	53.79
Kansas	0.271	-0.457	3.92	21.21	-24.91
Kentucky	-0.457	-0.457	0.00	0.00	61.33
Louisiana	-0.358	-0.457	0.05	2.54	1.88
Massachusetts	0.329	-0.457	15.45	36.66	-20.24
Maryland	0.402	-0.457	12.73	35.65	-29.18
Maine	0.068	-0.457	7.70	17.02	-17.02
Michigan	-0.223	-0.457	6.94	8.62	-5.95
Minnesota	-0.149	-0.457	2.50	11.35	-11.33
Missouri	-0.457	-0.457	0.00	0.00	35.55
Mississippi	-0.220	-0.457	0.40	9.12	55.17
North Carolina	-0.332	-0.457	0.23	5.55	6.71
North Dakota	-0.355	-0.457	0.68	3.38	-2.53
Nebraska	0.171	-0.457	0.86	19.38	-21.09
New Hampshire	-0.457	-0.457	0.00	0.00	104.21
New Jersey	0.377	-0.457	10.57	31.03	-6.53
New Mexico	-0.457	-0.457	0.00	0.00	10.86
Nevada	-0.341	-0.457	1.09	2.67	-4.60
New York	0.308	-0.457	6.45	19.62	-16.88
Ohio	-0.166	-0.457	1.81	13.02	-3.63
Oklahoma	-0.439	-0.457	0.06	0.70	10.44
Oregon	-0.453	-0.457	0.08	0.47	2.51
Pennsylvania	0.041	-0.457	2.44	13.07	0.44
Rhode Island	0.456	-0.457	4.85	34.85	-20.39
South Carolina	-0.111	-0.457	1.08	14.46	-6.15
South Dakota	-0.244	-0.457	0.49	7.71	-5.48
Tennessee	-0.022	-0.457	5.03	20.41	-6.62
Texas	-0.335	-0.457	1.62	5.69	-3.69
Utah	-0.270	-0.457	1.80	4.42	-6.01
Virginia	-0.244	-0.457	1.40	8.46	65.54
Vermont	-0.317	-0.457	1.24	4.55	18.10
Washington	-0.277	-0.457	7.93	11.84	-5.62
Wisconsin	-0.214	-0.457	0.89	8.63	1.98
West Virginia	0.108	-0.457	1.95	26.15	35.16

[1] Source: elasticity estimates come from the Author's calculation of equations (3.8) and (3.9) in the text using parameter estimates from Panel C, column (iv) of Table 3.6. Smoking increases are calculated from equation (3.10) in the text and smuggling percentages from equation (3.11) in the text using the parameter estimates from Panel C, column (iv) in Table 3.6 as well.
[2] All estimates are for years in which a state is not the lowest priced state

Figure 3.1: Distribution of Distance to a Lower-price Border



Source: Author's calculations as described in the text.

Appendix A: Native American Reservation Tax Enforcement Regimes and Data

The responsibility for remitting cigarette excise taxes to the state governments falls on the cigarette wholesalers who sell directly to vendors. This method of taxation is more efficient than allowing individual vendors to remit the taxes as there are fewer wholesalers, making them easier to monitor. The method of enforcement for the collection of non-tribal sales and excise taxes on reservations mostly works through the wholesalers as well. For example, in Michigan, most of the individual tribal compacts allow the tribe to choose a state-approved wholesaler from whom to purchase non-taxed cigarettes. Tribes can either purchase a fixed quota per year or negotiate a tax refund ceiling with the state. In either case, they must provide proof that all sales of tax-free cigarettes were made to tribal members. In Oklahoma, tribal wholesalers remit a tax of 75 percent of the full per pack tax. If a tribe can show proof that more than 25 percent of its sales were to tribal members, they receive a proportional refund from the state. In Minnesota, each tribal compact requires the wholesaler to remit the full amount of the tax, and the tribes are responsible for submitting proof of sales to tribal members to obtain a tax refund from the state for those sales.

These examples underscore the differences across states in the method of tax enforcement for sales on Native American Reservations. It is reasonable to expect these differences to have varying implications for the effectiveness of taxing these sales. For example, a state like Minnesota may have less illegal sales on their reservations than Michigan as Minnesota tribes must apply for a refund on all tribal sales whereas Michigan tribes receive a fixed quota of tax free cigarettes. The exclusion of reservations in these states from the analysis may be extreme as some illegal tax-free sales may still occur depending on the level and effectiveness of enforcement. As noted in the main text, these cross-state differences in enforcement regimes are not included in the analysis, predominantly because of sample size restrictions.

There is no published price series on Native American Reservation cigarette prices. Because these reservations are allowed to sell cigarettes tax free, I apply pre-tax state average prices to all reservations within a given state; the savings for an individual who purchases cigarettes on a reservation in their home state is the tax.[33] There are reasons to doubt the pre-tax price is the correct price to apply to these sales. Some tribes levy their own tax on reservation sales, but there are no prevalent data on which tribes do so and the level of these taxes. In addition, most reservations are sparsely populated relative to states and are run by a more homogenous tribal government. The fact these tribes can sell cheaper cigarettes gives them geographic market power. It is unlikely none of the rents from this market power are captured by the tribes through higher prices. The price difference variable is therefore biased upward. As long as such a bias is uncorrelated with cigarette demand, the measurement error will cause an attenuation bias in the price difference coefficient. However, as I cannot measure Native American Reservation sale prices, the state average price is the best alternative.

The distance from a reservation to an MSA is calculated in the same manner as the distance to a lower-price state. I use 2000 Census geographic data on Native American Reservations to determine their location. Only Native American areas coded as "reservations" or "tribal lands" are included in the analysis as these are the areas over which tribes have jurisdiction. One of the main concerns with my methodology is reservations often consist of sections of non-contiguous land on which few individuals live. In order to make a more accurate calculation of distance, I include only those sections of tribal lands that have a non-zero population living within them. If no major road runs through the reservation, I use the geographic center. As each piece of these reservations is quite small, this method should not yield large errors. A more pressing problem is it is not obvious from which areas cigarettes are sold. The distance measure when reservations are included are likely to contain more measurement error because I am unable to determine

[33] The FACT Alliance for the Fair Application of Cigarette Taxes (2005) reports a carton of cigarettes on Native American Reservations in New York State can be purchased for close to $30. This is consistent with full tax savings for these sales.

the location of purchase points. As before, distance here is defined as the shortest distance to a lower-price reservation or lower-price border state.

Appendix B: Sensitivity Analysis for the Relationship Between Distance and Smuggling

The reduced form cigarette demand function given by equation (3.7) in the main text assumes a log linear relationship between distance and the probability of smuggling. This functional form is advantageous due to its simplicity and for the implication that distance increases have less of an effect on the propensity to smuggle the farther one lives from a lower-price border. Because I assume the probability of smuggling is given by a linear probability model, the log linear distance assumption also implies as distance gets large enough, expected consumption will become arbitrarily small. This is potentially problematic as the model can predict negative consumption for those living far from lower-price localities. Put differently, one may think it unlikely border distance plays a role in cigarette demand even in the upper tails of the distance distribution.

In this appendix, I performs two tests to determine whether the log linear function masks potential nonlinearities in the upper part of the distance distribution. Table 3.B.1 presents results from regressions similar to those from Panel C, column (iv) in Table 3.6. The dependent variable is log mean cigarette consumption in a given MSA. The regressions in Table 3.B.1 differ from those in Table 3.6 in that distance is entered linearly and the effect of distance on smuggling is assumed only to be relevant for those living "close" to the border. In each of the three columns, I define "close" to mean 75 miles, 100 miles and 150 miles respectively. Note the cutoffs in columns 1 and 3 represent the median and third quartile of distance respectively.

The results from Table 3.B.1 are similar to those presented in Tables 3.6 and 3.7. The signs and magnitudes of the coefficients are impacted little by setting smuggling to zero for those MSAs that are not close to lower-price borders. I also calculate home state price elasticities, smoking increases from smuggling and percent smuggled. For those not within the distance cutoffs, the home state price elasticity is set to the full price elasticity, and the smoking increases and smuggling percentages are set to zero. As the table illustrates, the home state price elasticities become less negative the smaller the distance cutoff and the estimated smuggling percentage is higher than in Table 3.7, but the results are generally consistent with the estimates presented in the text.

The decision of whether to smuggle is a function of both the price difference and the distance to the border. Thus, it may be more appropriate to impose cutoffs based on relative savings rather than on distance alone. I calculate average savings per mile in each MSA using the formula

$$\frac{\text{savings}}{\text{mile}} = \bar{Q} * 365 * \frac{2}{3} * (P_h - P_b)$$

where $\bar{Q}$ is average daily cigarette consumption. I further assume a cigarette shelf life of eight months (Wong, Ashcraft and Miller, 1991), meaning the fixed cost of smuggling must be born every eight months.

Table 3.B.2 presents results from demand regressions using differing savings per mile cutoffs. The cutoff in the first column of the table is the median per mile savings and column 3 uses the 75^{th} percentile per mile savings as a cutoff. Relative to the results in Panel C, column (iv) of Tables 3.6 and 3.7, restricting smuggling to occur only in the high relative savings MSAs yields similar results. The coefficients on difference in log price are smaller and not significant and the home state price elasticities are larger in absolute value, but the qualitative results are consistent across methodologies. Further, increasing the savings per mile cutoffs does little to change the results.

Taken together, Tables 3.B.1 and 3.B.2 suggest the log linear distance assumption used in the main text is not driving the results and conclusions of the paper. Restricting smuggling to be in MSAs that are close to borders or for which the per-mile savings are large yields similar qualitative results to those presented in Tables 3.6 and 3.7. Thus, the simplifying assumption of a log linear relationship between smuggling and demand is innocuous with respect to the central results presented above.

Table 3.B.1: IV Estimates of the Full Cigarette Demand Model with Distance Cutoffs

Independent Variable	D<150	D<100	D<75
Log Home State Price	-0.506**	-0.483**	-0.637**
	(0.140)	(0.148)	(0.177)
Difference in Log Price	1.332**	1.539**	1.563**
	(0.536)	(0.595)	(0.687)
Difference in Log Price Squared	-0.572	-0.690	-0.808
	(1.258)	(1.348)	(1.636)
Distance x Difference in Log Price	-0.012**	-0.015**	-0.012
	(0.003)	(0.005)	(0.008)
Home State Price Elasticity	-0.073**	0.011	-0.092
	(0.218)	(0.242)	(0.292)
Smoking Increase from Smuggling	0.068	0.083	0.112
Smuggling Percentage	0.394	0.366	0.398

[1] Source: parameter estimates from the Author's estimation of equation (3.7) in the text using the 1992-2002 Current Population Survey Tobacco Supplements. In each column, the distance to a lower-price border is set to zero if this distance is greater than the cutoff. Only those living in identified MSAs are included in the regressions.

[2] The regressions include fixed effects for each unique state-MSA combination and are weighted by the number of observations that constitute each MSA-level mean. MSA means of the following variables are also included in the regressions: age, percent male, percent married, weekly wage, percent Black, percent Native American, percent Hispanic, percent Asian, percent high school diploma, percent some college, percent associates degree, percent BA, percent graduate school, percent working, and percent unemployed. Full regression estimates are available from the author upon request.

[3] Price variables are instrumented with tax variables as described in the text.

[4] Robust standard errors are in parentheses: * indicates significance at the 10 percent level and ** indicates significance at the 5 percent level.

Table 3.B.2: IV Estimates of the Full Cigarette Demand Model with Savings per Mile Cutoffs

Independent Variable	Savings/Mile>1.805	Savings/Mile>3	Savings/Mile>5
Log Home State Price	-0.512**	-0.760**	-0.822**
	(0.159)	(0.200)	(0.252)
Difference in Log Price	0.401	0.572	0.329
	(0.566)	(0.771)	(1.194)
Difference in Log Price Squared	1.486	1.264	1.707
	(1.469)	(1.775)	(2.728)
Distance x Difference in Log Price	-0.013**	-0.015**	-0.021*
	(0.004)	(0.006)	(0.011)
Home State Price Elasticity	-0.370	-0.568	-0.707
	(0.208)	(0.292)	(0.476)
Smoking Increase from Smuggling	0.033	0.052	0.035
Smuggling Percentage	0.183	0.182	0.093

[1] Source: parameter estimates from the Author's estimation of equation (3.7) in the text using the 1992-2002 Current Population Survey Tobacco Supplements. In each column, I impose the restriction that smuggling is zero if the savings per mile from purchasing in a cross-border locality is less than the designated cutoff. Only those living in identified MSAs are included in the regressions.

[2] The regressions include fixed effects for each unique state-MSA combination and are weighted by the number of observations that constitute each MSA-level mean. MSA means of the following variables are also included in the regressions: age, percent male, percent married, weekly wage, percent Black, percent Native American, percent Hispanic, percent Asian, percent high school diploma, percent some college, percent associates degree, percent BA, percent graduate school, percent working, and percent unemployed. Full regression estimates are available from the author upon request.

[3] Price variables are instrumented with tax variables as described in the text.

[4] Robust standard errors are in parentheses: * indicates significance at the 10 percent level and ** indicates significance at the 5 percent level.

Appendix C: Second Order Demand Function Approximation Parameter Estimates

Table 3.C.1: Implied Elasticities from the Full Log-Linear Second Order Demand Function Approximation

Panel A: Intensive Margin						
Elasticity	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Mean Home State	-0.148**	-0.058	-0.071	0.033	-0.080	-0.001
Price Elasticity	(0.023)	(0.048)	(1.807)	(1.670)	(0.533)	(0.535)
Mean Full Price	.	.	-0.181*	-0.082	-0.142**	-0.061
Elasticity	.	.	(0.107)	(0.118)	(0.042)	(0.044)

Panel B: Extensive Margin						
Elasticity	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Mean Home State	-0.210**	-0.176**	0.010	0.030	-0.051	-0.039
Price Elasticity	(0.030)	(0.061)	(2.178)	(2.161)	(0.679)	(0.682)
Mean Full Price	.	.	-0.247**	-0.227**	-0.154**	-0.142**
Elasticity	.	.	(0.118)	(0.126)	(0.053)	(0.058)
Panel C: Full Margin						
Elasticity	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Mean Home State	-0.437**	-0.326**	-0.071	0.033	-0.080	-0.002
Price Elasticity	(0.046)	(0.093)	(4.639)	(4.409)	(1.091)	(1.098)
Mean Full Price	.	.	-0.628**	-0.473*	-0.342**	-0.182**
Elasticity	.	.	(0.222)	(0.244)	(0.087)	(0.092)

[1] Source: elasticity estimates come from the Authors' calculation of equations (3.8) and (3.9) in the text using parameter estimates from estimation of equation (3.7) including all cross product and squared terms: $ln(P_h)^2$, $ln(P_h) * ln(D)$, $ln(P_h) * (ln(P_h) - ln(P_b))$, and $ln(D)^2$.
[2] All means in the table are calculated over state-MSA and year and are weighted by the number of observations that constitutes each state-MSA observation.
[3] Robust standard errors are in parentheses: * indicates significance at the 10 percent level and ** indicates significance at the 5 percent level.

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CHAPTER IV

Understanding the Increased Time to the Baccalaureate Degree

4.1 Introduction

Time to completion of the baccalaureate (BA) degree has increased markedly among college graduates in the United States over the last three decades. Among high school graduates from the class of 1972 enrolling in college, about 29% completed BA degrees within four years; two decades later, among graduates of the class of 1992, college completion within four years had slipped under 20%. With both college graduation rates and time to degree prominent in recent policy reports and press accounts,[1] a central question is whether the slowdown in the rate of collegiate attainment is caused by a shift in who goes to college, a decline in the resources available for post-secondary education, or other factors.

The time that it takes individuals to complete an undergraduate college degree reflects both individual choices about the "intensity" of study and the availability and price of courses offered by colleges and universities. Among the reasons students may extend collegiate experiences beyond the four-year norm include a need for academic remediation, which lengthens the course of study, inability to finance full-time attendance resulting in part-time enrollment and employment, or simply a desire to extend the consumption experience of collegiate life. It is also possible, particularly in a collegiate market dominated

[1] A recent front page article in the *New York Times* focused on the low graduation rates and extended time to degree for students completing degrees at many urban universities. For example, the graduation rate at Chicago State University was 16% after six years, rising to 35% after seven years (Finder, 2006). Similarly, one of the findings of the recently released Spellings Commission report was "While educators and policymakers have commendably focused on getting more students into college, too little attention has been paid to helping them graduate. The result is that unacceptable numbers of students fail to complete their studies at all" (U.S. Department of Education, 2006).

by public and non-profit providers, that reductions over time in resources per student at the institutional level limit the extent to which students are able to complete the courses necessary to graduate in four years.

Time to completion of the undergraduate degree is an important outcome in higher education, ultimately affecting the private and social returns to investment in higher education. While it is possible that increased time to degree might reflect greater human capital acquisition, the empirical evidence we produce suggests this positive interpretation is unlikely, as increased time to degree has been associated with slower accumulation of degree credits. That students are accruing credits more slowly implies deferral of the higher wages associated with college completion, lowering the rate of return to college, and reductions in the availability of college-educated workers in the labor market.

We find both increased time to degree and decreased completion rates have been disproportionately concentrated among those students starting at community colleges and public colleges and universities outside the most selective few. However, we find that changes in individual characteristics taken together, including college preparedness and parental education, explain none of the decline in the percentage of college entrants receiving a BA nor the outward shift in the time to degree distribution. The shift toward initial enrollment at two-year institutions rather than four-year institutions can account for some of the decline in completion rates.

Our analysis suggests reductions in collegiate resources that limit the availability of courses and the progression through degree programs, combined with increases in college costs relative to family incomes, are primary explanations for the observed increases in time to degree. We find increases in time to degree are most concentrated in states that have experienced rapid growth in the size of the college age population. This finding is consistent with the hypothesis that larger cohorts generate "crowding" in higher education as resources per student in public higher education do not fully match increases in enrollment demand (Bound and Turner, 2007).

Employment while in college has increased markedly along with time to degree. Given the endogeneity of hours worked, it is hard to quantify the effect this shift might have had on the pace of collegiate attainment. Still, the magnitude of the shift is large enough that it is hard to imagine this shift has not played a role as a proximate factor in increased time to degree. While more favorable macroeconomic conditions in the 1990s relative to the 1970s can explain some of the increase in hours worked among college students, we find the observed change in hours worked is much too large to be explained by such factors alone. We argue the reduction in family incomes relative to college costs for many families will, under plausible assumptions, generate an increase in work effort among college students.

The rest of this paper is organized as follows: Section 4.2 describes the increase in time to degree and decline in completion rates found in the data. Section 4.3 outlines the potential explanations for these trends that inform our empirical analysis. Section 4.4 presents the results from our empirical analysis, and Section 4.5 concludes.

4.2 Increased Time to Degree and Reduced Collegiate Attainment

Evidence of increased time to degree and reductions in college completion conditional on enrollment can be found in a range of data sources. The Current Population Survey (CPS) provides a broad overview of these trends in collegiate attainment by age (or birth cohort). While the share of the population with some collegiate participation increased substantially between the 1950 birth cohort and the 1975 birth cohort, Figure 4.1 shows that the share of the birth cohort obtaining the equivalent of a college degree by age 23 increased only slightly over this interval. Looking at college completion through age 28, however, shows the proportion of college graduates rising more significantly among recent birth cohorts. Taken together, the inference is that time to degree has increased markedly.[2]

To measure changes in time to degree in connection with micro data on individual

[2]Data from cross-sections of recent college graduates assembled by the Department of Education from the Recent College Graduates and Baccalaureate & Beyond surveys corroborate this finding. For example, from 1970 to 1993, the share of graduates taking more than six years rose from less than 25% to about 30%, while the share finishing in four years or less fell from about 45% of degree recipients in 1977 to only 31% in the 1990s (see McCormick and Horn, 1997 and Bradburn et al., 2003).

and collegiate characteristics, this analysis utilizes the National Longitudinal Survey of the High School Class of 1972 (NLS72) and the National Educational Longitudinal Study (NELS:88). These surveys draw from nationally representative cohorts of high school and middle school students, respectively, and track the progress of students longitudinally through collegiate and employment experiences. To align these surveys, we focus on outcomes within eight years of cohort high school graduation for the sample of high school graduates who entered college within two years of their cohort's high school graduation.[3]

The college attendance rate measured among high school graduates from the NLS72 and NELS:88 microdata increased substantially over the two decades of analysis, as shown in Table 4.1. For the high school class graduating in 1972, 53.1% entered college within two years, with this rate rising to 80.8% for those graduating in 1992. While overall college participation rates have increased, the rate at which beginning college students complete the requirements for the BA degree has declined in recent decades. Focusing on completion of college within eight years, there is a more than 5 percentage point decline from 51.1% to 45.3%.

As college attendance rates have increased over this interval, the distribution of students across institutions has shifted. While about 71% of students started at four-year institutions from the 1972 cohort, only about 61% of students in the later cohort started at a four-year public or private institution (Table 4.1, columns (ii) and (v)). Concurrently, the share starting at a two-year institution rose from 29% to 39%.[4] The share of students starting at private colleges and universities decreased only slightly from 20.0% to 17.5%.

[3]Cohort high school graduation is June 1972 for NLS72 respondents and June 1992 for NELS:88 respondents. We define time to degree as the elapsed time from high school cohort graduation. An alternative would have been to measure time to degree from the point of college entry. The results are not sensitive to this choice and, with the NELS:88 cohort only followed for eight years after high school graduation, our approach is consistent in affording eight years of observation for both cohorts. While we measure time to degree from cohort high school graduation, the sample includes those who do not graduate high school on time. Because the NLS72 survey follows a 12^{th} grade cohort and the NELS:88 survey follows an 8^{th} grade cohort, there are more late high school completers in the latter sample. However, when one conditions on college completion within eight years, over 99% of respondents finish high school on time in both samples. See Appendix B for further discussion of the NLS72 and NELS:88 datasets.

[4]One point of note is that tabulations from institutional sources based on counts of first-time students in higher education would suggest that the shift occurred somewhat earlier. For example, tabulations in the *Digest of Education Statistics* show the fraction of first-time students starting at community colleges rising most dramatically between 1960 and 1970 (from 20% to 41%), with a peak of 51% reached in 1975 before returning to 45% in 1992 [See: http://nces.ed.gov/programs/digest/d04/tables/xls/tabn180.xls]. Notably, measures from sources like the CPS that allow for the consideration of a single age cohort confirm the substantial rise in attendance at community colleges among recent high school graduates between 1972 and 1992.

Panel B of Table 4.1 illustrates the dramatic differences in the likelihood of BA completion associated with type of first institution. Students starting at community colleges are considerably less likely to complete than students starting at four-year institutions in the public and private sectors. For the high school cohort of 1992, the BA completion rate among those starting at public two-year institutions slipped to 17.4% from the 23.2% level for those graduating in the high school class of 1972. At the other extreme, completion rates among students starting at private four-year institutions increased across cohorts from 68.4% to 78.2%. To wit, there are substantial and increasing differences in the likelihood of degree completion associated with a student's initial college choice.

4.2.1 Time to Degree

We measure time to degree in each survey as the number of years between cohort high school graduation and BA receipt.[5] Because the last NELS:88 follow-up was conducted in 2000, we are forced to truncate the time to degree distributions at eight years, reflecting the time between cohort high school graduation and the last follow-up. We are therefore truncating on a dependent variable, which may introduce a bias into our analysis if the truncation occurs at different points in the full degree time distribution for the two cohorts. Empirically, the proportion of eventual college degree recipients receiving their degrees within eight years has not changed appreciably. The National Survey of College Graduates (2003) allows us to examine year of degree by high school cohort. For the cohorts from the high school classes of 1960 to 1979 for which there are more than 20 years to degree receipt, we find the share of eventual degree recipients finishing with eight years holds nearly constant at between 0.83 and 0.85. Focusing on more recent cohorts (and, hence, observations with more truncation) we find that in the 1972 high school graduating cohort, 92.3% of those finishing within twelve years had finished in eight years, with a figure of 92.4% for the 1988 cohort. This evidence supports the assumption made throughout this

[5] Given our measure of time to degree , it is natural to wonder if some of the differences across cohorts could be accounted for by delayed entry into college. Empirically, the shift that occurred was too small to account for any of the shift in time to degree. In our samples of those that finished college, only 5.5% of the NLS72 cohort and 7.4% of the NELS:88 cohort delayed initial entry.

analysis that the eight year truncation occurs at similar points in the time to degree distribution in both surveys.

To understand the change in the timing of degree receipt across surveys, we show the cumulative share of eight-year degree recipients receiving their degree in years four through eight beyond their cohort's high school graduation[6] (see Appendix B for details on the construction of the time to degree variables). Table 4.2 presents cumulative distributions for the full sample and by initial school type. For each year of the time to degree distribution, we calculate the difference between the proportion having graduated in that year or earlier in the NELS:88 survey and the NLS72 survey, along with the p-value of the test that this difference is statistically different from zero. We also present the p-value of a test that the NLS72 distribution first-order dominates the NELS:88 distribution for years 4, 5, and 6 post high school cohort graduation. Appendix C outlines the calculation of p-values and other statistical issues related to the distributional comparisons.

There is a significant increase in time to degree over the time period studied in this analysis. While more than one half (56.8%) of degree recipients finished within four years from the 1972 high school cohort, the share finishing within four years dropped to 43.6% for the 1992 high school cohort. It is unambiguous that there has been a shift in the distribution of time to degree over this interval as the cumulative distribution function for NLS72 first-order dominates the observed NELS:88 time to degree distribution.

The extension of time to degree is far from uniform across types of undergraduate institutions, with much larger increases among students starting at public 4-year and 2-year institutions than among those starting at private colleges and universities, as shown in the additional panels of Table 4.2. The fraction of observed degree recipients finishing within four years falls from 54.6% to 37.1% among students starting at 4-year public universities and from 35.9% to 21.0% at the 2-year publics.[7] Among students enrolling

[6]A concurrent trend over the time period covered by our analysis is the delay of initial kindergarten or primary school entry. Because our analysis focuses on eighth and twelfth grade cohorts, our estimates should not be affected by this shift. However, the increase in "academic red-shirting" is further justification for using grade-specific rather than age-specific measures of time to degree.

[7]All references to two-year schools and community colleges refer to public institutions only.

at private institutions, the decline in 4-year completion is a more modest 5.3 percentage points, and the NLS72 distribution does not first order dominate the NELS:88 distribution for this group.

Public colleges and universities are central to our analysis because the majority of undergraduate degrees are awarded by these institutions, and this is the institutional sector where changes in time to degree are most pronounced. Given the considerable heterogeneity in program offerings and resources among BA-granting colleges and universities within the public sector, we present differences in the rate of collegiate attainment and distribution of time to degree by rank of the initial institution attended by respondents whose first collegiate attendance was at a four-year public institution in Table 4.3.[8] Notably, students at top-ranked public universities are only a small share of all undergraduate enrollment (2.9% for the high school class of 1992) and BA recipients (5.8%). Moving down Table 4.3, the higher ranked group becomes less selective, from the top 15 to the top 50 public schools. It is only outside the top 15 public universities where some erosion in time to degree is apparent, though the largest changes occur outside the top 50. To illustrate, while the proportion of BA recipients receiving degrees within four years increased from 53.8% to 54.8% between cohorts among those whose initial institution is a top-15 public university, the proportion of BA recipients receiving degrees within 4 years decreased from 54.7% to 35.0% at public universities outside the top 15. Later in the analysis, we consider the institutional features contributing to greater stratification in inputs and academic outcomes within public colleges and universities.

4.2.2 Credit Attainment

Given observed increases in time to degree, it is natural to ask whether these changes reflect increased difficulty in passing through the course sequences or increased course

[8]We employ the rankings of public universities assembled by *U.S. News and World Report* in 2005. Other metrics such as resources per student or selectivity in undergraduate admissions give similar results. The top 15 public universities are, in order: University of California - Berkeley, University of Virginia, University of Michigan, University of California - Los Angeles, University of North Carolina, William and Mary, University of Wisconsin, University of California - San Diego, University of Illinois, Georgia Institute of Technology, University of California - Davis, University of California - Irvine, University of California - Santa Barbara, University of Texas, and University of Washington.

taking. At the extreme, if increased time to degree were to capture only more attainment in the form of course credits, then policy concern over the effects of time to degree would be misplaced as the additional attainment should also translate to labor market benefits. With access to transcript data, we are able to chart the time path of credit accumulation in order test for such an explanation. Average credit accumulation schedules by initial school type are presented in Figure 4.2. What is evident from the figure is a slower pace of credit accumulation in the 1992 cohort relative to the 1972 cohort for students at four-year public institutions outside the most selective group as well as at community colleges. In the first year after high school graduation, students beginning at two-year schools accumulated, on average, about 4.6 fewer credits in the later cohort relative to the class of 1972, while students starting at four-year public schools outside the top 15 accumulated, on average, about 4.1 fewer credits.[9] For students at public non-top 15 universities, differences largely converge after 8 years with little net change in total credit accumulation. However, students beginning at two-year schools accumulate over 6 fewer credits on average after eight years in the NELS:88 survey.

These changes in credit accumulation are evident in the distributions of earned credits as well as in the yearly means. Figure 4.3 shows the distribution of total credits four years after cohort high school graduation. As the figure illustrates, the earned credit distribution for NLS72 respondents is above that of the NELS:88 respondents in the sectors in which the time to degree increase was most pronounced.[10] However, as Figure 4.4 indicates, eight years after cohort high school graduation, the credit distributions converge fully for the public non-top 15 sector but not for the two-year sector.

Taken together, Figures 4.2-4.4 indicate the observed increases in time to degree reflect a reduction in the rate of human capital accumulation rather than an increase in human

[9] One point of note is that credits completed actually increase moderately among those beginning at the top public universities and private institutions. We believe this shift reflects several related trends: first, students at these institutions may be more selected (and higher achieving) than in previous cohorts; secondly, students at these institutions are increasingly likely to pursue double majors that require higher total credit accumulation; and, finally, a number of these institutions offer five-year programs that allow students to combine an undergraduate degree with a one-year MA in areas like education or engineering.

[10] We have also explored changes in the ratio of attempted credits to credits completed and find only a modest increase; these changes are not large enough to explain much of the increase in time to degree.

capital outside the top public schools and private institutions. Given the evidence regarding the extent of the time to degree and completion rate changes and the types of schools most affected, our objective in the remainder of this paper is to consider how changes in pre-collegiate characteristics, institutional conditions and other market forces can explain these trends.

4.3 Potential Explanations for Increased Time to Degree and Decreased College Completion

In this section, we consider multiple theoretically plausible explanations for the changing rate of college completion and time to degree. One class of explanations focuses on the characteristics of students at the margin of college completion: if the pre-collegiate achievement of these individuals eroded, time to completion would be expected to increase. An alternative explanation for increased time to degree may rest with changes in constraints in the market for higher education. First, because much of higher education is publicly produced with substantial reliance on public subsidies, changes in demand may not be fully accommodated with changes in the supply of undergraduate collegiate opportunities (Bound and Turner, 2007). Second, students may find it increasingly difficult to finance college. With limited access to credit markets to fund an investment like education that is impossible to collateralize, students may need to increase hours worked to pay for living expenses and tuition, thereby potentially decreasing the rate of collegiate attainment.

4.3.1 Changes in Student Characteristics at the Margin

With increased returns to a college education inducing more high school graduates to pursue college, students drawn to attend college because of the higher rewards may be less adequately prepared than the infra marginal student. A simple selection model illustrates the connection between changes in the pool of students entering college, the college completion rate and time to degree. We begin with the observation that changes in the returns to college (or reductions in the cost of college) both induce more students to enroll in college and potentially encourage greater attainment among those already enrolled but not completing the BA. How the completion rate changes depends ultimately on the relative magnitudes of the change in the number of students enrolling in college relative to the number of students induced to complete college. Note it is likely to be the exception not the norm that these two changes would be exactly offsetting.[11] Changes in college completion may affect time to degree to the extent the same underlying characteristics that operate at the margin of college completion affect the rate at which individuals progress through degree programs.

4.3.2 Resources per Student and Institutional Constraints

The supply-side of the market for higher education defined in terms of the quantity and quality of enrollment opportunities at any point in time is an important determinant of both completion rates and time to degree. With colleges and universities receiving considerable subsidies from state, federal, and private sources, consumers pay only a fraction of the cost of production, as student fees cover only about 12% percent of total educational costs at public colleges and universities in the U.S. (Winston, 1999). Moreover, total resources and public subsidies are highly stratified across institutions within states, with expenditures per student in public universities more than double those in community colleges (Courant, McPherson and Resch, 2006). There also exists considerable variation across states and within states over time in the level and distribution of public subsidies.

Public colleges and universities are unlikely to accommodate fully changes in demand. First, non-tuition revenues and capital stock - including state appropriations, donations from private sources, and campus infrastructure - are unlikely to respond in full to short run changes in demand. In addition, tuition charges, particularly at public institutions under significant political pressure are unlikely to increase such that enrollment is regulated through the price mechanism.

[11]Even in the stylized case when a model might yield the prediction of proportionate changes in new collegiate entrants and new collegiate graduates, the relative changes in the underlying characteristics (such as achievement) are likely to be smaller (larger) depending on the proximity or distance to the median. Changes occurring in the tails imply much larger changes in the underlying characteristic or precollegiate indicators.

The adjustment of public colleges and universities to demand increases takes somewhat different forms across the strata of higher education. For top-tier colleges and universities in both the public and private sectors, there is little adjustment in degree (or enrollment) outcomes to demand shocks. To the extent these institutions use selectivity in admissions (which increases with increases in demand) to regulate enrollment, it is likely outcomes such as completion rates and time to degree are unchanged, or, perhaps, even improve, with increased demand. At the same time, enrollment is relatively elastic among public universities outside of the most selective few. Here, we expect increased demand to lead to increased enrollment and consequent reductions in resources per student.[12]

Increasing enrollment at community colleges relative to universities will tend to reduce the overall resources per college student within a state. In addition to changes in outcomes linked to the availability of public subsidies within institutions, increases in collegiate demand may shift the distribution of college enrollment within a state to open-access four-year institutions and community colleges. Yet, while some shifts in the distribution of students toward community colleges may reflect the constraints of the supply-side of the market, it is difficult to distinguish these outcomes from shifts generated by increases in demand for sub-baccalaureate training among students not intending to complete a BA degree.[13]

Queuing and enrollment limits in response to limited resources in the public sector may affect the pace of degree receipt and the overall college completion rate. Despite nominal claims of "open enrollment," there is ample evidence of enrollment limits and course

[12]Bound and Turner (2007) emphasize that direct assessment of the effect of resources per student on degree outcomes using measures of current expenditures or state appropriations is difficult because only part of any observed change in current expenditures is likely to be exogenous, and expenditures translate into resources with long lags. They use variation in the size of the college-age cohort within states to generate plausibly exogenous variation in the availability of higher education resources per student. They find that the elasticity of undergraduate enrollment with respect to cohort size is close to 0.2 at flagship public universities, and the corresponding elasticities are 0.8 at community colleges and about 0.6 at non-flagship public universities.

[13]Estimating the causal impact of type of collegiate experience on attainment and earnings is difficult given the endogeneity of college choice, with some students attending community colleges likely to be systematically different than those attending four-year colleges. A number of researchers have found college students starting at two-year schools are much less likely to complete the BA than their peers beginning at four-year schools. Reynolds (2007) uses matching estimators to approach this question, while earlier work uses regression techniques to adjust for observable differences between those starting at two and four-year schools (Rouse, 1995, Leigh and Gill, 2003 and Sandy, Gonzales and Hilmer, forthcoming). The results in Reynolds (2007) underscore the importance of common support in estimating the effect of college type on outcomes, finding that among students expecting to complete less than a college degree there is little power for comparison with students starting college at four-year institutions as only about 5% have initial expectations of educational attainment below the BA.

closings, particularly in high growth states. Case studies with press clippings from high growth states such as California, North Carolina and Utah are instructive and presented in Appendix A. Some institutions become more selective in response to increases in demand as illustrated by the cases of the University of Utah and Utah State.[14] In turn, courses at the community college and open access four-year institutions fill up rapidly, leaving many students "admitted" but unable to enroll in needed classes. For example, the registrar at Utah Valley State noted,

> We admit everyone, but if they come and find that the courses they want are already full, they may either look at other options or wait until the next semester. Although 'open enrollment' remains the objective, the availability of classes is a limiting factor (Van Leer, 2002).

It is straightforward to see how such institutional barriers lead to delays in degree progress. While queuing and shortages of courses are inefficient, such limitations may be inevitable in the absence of adjustments in tuition and enrollment at public universities when appropriations per student decrease, leading to increases in time to degree.

4.3.3 Student Responses to Increases in College Costs

That college costs have increased dramatically in recent years has been widely noted by the press and policy makers. The rate of real tuition increases has been sizeable at all types of institutions, with tuition costs rising by about 240% between 1976 and 2003 at four-year institutions in both the public and private sectors. Despite substantial relative increases over time, average tuition at public 2-year schools was $2,191 and was $5,491 at public 4-year schools for 2005-2006 (College Board, 2006), suggesting it is unlikely tuition alone is the binding constraint affecting college completion and time to degree. Yet, with tuition only a fraction of the total cost of full-time attendance (including room and board) it would seem plausible that some students may be credit constrained.

[14] For example, one report notes

> Both the University of Utah and Utah State University are raising the bar for enrollments this fall. Other colleges and universities in the system have traditionally tried to take all comers, but they are warning ahead of time that budget cuts, coupled with unprecedented demand, may limit enrollment as classes fill and potential students are turned away (Van Leer, 2002).

Absent credit constraints, the student response to increases in college costs may well be ambiguous, depending in part on the pricing structure chosen by colleges and universities. One point of distinction across the sectors of higher education is the extent to which institutions price per unit or per term, with students effectively allowed to take as many courses as they can handle within the academic term. If students are free to select the number of credits taken in each term, increases in tuition charges provide an incentive to substitute away from leisure toward academic study; for some students, increased intensity of study can reduce time to degree and, in turn, total college costs.[15] Examples of institutions with this type of "fixed term" pricing structure include many residential private institutions such as Princeton, Harvard, Amherst, and Williams and selective public universities like the University of Virginia. Institutions serving constituencies focused on full-time residential undergraduates are much more likely to post "flat fee" tuition schedules while those with many working and adult students tend to offer pricing per credit hour. Looking at tuition structures offered by public four-year colleges and universities in 1992, more than 52% report a per credit pricing system. Yet, when we focus on the more selective top-15 public institutions, only 1 institution reported tuition on a per credit basis.[16]

Models based on full flexibility in the allocation of hours between academic study, work and leisure typically assume the capacity to finance collegiate study with intertemporal borrowing and full access to credit markets. In practice, it is quite likely students face some limits in access to capital markets (Becker, 1993). With relatively modest availability of federal aid and limited institutional financial aid funds outside the most affluent colleges and universities, it is plausible many students attend college part time - thus extending time to degree - because they are credit constrained and unable to borrow to finance full-time attendance.[17] If tuition increases combined with imperfect credit markets induce

[15] A notable distinction among these institutions is the extent to which AP credits and prior coursework count as credit toward the degree. At many of the selective privates (where it may be that universities have a vested interest in collecting exactly four years of tuition, room and board), advanced standing credit may be used for placement purposes but does not reduce the total courses a student is required to complete. This is true, for example, at Amherst and Williams.

[16] While historical trends in tuition structure are difficult to find nationally, we collected information from Michigan, California, and Virginia for all public universities going back to the 1970s. The data suggest the structure of tuition is remarkably stable within institution with respect to charging per term or per credit hour.

[17] With the maximum Pell grant at $4050 and the borrowing under the Stafford program limited to $2625 for first year

students to work more hours while in college to finance attendance, time to degree may increase if working time crowds out time that would be spent on academic pursuits.[18]

In the context of the Becker-Tomes (1979) model of intergenerational transfers (see also Solon, 2004 and Brown, Mazzocco, Scholz, and Seshadri, 2006) rising tuition charges and falling family income lead to the expectation that, under these circumstances, students will be expected to shoulder a higher fraction of college costs. As such, if students are limited in their capacity to borrow, rising college costs may lower attainment, extend time to degree and increase the incidence of employment while in school.

In sum, theoretically plausible explanations for declines in completion rates and increases in time to degree include demand-induced shifts in the characteristics of new college entrants and students at the margin of college completion, changes in the supply-side of higher education reducing resources per students, and increased difficulties in paying for college that may lead to increased employment and reductions in the rate of collegiate attainment. In the next section, we evaluate the empirical evidence in support of these explanations.

4.4 Empirical Analysis of Increased Time to Degree and Decreased Completion Rates

4.4.1 The Role of Changing Student Attributes

4.4.1.1 Methodology to Assess the Role of Changing Student Attributes

Our methodological approach begins with the investigation of the relationship between changes in the pre-collegiate characteristics of college students and the outcomes of college completion and time to degree. The motivation is similar to the Blinder-Oaxaca decomposition in that our objective is to determine the extent to which changes in the distribution of student attributes can explain the observed changes in the distribution of time to de-

dependent students, a low-income student hoping to attending a residential college full time would face substantial unmet need. See Fitzpatrick and Turner (2007) for further discussion.

[18] Various researchers have found evidence that the resources available to students have important effects on behavior. For example, Kane (1996) finds high tuition at public colleges tends to induce students to postpone college, while Christian (2007) finds evidence college enrollment tends to be pro-cyclical for students from low income families. Keane and Wolpin (2001) show that optimizing, forward looking agents will tend to both postpone attending college and increase hours of work in response to tuition increases.

gree. We re-weight the NELS:88 time to degree distribution using the characteristics of students from the NLS72 survey. This calculation leads to a counterfactual time to degree distribution in which the proportion of students with a given characteristic has not changed between the two surveys. By comparing the observed NELS:88 time to degree distribution and the re-weighted NELS:88 time to degree distribution, we can determine the proportion of the difference between the distributions that is due to changes in the mix of students with a given attribute attending college. The remainder, or the difference between the reweighted distribution and the observed distribution, reflects changes in other determinants of time to degree or changes over time in how a given characteristic affects degree completion.

We perform re-weighting calculations separately for specific characteristics such as ethnicity, family income and pre-collegiate achievement and for all characteristics together. The latter methodology parallels the semi-parametric re-weighting pursued in DiNardo, Fortin, and Lemieux (1996), Heckman, Ichimura and Todd (1987, 1988) and Barsky, Bound, Charles, and Lupton (2002). While these studies apply the re-weighting technique to a continuous outcome variable (wages or wealth), the distribution of time to degree is discrete: students can only graduate at the end of a term. However, the discretization of the outcome variable is a straightforward extension of the basic econometric strategy.

4.4.1.2 Data Used in the Re-weighting Analysis

The student attributes we analyze are high school math test quartile, high school reading test quartile,[19] father's education level, mother's education level, parental income levels, gender, and the representation of historically underrepresented racial and ethnic groups; see Appendix B for further discussion. For the measurement of family income, we are

[19]The math and reading tests refer to the NCES-administered exams that were given to all students in the longitudinal surveys in their senior year of high school. Because the tests in NLS72 and NELS:88 covered different subject matter, were of different lengths, and were graded on different scales, the scores are not directly comparable across surveys. Instead, we construct the quartiles of the score distributions for each test type and for each survey. The comparison of students in the same test quartile across surveys is based on the assumption overall achievement did not change over this time period. This assumption is supported by the observation that there is little change in the overall level of test scores on the nationally-representative NAEP over our period of observation. Similarly, examination of time trends in standard college entrance exams such as the SAT provides little support for the proposition that achievement declined appreciable over the interval within test quartiles.

interested in assessing parents' ability to finance college and the variable of interest is the real income level, not one's place in the income distribution. We align the income blocks representing responses to categorical questions across the two surveys using the CPI for higher education.

The NLS72 and NELS:88 datasets contain a significant amount of missing information on test scores, parental education, and parental income brought about by item non-response. While a very small share of observations are missing all of these variables (in NLS72 and NELS:88, respectively, 0.60% and 1.26% have no information on any of these variables), a substantial number of cases are missing either test scores, parental education or parental income. For example, in NLS72, 40% of those who enroll in college and 39% of those receiving a BA within eight years of cohort high school graduation are missing information on at least one of these background characteristics. These percentages are 51 and 43, respectively, in NELS:88. Because the data are not missing completely at random, case-wise deletion of observations with missing variables will bias the unconditional sample means of completion rates and time to degree. We use multiple imputation methods (Rubin, 1987) on the sample of all high school graduates to impute missing values using other observable characteristics of each individual;[20] see Appendix B for complete details.

Table 4.4 presents the changes in background characteristics and measured ability of college attendees and graduates across the NLS72 and NELS:88 surveys. The table shows the substantial shift in background characteristics of students entering college over time to include those from lower on the high school test score distribution and those from historically underrepresented minority groups. For example, the proportion of college attendees from the highest math test quartile dropped from 40.6% to 35.3% and from the lowest math test quartile increased from 11.2% to 14.2%. For reading tests, the proportion in the top quartile dropped from 35.0% to 31.4% and the proportion in the bottom quartile increased from 14.5% to 18.0%. Similarly, the percent of college attendees with race

[20] Under the assumption the data are missing conditionally at random, multiple imputation is a general and statistically valid method for dealing with missing data (Rubin, 1987; Little, 1982). The relative merits of various approaches for dealing with missing data have been widely discussed (e.g. Little and Rubin, 2002; Schafer, 1997).

classified as white decreased from 85.2% to 73.8%.

Table 4.4 also illustrates these changes in the demographics of college attendees were not translated fully into changes among college graduates. Among BA recipients, the proportion in the highest math quartile decreased by less than 2 percentage points and increased for reading, while the percent white declined by only 7 percentage points. The implication of comparisons across columns in Table 4.4 is while many students were pulled into the higher education system between 1972 and 1992, many of them from historically underrepresented minority groups and from lower on the measured ability distribution did not obtain a BA.

For both college attendees and graduates, the direction of the change in outcomes caused by the change in observables is hard to predict because some variables, such as family income and parental education, became more favorable among college graduates. Echoing the general increase in educational attainment during the post-war period, the proportion of college attendees whose father (mother) had at least a BA increased by 8.1 percentage points (12.9 percentage points) for all college attendees and 15.6 percentage points (20.8 percentage points) for BA recipients.[21] Such shifts implicitly go in the "wrong direction" to explain the observed changes in either time to degree or completion rates.

4.4.1.3 Results from Multivariate Reweighting using Individual Characteristics

To understand how the change in the distribution of individual characteristics affected collegiate attainment, we use a logit estimation to generate weights based on cohort of observation. These weights are used to generate the reweighted distributions shown in Table 4.5, reflecting the distribution of completion expected if the distribution of individual characteristics in 1992 resembled the distribution of individual characteristics in 1972.[22]

[21] To assess whether the cross-sectional relationship between parental outcomes and education changed with the expansion of education among parents in the later survey, we estimate logit regressions of the completion outcome on parental education levels. We perform this analysis both with and without the other covariates listed in Table 4.4 and find stable cross-sectional relationships across the surveys.

[22] As with all reweighting analyses, the choice of indexing is arbitrary. We chose to reweight the NELS:88 distribution due to ease of interpretation, but we obtain virtually identical results when we reweight the NLS72 outcomes using the distribution of observable characteristics in NELS:88. Results from reversing the indexing are presented in Appendix D, Tables 4.D.4 and 4.D.5.

We also present the p-value on the test that the difference between the NELS:88 reweighted distribution and the NLS72 distribution at each point is statistically significant. The p-value of the test for first order stochastic dominance is presented in the final column of Table 4.5.

Panel A of Table 4.5 shows the reweighted time to degree distribution and completion percentages for the full sample of those that attend college within two years of cohort high school graduation. In words, these estimates show the distribution of time to degree that would have been expected to prevail if individual attributes had remained at their 1972 level and students had completed degrees according to the pattern observed for observationally similar students from the 1992 high school cohort. Overall, changes in background characteristics of college graduates go in the opposite direction to explain the increase in time to degree; the reweighted proportion of four-year BA recipients declines from 43.6% to 39.6%. Given the shift in the nature of the college going population, we found this result surprising. However, we found that regardless of which variables we standardized on (Appendix D, Table 4.D.3, contains univariate reweighting results), or how we did the standardization, changes in the characteristics of students graduating from college could not explain more than a trivial amount of the increased time to degree. While it is true that the distribution of the characteristics of those enrolling in college shifted towards groups that, along some dimensions, would have been expected to take longer to get through college, the distribution of those finishing changed much less.

In addition, changes in observables explain none of the drop in the completion rate, which is due to the fact that the shifts in parental education and income fully counteract the shifts in test scores and ethnic makeup of students.[23]

In Panels B and C, we perform the reweighting analysis separately for those whose initial institution is a public non-top 15 university and those whose initial institution is a two-year college, respectively. For the time to degree analysis, results are similar to those

[23]One can see this more clearly when looking at the univariate reweighting results in Appendix Table 4.D.3.

reported in Panel A. Changes in the characteristics of BA recipients explain none of the increase in time to degree across cohorts, with point estimates suggesting slightly larger predicted increases in time to degree in the comparison between the observed outcome for the 1992 cohort and the reweighting of these data to reflect the characteristics of the 1972 cohort. While reweighting by individual characteristics explains none of the drop in completion rates for the public non-top 15 sample, it explains about 47% of the decrease in the completion rate for the two-year sample. These changes are reflected in the shifts in characteristics for individuals in these samples that are reported in Appendix D, Tables 4.D.1 and 4.D.2.

Overall, changes in individual characteristics explain none of the upward shift in the cumulative time to degree distribution between NLS72 and NELS:88. We now turn to an examination of institutional factors to understand their role in increasing time to degree and decreasing completion rates.

4.4.2 The Role of Institutional Type and Resources at Public Universities

4.4.2.1 Initial Institution Type Re-weighting Results

Whether college students at different points in time face the same options and levels of public support in the higher education market is a matter of considerable uncertainty. As discussed in Section 4.3, the supply-side of the market in higher education may contribute to the observed increased time to degree for students at public universities if there have been declines in resources per student which retard degree completion.

Suppose the class entering college from the high school class of 1992 was distributed among colleges in the same way as the class of 1972. How would this reweighting predict completion and time to degree? Results from reweighting by initial school type, both with and without additional controls for variation in individual characteristics, are shown in Table 4.6. With a predicted completion rate of 48.0% and 49.2% under the prior distribution of institutional type when individual covariates are included and excluded, respectively, the shift in type of institution - largely the shift toward entry at two-year schools - explains between 49.2% and 68.9% of the observed decline in completion rates. Because shifts in the type of institution that first time college students select reflect both individual characteristics and supply-side adjustments in the higher education market, these estimates are likely to over-estimate the causal effect of the shift on completion rates. However, as long as there is an effect on the probability of completing college of starting at a two-year school, these estimates suggest the shift towards two-year schools has contributed to declining completion rates.[24]

Yet, these shifts explain none of the change in the timing of college completion. In effect, type of initial collegiate institution captures considerable self-selection and, with low completion rates among students starting at community colleges, there is little effect of increased enrollment in community colleges on the time to completion margin.

4.4.2.2 Institutional Crowding Estimates

The persistence of the increase in time to degree among students attending public universities leads to the question of whether declines in resources within this sector over the period of observation might adversely affect the progression of students. We follow Bound and Turner (2007) and use the percentage change in the number of 18 year olds in each state between 1972 and 1992 as a determinant of exogenous changes in resources per student. Our analysis is thus at the state level, as this is the governmental level of control for public universities and, in turn, the division used in determining access for in-state tuition and fees. To investigate the role of school-level resources, we focus on each of seven different variables that measure time to degree and college completion: the eight-year college completion rate for all high school graduates; the eight-year college completion rate for all college attendees; the eight-year college completion rate for all college attendees who begin their postsecondary education at a public non-top 15 institution; the four-year completion rate for those who graduate within eight years; the four-year completion rate

[24] While it is possible that some students enter community colleges for sub-baccalaureate vocational training, the majority (64.12%) of community college entrants in the NELS:88 cohort intended to complete at least a BA degree. A similar question was not asked in the NLS72 survey so it is not possible to compare responses across time.

for those who graduate within eight years and whose initial institution is a public non-top 15 university; average time to degree, in years, truncated at eight years; and average time to degree for the public non-top 15 sample.[25]

A potential confounding factor in analyzing the relationship between the change in the time to degree measures and the 18-year old population is the role of changing demographic characteristics within each state. For example, if states that witness an increase in their 18-year old population also experience an increase in their population from the lower parts of the income distribution, and if more high school students are pulled from this group, we should observe a time to degree increase regardless of the effect on resources per student.

Using a two-stage estimator, we first regress the dependent variable of interest on observable demographic characteristics (the same ones used in the multivariate reweighting analysis), a state specific indicator variable, a cohort specific dummy (NELS:88 =1), and state-cohort interaction terms at the individual level. In the cases where the dependent variable is binary, we use a logit to estimate the parameters of this regression, otherwise we use OLS.[26] Our goal is to compare the observed NLS72 outcome and the counterfactual outcome for NELS:88 if observable characteristics of students had remained unchanged over time. To obtain this counterfactual, we generate state and cohort level fitted values from each regression, and then, for the NLS72 observations, add in the cohort and relevant state x cohort fixed effect for each observation. When the regression is a logit, these variables are added into the logistic function. We then take state-level means of the observed outcomes and the counterfactual outcomes.

Taking the changes in these state-level estimates as our dependent variables, we run second stage regressions of the effect of the change in log population 18 in each state. Results are reported in Table 4.7. In the first column, the dependent variables are the change

[25] We lack sufficient observations of those who start at two-year institutions within each state to separately identify crowding estimates for this group.

[26] With the objective of estimating how state level variation in resources generated by changes in cohort size affects time to degree, we follow a two-step estimation process, first regressing individual educational outcomes on covariates and state effects: $\ln \mathrm{TTD}_{ijt} = \alpha + \phi X_{ijt} + \gamma_j S_j + \lambda_j S_j \mathrm{D92} + \epsilon_{ijt}$ and then recovering the year-state interactions to estimate the second stage: $\ln(\mathrm{TTD}_{j72} - \mathrm{TTD}^{92}_{j72}) = \alpha + \beta dlnP_{jt} + \epsilon_{jt}$. Note that we lack sufficient observations within states to employ the non-parametric reweighting strategy within states.

in actual state-level outcomes that are not regression-adjusted, NELS:88-NLS72. In the second column, the dependent variables are the difference between the NELS:88 counterfactual and the actual NLS72 value of the outcome variable. This difference represents the average change within each state in the outcome variable that is not attributable to changes in observable background characteristics. Graphically, the same story is present in Figure 4.5, which shows the change in cohort size on the x axis and the change in time to degree on the y axis.

Taken as a whole, the results are consistent with the hypothesis that time to degree has expanded the most in states where cohort size has increased, in turn reducing resources per student. For example, we find a 1 percent increase in a state's 18-year old population increases time to degree by 0.112 percent for the non-regression adjusted measure and by 0.094 for the regression adjusted measure. These effects increase to 0.129 percent and 0.132 percent, respectively, when we restrict the sample to the public, non-top 15 sample. At the margin of college completion, there is also evidence that increases in the size of the college age cohort lowers completion rates, particularly within four years.[27]

Additional evidence from the College Board's *Annual Survey of Colleges*, which includes institution-level characteristics of the undergraduate class and admission process from 1986-2000, supports the proposition that growth in the number of potential college students within a state accords with an increase in the measured qualifications of entering students. We find a 10% increase in a within-state cohort size is associated with a roughly 1.1 (0.209) percentage point rise in the fraction of college students at public colleges and universities ranked in the top ten percent of high school classes. We find similar evidence of an increase in the requirement of college admissions tests and higher scores on standardized tests among entering students at public universities.[28] Given the problems with these data,

[27]Evidence of a link between eight-year completion rates and cohort size is appreciably weaker when we adjust for within-state changes in cohort characteristics. Changes in the representation of Hispanic students and parental education within states, variables also linked to student completion, are correlated with changes in cohort size. As a point of further consistency implicit in these results, we have estimated these models with the dependent variable expressed as a log differences; the elasticity of cohort size on eight-year completion with adjustment for demographic changes is 0.39 and close to the preferred estimate of 0.4 in Bound and Turner (2007).

[28]These effects are particularly prominent in the within-institution changes in the 25^{th} percentile SAT scores, where a 10% increase in the size of the cohort age 18 within a state predicts an increase in verbal SAT scores of 2.27 (0.537) at all public

including considerable missing data and a relatively short time horizon, we do not want to overemphasize these results. However, they are consistent with the notion that the public universities are not able to accommodate fully increases in student demand with increases in institutional capacity.

Our regression results support the hypothesis that shifts in the number of students attempting to enroll in public institutions within a state may reduce resources per student and, in turn, affect the pace at which students are able to complete their studies. The discussion of changes in resources over time requires particular attention to differences in changes across institutions. Taken as a whole, resources either increased or held constant on a number of widely reported scales. To illustrate, constant dollar current expenditures per student at public colleges and universities have risen from $14,610 in 1970-71, to $17,606 in 1990-91, to $22,559 in 2000-01 (Snyder, Tan, and Hoffman, 2006, Table 339). Such measures miss two fundamental changes occurring over this period: first, the stratification in resources across institutions increased over this period, with dramatic increases in resources at some institutions combined with stagnation and decline in resources at other institutions; and secondly, changes in spending per student combine changes in the price of educational inputs with changes in quantities. While the employment of a price index specific to the overall mix of inputs employed by colleges and universities (e.g., HEPI) reduces the constant dollar growth in expenditures, it is likely that faculty salaries and the cost of laboratory equipment at research universities have outpaced this general index.

When we use institutional level data from the HEGIS-IPEDS institutional surveys, we find real state appropriations per student declined by about 10% between 1976 and 1996 at public four-year institutions outside the most highly ranked institutions. Focusing on faculty inputs in Table 4.8, we present undergraduate student-faculty ratios at the BA-granting institution by initial school type weighted by student enrollment and find a pattern consistent with the hypothesis that resources outside the most selective public

universities and 5.90 (0.239) at the top 15 public universities and an increase in math SAT scores at the 25^{th} percentile of 2.61 (0.507) at all public universities and 3.49 (0.267) at the top public universities.

institutions were more thinly spread in the later period. While student-faculty ratios at the graduating institution actually fell somewhat for those starting at a top 15 public institutions (declining by about 9%), student faculty ratios rose at other public institutions with a rise in the mean of about 15% from 19.2 to 22.0 students per faculty member and yet larger relatively increases at below median institutions. Similarly, BA recipients beginning at a two-year school transferred into four year schools that experienced a 31% increase in student-faculty ratios, from 18.8 to 24.5. Consistent with our results, smaller increases in student-faculty ratios occurred in the private sector of higher education.

These changes in student-faculty ratios reflect the differential increase in enrollment over time across institutions. For example, while undergraduate enrollment increased by 11% between 1972 and 1992 at public top 15 universities, enrollment increased by 29% in non-top 15 public institutions and by 116% in community colleges over this time period.[29] This differential increase in enrollment is consistent with the dilution of resources per student and the consequent increases in time to degree in the non-top 15 public and two-year colleges and universities found in the data.

We also considered the extent to which press accounts identify queuing and excess demand. These qualitative accounts provide unambiguous evidence that enrollment limitation occurs in higher education, even in those institutions like community colleges purported to be "open access" (see Appendix A). In short, supply in higher education is not perfectly elastic. Within states, where the local market is likely to define collegiate options for the marginal college student, supply constraints appear to turn many students away from even community colleges, presumably limiting degree completion and slowing time to degree for those who do complete.

4.4.3 Increased Time Spent Working and Potential Credit Constraints

Between 1972 and 1992, average weekly hours worked (unconditional) among those enrolled in college increased by about 2.9 hours, from 9.5 to 12.4, as measured for 18-21

[29]Authors' calculations from the 1972-1992 HEGIS/IPEDS surveys.

year old college students in the October CPS, with a further increase to 13.2 hours per week evident in 2005. Consistent with observations from the CPS, the comparison of the NLS72 and NELS:88 cohorts also show hours worked rose sharply for students in their first year of college.[30] For the full sample, average unconditional weekly hours worked increased from 7.1 to 14.9 and increased from 22.4 to 28.6 on the intensive margin. This increase in working behavior occurred differently across initial school types. For the public non-top 15 sample, average hours increased from 6.0 to 12.8 and from 11.0 to 21.0 hours for students in the sample entering two-year colleges. In the public top 15 and private sectors, the increases were more modest, from 4.9 to 9.3 and from 2.4 to 5.1 hours, respectively.

Figure 4.6 shows the distribution of work hours for those enrolled in college in the first year post high school cohort graduation by initial school type. As shown in the figure, there is a marked increase in the share of students enrolled in college who are also working (hours greater than zero) across all school types, though the increases are greatest among the two-year and non-top 15 public samples.

Figure 4.7 contains similar employment information by school type from the CPS, which contains much better measures of hours worked than in the NCES surveys. Panel A of Figure 4.7 shows the steady rise in employment rates among 18-19 year old college students, particularly in the two-year and four-year public sectors. Panel B further explores these trends by presenting the percent of enrolled students working more than 20 hours per week. The figure reinforces the distinct separation in employment behavior between students at two-year and four-year institutions, with the former group systematically more likely to be employed and working more than 20 hours per week. Focusing on differences among students at four-year institutions, students at public and private four-year institutions demonstrate very similar employment behavior in the early 1970s yet, beginning in the 1980s, students at the public institutions are not only more likely to be employed but also more likely to work more than 20 hours relative to those at private institutions.

[30]The NELS:88 survey does not allow one to track work histories fully between the 1994 and 2000 follow-ups. Thus, we restrict the analysis of working hours in both surveys to those enrolled in college in the first year following high school cohort graduation.

While data restrictions limit our ability to disentangle whether increases in hours employed take time away from collegiate progress or simply reduce leisure, we can use the observation of increased employment to bound the potential effects of hours worked on time to degree. Consider a student with a time budget of 60 hours per week available for course work and employment. With this fixed budget, increased hours worked necessarily reduce the time available for study. We measure the extent to which "effective time to degree," measured as the amount of non-working time, in years, it takes each individual to obtain a baccalaureate degree out of high school, has changed over time. For example, if a student works twenty hours a week, he then will have only $\frac{2}{3}$ of his time for study. If we observe it takes this student five years to graduate, then we calculate the effective time to degree as: $\frac{2}{3} * 5 = 3\frac{1}{3}$. We make this calculation for each student in our samples and compare outcomes for the 1972 and 1992 high school cohorts. Assuming working hours fully crowd out time spent on academic pursuits, we estimate a decrease in "effective" time to degree from 4.24 to 4.04 years for the full sample of BA recipients. For the public non-top15 sample, the decrease is somewhat smaller, from 4.26 to 4.15 but is 4.32 to 4.04 for the two-year sample. Our estimates imply the changes in working behavior between students in the NLS72 and NELS:88 cohorts are sufficiently large to explain the average time to degree increase observed in the data across all initial school types.

The observation of increased employment while enrolled in college is consistent with the hypothesis that increased hours in the labor force essentially displace hours available for study, thus prolonging time to degree. Yet, increased employment among college students certainly does not present causal proof of this hypothesis.[31] At issue is whether additional hours at work take time away from leisure or schooling. To the extent additional hours in the labor force do reflect a tradeoff with collegiate hours, the causes are superficially indeterminate, potentially reflecting difficulties in college financing, changes in macro eco-

[31] Credibly estimating the effect of working while in school on college performance is difficult, given the potential endogeneity of the decision to work and hours of employment. Stinebrickner and Stinebrickner (2003) use data from Berea College in Kentucky where all students are expected to work and all admitted students have financial need. Random assignment of first year students to various jobs when they arrive at the college provides an instrument for hours worked, and their IV estimates imply a strong negative effect of hours worked on academic performance.

nomic conditions or constraints on the supply side of higher education. While cyclical downturns do reduce employment of college students, the differences in macro economic conditions faced by these cohorts are sufficiently small to explain less than 15% of the observed increase in employment.[32]

During the 1980s and 1990s, the family incomes of those attending college increased less rapidly than the rise in college costs. While tuition and room and board expenses at public four-year colleges increased by a factor of three in nominal terms from 1986 to 2005, family incomes - particularly below the median - increased much less rapidly.[33] Such shifts are predicted to lead to a reduction in college enrollment among students from relatively low income families and an increase in the fraction of college costs paid by students relative to parents, with increased student employment resulting from limited credit markets. Belley and Lochner (2007) show that family income has become a more important determinant of college enrollment and attainment over the last two decades, while student employment among high achieving low income youth has increased.[34] Indeed, Keane and Wolpin (2001) predict that, in a forward-looking dynamic model with limited access to credit, increases in employment while enrolled in school are the expected response to tuition increases.

An alternative explanation is that the mechanism of institutional crowding discussed in the prior section leaves students with more time for employment if they are limited in their access to courses for full-time continuous enrollment. In this context, we explore whether substantial growth in the college-age population corresponds with increased employment and hours among those enrolled. Results are reported in Table 4.9 and show

[32]Regressing hours employed among those enrolled in college on the state level unemployment rate from 1977 to 2006 yields a coefficient of -0.417 (0.049), with state fixed effects, and -0.284 (0.043), with state effects and a linear trend. Assuming an average decrease in the unemployment rate of about 1 percentage point from the period of enrollment for the class of 1972 to the class of 1992 would imply that no more than 15% of the increase in employment could be attributed to changes in macro economic conditions.

[33]Combined tuition, room and board at public universities rose from $4146 to $12,604, a change of over 300%, at public universities (Table 31, *Digest of Education Statistics*), while nominal family incomes for those enrolled in college increased more modestly, with increases of 40% and 73% for the 25^{th} and 50^{th} percentiles (authors' tabulations from the March CPS). It is possible that increases in tuition at institutions charging by the term might well lead to reductions in time to degree as students give up current leisure to reduce the tuition burden. Empirically, while the 4-year completion rate fell from 57% to 51% at those institutions with flat fee structures, it fell from 59% to 36% at those institutions with per credit pricing structures. We hesitate to infer too much from this evidences as fee structure may well be correlated with other institutional circumstances, as well as changes over time in the generosity of institutional aid and the selection of the student body.

[34]Scott-Clayton (2007) suggests credit constraints may be an important explanation for increased student employment and, using CPS data that show no shifts toward higher skill jobs based on industry and occupation codes, she finds no support for the alternative explanation that increased employment is linked to a rising return to work experience.

within state changes in cohort size are strongly related to hours and employment. Further, the effects are most pronounced for students at public four-year colleges and universities. These estimates are consistent with the interpretation that the crowding mechanism is partly responsible for inducing students to work more while in school. Qualitative evidence makes the proximate connection among supply constraints, working, and degree completion. Responding to the overcrowding and constraints at local community colleges in Riverside California, one high school counselor noted, “They [students] get jobs, they get apartments and then have bills to pay. It is easy for them to get off track, and hard to get back on” (Peoples, 1995).

We identify institutional resource constraints and difficulties faced by individuals in financing college as plausibly significant factors in the increased employment of students while in college and, in turn, we suspect both operate through increased employment to increase time to degree and reduce completion rates.

4.5 Discussion

There is no ambiguity in the data with respect to the growth over the last three decades in the time elapsed for college graduates between high school completion and the receipt of the BA degree. While we focus our analysis on the inter-cohort comparison afforded by NLS72 (the high school class of 1972) and NELS:88 (the high school class of 1992), this finding is reiterated in other data sets including the CPS and the National Survey of College Graduates. By looking at the type of college at which individuals begin their postsecondary careers, it is clear the rise in time to degree is largely concentrated among students beginning at non-top 15 public universities and two-year colleges.

We find no evidence that changes in the characteristics of college entrants explain the drop in completion rates or outward shift in the time to degree distribution for the full sample. In fact, among students starting at four-year institutions, changes in characteristics of college graduates explain none of the observed increase in time to degree or decrease in completion rates. We do, however, find changes in background characteristics of students explain about half of the completion rate decline among those whose initial institution is a two-year school.

We have found evidence the observed increase in time to degree is tied to growth within states in the college-age population. This is consistent with the notion that "cohort crowding" at the college level has served to limit resources available to students and to slow their progress through school. The mechanisms by which this might occur, however, deserve more attention in future research.

Lastly, students in the more recent cohorts are working a significant number of hours while they are in school. For many students, family economic circumstances have eroded while college costs have increased, contributing to the need to increase employment to cover a greater share of college costs through wage earnings. While the magnitude of the effect of increased employment on degree progress is hard to ascertain with precision, the direction of the effect is unambiguous.

That increases in time to degree are not tied directly to declines in the pre-collegiate preparation of students suggests the underlying rate at which students complete college studies may be impeded by limited availability of courses and institutional resources more generally at public colleges as well as increased difficulties faced by individuals in financing full-time collegiate study. That increases in time to degree are concentrated among students attending public colleges and universities outside the most selective few is grounds for particular concern. To the extent budget pressures in the funding of public higher education extend time to degree, the inequality in the distribution of resources across sectors in higher education has increased. As such, increased time to degree among students beginning college at these institutions suggests a need for more attention to whether these institutions are able to meet student demand and to how students at these colleges finance higher education.

Acknowledgments

This chapter is co-authored with Professor John Bound and Professor Sarah Turner.

Table 4.1: Changes Over Time Between First Institution and Graduating Institution Types for Those Obtaining a BA Within Eight Years of Cohort High School Graduation

Panel A: Full Sample						
	NLS72 Cohort			NELS:88 Cohort		
	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Initial Institution Type	Enrollment Rate of High School Graduates	College Attendees	Eight Year BA	Enrollment Rate of High School Graduates	College Attendees	Eight Year Year BA
Four-Year Public	26.5%	51.2%	60.2%	33.8%	43.9%	54.9%
Four-Year Private	9.9%	20.0%	26.7%	13.1%	17.5%	30.3%
Two-Year	16.7%	28.8%	13.1%	33.9%	38.5%	14.8%
Total	53.1%	100%	100%	80.8%	100%	100%

Panel B: 8-Year College Graduates						
	NLS72 Cohort			NELS:88 Cohort		
	(i)	(ii)	(iii)	(iv)	(v)	(vi)
		Institution of BA			Institution of BA	
Initial Institution Type	Completion Rate: 8 Year	Four-Year Public	Four-Year Private	Completion Rate: 8 Year	Four-Year Public	Four-Year Private
Four-Year Public	60.1%	65.0%	35.0%	56.7%	93.3%	6.7%
Four-Year Private	68.4%	12.9%	87.1%	78.2%	10.1%	89.9%
Two-Year	23.2%	64.5%	35.5%	17.4%	73.8%	26.2%
Total	51.1%			45.3%		

[1] Source: Authors' calculation from the NLS72 and NELS:88 surveys. NLS72 calculations were made using the fifth follow-up weights included in the survey. Fourth follow-up weights were used for the NELS:88 survey calculations. Only those participating in these follow-ups are included in the tabulations.

[2] The NLS72 and NELS:88 samples are restricted to those who attend college within 2 years of cohort high school graduation. Cohort high school graduation is defined as June 1972 for the NLS72 sample and June 1992 for the NELS:88 sample.

Table 4.2: **Cumulative Time to Degree Distributions for College Graduates Within 8 Years of Cohort High School Graduation for Full Sample and by First Institution Attended**

Full Sample	**Years out of High School** 4	5	6	7	8	**Completion Rate**	**Mean**	**Test for First Order Dominance**
NLS72	56.8%	83.8%	92.4%	97.0%	100%	51.1%	4.67	
NELS:88	43.6%	76.2%	89.6%	95.9%	100%	45.3%	4.93	
Difference	-13.2%	-7.6%	-2.8%	-1.1%	0.0%	-5.8%	0.26	
P-Value	0.000	0.000	0.001	0.059		0.000		0.000
Public 4-Year	**Years out of High School** 4	5	6	7	8	**Completion Rate**	**Mean**	**Test for First Order Dominance**
NLS72	54.6%	84.6%	92.9%	97.0%	100%	60.1%	4.68	
NELS:88	37.1%	76.6%	90.3%	95.7%	100%	56.7%	4.99	
Difference	-17.5%	-8.0%	-2.6%	-1.3%	0.0%	-3.4%	0.31	
P-Value	0.000	0.000	0.020	0.117		0.122		0.015
Private 4-Year	**Years out of High School** 4	5	6	7	8	**Completion Rate**	**Mean**	**Test for First Order Dominance**
NLS72	71.7%	89.9%	94.9%	98.4%	100%	68.4%	4.42	
NELS:88	66.4%	88.4%	95.4%	98.9%	100%	78.2%	4.47	
Difference	-5.3%	-1.5%	0.5%	0.5%	0.0%	9.8%	-0.05	
P-Value	0.040	0.388	0.688	0.368		0.000		1.000
Two Year	**Years out of High School** 4	5	6	7	8	**Completion Rate**	**Mean**	**Test for First Order Dominance**
NLS72	35.9%	67.7%	85.2%	93.8%	100%	23.2%	5.15	
NELS:88	21.0%	49.8%	75.4%	90.3%	100%	17.4%	5.62	
Difference	-14.9%	-17.9%	-9.8%	-3.5%	0.0%	-5.8%	0.47	
P-Value	0.001	0.000	0.002	0.071		0.001		0.000

[1] Source: Authors' calculation from the NLS72 and NELS:88 surveys. NLS72 calculations were made using the fifth follow-up weights included in the survey. Fourth follow-up weights were used for the NELS:88 survey calculations. Only those participating in these follow-ups are included in the tabulations.

[2] The NLS72 and NELS:88 samples are restricted to those who attend college within 2 years of cohort high school graduation. Cohort high school graduation is defined as June 1972 for the NLS72 sample and June 1992 for the NELS:88 sample.

[3] P-values in the table refer to tests for statistical significance of the differences. The p-values of the test for first-order dominance are based on 5000 bootstrap replications of the data, clustered at the high school level, and show the percentage of the replications for which the NLS72 distribution is less than the NELS:88 distribution for years 4, 5 and 6.

Table 4.3: Cumulative BA Distributions, Public Universities by Rank of First Institution Attended

Panel A: Public 4-Year								
	Years out of High School					**Completion**		**Test**
Cohort	4	5	6	7	8	**Rate**	**Mean**	**for First**
NLS72	54.6%	84.6%	92.9%	97.0%	100%	60.1%	4.68	**Order**
NELS:88	37.1%	76.6%	90.3%	95.7%	100%	56.7%	4.99	**Dominance**
Difference	-17.5%	-8.0%	-2.6%	-1.3%	0.0%	-3.4%	0.31	
P-Value	0.000	0.000	0.020	0.117		0.122		0.015
Panel B: Top 15-Non-top 15 Public 4-Year								
Top 15	**Years out of High School**					**Completion**		**Test**
Public	4	5	6	7	8	**Rate**	**Mean**	**for First**
NLS72	53.8%	80.3%	90.9%	96.1%	100%	76.1%	4.77	**Order**
NELS:88	54.8%	90.1%	98.1%	98.9%	100%	90.6%	4.57	**Dominance**
Difference	1.0%	9.9%	7.2%	2.8%	0.0%	14.5%	-0.20	
P-Value	0.889	0.038	0.008	0.186		0.003		1.000
Non-top 15 Public								
NLS72	54.7%	84.8%	93.0%	97.1%	100%	59.4%	4.68	
NELS:88	35.0%	75.1%	89.4%	95.3%	100%	54.3%	5.04	
Difference	-19.7%	-9.7%	-3.6%	-1.8%	0.0%	-5.1%	0.37	
P-Value	0.000	0.000	0.003	0.057		0.022		0.002
Panel C: Top 50-Non-top 50 Public 4-Year								
Top 50	**Years out of High School**					**Completion**		**Test**
Public	4	5	6	7	8	**Rate**	**Mean**	**for First**
NLS72	54.3%	83.5%	92.7%	96.8%	100%	72.7%	4.70	**Order**
NELS:88	44.6%	84.3%	94.0%	96.5%	100%	83.7%	4.80	**Dominance**
Difference	-9.7%	0.8%	1.3%	-0.3%	0.0%	11.0%	0.10	
P-Value	0.014	0.766	0.438	0.838		0.000		1.000
Non-top 50 Public								
NLS72	54.7%	84.9%	92.9%	97.1%	100%	57.4%	4.68	
NELS:88	34.0%	73.5%	88.8%	95.4%	100%	50.0%	5.07	
Difference	-20.7%	-11.4%	-4.1%	-1.7%	0.0%	-7.4%	0.39	
P-Value	0.000	0.000	0.003	0.102		0.002		0.000

[1] Source: Authors' calculation from the NLS72 and NELS:88 surveys. School rankings are taken from the 2005 US News and World Report top college and university rankings. NLS72 calculations were made using the fifth follow-up weights included in the survey. Fourth follow-up weights were used for the NELS:88 survey calculations. Only those participating in these follow-ups are included in the tabulations

[2] The NLS72 and NELS:88 samples are restricted to those who attend college within 2 years of cohort high school graduation. Cohort high school graduation is defined as June 1972 for the NLS72 sample and June 1992 for the NELS:88 sample.

[3] P-values in the table refer to tests for statistical significance of the differences. The p-values of the test for first-order dominance are based on 5000 bootstrap replications of the data, clustered at the high school level, and show the percentage of the replications for which the NLS72 distribution is less than the NELS:88 distribution for years 4, 5 and 6.

Table 4.4: Means of Selected Variables

Variable	All College Attendees		College Graduates Who Obtain a BA Within Eight Years of Cohort High School Graduation	
	NLS72	**NELS:88**	**NLS72**	**NELS:88**
Average Time to Degree	4.674 (1.040)	4.929 (1.114)	4.674 (1.040)	4.929 (1.114)
Lowest Reading Quartile	0.145 (0.319)	0.180 (0.362)	0.088 (0.255)	0.081 (0.258)
Second Reading Quartile	0.212 (0.363)	0.229 (0.382)	0.173 (0.337)	0.162 (0.339)
Third Reading Quartile	0.292 (0.379)	0.277 (0.408)	0.297 (0.378)	0.289 (0.418)
Top Reading Quartile	0.350 (0.413)	0.314 (0.439)	0.441 (0.428)	0.468 (0.474)
Lowest Math Quartile	0.112 (0.289)	0.142 (0.334)	0.059 (0.214)	0.037 (0.175)
Second Math Quartile	0.216 (0.366)	0.219 (0.387)	0.149 (0.314)	0.152 (0.339)
Third Math Quartile	0.266 (0.392)	0.286 (0.424)	0.251 (0.384)	0.288 (0.426)
Top Math Quartile	0.406 (0.451)	0.353 (0.464)	0.541 (0.452)	0.522 (0.482)
Father – No HS Diploma	0.222 (0.414)	0.123 (0.292)	0.171 (0.375)	0.059 (0.207)
Father – HS Diploma	0.263 (0.438)	0.306 (0.418)	0.222 (0.414)	0.207 (0.373)
Father – Some College	0.242 (0.427)	0.216 (0.376)	0.248 (0.431)	0.219 (0.382)
Father – BA	0.162 (0.368)	0.187 (0.363)	0.200 (0.400)	0.251 (0.407)
Father – Graduate School	0.112 (0.315)	0.163 (0.355)	0.159 (0.366)	0.264 (0.425)
Mother – No HS Diploma	0.181 (0.383)	0.114 (0.295)	0.133 (0.338)	0.052 (0.200)
Mother – HS Diploma	0.391 (0.487)	0.348 (0.445)	0.368 (0.481)	0.261 (0.416)
Mother – Some College	0.267 (0.442)	0.248 (0.399)	0.283 (0.449)	0.262 (0.413)
Mother – BA	0.118 (0.322)	0.173 (0.355)	0.154 (0.360)	0.247 (0.407)

Mother – Graduate School	0.043	0.117	0.063	0.178
	(0.202)	(0.302)	(0.243)	(0.363)
Income 3,000/10,000	0.040	0.067	0.024	0.031
	(0.178)	(0.236)	(0.137)	(0.166)
Income 6,000/20,000	0.070	0.108	0.048	0.061
	(0.233)	(0.290)	(0.195)	(0.226)
Income 7,500/25,000	0.070	0.077	0.067	0.062
	(0.232)	(0.248)	(0.230)	(0.230)
Income 10,500/35,000	0.200	0.134	0.188	0.101
	(0.366)	(0.318)	(0.361)	(0.285)
Income 15,000/50,000	0.270	0.213	0.267	0.200
	(0.402)	(0.386)	(0.402)	(0.379)
Income 15,000+/50,000+	0.350	0.400	0.406	0.544
	(0.442)	(0.471)	(0.455)	(0.480)
Asian	0.014	0.047	0.019	0.059
	(0.111)	(0.212)	(0.129)	(0.235)
Hispanic	0.029	0.097	0.016	0.052
	(0.165)	(0.296)	(0.124)	(0.223)
African American	0.093	0.109	0.073	0.073
	(0.289)	(0.312)	(0.259)	(0.260)
White	0.852	0.738	0.884	0.812
	(0.356)	(0.440)	(0.320)	(0.391)
Native American	0.006	0.008	0.003	0.003
	(0.074)	(0.090)	(0.053)	(0.058)
Number of Observations	7107	8417	4284	4179

[1] Source: Authors' tabulations from the NLS72 and NELS:88 surveys. Standard deviations are in parentheses. NLS72 calculations were made using the fifth follow-up weights included in the survey. Fourth follow-up weights were used for the NELS:88 survey calculations. Only those participating in these follow-ups are included in the tabulations.
[2] The NLS72 and NELS:88 samples are restricted to those who attend college within 2 years of cohort high school graduation. Cohort high school graduation is defined as June 1972 for the NLS72 sample and June 1992 for the NELS:88 sample.

Table 4.5: **Cumulative BA Distribution, Multivariate Re-weighting NELS:88 using NLS72 Individual Background Characteristics**

Panel A: Full Sample							
	Years out of High School					Completion	P
Cohort	4	5	6	7	8	Rate	Value
NLS72	56.8%	83.8%	92.4%	97.0%	100%	51.1%	of FOSD
NELS:88	43.6%	76.2%	89.6%	95.9%	100%	45.3%	Test
NELS:88 Reweighted	39.6%	71.9%	87.1%	94.2%	100%	45.0%	
P-Value of NELS:88 Reweight - NLS72	0.000	0.000	0.000	0.000		0.000	0.000

Panel B: Public Four-Year Non-top 15 Sample							
	Years out of High School					Completion	P
Cohort	4	5	6	7	8	Rate	Value
NLS72	54.7%	84.8%	93.0%	97.1%	100%	59.4%	of FOSD
NELS:88	35.0%	75.0%	89.4%	95.3%	100%	54.3%	Test
NELS:88 Reweighted	34.4%	73.3%	87.8%	93.8%	100%	52.0%	
P-Value of NELS:88 Reweight - NLS72	0.000	0.000	0.000	0.001		0.000	0.000

Panel C: Public Two-Year Sample							
	Years out of High School					Completion	P
Cohort	4	5	6	7	8	Rate	Value
NLS72	35.9%	67.7%	85.2%	93.8%	100%	23.2%	of FOSD
NELS:88	21.0%	49.8%	75.4%	90.3%	100%	17.4%	Test
NELS:88 Re-weighted	19.9%	47.8%	74.0%	87.6%	100%	20.1%	
P-Value of NELS:88 Reweight - NLS72	0.000	0.000	0.000	0.002		0.042	0.000

[1] Source: Authors' calculation from the NLS72 and NELS:88 surveys. NLS72 calculations were made using the fifth follow-up weights included in the survey. Fourth follow-up weights were used for the NELS:88 survey calculations. Only those participating in these follow-ups are included in the tabulations.
[2] The NLS72 and NELS:88 samples are restricted to those who attend college within 2 years of cohort high school graduation. Cohort high school graduation is defined as June 1972 for the NLS72 sample and June 1992 for the NELS:88 sample.
[3] School type samples refer to first institution attended.
[4] All p-values in the table are based on 5000 bootstrap replications of the data, clustered at the high school level. The p-value for the test of first-order dominance shows the percentage of the replications for which the NLS72 distribution is less than the NELS:88 distribution for years 4, 5 and 6.

Table 4.6: Cumulative BA Distribution, Multivariate Re-weighting NELS:88 using NLS72 Initial School Type and Individual Background Characteristics

	Years out of High School					Completion	P
Cohort	4	5	6	7	8	**Rate**	**Value of FOSD Test**
NLS72	56.8%	83.8%	92.4%	97.0%	100%	51.1%	
NELS:88	43.6%	76.2%	89.6%	95.9%	100%	45.3%	
Initial School Type	42.2%	75.8%	89.4%	95.8%	100%	49.2%	
Initial School Type and Background Characteristics	39.9%	72.8%	87.6%	94.4%	100%	48.0%	
P-Value of NELS:88 Reweight with Initial School Type - NLS72	0.000	0.000	0.001	0.015		0.092	0.001
P-Value of NELS:88 Reweight with Initial School Type and Background Characteristics - NLS72	0.000	0.000	0.000	0.000		0.013	0.000

[1] Source: Authors' calculation from the NLS72 and NELS:88 surveys. NLS72 calculations were made using the fifth follow-up weights included in the survey. Fourth follow-up weights were used for the NELS:88 survey calculations. Only those participating in these follow-ups are included in the tabulations.
[2] The NLS72 and NELS:88 samples are restricted to those who attend college within 2 years of cohort high school graduation. Cohort high school graduation is defined as June 1972 for the NLS72 sample and June 1992 for the NELS:88 sample.
[3] School type indicators included in the weighting regressions are: public non-top 15, public top 15, private 4-year and two-year. School type indicators and samples refer to first institution attended.
[4] All p-values in the table are based on 5000 bootstrap replications of the data, clustered at the high school level. The p-value for the test of first-order dominance shows the percentage of the replications for which the NLS72 distribution is less than the NELS:88 distribution for years 4, 5 and 6.

Table 4.7: State-level Estimates of the Effect of Crowding On Multiple Time to Degree Measures

Independent Variable: Change in Log 18-Year Old Population (1992-2002)		
	2^{nd} Stage	
Dependent Variable	**Actual - Actual Coefficients**	**Counterfactual 92 - Actual 72 Coefficients**
8 Year Completion Indicator - All HS Graduates	-0.348**	-0.120**
	(0.093)	(0.071)
8 Year Completion Indicator - All College Attendees	-0.264**	-0.087
	(0.107)	(0.100)
8 Year Completion Indicator - Public Non-top 15	-0.051	0.042
	(0.138)	(0.128)
4 Year Completion Indicator	-0.181*	-0.134
	(0.092)	(0.097)
4 Year Completion Indicator - Public Non-top 15	-0.229**	-0.261**
	(0.103)	(0.119)
Log TTD	0.112**	0.094**
	(0.041)	(0.043)
Log TTD - Public Non-top 15	0.129**	0.132**
	(0.048)	(0.053)

[1] Source: Authors' calculations as described in the text from the NLS72 and NELS:88 surveys. NLS72 calculations were made using the fifth follow-up weights included in the survey. Fourth follow-up weights were used for the NELS:88 survey calculations. Only those participating in these follow-ups are included in the regression.

[2] Robust standard errors are in parentheses: ** indicates significance at the 5 percent level and * indicates significance at the 10 percent level.

[3] The regressions using the four-year completion indicators as dependent variables include only those who obtain a BA within 8 years of cohort high school graduation. All samples include only those who begin college within 2 years of cohort high school graduation. Cohort high school graduation is defined as June 1972 for the NLS72 cohort and June 1992 for the NELS:88 cohort. The Public Non-top 15 samples refer to initial institution of the respondent.

Table 4.8: **Undergraduate Student-Faculty Ratios at Graduating Institution by Initial School Type**

Public Non-top 15 Universities					
		Percentile			
Survey	**Mean**	**25^{th}**	**50^{th}**	**75^{th}**	**90^{th}**
NLS72	19.17	15.24	17.37	20.83	25.34
NELS:88	21.99	18.26	20.85	24.30	29.20
Public Top 15 Universities					
		Percentile			
Survey	**Mean**	**25^{th}**	**50^{th}**	**75^{th}**	**90^{th}**
NLS72	17.41	13.57	17.05	21.17	22.43
NELS:88	15.90	13.21	14.06	19.66	21.43
Private Universities					
		Percentile			
Survey	**Mean**	**25^{th}**	**50^{th}**	**75^{th}**	**90^{th}**
NLS72	16.62	13.20	15.69	18.78	23.02
NELS:88	18.63	13.06	16.50	20.68	26.73
Two-Year Universities					
		Percentile			
Survey	**Mean**	**25^{th}**	**50^{th}**	**75^{th}**	**90^{th}**
NLS72	18.76	15.81	18.16	21.34	22.67
NELS:88	24.53	18.45	21.41	25.42	35.32

[1] Source: Authors' calculations as described in the text from the NLS72 and NELS:88 surveys. NLS72 calculations were made using the fifth follow-up weights included in the survey. Fourth follow-up weights were used for the NELS:88 survey calculations. Only those participating in these follow-ups are included in the regression. Data on faculty are from the HEGIS/IPEDS surveys from the Department of Education.

[2] Tabulations include only those that graduate within 8 years of cohort high school graduation. Cohort high school graduation is defined as June 1972 for the NLS72 cohort and June 1992 for the NELS:88 cohort. All institution types refer to initial institution attended by respondent.

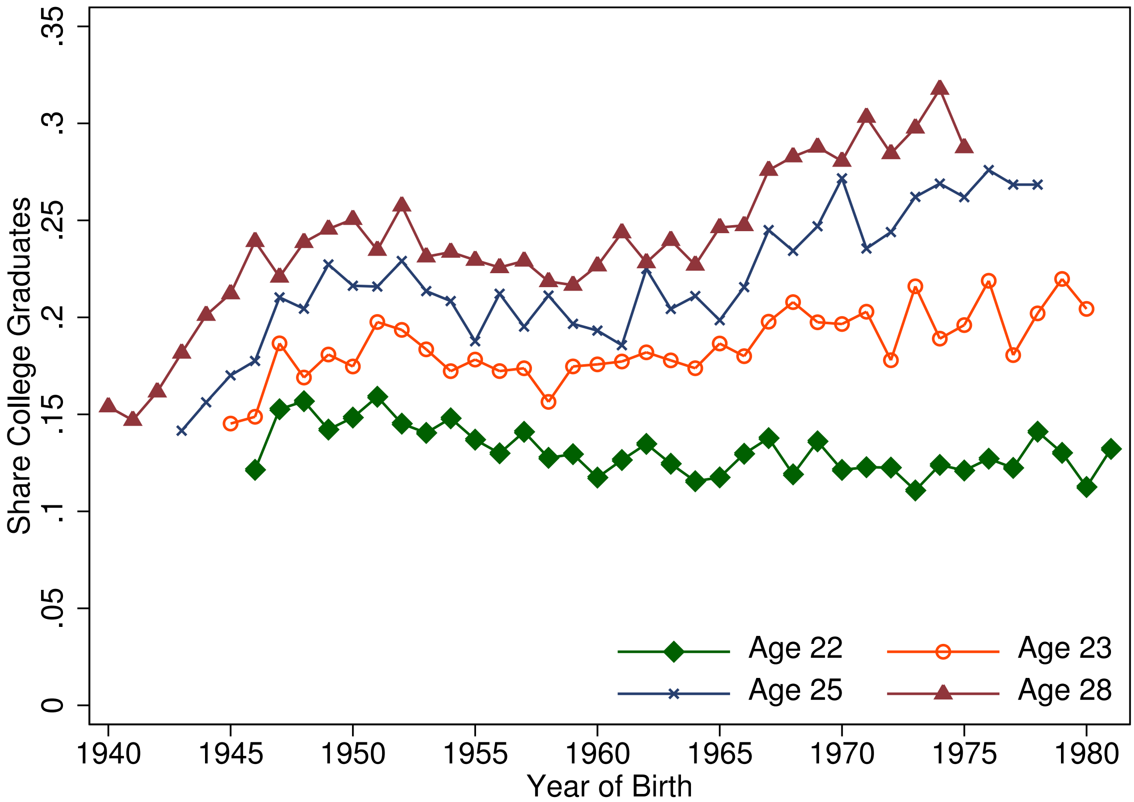
Table 4.9: Regressions of Hours and Employment on Cohort Size, by Type of Institution: 1976-2003

	Coefficient on Ln Pop 18		
Sample	**Hours (OLS)**	**Hours (Tobit)**	**Employment (OLS)**
All Enrollment	2.007**	3.417**	0.051*
	(0.845)	(1.595)	(0.031)
Public College	0.983	1.093	0.009
	(1.021)	(1.870)	(0.037)
Public 4-Year	4.150**	7.908**	0.101**
	(1.124)	(2.374)	(0.045)
Public 2-Year	0.111	0.405	-0.010
	(1.940)	(2.840)	(0.062)
State Fixed Effects	Y	Y	Y
Year Fixed Effects	Y	Y	Y
State Tuition	Y	Y	Y
Unemployment Rate	Y	Y	Y
Age Fixed Effects	Y	Y	Y

[1] Authors' tabulations as described in the text using CPS data from 1977-2003 for the sample aged 18-20. Each cell represents a separate regression and includes CPS sample weights.

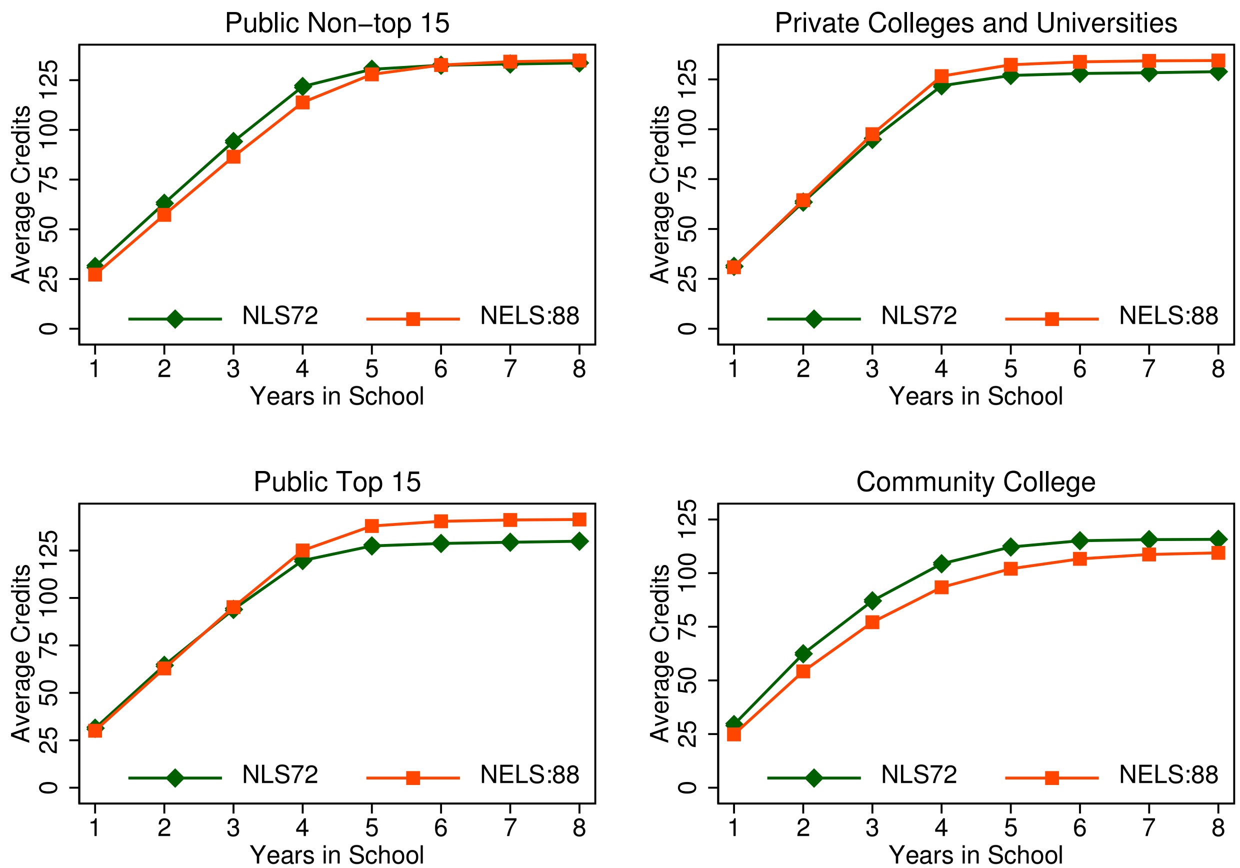
[2] Robust standard errors are in parentheses: ** indicates significance at the 5 percent level and * indicates significance at the 10 percent level.

Figure 4.1: College Completion Rate by Age, 1940-1980 Birth Cohorts



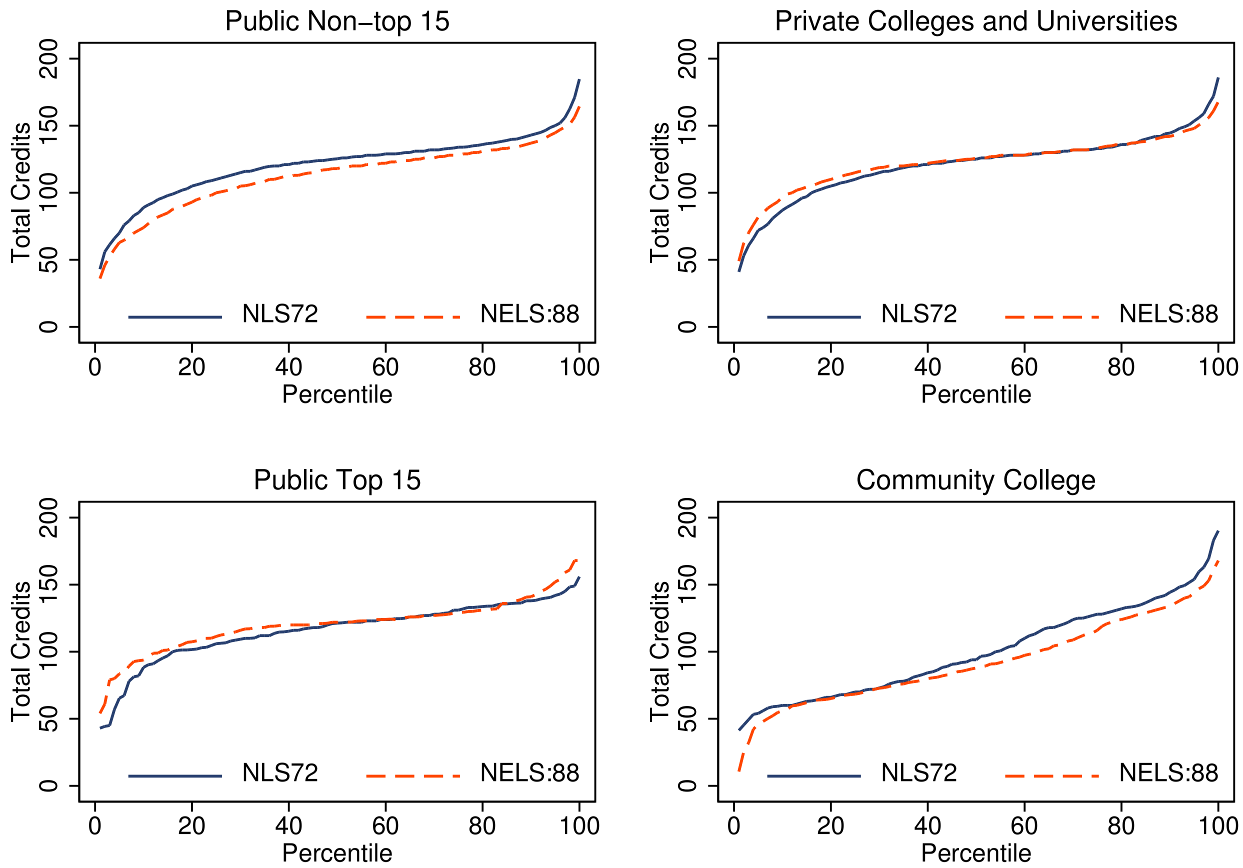
Source: Data are from authors' tabulations using the October CPS, 1968-2005. Individual weights are employed. See Turner (2005) for additional detail

Figure 4.2: Credit Accumulation by Type of Initial Institution for Eight-Year BA Recipients



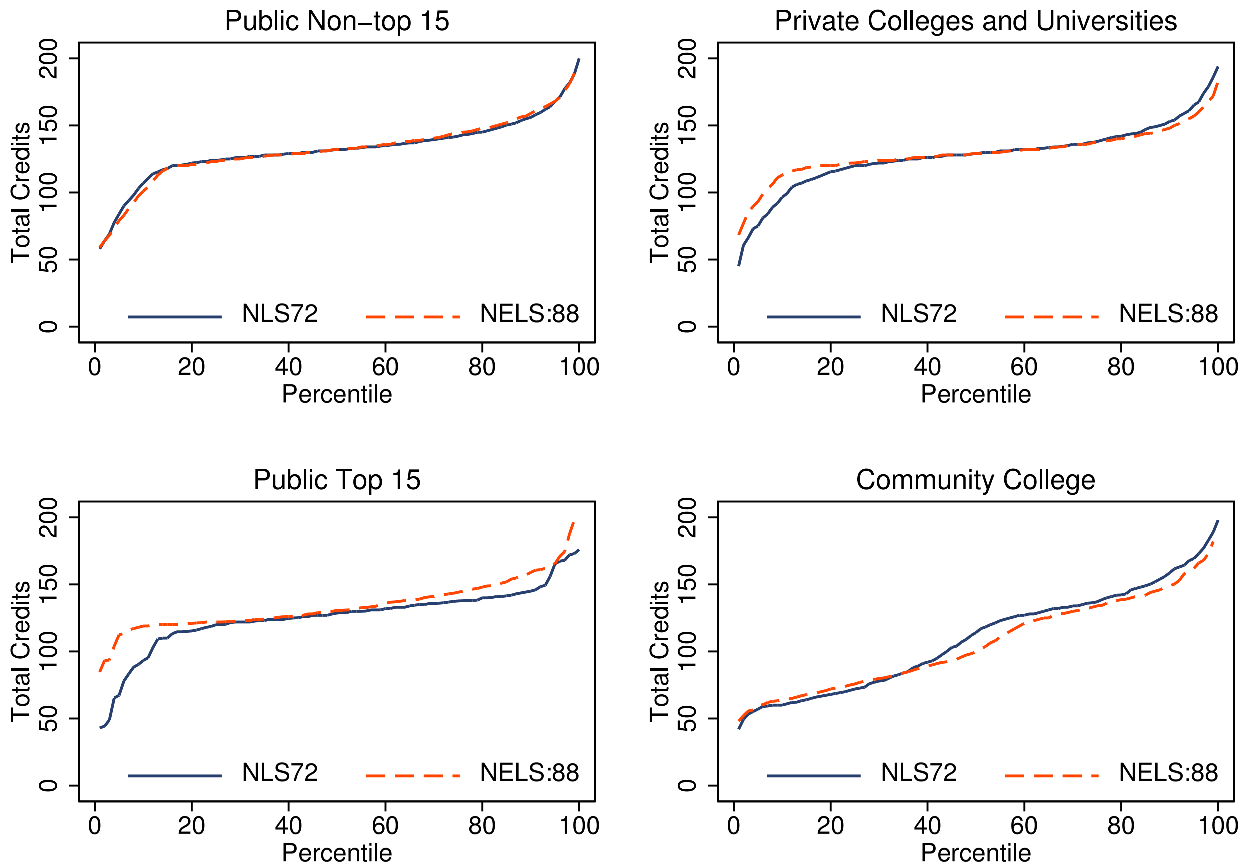
[1] Source: Authors' calculations from the NLS72 and NELS:88 transcript surveys. NLS72 calculations were made using the fifth follow-up weights included in the survey. Fourth follow-up weights were used for the NELS:88 survey calculations. Only those participating in these follow-ups are included in the tabulations.
[2] The credit accumulation schedules represent total average credits by years enrolled in college. For those who graduate in less than eight years, their number of credits are held constant for each subsequent year post graduation.
[3] The NLS72 and NELS:88 samples are restricted to those who attend college within 2 years of cohort high school graduation and receive a BA within eight years. Cohort high school graduation is defined as June 1972 for the NLS72 sample and June 1992 for the NELS:88 sample. The figure does not include outliers who accumulate more than 45 credits per year.

Figure 4.3: Distribution of Accumulated Credits by Initial School Type: 4 Years after Cohort High School Graduation for all BA Recipients



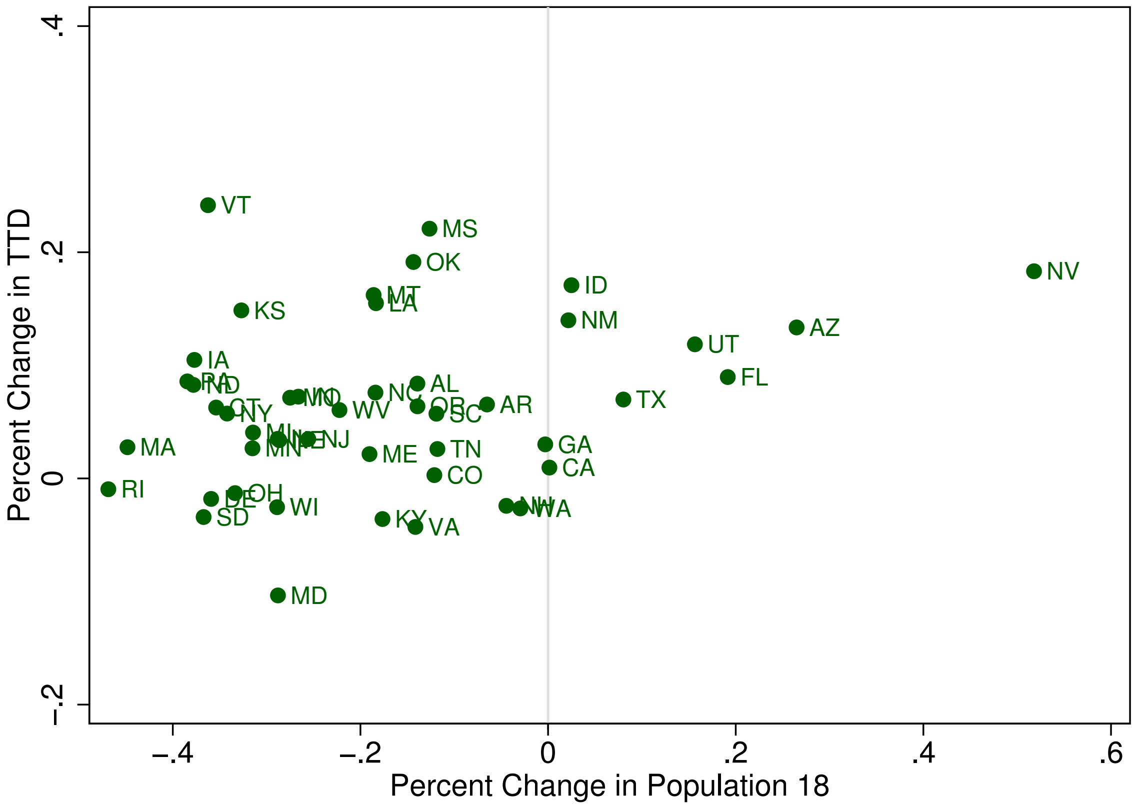
[1] Source: Authors' calculations from the NLS72 and NELS:88 transcript surveys. NLS72 calculations were made using the fifth follow-up weights included in the survey. Fourth follow-up weights were used for the NELS:88 survey calculations. Only those participating in these follow-ups are included in the tabulations.
[2] The credit distributions represent the CDF of total accumulated credits as of the fourth year after cohort high school graduation.
[3] The NLS72 and NELS:88 samples are restricted to those who attend college within 2 years of cohort high school graduation and receive a BA within eight years. Cohort high school graduation is defined as June 1972 for the NLS72 sample and June 1992 for the NELS:88 sample. The figure does not include outliers who accumulate more than 45 credits per year.

Figure 4.4: Distribution of Accumulated Credits by Initial School Type: 8 Years after Cohort High School Graduation for all BA Recipients



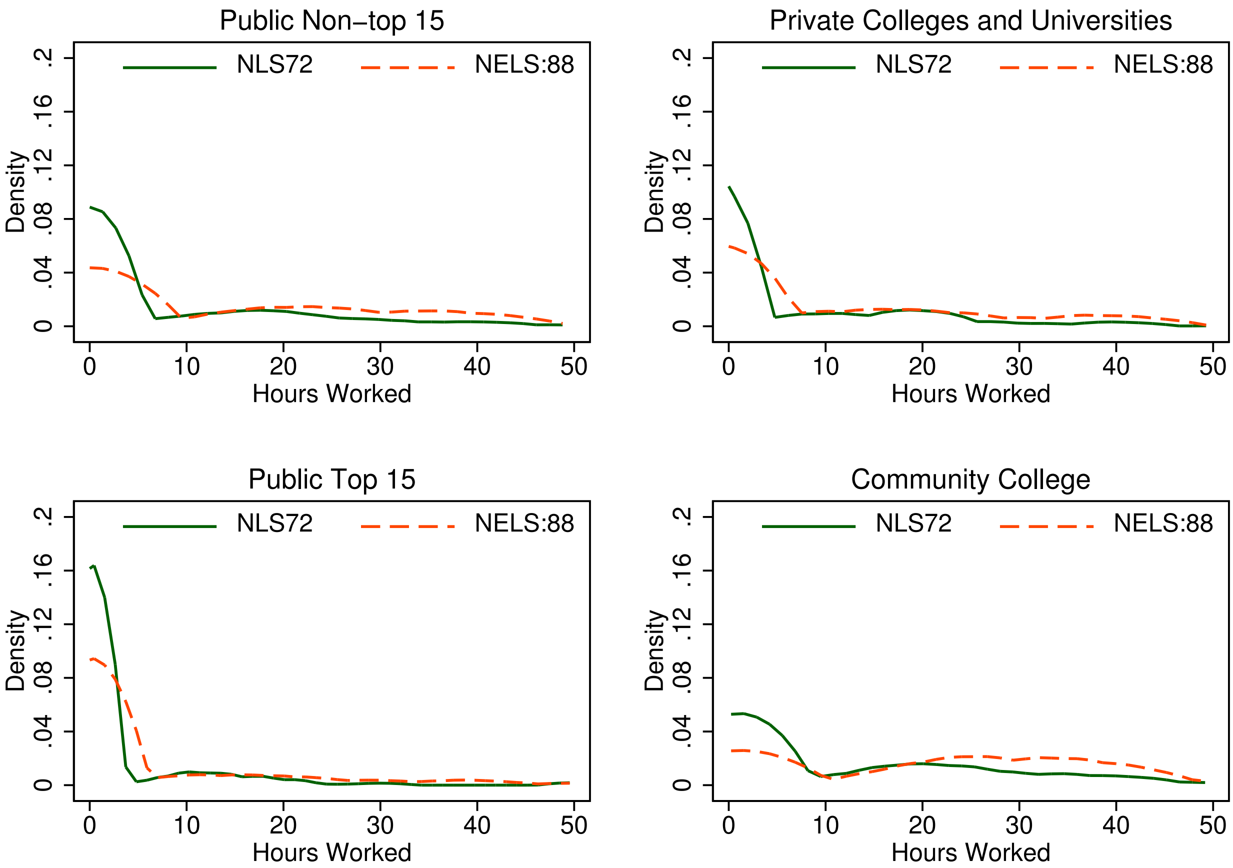
[1] Source: Authors' calculations from the NLS72 and NELS:88 transcript surveys. NLS72 calculations were made using the fifth follow-up weights included in the survey. Fourth follow-up weights were used for the NELS:88 survey calculations. Only those participating in these follow-ups are included in the tabulations.
[2] The credit distributions represent the CDF of total accumulated credits as of the eighth year after cohort high school graduation.
[3] The NLS72 and NELS:88 samples are restricted to those who attend college within 2 years of cohort high school graduation and receive a BA within eight years. Cohort high school graduation is defined as June 1972 for the NLS72 sample and June 1992 for the NELS:88 sample. The figure does not include outliers who accumulate more than 45 credits per year.

Figure 4.5: Percent Change in Time to Degree vs. Percent Change in 18-Year Old Population by State



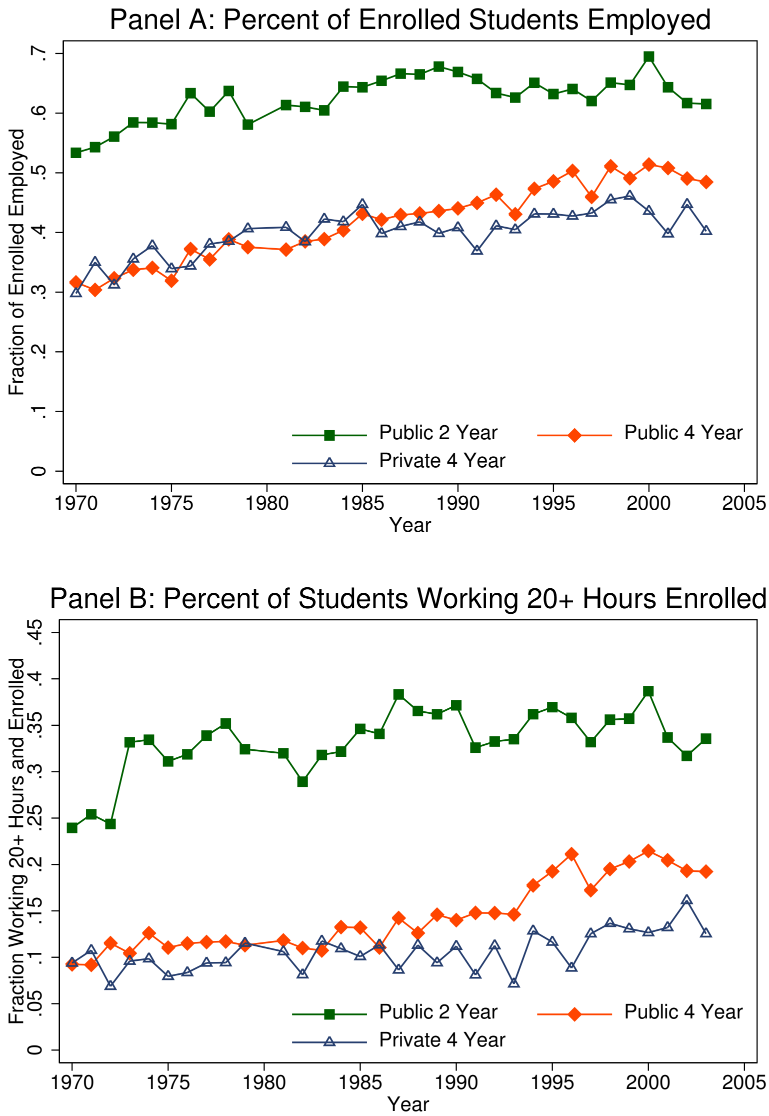
Source: Authors' calculation from the NLS72 and NELS:88 surveys and the U.S. Census of Population and Housing. NLS72 calculations were made using the fifth follow-up weights included in the survey. Fourth follow-up weights were used for the NELS:88 survey calculations. Only those participating in these follow-ups are included in the figure.

Figure 4.6: Distribution of Hours of Work in the First Year after High School Cohort Graduation for Students Enrolled in College, NLS72 and NELS:88 by Initial School Type



Source: Authors' calculations from the NLS72 and NELS:88 surveys. NLS72 calculations were made using the fifth follow-up weights included in the survey. Fourth follow-up weights were used for the NELS:88 survey calculations. Only those participating in these follow-ups are included in the tabulations.

Figure 4.7: Employment and Hours Worked Among Those Enrolled in College by Type of Institution, October CPS



Source: Data are from authors' tabulations using the October CPS. Individual weights are employed.

Appendix A: Press Accounts of Queuing and Enrollment Constraints

In addition to the quantitative work presented in the text linking expansions in cohort size to reductions in resources per student and, in turn, the outcomes of time to degree and college completion, we have explored press accounts over the past two decades to identify descriptions of cases in which it appears that resource constraints limit enrollment at public colleges and universities. Below, we present excerpts from a number of articles from *The Chronicle of Higher Education* and regional newspapers illustrating these conditions in California, North Carolina and Utah. In addition to the cases noted below, we have employed a systematic Lexis-Nexis search for articles including "overcrowding" or "enrollment limit" in conjunction with "higher education", "college" or "university," which returned 375 citations.

A.1 California

> Under a law passed more than 30 years ago, qualified students in California are promised admission to the University of California, to California State University, or to a community college.
>
> The law remains in effect. But already this academic year, eight institutions in the state-university system have announced they will not accept any new applicants in the spring semester, and the president of the University of California has said that system may soon start rejecting qualified applicants.
>
> Community colleges have yet to turn students away, but a legislative staff member says the absence of formal enrollment caps is "just a facade."
>
> Students are being admitted, "but when they go to register for classes there just aren't any," says Ann Blackwood, who works for the Assembly Committee on Higher Education.
>
> Officials in all three of the California systems say the budget cuts brought on by the state's financial crisis are to blame. The heads of the three systems have publicly asked Gov. Pete Wilson to meet with them to discuss a solution.
>
> David Mertes, chancellor of the community colleges, has also asked each college for proposals that might become part of a new systemwide policy in January. Options under consideration include: limiting enrollment of out-of-state students, allowing students to take only a certain number of classes, and offering fewer basic-skills classes (Blumenstyk, 1991).

In California, the state's master plan for higher-education prohibits community colleges from capping enrollment, but that hasn't stopped the Legislature from limiting the number of students they will reimburse the colleges for when dollars in the budget are tight. As a result, last year, there were more than 40,000 students at the state's 108 community colleges for which the institutions received no state money. Officials in the state system's office estimate that the colleges lost out on some $120.8-million in state funds.

Some 17,000 of those students were in the Los Angeles Community College District, the state's largest district. Consequently, it lost out on more than $46.6-million in state funds that the colleges had to make up on their own. And although California's public colleges enjoyed several years of robust state budgets in the late 1990s, officials say the few good years did not make up for the lean spending plans that lawmakers adopted when times were tough (Evelyn, 2002b).

Thousands of Riverside County high school graduates are being turned away every year from overcrowded community college classes, as a giant hole is ripped in the higher education 'safety net' for those with neither the money nor the grades for four-year schools.

Educators say many of the students who do not get in will never come back.

"They get jobs, they get apartments and then have bills to pay," said Billie Rogers, a veteran counselor at Corona High School. "It is easy for them to get off track, and hard to get back on."

Community colleges statewide are limiting classes as a result of budget cuts and an enrollment cap that was imposed in 1981. But nowhere is the problem more acute than in Riverside County, where thousands of students are turned away from classes each year.

"A community college is supposed to accommodate the community," said Salvatore Rotella, president of Riverside Community College. "But in this county, demand far outstrips the supply of seats at the college."

All students over the age of 18 - and many who are still in high school with a counselor's permission - are "accepted" but may find it tough to get classes, he said.

That is especially true for general education courses - English, math and science - needed to transfer to a four-year college or university.

Twenty-year-old Andrew Shouse will start his third year at RCC in the fall. He has been trying since the fall of 1993 to complete enough general education requirements to transfer to UCR as a junior. His first semester, Shouse managed to get into one English class, but had to take a computer class and an art course. "Sometimes you have to take cheesy classes just to get credits," he said.

Most classes have about 40 seats, and there are always about 20 people trying to talk instructors into letting them squeeze in, Shouse said.

"Usually you get to the teachers before the regular start of classes, or during the previous semester before classes are over," he said. "If you keep pestering them, they might remember you and let you in."

Rotella estimated RCC may be turning away as many as 10,000 students a year. Students may enroll by telephone or in person, but an unknown number never make it into classes.

As of June 21, RCC had about 20,000 applications on file for September classes. Rotella figured about half those students would get in (Peoples, 1995).

California's community colleges are bracing for a 25 percent enrollment increase in the next decade that will explode demand for teachers, classroom space, child care, financial aid and remedial education.

That explosion will rock a system already strained by waiting lists, crowding and tight budgets.

"We're pretty full," said Bill Stewart, chancellor of the State Center Community College District, which includes Kings River Community College in Reedley as well as Fresno City College. "It's going to take a lot of creativity on our part" (Coleman, 1996).

It will take three years, but students at State Center Community College District's Clovis Center are relieved a new campus is going to be built.

They endure crowded classes, and many students who live in Clovis and northeast Fresno commute to Fresno City College for courses Clovis Center has no space to provide.

"The campus is too small for the amount of people who come here," student Elie Lipson, 20, said while taking a moment away from a computer terminal before the start of a class (Benjamin, 2002).

CSU is becoming much harder to get into. The number of impacted campuses, those that receive more applications from qualified students than they can accept, has jumped from five to eight this year: Cal Poly Pomona, Cal State Fullerton, Cal State San Marcos, San Diego State, Cal State Long Beach, Cal Poly San Luis Obispo, Sonoma State and Chico State.

Already, 15 of the 23 campuses have closed their application period for the spring term for first-time freshmen, which is unusual....

Now is the heart of college application season, with students taking SATs and writing essays. UC has an application deadline of Nov. 30. With CSU, it used to be that prospective students could sometimes wait until summer to apply to certain campuses. Those days are over.

"Many of our campuses will stop accepting applications earlier than ever before," said CSU Assistant Vice Chancellor Allison Jones. "We just don't have the same degree of open access CSU has historically had" (Sturrock, 2003).

The state's community colleges are preparing for a crush of students who were turned away from the state's public universities because of state budget cuts.

More than 10,000 students who had the grades and SAT scores to enroll in the University of California or California State University systems are being told to apply instead to community colleges, then transfer to the campus of their choice in two years. That's further crowding a community college system that last year turned away tens of thousands of students due to overcrowding.

Enrollment opens Monday at some of the state's community colleges, and officials are worried about whether they'll be able to handle so many more applicants when they can't afford to add classes. They urged students to sign up quickly.

"If they wait, then there's nothing we can do for them. The classes will be gone," said Darroch "Rocky" Young, acting senior vice chancellor for the nine-member Los Angeles Community College District. Last year lack of money forced the district to turn away 5,000 students and cut 1,000 classes (The Associated Press State & Local Wire, 2004).

A.2 North Carolina

In Florida and North Carolina, community colleges also have limited enrollment – or are preparing to do so – despite the "open admissions" principle under which they have long operated....

Forsyth Technical Community College, in North Carolina, already has had to turn away about 100 students seeking remedial classes because the school had no money to hire the instructor. "That to me is pretty drastic in an urban community like ours," says the president, Bob H. Greene.

Institutions with admissions requirements have other means to limit enrollment, and many are using them (Blumenstyk, 1991).

This year Cape Fear had to add more than 80 course sections more than the college had originally planned for, and Cape Fear still had to turn away students. For community colleges, selling out on courses sometimes means feeling like they are selling out on their open-admissions values. Campus administrators say it wasn't easy telling students they couldn't accommodate them. But with the state budget shortfall taking a 3-percent chunk out of the college's $25-million operating budget, officials here say they were hard pressed to come up with the resources to handle any more.

"It was just amazing to see the lines at registration come outside the buildings and wrap around campus blocks," Mr. McKeithan says. "Many of those students waited in line for seven or eight hours only to find out that most of the classes they wanted were sold out. It really hurt to watch that happen" (Evelyn, 2002a).

When Tony Zeiss, the president of Central Piedmont Community College, heard about North Carolina's grim budget forecast for next year, he got on the phone. Not to legislators or his state's community-college system office. Instead, he called the Pepsi-Cola Bottling Company of Charlotte and other local businesses to ask them if they would be willing to donate $1,650 each to sponsor a course at the college.

Mr. Zeiss says his hands were tied. Because of a 5.5-percent reduction in his institution's budget last year, he had already canceled 110 courses – out of some 2,400 courses that the college offered – although enrollment had jumped 10 percent. He now faced cutting another 220 courses, even though projections for this fall's enrollment were already showing another 10-percent rise, as some politicians called for additional budget cuts of up to 15 percent. As a result, Mr. Zeiss is growing increasingly concerned about the extent to which he will have to do something that flies in the face of the historical mission of community colleges: Turn students away (Evelyn, 2002b).

A.2 Utah

Lynn Cundiff, the president of Salt Lake Community College, says that with state lawmakers weighing a 4-percent cut to the institution's budget, he is considering imposing admissions standards.

"If it gets much worse, something will have to give," Mr. Cundiff says.

He doesn't want a repeat of last year, when he estimates the college turned away 500 students after state officials reduced the institution's appropriation by some 4 percent.

Mr. Cundiff says that as it is, the college will likely turn away another 500 or so students this fall. If state revenues take another dive, he says, that number may increase. To stem growth, he is looking at his options: an official enrollment cap or even instituting admissions standards, two things almost unheard of in the community-college world (Evelyn, 2002b).

An unexpected "wave" of students enrolling at Salt Lake Community College is stretching the school's resources during its first week of school.

Enrollment, which was expected to hold steady this semester, is up about 700 FTE, the equivalent of full-time students, or by 1,400 actual students over the first day of classes last year.

"As of (Tuesday) night, 6,125 people were on wait lists trying to get into classes they need" – a record number – said Judd Morgan, vice president for student services.

"It's a zoo today," college spokesman Jay Williams added. As many as 800 students are trying to get into basic English classes (Titze, 2000).

Even universities experiencing manageable growth, such as the University of Utah, are concerned about future projections. The U. is considering upping admissions standards to slow the flow of students.

Brigham Young University anticipates it will hit its enrollment cap of 30,000, and has no plans of opening up more seats.

The three schools hit the hardest also experienced dramatic growth last fall, and are projecting for it again this year.

Last year's dilemma was finding enough faculty members to open extra classes. This year, lack of space is the main constraint, said Lynn Cundiff, president of SLCC, which is reporting a 14 percent hike in the head count. Last year at this time, SLCC counted 19,759 students. This year, 22,533 have registered.

"We've got cars parked everywhere, even on the soccer field," said Cundiff. "We don't have classrooms to put [students] in."

The college is renting space from the LDS Institute on campus and even has held one class in an outdoor amphitheater – a quick fix that works now, said Cundiff, "but not when winter rolls around" (Stewart, 2001).

Both the University of Utah and Utah State University are raising the bar for enrollments this fall. Other colleges and universities in the system have traditionally tried to take all comers, but they are warning ahead of time that budget cuts, coupled with unprecedented demand, may limit enrollment as classes fill and potential students are turned away.

With three weeks still to go in the registration process, Utah Valley State College already has 14 percent more enrollments than at the same time last fall, said registrar Luann Smith. The Orem college has been the fastest-growing among the state institutions for a number of years.

"We admit everyone, but if they come and find that the courses they want are already full, they may either look at other options or wait until the next semester," she said. Although "open enrollment" remains the objective, the availability of classes is a limiting factor, she said (Van Leer, 2002).

Salt Lake Community College expects about 3,000 additional students on its campuses for the fall 2003 semester due, in part, to the University of Utah's enrollment cap.

"We will have to do more with less," Interim President Judd Morgan told SLCC's board of trustees Wednesday. "We will see larger class sizes because we want to fill up those we already offer instead of creating new ones" (Sykes, 2003).

Appendix B: Data Appendix

B.1 Time to Degree and Degree Completion

Time to degree and degree completion are calculated using NLS72 and NELS:88 survey responses from the first through fifth follow-ups in NLS72 and the fourth follow-up in NELS:88. The NLS72 study participants were seniors in high school in the spring of 1972. Following the base year interview, participant follow-up surveys were administered in 1973, 1974, 1976, 1979, and 1986 (for a subsample), with questions covering collegiate participation and degree attainment. In addition, detailed high school records and postsecondary transcripts were collected by the Department of Education.

The NELS:88 survey started with students who were in the eighth grade in 1988 (high school class of 1992) and conducted follow-up surveys with participants in 1990, 1992, 1994, and 2000. Similar to the NLS72 survey, NELS:88 contains high school records and collegiate transcripts as well as a host of background information that may be relevant to time to degree.

Although degrees can be awarded throughout a year, we record the timing of degree receipt in discrete units of years since cohort high school graduation. Cohort high school graduation is defined as June 1972 for the NLS72 sample and June 1992 for the NELS:88 sample. The cut-point in each survey that defines a new year is the end of August (and thus the start of the new academic year). For example, NLS72 respondents who received a degree in January and June of 1976 would both be classified as taking 4 years to obtain a BA. However, a student who received a degree in September 1976 would be classified as taking 5 years.

Because the NELS:88 survey is comprised of eighth graders from 1988 and the NLS72 survey follows twelfth graders from the class of 1972, the NELS:88 survey contains more students who graduate college after their cohort's high school graduation. In our base sample, 1.3% of respondents in NLS72 and 4.4% of respondents in NELS:88 finish high school after June of their respective cohort graduation year. However, looking only at eight-year BA recipients, 0.3% and 0.6%, respectively in NLS72 and NELS:88 did not finish high school on time. It is therefore unlikely the larger preponderance of late high school graduates in the NELS:88 survey biases our time to degree calculations.

Table 4.B.1 contains variable names and definitions used to define the sample and to calculate time to degree and degree completion in both the NLS72 and NELS:88 surveys.

B.2 School Type and Collegiate Start Dates

Self-reported enrollment records from the first through fourth follow-up surveys for the NLS72 survey and from NCES-aggregated responses in the NELS:88 survey are used to define the type of institution of initial collegiate enrollment.

In the NLS72 survey, we first determine the year in which a student first enrolls in an academic post-secondary institution, where "academic" is defined as granting at least an associates degree or BA. In each follow-up, students were asked about schools they attended (up to three) in each year since the previous survey. The first school attended is defined as the first time a student reports attending an academic institution. Note that when we focus on those students who enroll in college, we restrict the sample to those who start at an academic institution within 2 years of cohort high school graduation. Identifying the first institution attended by FICE (Federal Interagency Committee on Education) code, we merge institutional-level information that contains public/private and 2-year/4-year identifiers and identify top 15 and top 50 public 4-year institutions by respondent's initial institution's FICE code.

In the NELS:88 survey, NCES has constructed variables that aggregate the individual reported enrollment histories in order to determine the type of first institution attended. These variables are f4efsect, psefirty, and levlcont. These variables are used in ascending order: information from f4efsect is used as the primary source, then information from psefirty, and then levlcont is used. Further, there are some individuals who have a start date prior to their high school graduation date as they take classes at community or local colleges and this is reflected in the above variables. [Note this is not a problem for the NLS72 measure because it incorporates only institutions attended after high school graduation.] For students with post-secondary experience preceding high school graduation, we use the first start date and institution after high school graduation taken from the post-secondary transcript files. For all other students in the NELS:88 survey, first start date is identified by f4efmy, which is the NCES-constructed date of first post-secondary attendance. In order to identify individuals whose first institution is a top 15 or top 50 public school, we use the first school attended post-high school cohort graduation listed in the post-secondary transcript files.

B.3 Background Characteristics

Math and Reading Tests

In both surveys, tests of academic achievement were administered to students in the senior year. The NLS72 exam was administered as a 69-minute test book with sections on vocabulary, picture numbers, reading, letter groups, mathematics, and mosaic comparisons. Each section was 15 minutes (except for the mosaic comparison which was 9 minutes). The math test included 25 items and the reading test included 20 items. While the reading test focused on analysis, interpretation and comprehension of short reading passages (100-200 words), the math test contained only quantitative comparisons in order to measure basic quantitative competence. We use the reported scaled math scores (scmatsc) and scaled reading scores (scrdsc) as our test score measures in NLS72.

The NELS:88 cognitive test batteries were administered in each of the first three waves, with sections on reading, math, science and social studies. The tests were 85 minutes and consisted of 116 questions, 40 of which were on math and 21 of which were on reading comprehension. Unlike the NLS72 exams, the NELS:88 tests covered more material and tested more skills. In reading comprehension, students were tested on word meaning, figures of speech, author's perspective, and comprehension. The math exam consisted of word problems, graphs, equations, quantitative comparisons, and geometric figures. Further, because the NELS:88 tests were given in subsequent waves, students were given harder or easier tests in the first and second follow-ups depending on their scores in the previous wave to guard against floor and ceiling effects. We use the math IRT theta score (f22xmth) and the reading IRT theta score (f22xrth) from the second follow-up as the base measure of test scores. These scores are psychometric evaluation scores of each student's ability that account for the difficulty of the exam.

Because the tests in NLS72 and NELS:88 covered different subject matter, were of different lengths, and were graded on different scales, the scores are not directly comparable across surveys. Instead, we construct the quartiles of the score distributions for each test type and for each survey. The comparison of students in the same test quartile across surveys is based on the assumption overall achievement did not change over this time period. This assumption is supported by the observation that there is little change in the overall level of test scores on the nationally-representative NAEP over our period of observation. Similarly, examination of time trends in standard college entrance exams such as the SAT provides little support for the proposition that achievement declined appreciable over the interval within test quartiles. For the SAT, the ratio of test takers to high school graduates increased from 33% to 42% while mean math scores declined from 509 to 501 and mean verbal scores decline a bit more, from 530 to 500 over the 1972 to 1992 interval (Digest of Education Statistics, 2005, Table 129).

In the NLS72 survey, we use high school GPA as an imputation variable in order to measure pre-collegiate academic ability for students with missing test scores. The GPA measure we use is "imptaver" from the NLS72 survey. In the multiple imputation of missing variables in the NELS:88 survey, we use IRT theta test scores from the first follow-up for math (f12xmth) and reading (f12xrth) and from the base year for math (by2xmth) and reading (by2xrth). The IRT theta scores are scaled to a common metric across years by NCES. The imputed math and reading test scores from the senior year in each survey are used to construct the test quartiles used in the main analysis.

Parental Education

We obtain student reported measures of father's and mother's education separately. In the NLS72 survey, we have three different measures of this variable. For mother's education, we use the variables cmoed, bq90b, and fq78b. For father's education, we use the variables cfaed, bq90a, and fq78a. If there are disagreements across measures, fq78b and fq78a take precedence.

In the NELS:88 survey, we also use student reports of father's education (bys34a) and mother's education (bys34b). For the multiple imputation model, we include parent self-reports of their own education from the base year and second follow-up parental surveys. In the base year parent survey, we combine information on whether the respondent and his/her spouse is the father or mother (byp1a1 and byp1a2) with reported self (byp30) and spouse (byp31) educational attainment. A similar methodology is used for the second-follow up parent survey, using f2p1a and f2p1b to identify the gender of the respondent and the spouse, respectively, and f2p101a and f2p101b to identify educational attainment of the respondent and the spouse, respectively. The base year and second follow-up parental education information is aggregated into two variables, father's education and mother's education, used in the multiple imputation model.

Parental Income Levels

The parental income variables are bq93 for NLS72 and f2p74 for NELS:88. The former is reported by the student while the latter is reported by the parents. Unfortunately, NLS72 does not contain a parent-reported measure and the NELS:88 survey does not contain a student-reported measure, so these variables are the most closely aligned parental income measures across the two surveys.

Rather than asking directly for parental income levels, the NELS:88 and NLS72 surveys ask for income ranges from respondents. Because we are interested in measuring parents' ability to finance college, the variable of interest is the real income level, not one's place in the income distribution. We thus align the income blocks across the two surveys using the CPI for higher education. In NLS72, the income groups are less than \$3000, \$3000-\$6000, \$6000-\$7500, \$7500-\$10500, \$10500-\$15000, and greater than \$15000. In NELS:88, the income blocks are less than \$10000, \$10000-\$20000, \$20000-\$25000, \$25000-\$35000, \$35000-\$50000, and greater than \$50000. These six income groups are grouped together, from high to low, and treated as similar real income ranges across the surveys.

Race

Race is measured in the NLS72 survey using "crace" and "race86." The latter is used if the former is blank due to non-response. In the NELS:88 survey, race is measured using the "race" variable available in the data files.

B.4 Multiple Imputation

There is a considerable amount of missing data in the NLS72 and NELS:88 surveys. Table 4.B.2 presents the number of unweighted missing observation by variable and survey. These observations are not missing completely at random; respondents who have no math or reading test scores are less likely to finish college and less likely to finish in four years conditional on starting.

Casewise deletion of missing observations with therefore cause a bias in the calculation of the base trends we are seeking to explain in this analysis. To deal with this problem, we use the multiple imputation by chained equation (MICE) algorithm developed by Van Buuren, Boshuizen, and Knook (1999) that is implemented through the STATA module "ICE" (see Royston (2004) for a detailed discussion of ICE).

MICE is implemented by first defining the set of predictor variables $(x_1 \ldots x_k)$ and the set of variables with missing values to be imputed: math test scores, reading test scores, father's education, mother's education, and parental income levels $(y_1 \ldots y_5)$. The MICE algorithm implemented by ICE first randomly fills in all missing values from the posterior distribution of each variable. Then, for each variable with missing data, y_i, STATA runs a regression (or ordered logit) of y_i on $y_{\sim i}$ and $x_1 \ldots x_k$ and updates the randomly imputed missing values. A sequence of regressions for each y_i is a cycle, and this process is repeated for 10 cycles. In each cycle, the imputed values from the previous cycle are used in the regressions and updated. The imputed values after 10 cycles constitute 1 imputed data set, and this process is repeated 5 different times to generate 5 imputed data sets.

There are two important specifications in implementing MICE: determination of the predictor variables and determination of the imputation models. Because of the different structure of the two surveys, different variables are used in the imputation procedure across surveys. In both surveys, we include dummy variables for cumulative time to degree from four to eight years, dummy variables for initial school type, interactions between these variables, an indicator for college attendance within two years of cohort high school graduation, as well as race and gender indicators.

For imputations with the NLS72 sample, we include a measure of high school GPA in order to proxy for unobserved ability among those without test score information. Due to the structure of the NELS:88 survey, there is more background information with which to impute missing data. We utilize 8^{th} and 10^{th} grade math and reading test scores, parental reports of their education from the base year and second follow-up parent surveys, and parental reports of their income level from the base year parent survey. The definitions of the variables used in the imputation models are discussed in the preceding section.

Because the math and reading test scores are continuous variables, we use OLS regressions to impute these variables. Mother's and father's education and income, however, are categorical variables. Because of the ordered nature of these variables, we use ordered logits to impute the missing values of these variables. While these model choices are reasonably arbitrary, they are only used to draw ranges of plausible estimates of missing data.

B.5 Eighteen Year Old Population

For the crowding regressions, we calculate the number of eighteen-year olds in each state in 1972 and 1992, which are the cohort high school graduation years in NLS72 and

NELS:88 respectively. To measure the population of eighteen year olds in each state, we use data on population by state and single year of age that are available through the Bureau of the Census website.[35] The change in the log of the eighteen year old population at the state-level constitutes the independent variable of interest in the crowding regressions.

B.6 Dropped Observations and Missing Transcript Data

The base sample in this analysis consists of all respondents who graduate high school and attend college within two years of their cohort's high school graduation. We further restrict the sample to exclude those whose only enrollment over this time period is at a private two-year institution as these schools are predominantly professional without a BA track. Table 4.B.3 presents information on the number of observations that are dropped by survey and the reason for dropping the observation. For example, 168 respondents are dropped because they are not high school graduates in NLS72 whereas 722 are dropped in NELS:88 for this reason. The apparently higher dropout rate in NELS:88 is because the universe of students are all those enrolled in the 8^{th} grade in 1988, whereas the universe in NLS72 are all those enrolled in 12^{th} grade in 1972.

In the NLS72 survey, 99 observations are dropped because they report attending college but provide no information on either the type of institution or the date they first began attending this institution. In the NELS:88 survey, 195 observations were dropped because they were not in all four waves of the survey. In other words, they have a sample weight of zero.

Of potential concern in constructing our sample is the exclusion of those beginning college more than two years post-high school cohort graduation. We exclude these observations because we are interested in the truncated, eight-year time to degree distribution and the eight-year completion rate. These statistics have a different interpretation for a student who began college directly after high school than for a student who began college five years after high school. While 613 and 795 respondents in NLS72 and NELS:88, re-

[35] Data for 1972 are available at: *http://www.census.gov/popest/archives/pre-1980/e7080sta.txt* and data for 1992 are available at: *http://www.census.gov/popest/archives/1990s/st_age_sex.html*.

spectively, attend college more than 2 years after their cohort's high school graduation, the eight year completion rates for these groups are 0.73% and 0.57%, respectively.

For the analysis of credit accumulation, we employ postsecondary transcript data from the postsecondary transcript studies in both the NLS72 and NELS:88 surveys. While the goal of these studies was to obtain all student transcripts, there are missing transcripts. Among all eight-year college graduates in the analysis sample, there are no transcripts for 454 respondents in the NLS72 sample nor for 182 respondents in the NELS:88 sample. Representing 10.6% and 4.4% percent of the relevant NLS72 and NELS:88 samples, respectively, these observations were dropped from the analysis of credit attainment.

With respect to collegiate outcomes, the observations with missing transcript data look similar to the observations without transcript data in NLS72. Average time to degree among the two groups is 4.68 and 4.67, respectively, while the four-year completion rate is 61% and 56%, respectively. Further, the initial school types for respondents with and without post-secondary transcripts are similar.

In contrast, the missing NELS:88 transcripts are not missing at random with respect to the outcome variables. Observations with missing transcript data have a higher time to degree (5.18 vs. 4.88), a lower four-year completion rate (30% vs. 44%) and are more likely to begin their postsecondary career at a two-year school. While these observations constitute a relatively small portion of total college graduates in NELS:88, the nature of the missing data suggests we overstate the rate of credit accumulation in NELS:88 and thus understate the reduction in the rate of credit attainment across the two surveys in our analysis.

Table 4.B.1: Variable Names and Definitions for Calculation of Time to Degree in NLS72 and NELS:88

Panel A: NLS72		
Variable Name	**Variable Definition**	**Follow Up**
Fq2	High school completion dummy	2
Edatt86	Educational attainment as of 1986	1-5
Fq3a	High school graduation month	2
Fq3b	High school graduation year	2
Tq48ea	BA completion dummy as of 10/1/1976	3
Tq48eb	Month BA received as of third follow-up	3
Tq48ec	Year BA received as of third follow-up	3
Ft76ea	BA completion as of fourth follow-up	4
Ft76eb	Month BA received as of fourth follow-up	4
Ft76ec	Year BA received as of fourth follow-up	4
Fi19b1ey - Fi19b4ey	Year ended most recent school attended, first through fourth time	5
Fi19b1em - Fi19b4em	Month ended most recent school attended, first through fourth time	5
Fi19h	Course of study in most recent school attended	5
Fi19i	Completed requirements in most recent school attended	5
Fi20b1ey - Fi20b4ey	Year ended 2nd most recent school attended, first through fourth time	5
Fi20b1em - Fi20b4em	Month ended 2nd most recent school attended, first through fourth time	5
Fi19h	Course of study in 2nd most recent school attended	5
Fi19i	Completed requirements in 2nd most recent school attended	5

Panel B: NELS:88		
Variable Name	**Variable Definition**	**Follow Up**
F4hsgradt	High school graduation date	4
F4ed1	Degree receipt date - first degree received	4
F4edgr1	Degree type received - first degree	4
F4ed2	Degree receipt date - second degree received	4
F4edgr2	Degree type received - second degree	4
F4ed3	Degree receipt date - third degree received	4
F4edgr3	Degree type received - third degree	4
F4ed4	Degree receipt date - fourth degree received	4
F4edgr4	Degree type received - fourth degree	4
F4ed5	Degree receipt date - fifth degree received	4
F4edgr5	Degree type received - fifth degree	4
F4ed6	Degree receipt date - sixth degree received	4
F4edgr6	Degree type received - sixth degree	4

Table 4.B.2: Number of Imputed Observations by Survey and Variable (Unweighted)

Variable	Number of Imputed Observations NLS72	NELS:88
Math Test Score	1,940	1,636
Reading Test Score	1,940	1,638
Mother's Education	46	1,231
Father's Education	45	1,445
Parent Income	1,612	1,193
Total	2,886	3,620

Observation counts include only those respondents who enroll in college within two years of cohort high school graduation at a four-year institution or a non-private two-year college.

Table 4.B.3: Number of Dropped Observations by Category (Unweighted)

NLS72		
Sample Change	**Dropped Observations**	**Remaining Observations**
Original Base - 5^{th} follow-up Sample		12,841
High School Dropouts	168	12,673
Missing Initial School Information	99	12,574
Never Attended College	4,734	7,840
Time Between HS and College >2 Years	613	7,227
1^{st} School Type is 2 Year Private	120	**7,107**
NELS:88		
Sample Change	**Dropped Observations**	**Remaining Observations**
Original Base - 4^{th} follow-up Sample		12,144
High School Dropouts	722	11,422
Observations not in all 4 Waves	195	11,227
Never Attended College	1,918	9,309
Time Between HS and College >2 Years	795	8,512
1^{st} School Type is 2 Year Private	95	**8,417**

Appendix C: Statistical Appendix

C.1 Calculation of P-Values Without Multivariate Reweighting

In order to test whether the NLS72 and NELS:88 distributions are different at a given point in the distribution, we calculate the difference and the standard error of this difference between the NLS72 and NELS:88 distributions at each year since high school cohort graduation in Tables 4.2 and 4.3. For each year since high school cohort graduation from 4-8, we regress a dummy variable equal to 1 if the respondent's time to degree is less than or equal to that number of years on a dummy variable for whether the respondent was in the NELS:88 survey as well as a constant term. We run a similar regression for the completion rate, where the dependent variable is an indicator equal to 1 if the respondent received a BA within 8 years of cohort high school graduation. All regressions are weighted by the sample weights used throughout and standard errors are clustered at the high school level. The standard errors are then converted into p-values of the probability one can reject the null hypothesis the difference is equal to zero. These p-values are reported in Tables 4.2 and 4.3.

C.2 Calculation of P-Values With Multivariate Reweighting

When we calculate p-values in the multivariate reweighting analysis, we bootstrap the difference in the distributions in order to take into account both the sampling variability of the weights as well as the fact the data from which the weights are calculated are from multiply imputed datasets.

We replicate the data across individuals, not across imputations, so each observation in each replication of the data contains all 5 imputed datasets. However, similar to the calculation of standard errors in the regressions discussed in the previous section, the bootstrap replications are clustered at the high school level. Clustering at the high school level amounts to replicating the data across high schools, not across individuals, with each observation within each cluster and replication containing all 5 imputed data sets.

For each replication and for each imputed data set, we generate the weights used for reweighting by running a logit of a dummy variable equal to 1 if the individual is in the NLS72 survey on the demographic characteristics described in the text and in Appendix B. The weights are the ratio of the predicted value to one minus the predicted value from each of these logits. We next calculate the difference between the NLS72 and reweighted NELS:88 distributions at each point in the distribution from four to eight (or similarly the difference in the 8-year completion rate) and take the average of these differences across imputations. Similarly, we calculate the difference between the NELS:88 reweighted distribution and the NELS:88 distribution for each year from 4 to 8 as well as for completion rates and take the average of these differences across the five imputations. We generate distributions of these various differences by bootstrapping this process using 5000 replications. The p-values of the test for statistical significance of the difference between the NLS72 and NELS:88 reweighted distributions are reported in Table 4.5 and Table 4.6. The p-values for statistical significance of the difference between the reweighted NELS:88 and actual NELS:88 distributions are discussed in the text only and are available from the authors upon request.

C.3 Calculation of P-Values for Tests of First Order Stochastic Dominance

We limit our test of first order stochastic dominance to the range of completion in years 4, 5 and 6. The null hypothesis of the test for first order dominance is that for years 4, 5, and 6, the NLS72 and NELS:88 distributions cross (i.e., at some point in the distribution, the NLS72 distribution is less than the NELS:88 distribution). In order to test this null hypothesis, we bootstrap the cumulative time to degree distributions in the two surveys and calculate the percentage of times a "crossing" occurs. We define a "crossing" as any time the NLS72 distribution lies below the NELS:88 distribution.

The bootstrap process is similar to the one discussed above, however there is only one data set as the collegiate outcome variables were not imputed. For each replication, we calculate the proportion finishing in at least 4 years to at least 6 years for all those who complete within 8 years of cohort high school graduation separately for NLS72 and NELS:88. We then generate the distribution of these proportions by bootstrapping this process using 5000 replications. These bootstraps are clustered at the high school level. Finally, we count for how many replications there is a crossing of the time to degree distributions. The percentage of times a crossing occurs is the p-value reported in the tables and represents the probability the null hypothesis that the NLS72 distribution does not first order stochastically dominate the NELS:88 distribution is false. In other words, the p-value tells one with what certainty one can reject the null hypothesis of no first order stochastic dominance. Note this is a one-sided test as it tests whether the NLS72 distribution lies above the NELS:88 distribution, not whether one distribution lies above the other. In a case where the NELS:88 distribution first order dominates the NLS72 distribution, our test would fail to reject the null hypothesis.

Appendix D: Supplemental Tables

Table 4.D.1: Means of Selected Variables – Public Non-top 15 Sample

Variable	All College Attendees NLS72	All College Attendees NELS:88	College Graduates Who Obtain a BA Within Eight Years of Cohort High School Graduation NLS72	College Graduates Who Obtain a BA Within Eight Years of Cohort High School Graduation NELS:88
Average Time to Degree	4.677	5.044	4.677	5.044
	(1.017)	(1.084)	(1.017)	(1.084)
Lowest Reading Quartile	0.126	0.146	0.093	0.078
	(0.300)	(0.333)	(0.263)	(0.256)
Second Reading Quartile	0.192	0.207	0.164	0.160
	(0.348)	(0.371)	(0.328)	(0.337)
Third Reading Quartile	0.299	0.302	0.310	0.316
	(0.375)	(0.420)	(0.381)	(0.427)
Top Reading Quartile	0.383	0.345	0.433	0.446
	(0.417)	(0.449)	(0.424)	(0.468)
Lowest Math Quartile	0.096	0.122	0.067	0.033
	(0.269)	(0.310)	(0.226)	(0.167)
Second Math Quartile	0.196	0.203	0.146	0.153
	(0.351)	(0.373)	(0.313)	(0.341)
Third Math Quartile	0.259	0.299	0.246	0.304
	(0.387)	(0.429)	(0.382)	(0.434)
Top Math Quartile	0.449	0.376	0.542	0.510
	(0.455)	(0.468)	(0.453)	(0.483)
Father – No HS Diploma	0.220	0.107	0.190	0.056
	(0.412)	(0.276)	(0.391)	(0.210)
Father – HS Diploma	0.256	0.293	0.222	0.215
	(0.435)	(0.415)	(0.414)	(0.381)
Father – Some College	0.242	0.224	0.246	0.235
	(0.428)	(0.381)	(0.430)	(0.393)
Father – BA	0.170	0.200	0.201	0.255
	(0.375)	(0.373)	(0.400)	(0.409)
Father – Graduate School	0.112	0.175	0.141	0.239
	(0.315)	(0.362)	(0.348)	(0.414)
Mother – No HS Diploma	0.177	0.099	0.150	0.047
	(0.380)	(0.281)	(0.356)	(0.195)
Mother – HS Diploma	0.394	0.330	0.383	0.278
	(0.487)	(0.443)	(0.485)	(0.426)
Mother – Some College	0.279	0.263	0.278	0.280
	(0.448)	(0.411)	(0.447)	(0.424)

Mother – BA	0.110	0.183	0.135	0.230
	(0.312)	(0.365)	(0.340)	(0.397)
Mother – Graduate School	0.041	0.124	0.054	0.165
	(0.197)	(0.314)	(0.226)	(0.356)
Income 3,000/10,000	0.039	0.058	0.023	0.030
	(0.176)	(0.220)	(0.134)	(0.162)
Income 6,000/20,000	0.070	0.111	0.047	0.074
	(0.234)	(0.292)	(0.193)	(0.248)
Income 7,500/25,000	0.072	0.077	0.075	0.061
	(0.236)	(0.252)	(0.242)	(0.231)
Income 10,500/35,000	0.201	0.133	0.205	0.104
	(0.368)	(0.318)	(0.374)	(0.288)
Income 15,000/50,000	0.267	0.207	0.271	0.206
	(0.402)	(0.381)	(0.402)	(0.385)
Income 15,000+/50,000+	0.351	0.413	0.380	0.525
	(0.443)	(0.473)	(0.449)	(0.484)
Asian	0.009	0.039	0.012	0.044
	(0.093)	(0.193)	(0.107)	(0.205)
Hispanic	0.022	0.079	0.014	0.048
	(0.146)	(0.270)	(0.117)	(0.214)
African American	0.097	0.134	0.082	0.083
	(0.295)	(0.341)	(0.273)	(0.276)
White	0.860	0.741	0.884	0.819
	(0.347)	(0.438)	(0.320)	(0.385)
Native American	0.005	0.006	0.002	0.005
	(0.071)	(0.078)	(0.045)	(0.072)
Number of Observations	3570	3456	2418	1989

[1] Source: Authors' tabulations from the NLS72 and NELS:88 surveys. Standard deviations are in parentheses. NLS72 calculations were made using the fifth follow-up weights included in the survey. Fourth follow-up weights were used for the NELS:88 survey calculations. Only those participating in these follow-ups are included in the tabulations.

[2] The NLS72 and NELS:88 samples are restricted to those who initially attend a non-top 15 public college within 2 years of cohort high school graduation. Cohort high school graduation is defined as June 1972 for the NLS72 sample and June 1992 for the NELS:88 sample.

Table 4.D.2: Means of Selected Variables – Public Two-Year Sample

Variable	All College Attendees NLS72	All College Attendees NELS:88	College Graduates Who Obtain a BA Within Eight Years of Cohort High School Graduation NLS72	College Graduates Who Obtain a BA Within Eight Years of Cohort High School Graduation NELS:88
Average Time to Degree	5.150	5.624	5.150	5.624
	(1.221)	(1.260)	(1.221)	(1.260)
Lowest Reading Quartile	0.209	0.272	0.120	0.159
	(0.370)	(0.418)	(0.279)	(0.345)
Second Reading Quartile	0.270	0.289	0.237	0.219
	(0.398)	(0.406)	(0.383)	(0.380)
Third Reading Quartile	0.292	0.268	0.300	0.349
	(0.389)	(0.402)	(0.381)	(0.442)
Top Reading Quartile	0.228	0.171	0.343	0.273
	(0.361)	(0.349)	(0.400)	(0.421)
Lowest Math Quartile	0.174	0.265	0.078	0.091
	(0.349)	(0.417)	(0.247)	(0.262)
Second Math Quartile	0.301	0.312	0.220	0.229
	(0.409)	(0.423)	(0.356)	(0.393)
Third Math Quartile	0.268	0.281	0.303	0.362
	(0.396)	(0.413)	(0.401)	(0.447)
Top Math Quartile	0.257	0.143	0.399	0.318
	(0.400)	(0.334)	(0.433)	(0.433)
Father – No HS Diploma	0.273	0.170	0.176	0.090
	(0.444)	(0.335)	(0.380)	(0.250)
Father – HS Diploma	0.307	0.385	0.273	0.313
	(0.460)	(0.434)	(0.445)	(0.413)
Father – Some College	0.239	0.227	0.269	0.268
	(0.426)	(0.377)	(0.443)	(0.401)
Father – BA	0.110	0.131	0.155	0.155
	(0.312)	(0.309)	(0.362)	(0.362)
Father – Graduate School	0.070	0.087	0.128	0.128
	(0.256)	(0.260)	(0.335)	(0.335)
Mother – No HS Diploma	0.225	0.163	0.143	0.110
	(0.417)	(0.342)	(0.349)	(0.280)
Mother – HS Diploma	0.432	0.427	0.419	0.359
	(0.494)	(0.455)	(0.493)	(0.446)
Mother – Some College	0.235	0.241	0.259	0.264
	(0.424)	(0.387)	(0.438)	(0.409)
Mother – BA	0.074	0.105	0.104	0.166
	(0.261)	(0.282)	(0.305)	(0.355)

Mother – Graduate School	0.034	0.064	0.174	0.100
	(0.180)	(0.224)	(0.263)	(0.279)
Income 3,000/10,000	0.040	0.092	0.030	0.043
	(0.175)	(0.272)	(0.157)	(0.191)
Income 6,000/20,000	0.086	0.126	0.071	0.060
	(0.253)	(0.311)	(0.235)	(0.222)
Income 7,500/25,000	0.076	0.086	0.057	0.084
	(0.240)	(0.254)	(0.209)	(0.240)
Income 10,500/35,000	0.229	0.160	0.209	0.148
	(0.380)	(0.340)	(0.371)	(0.335)
Income 15,000/50,000	0.287	0.233	0.277	0.234
	(0.410)	(0.398)	(0.411)	(0.397)
Income 15,000+/50,000+	0.281	0.302	0.355	0.440
	(0.414)	(0.438)	(0.443)	(0.471)
Asian	0.018	0.044	0.031	0.081
	(0.127)	(0.206)	(0.164)	(0.273)
Hispanic	0.053	0.131	0.031	0.071
	(0.219)	(0.338)	(0.173)	(0.257)
African American	0.089	0.093	0.052	0.043
	(0.282)	(0.290)	(0.221)	(0.202)
White	0.823	0.718	0.869	0.805
	(0.382)	(0.450)	(0.338)	(0.396)
Native American	0.007	0.014	0.009	0.000
	(0.084)	(0.116)	(0.094)	(0.000)
Number of Observations	1899	3020	582	591

[1] Source: Authors' tabulations from the NLS72 and NELS:88 surveys. Standard deviations are in parentheses. NLS72 calculations were made using the fifth follow-up weights included in the survey. Fourth follow-up weights were used for the NELS:88 survey calculations. Only those participating in these follow-ups are included in the tabulations.
[2] The NLS72 and NELS:88 samples are restricted to those who initially attend a two-year public college within 2 years of cohort high school graduation. Cohort high school graduation is defined as June 1972 for the NLS72 sample and June 1992 for the NELS:88 sample.

Table 4.D.3: Cumulative BA Distribution, Univariate Re-weighting NELS:88 using NLS72 Characteristics

Panel A: Full Sample						
	Years out of High School					Completion
Cohort	4	5	6	7	8	**Percent**
NLS72	56.8%	83.8%	92.4%	97.0%	100%	51.1%
NELS:88	43.6%	76.2%	89.6%	95.9%	100%	45.3%
Background Characteristic Reweighting Variable	**NELS:88 Distribution Reweighted by NLS72 Background Characteristic**					
Math Quartile	43.6%	75.9%	89.5%	95.8%	100%	49.5%
Reading Quartile	43.0%	75.9%	89.5%	95.8%	100%	47.3%
Father's Education Level	40.8%	73.1%	88.1%	95.1%	100%	41.8%
Mother's Education Level	40.3%	73.4%	87.7%	94.9%	100%	40.4%
Parent Income Level	42.2%	74.8%	89.0%	95.5%	100%	45.1%
Male Dummy	42.8%	75.8%	89.5%	95.8%	100%	45.2%
Racial Composition	44.2%	76.9%	90.1%	96.0%	100%	47.2%

Panel B: Public Non-top 15 Sample						
	Years out of High School					Completion
Cohort	4	5	6	7	8	**Percent**
NLS72	54.7%	84.8%	93.0%	97.1%	100%	59.4%
NELS:88	35.0%	75.0%	89.4%	95.3%	100%	54.3%
Background Characteristic Reweighting Variable	**NELS:88 Distribution Reweighted by NLS72 Background Characteristic**					
Math Quartile	35.0%	74.6%	89.3%	95.1%	100%	56.7%
Reading Quartile	34.8%	74.9%	89.3%	95.3%	100%	57.6%
Father's Education Level	33.8%	73.1%	88.2%	94.8%	100%	50.3%
Mother's Education Level	34.0%	73.2%	87.9%	94.3%	100%	49.1%
Parent Income Level	34.0%	73.5%	88.8%	94.8%	100%	53.9%
Male Dummy	34.4%	74.7%	89.3%	95.2%	100%	54.1%
Racial Composition	35.6%	75.8%	89.9%	95.6%	100%	56.8%

Panel C: Two-Year Sample						
	Years out of High School					Completion
Cohort	4	5	6	7	8	**Percent**
NLS72	35.9%	67.7%	85.2%	93.8%	100%	23.2%
NELS:88	21.0%	49.8%	75.4%	90.3%	100%	17.4%
Background Characteristic Reweighting Variable	**NELS:88 Distribution Reweighted by NLS72 Background Characteristic**					
Math Quartile	22.2%	51.1%	75.6%	90.2%	100%	20.8%
Reading Quartile	22.0%	50.5%	75.7%	90.4%	100%	18.7%
Father's Education Level	19.4%	47.5%	73.7%	89.2%	100%	16.5%
Mother's Education Level	20.9%	48.9%	74.7%	89.9%	100%	16.4%
Parent Income Level	20.5%	49.6%	75.1%	90.6%	100%	18.0%
Male Dummy	20.3%	49.1%	75.6%	90.5%	100%	17.4%
Racial Composition	20.9%	49.6%	75.1%	88.8%	100%	18.0%

[1] Source: Authors' calculation from the NLS72 and NELS:88 surveys. NLS72 calculations were made using the fifth follow-up weights included in the survey. Fourth follow-up weights were used for the NELS:88 survey calculations. Only those participating in these follow-ups are included in the tabulations.
[2] The NLS72 and NELS:88 samples are restricted to those who attend college within 2 years of cohort high school graduation.
[3] School types refer to first institution attended.

Table 4.D.4: Cumulative BA Distribution, Multivariate Re-weighting NLS72 using NELS:88 Individual Background Characteristics

Cohort	Years out of High School 4	5	6	7	8	Completion Rate	P Value of FOSD Test
Panel A: Full Sample							
NLS72	56.8%	83.8%	92.4%	97.0%	100%	51.1%	
NELS:88	43.6%	76.2%	89.6%	95.9%	100%	45.3%	
NELS:88 Reweighted	59.7%	84.8%	93.5%	97.4%	100%	50.7%	
P-Value of NELS:88 Reweight - NLS72	0.000	0.000	0.000	0.006		0.000	0.000
Panel B: Public Four-Year Non-top 15 Sample							
NLS72	54.7%	84.8%	93.0%	97.1%	100%	59.4%	
NELS:88	35.0%	75.0%	89.4%	95.3%	100%	54.3%	
NELS:88 Reweighted	55.6%	85.5%	94.1%	97.6%	100%	59.9%	
P-Value of NELS:88 Reweight - NLS72	0.000	0.000	0.000	0.005		0.002	0.000
Panel C: Public Two-Year Sample							
NLS72	35.9%	67.7%	85.2%	93.8%	100%	23.2%	
NELS:88	21.0%	49.8%	75.4%	90.3%	100%	17.4%	
NELS:88 Re-weighted	38.3%	67.7%	85.2%	93.5%	100%	22.7%	
P-Value of NELS:88 Reweight - NLS72	0.000	0.000	0.001	0.070		0.002	0.001

[1] Source: Authors' calculation from the NLS72 and NELS:88 surveys. NLS72 calculations were made using the fifth follow-up weights included in the survey. Fourth follow-up weights were used for the NELS:88 survey calculations. Only those participating in these follow-ups are included in the tabulations.
[2] The NLS72 and NELS:88 samples are restricted to those who attend college within 2 years of cohort high school graduation. Cohort high school graduation is defined as June 1972 for the NLS72 sample and June 1992 for the NELS:88 sample.
[3] School type samples refer to first institution attended.
[4] All p-values in the table are based on 5000 bootstrap replications of the data, clustered at the high school level. The p-value for the test of first-order dominance shows the percentage of the replications for which the NLS72 distribution is less than the NELS:88 distribution for years 4, 5 and 6.

Table 4.D.5: Cumulative BA Distribution, Multivariate Re-weighting NLS72 using NELS:88 Initial School Type and Individual Background Characteristics

Cohort	Years out of High School					Completion Rate	P Value of FOSD Test
	4	5	6	7	8		
NLS72	56.8%	83.8%	92.4%	97.0%	100%	51.1%	
NELS:88	43.6%	76.2%	89.6%	95.9%	100%	45.3%	
Initial School Type	57.0%	83.6%	92.3%	97.0%	100%	47.5%	
Initial School Type and Background Characteristics	59.4%	84.3%	93.3%	97.2%	100%	48.0%	
P-Value of NELS:88 Reweight with Initial School Type - NLS72	0.000	0.000	0.001	0.031		0.055	0.001
P-Value of NELS:88 Reweight with Initial School Type and Background Characteristics - NLS72	0.000	0.000	0.000	0.013		0.024	0.000

[1] Source: Authors' calculation from the NLS72 and NELS:88 surveys. NLS72 calculations were made using the fifth follow-up weights included in the survey. Fourth follow-up weights were used for the NELS:88 survey calculations. Only those participating in these follow-ups are included in the tabulations.
[2] The NLS72 and NELS:88 samples are restricted to those who attend college within 2 years of cohort high school graduation. Cohort high school graduation is defined as June 1972 for the NLS72 sample and June 1992 for the NELS:88 sample.
[3] School type indicators included in the weighting regressions are: public non-top 15, public top 15, private 4-year and two-year. School type indicators and samples refer to first institution attended.
[4] All p-values in the table are based on 5000 bootstrap replications of the data, clustered at the high school level. The p-value for the test of first-order dominance shows the percentage of the replications for which the NLS72 distribution is less than the NELS:88 distribution for years 4, 5 and 6.

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CHAPTER V

Conclusion

This dissertation presents three contributions to the fields of public finance and labor economics in three separate papers. The first paper, entitled "The Effect of Teachers' Unions on Education Production: Evidence from Union Election Certifications in Three Midwestern States," explores the effect of teachers' unions on the allocation of educational inputs and on the productivity of those inputs. I use unique data I collected on the timing of unionization and find unions have little effect on my measures of educational resources, but there is evidence they have a positive effect on teacher productivity. These findings are in contrast to much of the previous literature in this field, which I argue is due to the more accurate union timing data utilized in this analysis.

The second paper in this dissertation is "How Far to the Border?: The Extent and Impact of Cross-Border Causal Cigarette Smuggling, 1992-2002." In this paper, I derive a model of cigarette demand that explicitly accounts for cross-border cigarette purchasing behavior brought about by cross-state excise tax differentials. I find smoking is quite sensitive to the distance individuals live from lower-price borders, and in the presence of cross-border price differences, cigarette taxes are largely ineffective at reducing smoking or raising revenue on average.

The third paper presented in this work is entitled "Understanding the Increased Time to the Baccalaureate Degree." This paper first documents the rising time to the baccalaureate degree and falling college completion rates in general in the U.S. over the past three decades and presents evidence this shift is localized in the non-top ranked public institutions and community colleges. Turning to explanations of this shift, the paper argues it is not due to changes in the demographics of the college population, but rather can be traced in part to reductions in per-student resources and increases in student working behavior at these institutions.