NORTHWESTERN UNIVERSITY

Essays on Education, Policy, and Household Formation

A DISSERTATION

SUBMITTED TO THE GRADUATE SCHOOL

IN PARTIAL FULFILLMENT OF THE REQUIREMENTS

for the degree

DOCTOR OF PHILOSOPHY

Field of Economics

By

Benjamin Walter Bohdan Passty

EVANSTON, ILLINOIS

December 2008

ABSTRACT

Essays on Education, Policy, and Household Formation

This thesis is organized into three essays, each with particular insights involving public policy and family formation:

In the first essay, I address the well-documented correlation between the prestige of the university and the labor market income of its graduates by investigating whether a possible medium for this effect is the choice of a major field of study. Using data from two Texas universities, I predict students' choice of major from their high school qualifications and demographic characteristics. Because of Texas' implementation of a Ten Percent Law–which guarantees admission to the more elite school for those students in the top decile of their High School graduating class–I am able to carry out a regression discontinuity design. I find that the more prestigious institution makes students 22% more likely to major in Business than in Education, Nursing, or Social work. Results are confirmed through multinomial logit analysis.

In the second essay, I examine how increases in women's education affect marriage rates of men. I use State-Level Broad-Based Merit Aid Programs as an instrument to increase the education levels of cohorts of women. Because men often marry younger women, there are a number of men who are too old to have been exposed to the scholarship programs, but whose prospective spouses would have been. I find that men out of this group with college degrees who receive the treatment see reductions in marriage rates by as much as 4% when compared with similar men in control states. This gives evidence that the gains-from-trade effect on marital surplus that is caused by wage differential is of stronger magnitude than that of matching of traits.

In the third essay, my co-author and I estimate the impact of the introduction of no fault divorce laws on divorce rates. In order to deal with the problems of law endogeneity and unobserved state heterogeneity, we employ a new method in which we use only data from counties that border adjacent states. We find no evidence of any long-term increase in divorce associated with the passage of these laws. This means that the Coase Theorem–predicting the ability of parties to compensate each other within contracts in response to law changes–holds in equilibrium.

Acknowledgements

Chris Taber, my advisor, was an irreplaceable source of advice and direction. His sensible approach to the world of research taught me as much as his brilliant labor economics course. I especially appreciated his efforts to help me finish this dissertation long distance during the final year.

Luojia Hu was always both knowledgeable and optimistic, able to turn occasions on which I felt most insecure about my work into positive meetings. Greg Duncan first offered to hear about my paper sight unseen over the phone during a year on leave from Northwestern. His kindness and thoughtfulness in responding to my work are equalled by none.

My mentor Joel Mokyr not only invested all the effort of a committee member during my last three years at Northwestern, but opened his office to me on demand during the rigors and trials of the first three. His patience with me and willingness to teach me are much appreciated.

In addition to my committee, I am grateful to my friend and co-author Mark Surdutovich, who has taught me much about the world of economics. I am also grateful to Jocelyn Renner, my future wife, for much support and for typesetting this manuscript.

Finally, my parents–both Ph.D.'s in their own right–taught me quite a bit about the academic world. Some of the things they told me to worry about even came true. I owe them a debt that can never be repaid.

Preface

The process of research often involves trying to go from point A to point B, ending up at F or G, and then writing a paper that says one was going for G all along. While I do not wish to take away from the *gravitas* of my research by dwelling on this subject, the creative spark for all three of these essays depended on being in that right place, at that right time, where I got asked that right question by a very smart person.

I am certain that there are geniuses–I suspect Srinivasa Ramanujan to have been one–who are capable of re-inventing much of significant scientific thought on their own. If you, dear reader, are one of these, there is no advice I can give you: otherwise, I urge you to seek every opportunity possible to share your work, discuss your work, verbalize your way through your work, and invite criticism of your work. Do whatever you can to force yourself into thinking through your work, for I have learned that this–not surveying literature, not data collection, or interviewing, or programming estimations–is the process of research.

Table of Contents

List of Tables

List of Figures

CHAPTER 1

Introduction

In the interdisciplinary dialog surrounding the study of education, one of the most suitable points economists can make is that educational attainment has a price. The earliest models of human capital accumulation mirrored those of physical capital accumulation: a measurement of the fiduciary costs of schooling was compared to the gain in income, and a return comparable to those of physical assets could be calculated.

Unfortunately, the factors involved in young peoples' educational attainment decisions are much richer: this program of research endeavors to come to a fuller grip of the additional dimensions of education choice; while I retain the perspective of an economist, I enlarge the scope.

Labor economists have always been interested in the income that a person earns through the supply of their own energies and effort. Education has intrigued them because of its association with higher income levels. However, it is imperfect as a proxy for human capital: one reason is the widespread heterogeneity in the product supplied to students by different schools. One factor differentiating educated workers is their choice of a course of study, and I examine this choice in my first essay.

In particular, I explore whether similar students at different universities choose different majors. Institution choice is important in that it is a bundle of so many factors–quality of peers, quality of teachers, and quality of support structures–that education researchers find important. My source of identification is the Ten Percent Law, which provides an admissions threshold for different institutions.

My second essay shifts the focus from educational inputs to broadening my examination of the returns to education. Claudia Goldin [1992] was the first to suggest that returns-to-schooling are earned not just through the labor market, but also through the marriage market.

I follow in this tradition by grappling with the question of how education affects marriage rates. The need to deal with confounding factors is resolved by looking at how women's education affects the marriage rates of older men. I rely for identification upon state-level scholarship programs, exploiting the age differential typical in most married couples.

Since both of the above papers rely upon policy for identification, gaining insight into the proper estimation of policy effects is paramount. Unfortunately, there are two common pitfalls in estimating these: my third essay–co-authored with Mark Surdutovich–addresses this issue by proposing a previously overlooked method: relying upon border areas of states for identification. These fringe areas–by virtue of geography–answer our dual critiques of unobserved state-level heterogeneity and law endogeneity. We test this method using the case of Unilateral Divorce Law for several reasons: first, the laws are operationalized at the state level; second, they are relatively homogenous when compared to other categories of policy; and third, they are a nice counterbalance to the subject of marriage in the second paper.

CHAPTER 2

Does Attending a Different School Affect Choice of Major

2.1. Introduction

The effects of investment of human capital have concerned economists for fifty years. However, the question of whether some types human capital are worth more than others has gained the interest of more and more scholars lately, as there are legitimate differences in the product sold by different universities. Two degrees having the same name are earned from institutions which can have different faculty, resources, and peers; those with better tend to cost more; however, the importance of student effort and choices on student outcomes is still very relevant, and several studies suggest that institutions may not matter at all.

Here, I look at whether attending a different school will affect the major that a student chooses. I present evidence that they do, having repercussions that will extend beyond the students' academic career into the labor market for years after the student graduates from the university in question. If institutional prestige affects major, this could mediate a causal effect on earnings. In table 2.1, it is clear that different majors see quite large differneces in salary.

There are several existing studies about how students choose majors: Peter Arcidiacono [2003] has constructed a dynamic model of institution choice and major choice.

his main finding is that–while substantial–the premia to higher-paying majors have little effect on empirical selection of major. This leaves much room for the institutional mechanisms–Liang Zhang [2005] refers to this "black box" as "institutional quality"–to influence this choice for some students. In addition, Basit Zafar [2007] has found in his survey of Northwestern University undergraduates that certain groups of undergraduates are influenced heavily by their own uncertain expectations when they select a major. Using data from South Korea, Changhui Kang [2004] finds that there is an impact of university prestige on both college major and the likelihood of large-firm employment after completion of schooling.

Where I differ from these studies, however, is in my use of a regression discontinuity design to address the selection problem associated with institution-choice: students are not exogenously assigned to their universities, but rather choose them–conditional upon admission[1]–so as to maximize their own interests. This means that estimated effects are likely to be biased when using cross-sectional data. My regression discontinuity design resolves this issue.

While there are many studies that consider the details of the impact of a college education on earnings, there are other difficulties to be surmounted [Hamermesh and Donald, 2006]. While they demonstrate the importance of future revenue in choice of major, Montmarquette et al. [1997] note the importance of recognizing that choice of major affects not only the value of success, but also the probability of degree completion. Berger's findings are similar [Berger, 1988]. However, the standard methodology remains a conditional logit approach on survey data, without accounting for unobservable influences

[1]Of course, the number of truly selective institutions in the US is very small; only 130 colleges out of more than 3500 admit fewer than half of their applicants, according to fairtest.org.

Major	Salary,
Architecture, Fine Arts	$53,214.29
Soft Business	$109,911.10
Hard Business	$124,372.20
Communications	$77,873.63
Education	$43,232.56
Engineering	$102,292.80
Liberal Arts	$56,524.19
Plan II Honors	$128,289.50
Social Sciences	$79,804.51
Natural Sciences	$91,796.15
Nursing, Social Work	$48,900.00

Table 2.1. Average salary by major for UT graduates; data are from Hamermesh and Donald [2006].

of major choice; my results from regression discontinuity are therefore more robust, as they will not suffer from the bias of selection on unobservables [Goldberger, 1972]. I find that a more prestigious institution–through any of a variety of mechanisms–can alter this choice of major for students.

To address this problem of selection, I use a regression discontinuity approach, exploiting a Ten Percent Law Texas implemented for those students graduating from High School in 1998 or later; this law promised admission to all Texas State Universities to students graduating in the top decile of their high school graduating class. This law is part of a movement among states to institute "Uniform Admissions Policies." Because I can observe class rank explicitly, this has the advantage of being unbiased and consistent in estimating the effects of selective institutions within a neighborhood of the cutoff point. Thus, I can examine the major choices of students just below- or above the cutoff point and infer an effect of institution.

Since schools generally serve to add human capital to their students, those schools providing a stronger education may better serve their students in preparing for more intensive careers depending on their undergraduate training, such as in academia or in medicine. In addition, the quality of one's alma mater serves as a signal of one's ability. For less prestigious institutions, the selection of a more rigorous course of study (such as engineering) or a more lucrative one (business) can likewise signal high ability.[2].

I build a model of college choice and major choice to reflect these dimensions of the schooling decision. Choices of a less- or more prestigious institution are based on consideration of individual ability balanced with increased difficulty of the institution and increased wages conditional on graduation. Choice of a less- or more lucrative major is driven by the same tradeoff. The main prediction of the model is that exogenously shifting a student from the less prestigious institution to the elite institution will make them less likely to choose the lucrative major.

The data that I use consist of students from two schools, a "treatment" school and a "control" school. The treatment school is the University of Texas at Austin, while the control school is Texas State University: San Marcos. I will show that the relationship between class rank and chosen major follows a very different dynamic at the treatment school than at the control school. In comparing these schools, I will show that the distribution of class ranks among students of the two schools has a discrete jump at the cutoff point in terms of relative probability of attending schools. I apply both "Sharp" and "Fuzzy" regression discontinuity methods to confirm that similar students at the two

[2]This reasoning is better elaborated in Turner and Bowen [1999]

institutions are making very different choices of college major. Finally, I employ logit estimation to confirm the above when controlling for covariates.

This is a question of interest because the American school system tends to delay specialization until as late or later than any other school system in the world. In addition, the United States system is designed so as to encourage all students to at least attend college.[3] For these reasons, the choice of college major represents a significant widespread step for students in transitioning to adulthood. In addition, Hamermesh and Donald [2006] have shown that the choice of major correlates greatly with earnings in later life. However, their argument is that this is mediated through the selection of courses, and they control for major to show that science courses predict earnings independent of major.

In what follows, I will first provide background information on the History of the Uniform Admissions Policy movement, with the focus on the Ten Percent Law in Texas. Then I will present my model of school and major choice and describe my data; for a more complete discussion of the data collection, please see the data appendix. Following, I will carry out the regression discontinuity analysis, first with raw data, then controlling for covariates, and finally using "fuzzy" regression discontinuity methods. I will show that very similar students–when selected into different universities–make different choices of majors, which contradicts the prediction of my model.

2.2. Literature Review

There are many studies that look at the effect of attending an elite school: While the earliest one was Weisbrod and Karpoff [1968], Caroline Hoxby [1998] documents that the graduates of selective colleges tend to earn significantly more over their lifetimes. These

[3]Nearly 70% of students who graduate from high school begin college within the next year.

findings are similar to those of Brewer et al. [1999], James Monks [2000], and Liang Zhang [2005], the last of whom estimates that each $1000 increase in tuition for private school students leads to an average additional $733 in annual earnings after graduation. One must consider how reasonable an annual return of 70% must seem.

All of these studies suffer from the selection problem, which would bias the estimates of institutional effect upwards:[4] students are not randomly sorted into schools, but choose them, perhaps for reasons that are not visible to the econometrician. Motivated by this concern, Stacy Dale and Alan Krueger use the admission decisions of colleges to more accurately gauge unobserved student ability: their conclusion was the the institution that provided the sheepskin to the student had no significant impact on later wages, once this ability was accounted for by other institutions that had accepted the students for admission previously [Dale and Krueger, 2002]. Conversely Dan Black and Jeffrey Smith use propensity score matching estimators to find that standard OLS estimates, such as those in all of the regression papers above, may actually be *underestimating* the true effect [Black and Smith, 2004]; while this seems counter-intuitive, Black and Smith point out that disproportionately many high-ability students attend low quality colleges (many more than the proportion of low-ability students who attend a high quality college). This effect may actually cause the ability-sorting bias to work opposite of the effect originally expected.

[4] Opinions vary as to how serious the bias from selection is; Zhang [2005] claim that estimates using various quasi-experimental methods are not substantially different from theirs, but do not present them because of a desire to “keep the technical aspect of this study as parsimonious as possible.”

There are several scholars who have studied those factors that affect choice of major: Basit Zafar [2007] used data from a survey only at one institution and found many immediate social factors and enjoyment of coursework that play an influential role in the choice of major, in addition to expectations of future income. Boudarbat and Montmarquette [2006] use data on several cohorts of Canadian students to examine how lagged labor market conditions affect this choice by affecting the expectations of students about their own prospects. They find that expected earnings do affect choices, although men are more influenced by these changes than women. While they circumvent the selection problem of majors by modeling the selection process in which students take prior cohorts' outcomes as a signal of their own counterfactual incomes in various specialties, they make no attempt to account for the effect that the particular university attended could have on the income of the alumnus. Berger [1988] uses NLSY data from the US and has a similar finding, although he attempts to differentiate between majors that are lucrative quickly (an example would be accounting) versus those that eventually pay a lot more after a delay (like medicine).

I implement my regression discontinuity approach to address these problems with ability sorting by providing estimates of the effect of institution that are unbiased within a neighborhood of an admissions cutoff point. In addition, my results suggest that studying at elite schools may mediate earnings through the major that a student chooses.

2.3. Background

2.3.1. History of Uniform Admissions Policies

In this section, I will give the reader a general understanding of Uniform Admissions Policies: first, I will give a very specific history of the Texas law in order to illustrate its interaction with issues relating to minority education. Then, I will provide details about the differences in the California, Florida, and Texas laws. Schools have always been a nexus of racial inequality. For this reason, the history of attempts in America to either diminish or maintain their perpetuation of inequality is too complex for me to claim that I am giving a thorough summary here. However, this principle means that schools were one of the *foci* of the Civil Rights movement:

Many schools, including those in Texas, were segregated by the constitution until 1969[Holley and Spencer, 1999]. In a landmark court case–*University of California Regents v. Bakke* (1978)–the Supreme Court of the US ruled in favor of Alan Bakke, who alleged that the use of race in admissions was a violation of the equal protection clause of the 14^{th} amendment. Nevertheless, the language of the ruling was flexible enough that institutions could still base admissions on race so as to generate the benefits that accrue to students from being part of a diverse student body[Tienda and Niu, 2003]. As the 1980's began, it was with this motivation that many institutions turned their attention to the persistent race gap of their students, particularly in the states of Florida, California, and Texas, in which minorities will soon represent the majority of people in the state[Horn and Flores, 2003].

2.3.1.1. The Case of Texas. The hand of Texas, in particular was forced by the threat of withdrawal of federal title VI funding[Holley and Spencer, 1999]. While it was worrisome to college presidents and public relations officers that minorities represented a smaller percentage of college students than they did percentage of population in Texas, it was even more worrisome once one takes into account high school graduation rates for minorities, which were much more similar to those of whites than were college matriculation rates[Kain and O'Brien, 2001]. In order to attempt to diversify their student bodies, they endeavored to admit certain minority groups in greater numbers than qualifications might otherwise dictate.

Of course, to those white students who felt as though they were denied admission in favor of less-qualified minorities, this most definitely seemed like a zero-sum game. With regard to their own denied admission to the University of Texas at Austin Law School, four students brought their case to the Texas Supreme Court in 1996. The resulting decision–known as the *Hopwood* decision[5]–rendered the use of race in admissions in Texas to be illegal[Holley and Spencer, 1999, Leicht and Sullivan, 2000].

As was expected,[6] the effect of this decision was to severely decrease minority enrollment in the flagship public institutions of Texas[Kain and O'Brien, 2001]. Although Tienda and Niu [2003] note that this is due not to segregation so much as the "concentrated disadvantage" of minority students, the main mechanism for this decrease in

[5] *Hopwood v. University of Texas*

[6] Jessica Howell [2004] simulates a 2.5% reduction in minority enrollment coming from the elimination of affirmative action; further simulation shows that intensified minority recruitment is a more effective strategy for counter-acting this than either a uniform admission policy or increased support for minorities already admitted.

enrollment was the shifting of minority enrollment from those colleges to other, less selective ones within Texas[Bucks, 2004]. However, it has not been ruled out that talented minority students chose to leave the state entirely. Perhaps the greatest concern of all is that talented students might be so discouraged by *Hopwood* as to not bother applying to college at all, although this concern was shown to be without support[Card and Krueger, 2005].

It became clear that something needed to be done to protect the minority students without overtly granting them discriminatory benefits so as to satisfy the *Hopwood* ruling. While a more detailed history of this process can be found in Holley and Spencer [1999], it is sufficient for the purposes of this paper to say that–after a considerable amount of legal jockeying between the state legislature and the Texas Higher Education Coordinating Board–the Ten Percent Law was signed into law by Governor George W. Bush in late 1997. Beginning in the Fall of 1998, all public state universities would have to open their doors to any student who graduated in the top 10% of his or her high school class.

Ironically, this debate seemed to largely ignore the fact that until 1995, the University of Texas at Austin had automatically admitted students in the top 10% of their high school graduating class anyway. So what made the Ten Percent Law so different?

> . . . in the wake of Hopwood it became important for the school to advertise the bill. The state legislature also recognized this need, given the lackluster results for Fall of 1998, and the legislature required all high schools to post a sign explaining the top ten percent bill. UT went further and prepared a letter to every parent of a top-ten-percenter, which was signed by Governor Bush but mailed with UT funds (but contained no logo nor other identifying

> message about UT – instead, all public institutions in the state were listed in an insert)."[Leicht and Sullivan, 2000]

This change in recruitment on the part of UT Austin (which was mirrored by other schools) makes the task of distinguishing effects of the law more difficult.

The Ten Percent Law essentially leverages the segregation in High School compositions in order to re-balance college admissions.[Tienda and Niu, 2003] It was hoped by law makers that the Ten Percent Law would both increase the number of total college applicants in the state, and–conditional on applying–increase minority matriculation into the most prestigious flagship institutions. There is little reason to believe that either of these happened immediately in Texas after its passage, as the same number of minority students applied to and were admitted to the flagship schools. There was no meaningful increase in the percent of minority students enrolled at either flagship in the fall of 1998. However, Holley and Spencer [1999] are quick to point out that even if every minority in the top 10% attended the flagship schools, there would be no significant increase.[7]

In addition, Texas state university tuition was capped by the legislature until 2003. While tuition was fixed, the flagship schools were unable to differentiate the price of their product from those of the less prestigious schools in Texas.

Ultimately, the Supreme Court of the United States struck down the *Hopwood* decision, meaning that at these schools race was once again considered for admissions in 2005[May, 2005]. However, the TPL provides a means of affirmative action that comes without the stigma of being given an unfair-appearing advantage. In addition, it could provide a clear means of demonstrating the need for reform of inequality among the secondary

[7]The numbers appear on p. 32.

schools in Texas. Having their best students suffer from poor preparation at the post-secondary stage could be serious wake-up-call for those schools who are underserving their students[Holley and Spencer, 1999].

2.3.1.2. Following Texas' Lead: Florida and California. The mechanisms used to end race-based admissions in other states were more complicated[8], but the results were the same: politically, the protection of minority enrollments required immediate action.

Florida and California, similar to Texas in a variety of ways–not limited to the last name of their then-governors–followed suit soon after, although with laws that differ substantially from that of Texas. The full differences in the laws are laid out in table 2.2. The most important of these is a guarantee of admission only to the state university system, rather than the Texas guarantee of admission to the campus of your choice within the system. This means that the swelling effects observed at the flagship campuses of Texas are not mirrored by the other states.

Why haven't other states implemented Uniform Admissions Laws? According to statements made by the University of Michigan president–in another state that has seen litigation for race-based admissions–Texas, California, and Florida share several characteristics that many other states lack: a largely self-contained population (i.e. students from the state match closely at HS and College Levels); strong public universities; and, perhaps most importantly, a high minority population, with schools that are very sharply segregated.[9] In addition, issues of financial aid and recruitment must be addressed, making the

[8] In California, the UC Board of Regents voted to conduct admissions that were race- and gender-blind, and in Florida, Governor Jeb Bush's One Florida initiative included both the end of race-based admissions and the beginning of the 20% rule as a pre-emptive measure to thward a referendum campaign by Ward Connerly[Horn and Flores, 2003].

[9] While I don't have statistics, the first of these requirements would intuitively align with the fact that only the 1^{st}, 2^{nd}, and 4^{th} largest states in the union have such policies.

	California	Florida	Texas
year of first high school graduates	2001	2000	1998
Who Gains Automatic Admission?	Top 4 percent of graduating students from each comprehensive public high school or private high school accredited by the Western Association of Schools and Colleges in California	Top 20 percent of graduating students from each public high school in Florida	Top 10 percent of graduating students from each public or private high school in Texas
How is Class Rank Calculated?	Participating schools must submit students' transcripts; the UC system administrators then determine the top 4 percent of students based on GPA for UC-approved coursework completed in 10^{th} and 11^{th} grades. UC notifies students of their ELC status at the beginning of their senior year	Each secondary school district determines how class rank will be calculated	The Texas school or school district from which the student graduated or is expected to graduate calculates the rank based on standing at the end of the 11^{th} grade, middle of the 12^{th} grade, or at high school graduation, whichever is most recent at the application deadline
Nature of Admission	A UC campus, although not necessarily the one of your choice	An SUS institution, although not necessarily the one of your choice	The public Texas university *of your choice*
What is the program to help Minorities preparation before college?	Expanding Educational Horizons: tutoring and school partnerships	*A+* reform and Bright Futures Scholarship Program: provide merit-based scholarships	Closing the Gaps: improve participation and planning for higher education

Table 2.2. Comparison of Uniform Admission Policies from relevant state coordinating boards, as qtd. in Horn and Flores [2003].

institution of such programs politically complex. In addition to academic strength, the university system should have the resources necessary to guarantee admission to enough students that it actually matters[Coleman, 2003]. While capacity constraints may worry some, the Texas Ten Percent Law provides a safety valve should demand for the flagship campuses exceed their capacities: all students qualified for admission are to be subjected to either a lottery or a queue.[10] However, despite fears that this may soon happen, the two flagship schools in Texas are only seeing ten percent matriculation rates of around 70%.

2.3.2. Measuring the effects of Uniform Admissions Policies

Horn and Flores are quick to point out two complications in analyzing the effects of these policies on minority enrollments, both of which should bias estimates upward: the cumulative effects of school and the changing minority recruitment strategies employed by colleges and universities.

All three states with Uniform Admissions programs also have active programs in improving the performance of minorities at lower levels, which could result in an increases in minority enrollment completely independent of improving the admission process for qualified minority students[Horn and Flores, 2003]. In addition, there is the issue of student awareness, which would bias the effect of an intent to treat downward. While not an immediate effect, [11] the flagship schools in Texas–particularly the University of Texas

[10]The Texas of the bill from the Texas Administrative code is available under Title 19, Part 1, Chapter 5, Subchapter A, Rule 5.5.

[11]Kain and O'Brien [2001] and Thompson and Tobias [2000] suggested that there was a delayed effect because of a lack of high school and minority awareness of the new law. This lack of awareness is part of what Leicht and Sullivan refer to as a "minority pipeline," in which the number of minorities comparable to whites decreases at every level so that–by the time students have graduated from high school, taken

at Austin[12]–have seen a marked increase in the percent of their students of all races who were in the top 10% of their class[Dickson, 2005].

This tipping in the proportion of 10% students at the flagship campuses has alarmed some, including Texas-Ex[13] Marta Tienda, who has called for a cap of 50% on the number of top-decile students admitted to flagship institutions[Sheth, 2004]. In addition, there is concern about whether talented students–particularly those in the second decile–are being "crowded out" of the top institutions and attending second-tier ones or else leaving the state entirely. While this phenomenon is referred to as the "brain drain" hypothesis, there is only mixed evidence that it actually happens. Marta Tienda co-authoring with Sunny Niu found that it does not[Tienda and Niu, 2004], while the same, co-authoring with several others[Tienda et al., 2003], expressed considerable concern about whether the "crowding out" effect was being deeply felt by minority students in the second decile. Regardless of the concern about this effect, there is evidence that–conditional on preference for the school and qualifications–the probabilities that a white student or a minority student will matriculate are no different[Niu et al., 2004]. It is difficult to distinguish these gains from those caused by more aggressive minority recruitment[Horn and Flores, 2003, Howell, 2004]–particularly when such disparities still exist among the races in terms of taking tangible steps to college [14]–and the increase in the state of minority high school

the SAT–which only 62% of them do–gone to college, and stayed in college, there are many fewer of them[Leicht and Sullivan, 2000].

[12]Most accounts–see the Dickson [2005] piece for starters–have UT Austin increasing from 41% of students in the top decile at their high schools to 70%[Sheth, 2004]. The effect at Texas A&M has been less.

[13]For reasons of custom, former UT students are members of the "Texas Exes" association, a name which is inclusive of former students who did not complete a degree.

[14]On page 23, Holley and SpencerHolley and Spencer [1999] report that minorities are much less likely to take the SAT and send scores to a four-year institution, both of which are tangible and neccesary steps toward matriculation.

graduates[Kain and O'Brien, 2001], as well as the increased support of minority students by programs such as the Longhorn Scholars' Program[Tienda et al., 2003], and–as of 2001–they had not yet returned to pre-Hopwood levels, particularly at the flagship schools[Kain and O'Brien, 2001]. Of particular note is that enrollments overall at the flagship schools have increased markedly since the enactment of the Ten Percent Law, and these enrollments have correlated with a substantial ramping up of recruitment efforts, particularly minority recruitment.[15]

Unfortunately, these factors also bias upwards our estimates of the effect of Uniform Admissions on geographic and economic diversity as well as ethnic diversity, as geography and standard of living are correlated with race[Leicht and Sullivan, 2000]: during the first six years' tenure of the Ten Percent Law, the number of high schools from which UT drew it's freshman class has increased more than 20%.[16].

2.3.3. Model

In this section, I build a model of the school and major choice by high school graduates. Both (a) the choice to attend a specific college or none at all, and (b) the choice of major field of study are included. The goal of this model is to predict the effect on major choice of a shock to the institutional choice of an agent[17].

There are five stages in my model:

[15]Marked increases in enrollment at the flagship schools in Texas were marked by smaller increases in enrollment in California and in Florida after their enactments; however, there were other years in those states with even high increases, so we must be cautious in causally assigning the increase in enrollment to these X-percent lawss.

[16]A table of schools whose students are clear benefactors–collected from reports from the admissions office–is available from the author upon request

[17]Normally, I would refer to these people as "students", but not all of them choose to continue their education.

(1) schools make admissions decisions

(2) agents attend the school of their choice, or none at all

(3) students (agents who attend a school) choose a major

(4) students' graduation/failure to graduate is resolved

(5) agents–whether they graduate or not–then receive the appropriate resulting wage

I will discuss these stages in reverse order:

2.3.3.1. Wage. All wages discussed in this section will be referred to relation to w, the base wage for a college graduate. Agents who have not completed college earn a wage θw, where $\theta \sim [0, 1]$ is a person-specific parameter representing natural ability. The intuition for this wage determination is that–in the absence of formal training–people work in a low-skill sector in which productivity–perfectly observed by employers–is completely determined by ability.

For those agents who have a college degree, wages are determined differently: they are able to earn jobs in a skilled labor sector in which their training, not their ability, functions as an asset. This asset comes in four flavors, depending upon which institution granted it (elite or local) and which course of study they pursued (lucrative or non-). In order to reflect the premia for school- and major choice, they are added to the base wage according to the schedule given in table 2.3. This addition may seem different in spirit from the multiplication of the base wage by ability in the low-skill sector, but the intuition is the same for both: for low-skill workers, their ability is their asset; for high-skill workers, their credentials are their assets. Note that the effect of ability on wages is mediated by the credentials.

	Premia			
	base	college	major=	total
no college	θw		$=$	θw
college	w		$=$	w
elite college	w	$+w_s$	$=$	$w + w_s$
lucrative major	w		$+w_m =$	$w + w_m$
elite college and lucrative major	w	$+w_s$	w_m $=$	$w + w_m + w_s$

Table 2.3. Theoretical schedule of wages depending on college attended and major chosen.

A criticism of these outcomes is that there is no reflection of the dynamic aspect of college wage enhancement, as there is not an intermediate period in which the students who don't attend college are earning wages while their more ambitious peers are ramping up their human capital. I answer this criticism by saying that the wages represent present values of the entire stream of income so as to simplify these issues of timing. The enhancement in wages that students who opt for the elite college or for the more lucrative major is motivated entirely by the extra risk they assume when they opt for those.

2.3.3.2. Probability of Graduation. Students who attend college graduated with a base probability θ equal to their innate ability. Unlike the base wage, this probability is idiosyncratic to the person. However, the selection of an elite college or demanding major modifies θ additively in the exact same way as w, with p_m being the decrease associated with the lucrative major and p_s being the decrease associated with the non-elite school; for example, students who attend the elite school and opt for the lucrative major will graduate with a sum probability of $\theta - p_m - p_s$

2.3.3.3. Choice of Major. Once students have matriculated, they choose a major based on maximizing their expected income, dependent upon the probability of graduation and their wage conditional on the outcome of graduation. Students at the elite school will choose the "hard" major if the expected wage is higher, accounting for the uncertainty about graduation $(\theta - p_m - p_s)(w + w_m + w_s) + (1 - \theta + p_m + p_s)\theta w > (\theta - p_s)(w + w_s) + (1 - \theta + p_s)\theta w$, while students at a non-elite school will choose the "hard" major if $(\theta - p_m)(w + w_m) + (1 - \theta + p_m)\theta w > \theta w + (1 - \theta)\theta w$. These conditions can be simplified to:

Condition 1. *Students at the elite school choose the hard major if* $\theta > \frac{p_m(w+w_m+w_s)+w_m p_s}{p_m w+w_m}$.

Condition 2. *Students at a non-elite school choose the hard major if* $\theta > \frac{p_m(w+w_m)}{p_m w+w_m}$.

2.3.3.4. Choice of School. Before they choose a major, students must select which institution to attend. I assume that they do this without any knowledge of majors,[18] so they can only choose an institution based on an objective function with knowledge of how the institutions affect their graduation probabilities and eventual wages. Students have access to one non-elite school–expected value of attending is $\theta w + (1 - \theta)\theta w$–and the elite school–value is $(\theta - p_s)(w + w_s) + (1 - \theta + p_s)\theta w$. The condition for attending the elite school simplifies to

Condition 3. *Students attend the elite college if* $\theta > \frac{p_s(w+w_s)}{p_s w+w_s}$

In order to have a positive measure of students attending both colleges and both majors within colleges,

Condition 4. $1 > \frac{p_m(w+w_m+w_s)+w_m p_s}{p_m w+w_m} > \frac{p_s(w+w_s)}{p_s w+w_s} > \frac{p_m(w+w_m)}{p_m w+w_m} > 0$

[18] The intuition here is that students' decisions about majors before matriculation are non-binding anyway. A more detailed model would reflect additional knowledge of their own abilities that students might gain in the first year of college.

Theorem 1. *Suppose that* $\frac{p_s}{p_m} = \frac{w_s}{w_m}$. *Then condition 4 is true.*

Proof. First, suppose the conditions that are given. Let $\frac{p_s}{p_m} = \frac{w_s}{w_m} = \alpha$. Note that $\alpha = \alpha$ and $\alpha + \alpha(1+\alpha) > \alpha$.

I take these (in)equalities to generate coefficients in the expression $\alpha(p_s w)^2 + \alpha p_s w w_s + \alpha p_s^2 w w_s + \alpha p_s w^2 > \alpha(w p_s)^2 + \alpha w w_s p_s + \alpha^2 p_s^2 w w_s + \alpha^2 w_s^2 p_s$. Factoring and rearranging these terms, I get $\frac{p_s w + p_s w_s}{p_s w + w_s} > \frac{\alpha p_s (w + \alpha w_s)}{\alpha p_s w + \alpha w_s}$. Finally, substituting $\alpha w_s = w_m$ and $\alpha p_s = p_m$, we have $\frac{p_s w + p_s w_s}{p_s w + w_s} > \frac{p_m (w + w_m)}{p_m w + w_m}$.

Using the original conditions on α, I can also generate the expression $(\alpha + \alpha(1+\alpha)) w w_s p_s^2 + (\alpha + \alpha(1+\alpha)) w_s^2 p_s + \alpha w w_s p_s + \alpha(p_s w)^2 > \alpha w w_s p_s^2 + \alpha w_s^2 p_s + \alpha w w_s p_s + \alpha(w p_s^2)$. Factoring and rearranging, I have $\frac{\alpha w_s p_s + (w + w_s + \alpha w_s)\alpha p_s}{w \alpha p_s + \alpha w_s} > \frac{(w + w_s) p_s}{p_s w + w_s}$. Substituting, I have $\frac{w_m p_s + (w + w_s + w_m) p_m}{w p_m + w_m} > \frac{(w + w_s) p_s}{p_s w + w_s}$.

$\square$

Note that while the condition that $\frac{p_m}{p_s} = \frac{w_m}{w_s}$ is sufficient to guarantee a positive measure of students in each major at each school, it is not necessary. Secondly, ordering the students by their ability θ will result in those students who are choosing the more difficult/lucrative major at the non-elite school bordering those students at the elite school who choose the less-demanding major.

2.3.3.5. Schools' Admission Decisions. Of course, there is the complication of admissions: assuming that institutions observe $\tilde{\theta}$ which is correlated with student ability, but differs by a random error term. This random term causes students to be exogenously shifted across the boundary line of θ between institutions. My model predicts that this will also alter the major they select.

Assumption 1. *The elite institution only accepts those students with* $\tilde{\theta} \geq \theta_0$

Students with $\tilde{\theta} < \theta_0$ may only attend a non-elite institution. In the case of the Ten Percent Law in Texas, $\tilde{\theta} = 0.9$, although I acknowledge reality to be much more complicated than this.[19]

What the model yields ultimately motivates my regression discontinuity test: class rank provides a noisy signal of student ability. Nevertheless, the Ten Percent Law allows a high class rank to be entirely sufficient for some students to be admitted to an elite institution. These students who "accidentally" find themselves among the elite will choose the less lucrative major so as to protect their probability of graduation. Meanwhile, there will be others of near-equal ability who instead find themselves among the most talented at the non-elite school. In order to earn higher wages later, they compensate by choosing the more rigorous, lucrative major.

2.3.4. Data

The data that I use for this paper are merged from two different institutional data-sets, one control and one treatment. The treatment school is the University of Texas at Austin, the flagship campus of the University of Texas system. UT is widely regarded as the most selective public institution in Texas.[20] The control school is Texas State University – San Marcos, a nearby public university, similar in location and applicant pool, but less selective and prestigious than UT; the two schools differ geographically by only 30 miles.

[19]See the fuzzy regression discontinuity section for more details on relaxing this complexity.
[20]*US News & World Report*

I will henceforth refer to them as UT and TX State. Further details of how the data were cleaned and merged can be found in the data appendix.

One obvious shortcoming of my dataset is that it only includes two of the 35 public universities in Texas. Without having the entire universe of options for high school graduates, I must alter the above model of college choice. Suppose that there is a third university that exists which is an alternative to university 2. If students have an exogenous reason (think of geography as the closest construct to this) for their preference between university 2 and university 3, it is not a concern at all; alternatively, assuming that $w_2 = w_3$ would also be sufficient to identify a treatment effect in my model.

The only difference between universities 2 and 3 is that the students in university 2 are observable to the econometrician while the students in university 3 are not; therefore, having the above equivalence drives the identification of the treatment effect below. While, it is also a very strong assumption, the value of the option to not attend college at all is empirically low–relatively speaking–for students in the top fifth of their high school graduating classes,

I present summary statistics of the students at these two schools in some measures of student achievement and ability in table 2.4. From the first two columns–which include all students–It is clear that the two schools do have fairly different student populations; individual characteristics of the students will ultimately be controlled for when comparing student performance at the two universities. In the second two columns, I give the summary statistics from the two schools only of the group of students who are in a neighborhood around the ten percent cut-off line. While differences narrow to some extent, particularly in the SAT score–an alternative ability measure to the High School class rank–there are still fairly substantial disparities in many characteristics, particularly those of race. While controlling for these observables in my estimations is necessary and justified, the unobserved factors that may differ between the groups of students are also of concern. In order to proceed, I now make explicit my assumption that these unobserved factors do not correlate with the treatment.

	TXState	UT	TXState	UT
	All Students		Class Rank 8-13	
Students in sample	9,802	1280	886	240
Age in Jan 2002	21.9	21.7	20.2	21.8
SAT Score	1021	1210	1048	1192
Spring '02 Hours	12.3	13.8	12.4	14.2
Spring '02 GPA	2.67	3.04	2.88	2.96
Mean Class Rank	35.6	14.8		
Racial Breakdown				
# Black (%)	726 (4.9)	52 (3.9)	55 (6.2)	12 (5.0)
# Hispanic (%)	2,675 (18.1)	208 (15.4)	180 (20.3)	37 (15.4)
# Asian (%)	469 (3.2)	203 (15.0)	33 (3.7)	42 (17.5)
# White (%)	10,948 (73.9)	886 (65.7)	618 (69.8)	149 (62.1)
family income				
% $< \$40,000$	23.4	12.0	30.3	9.9
% $> \$80,000$	39.7	64.6	25.7	62.7

Table 2.4. Summary statistics by institution for those students for whom birthyear, race, non-zero satscore, spring 2002 GPA and hours, and class rank are available; in second two columns, I use only students who are ranked 8-13 %.

2.4. Regression Discontinuity Analysis

For a first look at choice of major, I present table 2.5, which gives probabilities of majoring in science conditional on class rank; very different relationships are clear at the two institutions. For those students ranked in the top decile at TX state, a science major is actually more attractive than it is to their counterparts at UT. Note that these are students for whom admission to UT was available. This story is consistent with the prediction of my model: a more difficult major is more attractive to students at a less prestigious institution, as it is a way of signaling ability when one's degree is coming from the less prestigious university.

As class rank declines, however, the relative likelihood of a science major shifts, suggesting different dynamics in choice of major between the two schools. At TX state, there is a monotonic decrease in majoring in science corresponding to the decline in class rank, while at UT Austin there is sustained interest in the science major. The increased risk of non-completion more than outpaces the enhanced earning power for students in the 20^{th} to 50^{th} ranks. This could be caused by different kinds of students self-selecting into the two universities (while the top decile could reasonably have attended either school, it is less likely that these particular students at TX state would have been admitted into UT).

	UT	TXState
0-5	0.223	0.299
5-10	0.217	0.227
10-15	0.257	0.211
15-20	0.227	0.185
20-25	0.286	0.173
2nd quarter	0.185	0.155

Table 2.5. Probability of being a science major conditional on class rank

An alternative to identifying such systematic characteristics that differentiate these students' selections is to employ the Regression Discontinuity design, employing the structure of a cutoff point that mediates a difference in treatment. Here, my running variable–the criterion for treatment–is class rank, and the ten percent line is a criterion for applying guaranteed admission into my "treatment" school. The advantage of this design is that–under certain assumptions–estimates of the treatment effect that are free from selection bias are produced; Shadish et al. [2002] are quick to remind readers that these estimates are "stronger for causal inference than any design except for the randomized experiment." Later work [Hahn et al., 2001] shows how these conclusions can apply even in the case in which the treatment is a stochastic treatment with a discontinuous jump in probability at the cutoff point.

The use of this method is not without its shortcomings however: the method suffers to the extent that the support of the running variable is not continuous. While statistical power and interaction effects are strongest if the cutoff point is the mean of the running variable,[Shadish et al., 2002] this is not always possible. Misspecifications of functional form can result in a type I error in testing for a treatment effect.

One issue of interest in regression discontinuity studies is whether the presence of the cutoff affects the distribution of the running variable around the cutoff, for example by borderline students exerting extra effort to qualify for admission to UT, or by lenient teachers giving higher grades to these students. Justin McCrary [2005] suggests a test for this by examining the continuity of the density function of the running variable distribution at the cutoff point. In table 2.6 I present the densities per institution of the class rank variable.

CR	TX State	UT	P(UT)
8	123	57	0.317
9	124	51	0.291
10	111	40	0.265
11	113	31	0.215
12	120	42	0.259
13	139	30	0.178

Table 2.6. Tabulation of students by class rank and institution; note that highest possible class rank–the one ranked ahead of all others–is 1; focus here is on the neighborhood of the top decile cutoff.

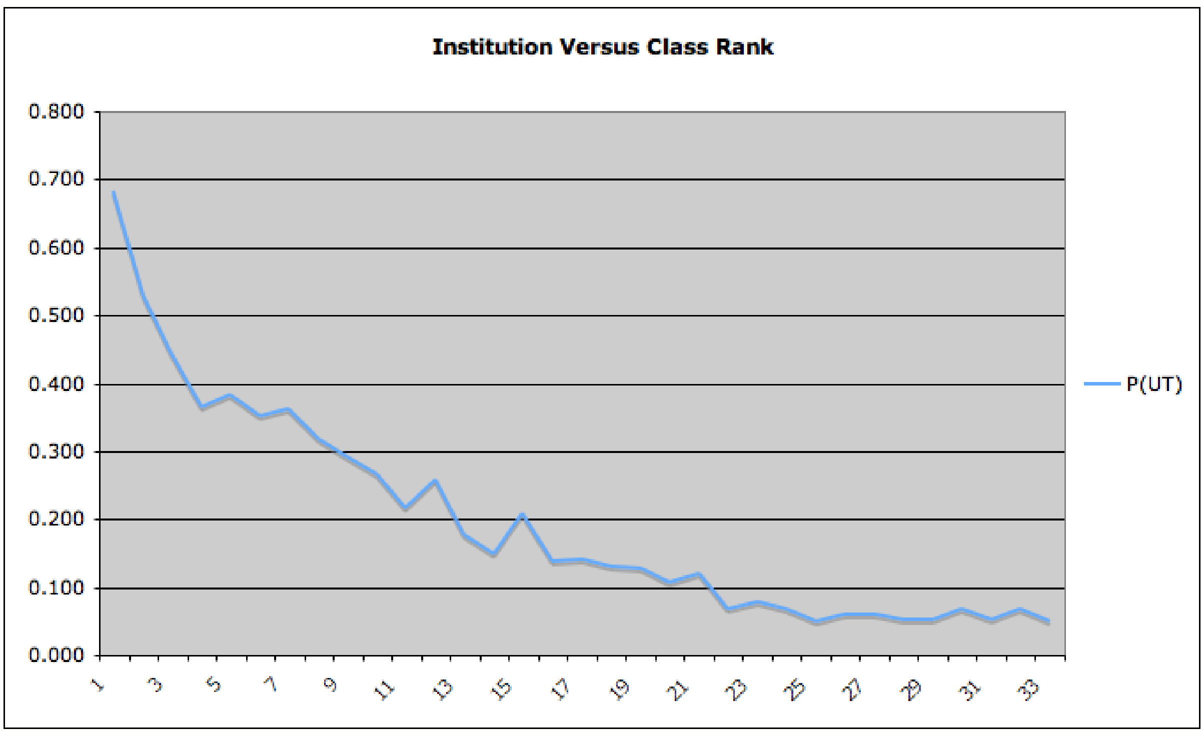

Figure 2.1. Probability of Institution Choice, conditional on class rank

The first point of interest is the ten percent class rank cutoff line. In figure 2.1 I present a graph showing how the probability of attending UT-Austin changes depending on class rank; ranks are presented as the ceiling of a non-integer class rank (i.e a student ranked 19^{th} out of 200 has class rank 10). As expected, there is a discontinuity right below the ten percent cutoff, although it is smaller than perfect: the unconditional probability of attending UT Austin goes from $\frac{40}{111}$ to $\frac{31}{113}$, a drop of 9%. Note that this is unconditional on any demographic controls.

In addition, there are signs of an "adjustment" in the ranks immediately below the ten percent line. The relative increase in $P(UT)$ going from rank 11 to rank 12 is a sign of the difficulty against which McCrary [2005] cautions: students who may naturally find themselves as 11%-ers may be exerting extra effort or seeking extra credit so as to gain that slight extra bump that will guarantee them admission into UT. One such method of "gaming the system"–changing high schools–is tested in Cullen et al. [2005] and found to take place.

In order to further examine the extent to which this cutoff line determines admission into UT, I estimate a Linear Probability Model with the dependent variable being the school a particular student has attended; in addition to including class rank, both in linear form and with an indicator for whether a student was in the top ten percent of his or her high school class, I include various predictors that could influence admission, whether explicitly or implicitly. The results of this model are presented in table 2.7.

As expected, being in the top ten percent provides a positive shock to the probability that a student will become a UT student; note that the negative coefficient on class rank is expected, as lower class ranks (i.e. 3) correspond to more qualified students.

Next to the randomized experiment, the regression discontinuity design is the most robust design for demonstrating a causal relationship[Shadish et al., 2002], allowing for fewer assumptions about the distribution of the error term, functional form, and exclusion restrictions. However, it is critical that the assignment to the treatment be based on a

	DV: College Choice
Class Rank	-0.0022***
	(0.0002)
SAT Score	0.0002***
	(0.0000)
Indicator for Ten Percent Rank	0.1880***
	(0.0107)
Hispanic	-0.0035
	(0.0167)
Asian	0.3234***
	(0.0221)
Caucasian	-0.0050
	(0.0150)
Female	-0.0351***
	(0.0070)
constant	0.8007**
	(0.2995)
Sample Size	7935

Table 2.7. Linear probability model for effect of student qualifications on University Choice; Dependent variable is indicator of University Choice (UT Austin is 1; TX state is 0); omitted race category is black

continuous and observable variable, at least in the neighborhood of the cutoff point for application of the treatment[Van der Klaaw, 2002, Hahn et al., 2001]. Nevertheless, concerns remain about the strength of assumptions[Hahn et al., 2001] and the reliability of implementing the treatment. In particular, expansion of the neighborhood around the cutoff point will typically require additional functional form assumptions. Nevertheless, estimation based on this method is becoming more widespread in economics circles,[21] because it is unbiased.

For this particular study, careful consideration of the treatment is required: as per the Ten Percent Law, students that are in the top decile of their graduating class are

[21]See, for example Jacob and Lefgren [2004] or DiNardo and Lee [2002].

guaranteed admission into every public university in Texas. This does not exclude other students from admission (except for capacity constraints), and does not force them to go. However–for those students whose top choice is one of the flagship institutions in the state (either UT-Austin or Texas A&M)–this law can provide access to a school which was otherwise closed to them, particularly if they are from a very inadequate high school.

Data suggest that this has impacted admission: both Marta Tienda and Julie Cullen have evidence that this affects roughly 1000 students per year. Admissions for UT-Austin show an increase in schools who've had at least one student admitted from 795 to 943, and I'd expect a similar increase at Texas A&M[Admissions, 2005]. The marginal schools are likely to be ones with students in the top decile who are benefitting from the ten percent law, and who would not otherwise be admitted.

I use the regression discontinuity design in two different ways, each of which identifies the effect of a slightly different treatment.

2.4.1. A "Sharp" Regression Discontinuity Estimation

In this section, I employ the "sharp" regression discontinuity design as described in Hahn et al. [2001] The treatment comes from receiving guaranteed admission to UT via the Ten Percent Law; the careful reader will have noted from table 2.6 that not all of those students who are guaranteed admission into UT because of their top decile status choose to attend. This creates issues of intent to treat for which I must account; my efforts to do so are described in section 2.4.2

In table 2.8, I look at purely unconditional cross-tabulations of majors with class ranks near the ten percent cutoff. Comparing the ten percent students at UT to the

	High School Class Rank											
	8		9		10		11		12		13	
	TX	UT	TX	UT	TX	UT	TX	UT	TX	UT	TX	UT
Number of Students												
Business	20	10	26	11	32	7	23	6	36	2	48	2
Commun.	11	10	3	10	14	4	10	5	13	7	19	4
ENS	22	3	30	1	25	1	25	4	36	5	24	4
Engineering	2	7	4	7	1	9	0	4	3	8	2	5
Fine Arts	19	1	13	0	10	1	19	0	9	4	12	1
Liberal Arts	30	9	36	13	41	11	32	5	29	7	32	8
Natural Sciences	29	17	27	10	39	6	27	8	22	9	37	6
Fraction out of 1												
Business	0.15	0.175	0.187	0.212	0.198	0.179	0.169	0.188	0.243	0.048	0.276	0.067
Communications	0.083	0.175	0.022	0.192	0.086	0.103	0.074	0.156	0.088	0.167	0.109	0.133
ENS	0.165	0.053	0.216	0.019	0.154	0.026	0.184	0.125	0.243	0.119	0.138	0.133
Engineering	0.015	0.123	0.029	0.135	0.006	0.231	0	0.125	0.02	0.19	0.011	0.167
Fine Arts	0.143	0.018	0.094	0	0.062	0.026	0.14	0	0.061	0.095	0.069	0.033
Liberal Arts	0.226	0.158	0.259	0.25	0.253	0.282	0.235	0.156	0.196	0.167	0.184	0.267
Natural Sciences	0.218	0.298	0.194	0.192	0.241	0.154	0.199	0.25	0.149	0.214	0.213	0.2

Table 2.8. Choice of major by class rank; Institution of 0 is Tx State, 1 is UT Austin.

11 percent students at TX State, it is clear that the major composition is very different for some majors: the percentage of students who are in the schools of business and communications are similar; however, those students who choosing ENS, engineering, fine arts, and natural sciences are wildly different. Even disputing the validity of a TX State engineering major[22], there are still a substantial number of students who are selecting into different fields right around the cutoff. I use the differences in distribution of majors within universities to construct a chi-squared statistic, which I compute to be 12.2 with six degrees of freedom; this is just short of the 12.59 cutoff to be able to reject that the distributions are the same at the 95% confidence level; rejection at the 10% level is achieved easily. Unconditional evidence suggests that students at the two different institutions are selecting different majors. As to whether we can explain these differing choices away through other characteristics, additional methods must be employed.

2.4.2. A "Fuzzy" Regression Discontinuity Estimation

Next, I relax the strong assumption that the treatment applies in a binary matter above- and below the cutoff point. Hahn et al. [2001] build on the work of Trochim [1984] and Jianquing Fan [1992] to develop an estimator for "Fuzzy Regression Discontinuity" that is more appropriate in this context, as the treatment of the Ten Percent Law merely increases probability of admission, without guaranteeing matriculation.

The ideal regression discontinuity setting is that of "sharp" regression discontinuity, in which the cutoff threshold exactly determines application of the treatment. "Fuzzy" regression discontinuity allows a weaker relationship: there is still a running variable and a

[22] As there was no engineering program at TX State in 2002, these students who are listed as majors would have completed their undergraduate course work at Texas A & M University in College Station.

cutoff point, but the cutoff point now merely represents a discrete shock in the probability of the treatment being applied. The advantage of this method is that it can be brought to bear in a wider set of applications; the disadvantage is that the imperfect application of the treatment requires slightly more sophisticated analysis.

First, I consider the question of how well the treatment actually bites conditional on the running variable. Using the methods developed by Hahn, et al., some degree of fuzziness is permitted, as long as there is a legitimate discontinuity in the expected application of the treatment at the point of interest. In order to gain a sense of the amount of fuzziness, I estimate the probability of treatment conditional on class rank ($P(UT|cr)$ non-parametrically within neighborhoods of the ten percent cutoff line.

In order to do so, I use a uniform Kernel with decreasing bandwidths around this cutoff point: the tabulation of students in my within these neighborhoods at each universities is given in table 2.9. It can be seen that–as students enter the top decile of their class, there is indeed a jump in the unconditional probability of attending UT from just over 10% to well over 16%. Equally interesting is the nature of its non-monotonicity: as class rank continues to decline past the cutoff, the probability of a student attending UT goes back up, before–and this is not shown in the table–it returns to declining in the late teens; this may be a sign of the irregularities cautioned against by McCrary. Note that this is all unconditional on other covariates.

Based on the results in section 2.4.1 , I expect that the action will be in the tradeoff between Business and Education/Nursing/Social Work majors. I repeat the non-parametric kernel estimations above using the ratio of these two majors in place of institutional

Table 2.9. Institution Chosen Conditional on Class Rank

bandwidth	Ranked < 10%			Ranked > 10%		
around 10%	UT	TX	%	UT	TX	%
3	147	457	0.243	104	519	0.167
2	90	301	0.230	74	324	0.186
1	39	147	0.210	32	169	0.159
$\frac{1}{2}$	15	63	0.192	14	81	0.147
$\frac{1}{4}$	6	30	0.167	7	46	0.132
$\frac{1}{8}$	2	10	0.167	3	26	0.103

choice, once again for increasingly small neighborhoods around the ten percent threshold; results are given in table 2.10.

Table 2.10. Fraction of Business Majors out of Business and ENS majors, conditional on class rank; tabulated by distance from the cutoff class rank

	UT Austin			TX State		
	p	n	se	p	n	se
-3	0.848	33	0.022	0.500	158	0.020
-2	0.895	19	0.022	0.532	111	0.024
-1	0.875	8	0.039	0.579	57	0.032
-1/2	1.000	3	0.000	0.680	25	0.044
-1/4	1.000	2	0.000	0.818	11	0.045
+3	0.435	23	0.051	0.507	215	0.017
+2	0.471	17	0.060	0.470	132	0.022
+1	0.600	10	0.076	0.466	58	0.033
+1/2	0.600	5	0.107	0.355	20	0.051
+1/4	0.500	2	0.177	0.300	10	0.066

It becomes clear immediately that behavior above and below the cutoff line is very different for these exogenously selected groups of students, at least as soon as I use a bandwidth large enough to include a fair number of observations. But exactly how different is this probability?

The computation of the the Hahn et al. [2001] estimator is built from the computations given above.

$$\beta_{HTV} = \frac{y^+ - y^-}{x^+ - x^-}$$

x^+ and x^- are the limits of the treatment probabilities from above and below the cutoff line respectively. From table 2.9, I take their difference as $0.192 - 0.147 = 0.055$. The numerator of this statistic is built from the change in relative probabilities of the two majors shown in table 2.10: I take $30\% - 13\% = 17\%$ and divide it by 5%, the difference in selection of the treatment probabilities. This gives me $\beta = 3.4$ for the coefficient on institution in the logit equation, which is the increase in log odds of majoring in business. To translate this into a treatment effect, I use the inverse of the natural log, giving me the result that students are 22% more likely to choose business over ENS when they attend UT Austin.

2.5. Multinomial Logit Estimations

Nevertheless, there are persistent observables–race, sex, and SAT score certainly affect the sorting of students into majors[Zafar, 2007]–that should be integrated into this analysis. In this section, I estimate a series of standard logit models for each major in order to test whether–controlling for observables–there is still an impact of institution on major choice.

The logit equation that I estimate for whether agent i chooses major j is of the form

$$y_{ij} = \begin{cases} 1 & \text{if } y_{ij}^{\bigstar} > 0 \\ 0 & \text{otherwise} \end{cases} \tag{2.1}$$

$$y_{ij}^{\bigstar} = \beta_j x_i + \delta\nu_j + \zeta\Psi_i + \gamma T_i + \xi_{ij} \tag{2.2}$$

Here, $y_i^{\bigstar}$ is unobservable latent utility for major j, but it determines an observable indicator Y_j representing the choice of major of person i that maximizes their objective function based on T_i, which represents the treatment effect of selection into a university based on the Ten Percent Law; x_i, which represents the individual characteristics of person i, except for class rank; and ξ_i, which represents those additional random components that are unobservable to both student and econometrician. I estimate one of these equations separately for each major, dividing my sample based on their selection into that major.

It bears repeating that I create T_i as an interaction of attending Texas-Austin and being in the top 10% of their high school class for a measure of the treatment effect when accepted: this is the effect that is most interesting here. Other observable characteristics that I use are age, family income, high school achievement as measured both by test scores and class rank, along with dummies representing which institution is attended, as well as a dummy for whether a student is in the top ten percent of his class, representing the intention to treat (the interaction represents the treatment accepted).

While I don't give the entire table for each estimation, I do present table 2.11 to summarize the coefficients for the T_i that are calculated for each major, along with their standard errors: In addition to the estimation performed over the entire sample, I give a second estimation just for borderline class rank students at the two schools. I do this by eliminating those students who are more than three percentile ranks away from the cutoff, as well as students whose behavior is opposite that of the intended regression discontinuity (i.e. students who are above the ten percent line but still chose the less prestigious school); therefore, this effect is essentially a "sharp" regression discontinuity estimation; these estimates lack the finer accounting for the probability of the treatment that is handled as part of the "fuzzy" methods, and they should be taken with this caveat.

Table 2.11. Estimation results for effect of ten percent status on selected major:

	Whole Sample		$8 \leq CR \leq 13$	
Major	Effect	SE	Effect	SE
Business	1.992	(0.254)	0.216	(0.592)
Communications	0.425	(0.247)	0.892	(0.710)
Ed/Nursing/Social Work	-0.253	(0.291)	-2.223	(0.747)
Fine Arts	-0.171	(0.350)	-3.189	(1.067)
Liberal Arts	0.197	(0.197)	-0.308	(0.587)

Based on the estimation results, it seems that business majors are most affected by the institution. Interestingly enough, students seem more likely to major in ENS when they go to TX state. Note that this is after controlling for HS achievement and other factors. This contradicts as well the prediction of Turner and Bowen [1999] and of my own model, which would suggest that students going to the less prestigious institution would need to adopt more demanding majors to signal their ability. If anything, it seems that identical students are actually assuming more demanding majors at the more demanding institution. Perhaps the competition of other higher ability students spurs them to a more demanding course of study. Or maybe the poor realization of college admission for the students who don't go to UT causes them to self-select out of the tournament for the most demanding career paths.

While there is a great concern with external validity between the border students and all students,[23] including those students far away from the cutoff point did not decrease the estimated effect of UT to encourage students to pursue a more demanding major, even controlling for race, family income, and the noisy signals of ability that are available.

2.6. Conclusion

In this study, I have presented evidence of a causal relationship between school attended and major selected. I build a model that predicts that exogenous shifts from a local university to an elite university result in a student finding the less lucrative major more attractive. When using a variety of regression discontinuity specifications with that data collected from the two aforementioned schools, I actually find the opposite of this prediction: students at UT Austin are more likely to abandon Education and Nursing in favor of majoring in Business.

This study joins a healthy literature examining the impacts of institutions on long-term academic and labor market outcomes: by prompting students towards more lucrative majors, I have identified one candidate medium through which elite institutions increase the lifetime earnings of their graduates.

[23]In particular, class rank might be a poorer signal of border students' ability to the extent that they might "game the system" or respond to the law in some other differentiated way.

2.7. Data Appendix

The data that I use from UT are those from Gerald Oettinger [2005]. Compiled with the help of Marc Musick, they include data on a random sample of students' academic performance in both high school and in college, as well as survey data on parents' finances and major, along with some demographic data. One concern is that these data only include students in their sophomore year and higher. At UT, ninety percent of freshman return for their sophomore year, the highest rate of any public university in Texas.[24] any other school.

The data from TX State were constructed with the intent of paralleling Oettinger's data. However, they are more comprehensive in some respects and less in others. Unlike Oettinger's data, no additional survey to students could be used to glean information about parents' finances and student work activity. However, I have data for more semesters[25] and for more students. In addition, I have high school identifiers for some TX State students[26] as well as the CIP code for major, a two-digit code that is much more specific than the college of major code in the UT data. Family income required a similar aggregation with the UT numbers, although reporting problems restricted information on income to fewer than half of the students in the samples from both institutions.

For purposes of this project, the two datasets needed to be merged. While most of the variables were rough equivalents of each other, this required a bit of decision-making: in terms of majors, the TXState dataset reported majors using CIP codes; these were

[24] This figure was released by the Texas Higher Education Coordinating Board; a table of retention rates for all texas universities is available from them online.

[25] Oettinger's data were just for Spring 2002, as well as a summary of prior course work; in addition, I have data for 11,000 students, versus the subsample of 1,100 that Oettinger used.

[26] Privacy regulations forbade TX State from releasing to me those identifiers for schools that sent fewer than five students to TX State.

linked to the broader categories of majors–based on academic college–of UT according to the schedule in table 2.12.

In addition, the class rank variable is differently specified in the two datasets. For the UT data, both a size of graduating class and a numerical rank in that class are provided; I divide the latter by the former in order to create an inverse percentile rank from 1-100, which I then round up to the nearest integer. The TX State data are reported as integer percentiles, so subtracting them from 100 produces the equivalent inverse percentile.

UT College	CIP Code	Major	UT College	CIP Code	Major
Business	1	agriculture	Communications	9	communications
	2	agriculture		10	comm tech
	3	conservation		25	library science
	8	sales/marketing			
	52	business management	Fine Arts	4	architecture
				50	performing arts
Education,	12	personal services			
Nursing,	13	education	Liberal Arts	5	cultural studies
Social Work	19	home econ		16	foreign language
	20	voc home econ		22	law
	31	Leisure/fitness		23	english
	43	protective services		24	liberal arts
	44	public admin		28	ROTC
	51	health professions		29	military tech
				38	philosophy/religion
Engineering	14	engineering		39	theology
	15	technology		42	psychology
	21	tech education		45	Social Science/history
	41	science tech			
	46	construction	Natural Sciences	11	computer science
	47	mechanics		26	biology
	48	precision production		27	mathematics
	49	transportation		30	interdisciplinary
				40	physical sciences

Table 2.12. CIP majors (from Texas State University) and their assignments to Colleges (UT Austin); note that there are no undergraduate library science students in either sample, even though it is classified.

CHAPTER 3

The Good Ones Go Fast: Education, Merit Aid, and Marriage Outcomes

3.1. Introduction

While still very young, many people make choices that will have implications for the remainder of their lives. Two major ones are how much education they will attain and whom they will marry. In this paper, I examine how these two decisions interrelate. In particular, I focus on whether the marriage rates of men are affected by the education levels of women. I confirm that–because it reduces the gains-from-trade to be enjoyed as part of the marriage surplus–women's enhanced education reduces the probability of marriage for men with college degrees.

While there is already a substantial literature on how education and marriage relate, the theoretical prediction is ambiguous because of the correlation between education and wages[Becker, 1973]. When looking at cross-sectional data on education and marriage, there is also a non-trivial selection problem on education. This problem is particularly pressing for this question because the same unobservable factors that affect educational attainments–value of the future, innate ability, and even lack of credit constraints–make someone more desirable as a marriage partner.

I address this selection problem by exploiting a well-documented source of variation in the overall education levels–State-level Broad-Based Merit Aid Programs–to increase the education levels of women[Dynarski, 2003].

Unfortunately, there is a second problem with which I must contend: when a woman has her education increased in this way, many suitors and competitors are also exposed to the scholarship program. The exposure of suitors will bias estimates of the effect on marriage upwards, while the exposure of competitors (i.e. other women in her cohort) will bias effects downwards.

In order to address these obstacles, I make a comparison between two groups of men who are defined by age cohorts: those men who are young enough that their prospective wives are exposed to the scholarship programs, and older men whose prospectives are too old to have been exposed to the scholarship programs. Because men usually marry younger women, neither group of men has been exposed to these same scholarship programs. This approach is diagrammed in figure 3.1, in which I have used different borders to indicate cohorts that correspond through the marriage market. I use the upper-most group of men as a control group for the "treatment" group of men in the middle row. This allows me to see the effect of a change in women's education rates on the marriage chances of corresponding cohorts of men.

My main result is that there is a decrease in the marriage rates of certain men whose potential mates have been exposed to this treatment. This decrease is felt entirely by the men who have a college degree and are old enough that their marriage pool consists almost entirely of women greater than 22 years of age, i.e. old enough to have graduated from college already. Since the education levels of those older men have not been affected by the scholarship programs, I observe the impact of women's education on men's marriage rates, while holding men's education fixed. In addition, I use the empirical probability distribution of age matching by spouses to build a weighted measure of the mean education of a man's potential mates, as well as their mean exposure to the scholarship programs; this allows me to test for changes using a finer measure than the presence in these cohorts as a robustness check to the above result, as well as hold constant the degree of competition from younger men who are themselves exposed to the scholarship programs.

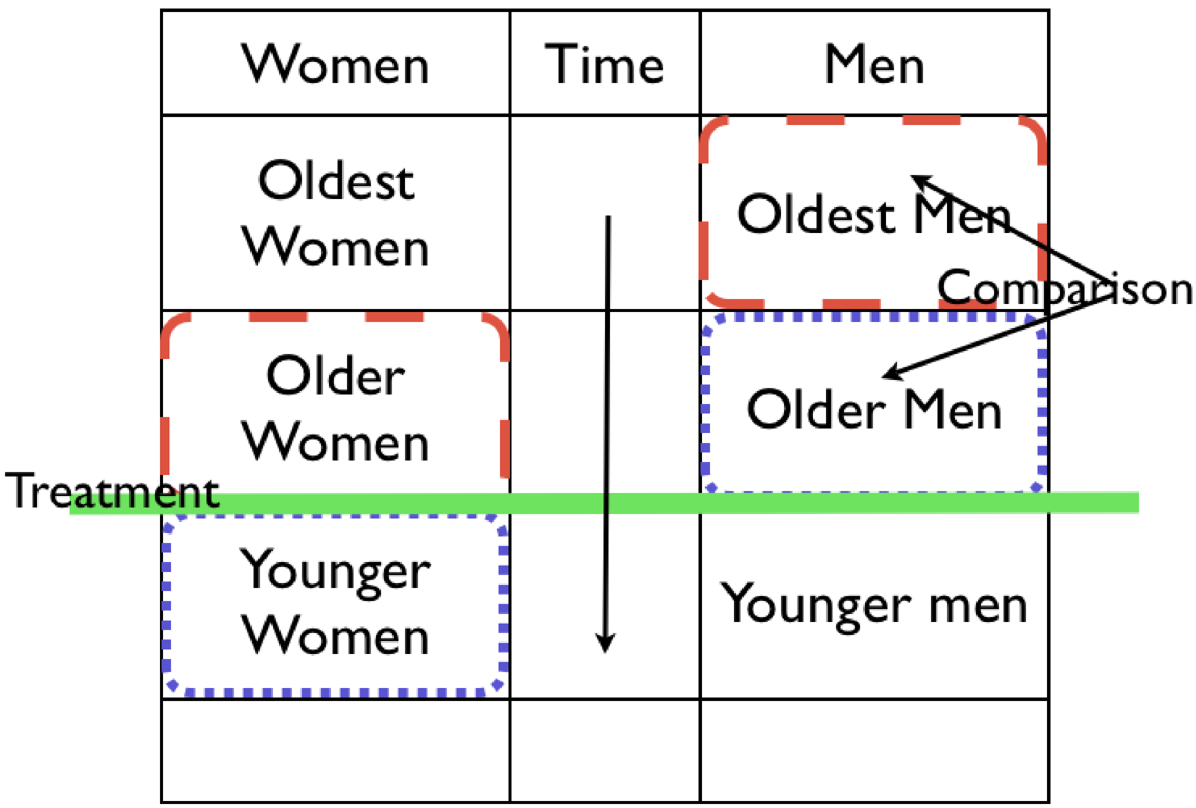

Figure 3.1. Empirical Strategy

My use of state policies to instrument for education means that my results will not suffer from endogeneity in choice of schooling level. Even without my own results, I would be confident in the effect of this instrument on education because of the sizable literature than shows that scholarship aid–generally–and these types of programs–specifically–increase attainments: Van der Klaaw [2002] uses a regression discontinuity approach to find and increase in the probability of a student attending college by 8.6% when there is a $1,000 increase in scholarship money; Kane [1995] finds similar. As far as Broad-Based Merit Aid Programs, the balance of the literature suggests that programs of this nature cause an increase in enrollment (on the extensive margin of college attendees) of anywhere from 2% to 6%,[1] they would have the same impact on enrollment as giving every student in the state access to a $581 scholarship.

Nevertheless, I check the effect myself using years of schooling attainment as my dependent variable; through a differences-in-differences estimation, I find that these programs cause an average increase of more than $\frac{1}{10}$ of a year in the educational attainments of young women in their state.

Once this effect is established, I exploit the fact that 55% of married men are more than 1 year older than their wives.[2] Therefore, I estimate a logit model for marriage over all men, omitting those men who have been exposed to the scholarship programs. While having more education increases the probability of marriage significantly for men, this effect is negated for those men who are in my treatment age cohorts; increasing women's education levels is actually decreasing the marriage rates of men with college degrees.

[1]For the state of Georgia, both [Dynarski, 2003] and [Cornwell et al., 2006] suggest an increase of college enrollees of more than 5% caused by the HOPE scholarship program. The finding of [St. John et al., 2003] for Indiana is roughly the same as is that of [Alee, 2004] for the Florida Bright Futures Program. In addition, there are other studies that find positive effects on year-by-year retention within college for these students, suggesting that these greater matriculation rates translate into greater rates of college completion[St. John et al., 2003].

[2]See table 3.4 for a complete age breakdown of married couples.

Recall that the average woman is now *more* educated than the average man; nevertheless, the average college graduate man still has more education than his prospective spouses.[3] It should therefore be unsurprising that an intervention to women's education would have differential marriage outcome effects on men depending on their educations. Both outcomes are consistent with the same theoretical statement: that the effect on marital surplus of wage differential–gains from exchange–is much stronger than the effect on marital surplus of the like trait preference matching.

An obvious shortcoming of this approach is that I must either assume that there is no change in the membership of the pool of potential marriage partners brought about by this education intervention or else document that change; while the initial results I present do the former, I explore the latter option in section 3.5.2, in which I find that–controlling for age and other factors–the effect on age difference of the scholarship programs is significant, but extremely small. In addition, I will provide other checks for robustness based on state similarity and women's own marriage rates.

Then, I look at the effect of Broad-Based Merit Aid Programs on the women's marriage outcomes, and I show that those are no different. This result suggests that the decline in marriage rates of men with college degrees is due to their own substitution of different spouses rather than delaying marriage or opting out of the market entirely.

Next, I create a continuous and direct measure of spousal exposure to scholarship programs, based on the married couple age distribution that I observe in the data. While this is a more sound measure than the treatment-cohort proxy used in the aforementioned

[3] See table 3.23.

results, it confirms their finding. This measure also allows me to account for the competition of men who have themselves increase their educational attainments in response to the scholarships. Even including a ratio of treated women (demand side) divided by treated men (competition on supply side), I still find the decreased marriage opportunities for men with college degrees. Finally checking the programs on a state-by-state basis allows me to identify a subgroup of states in which the Broad-Based Merit Aid Programs are particularly effective at increasing educational attainment: in those states, the marriage effect is even stronger, which allows me one final check of robustness.

The sum of these items is that an intervention increasing women's education levels will–*ceteris paribus*–cause them to substitute less-educated husbands than without the intervention. The remainder of this paper will proceed according to the following roadmap: In section 3.2 I will provide an intuitive framework for marriage; then I will give background information required to understand the instrument that I use, BBMA programs, and discuss my data source, the CPS October Supplement; next, I present my empirical work according to the plan given above. Finally, I will conclude in section 3.9.

3.2. Background

The past generation has seen dramatic changes in educational attainments: starting from parity in 1972, women have dramatically stepped up their educational attainments to the point that more than 4 women are enrolled in school for every 3 men; except for advanced graduate degrees, women outnumber men at every education level. This has happened during a time when median age at first marriage[4] for women has increased from 20.8 years to 25.3 years.[5]

3.2.1. Literature Review

The relationship between education and marriage has attracted interest from several scholars in the past, and they have written about many different time periods. The best known theoretical prediction is that of Becker [1973], which I discuss in more detail in section 3.2.2; in addition, there is a good deal of empirical work on the topic:

Historians have used the signatures on marriage registries in Victorian England to examine the degree to which literate- and illiterate people exhibit positive assortative mating, finding the stigma for illiteracy to be–at most–“small”[Vincent, 1989, Mitch, 2003].

These gains to marriage, however, could be a sign of same innate earnings potential that is observed by potential mates, but not by econometricians. Xie et al. [2003] use current earnings together with education and experience profiles to generate five separate

[4]There has recently been an increase in interest in studying cohabitation, particularly whether cohabitation is a substitute for singleness, for being married, or meant to be a transition between the two[Oppenheimer, 1997, Xie et al., 2003]. Indications are that financial considerations play a large role in young men's and women's choices to enter cohabitation instead of marriage[Wu, 2000, Kravdal, 1999], and this may account for the finding that cohabitation is more often chosen by less educated people as a substitute for marriage[Thornton et al., 1995]. However, it is not clear to me that these results are disentangled from an age effect; more scholarship here would certainly be helpful.
As far as the degree to which cohabiting couples may appear in the data, there are two types of households that are candidate households: first, households with younger, unmarried members of the opposite sex, and also households with children, as well as two older adult members of opposite sex. In the case of the first type, they would look similar to roommate relationships, while the second case may also be a situation in which a divorce prompts a newly single parent to move back in with other family members. While others might indeed be interested in the effects of education on every phase of the dating process, from first date through cohabitation or engagement to marriage and divorce, first marriage is certainly a significant enough milestone to warrant attention, so it will be the focus of this study.

[5]For a good series on this, see the *Historical Statistics of the United States*, Millennium Edition, Ae482.

measures of earnings potential, as a way of dealing with the latent unobserved earnings capacity. They find that these measures significantly increase the likelihood of marriage for men, but not for women. However, their results are biased upward by the selection effect of education and eventual earnings having to do with this latent unobserved earnings capacity. In addition, they use the *Intergenerational Panel Study of Mothers and Children*, which only interviewed white families.

In addition to the extra income, there are many other reasons educated people might make superior spouses: better health, more interesting conversation, and social status are just a few of these. In this light, it makes more sense to study education directly.

A study showing that educated women are more productive in the home is that of Currie and Moretti [2002] who find that–conditional on being mothers–educated women are more likely to be married and to have healthier babies. While their instrumental variable strategy[6] is a well-documented and sound way of dealing with the endogeneity of education levels, the selection inherent in their data–tabulation of birth certificates–means more direct work on marriage is still needed.

One concern in a literature for which longitudinal data are more necessary than available is making a distinction between the delay of marriage and abandonment of marriage altogether. There is much work supporting the claim that the demands of additional human capital investment early in life do delay marriage, but the extra human capital ultimately makes it easier for these people to find a marriage partner.[Blossfeld and Huinik, 1991, Blossfeld and Jaenichen, 1992, Goldstein and Kenney, 2001] In this paper, I don't have longitudinal data, meaning that I am forced to sidestep this issue by controlling

[6]They use the presence of a college in county of residence as an instrument for educational attainment.

for age and treating age cohorts separately. By using age cohorts in this way, I am able to be agnostic about which phenomenon I am observing in my data, considering only a probability of being married at any particular age. [7]

Lefgren and McIntyre [2006] take up the same question with 1980 US census data; and they show that women's education is highly positively correlated with with their husband's income, with "up to half of the correlation between education and consumption operating through the marriage market". Because they use an IV specification based on quarter of birth of the women, they are confident that their estimates represent a causal relationship.

Accounting for marriage is necessary to produce a truer estimate of the returns-to-schooling. In this vain, Chiappori et al. [2006] present a theoretical model with a joint decision of education together with marriage. In their model, more educated women exploit their labor market outside option to improve the share of surplus they enjoy within marriage; this increases their overall returns-to-education, which is sufficient motivation for their college attendance, even independent of labor market participation. Suqin Ge takes this one step farther, using NLSY data to estimate that more than $\frac{1}{6}$ of women with college degrees would not have gotten them if marital returns to a college degree were zero[Ge, 2007].

[7]Since marriage rates do not increase linearly with age, I generally implement a higher-order polynomial on age, most often a squared term. Results don't change significantly with higher order terms. Bloom and Bennett [1985] use three parameters to describe the distribution of age-at-first-marriage among people: a mean and standard deviation to identify a normal distribution, as well as an overall parameter E representing the probability of ever-getting married; this framework can be identified with panel data over individuals, but I do not have the luxury of panel data, so I must use a conceptualization with fewer parameters. I instead consider waves of people having experienced a transition probability–specific to age and the particular marriage market–each year that is realized by a movement from single status to married status. This conceptualization has no parameter that is the equivalent of E.

These concerns are in line with the findings of Blau et al. [2000], who show that good female labor markets–together with bad male labor markets–tend to lower marriage rates, although these results are much weaker for African Americans. Meanwhile, Choo and Siow [2006] find a plummeting in returns to marriage during the decade of the 1980s, a time when educational attainment was on the rise?

Finally, there is considerable discussion about whether increasing human capital will have equivalent- or even same direction effects on various demographic sub-groups. Anecdotally there is much reason to believe this to be different for men versus women: in a *New York Times* piece, Maureen Dowd talks about alumnae of Harvard Business School doing their utmost to prevent men from learning of their alma mater lest they flee[Dowd, 2005].[8]

Rose [2005] confirms this, saying that–while most über-successful men are happy to marry down to ally forces with a "power behind the throne"–career women would prefer to marry up, only they often have no choice other than a hypogamic match if they want to marry at all. Her data confirm that–when faced with the choice between ignorant men and no men–women are choosing these hypogamic matches in greater numbers than ever before. Despite this, she still finds clear evidence that those men who are at the bottom of the educational totem pole are being left out of the marriage market, although she admits causality to be a major caveat of her study. By using this BBMA Program instrument, I resolve this problem of causality and find evidence that more women making hypogamic matches because their increased education means they can afford to do so.

[8] Colloquially they refer to saying they went to Harvard as "dropping the H-bomb."

Considering that many of the Broad-Based Merit Aid Programs provide scholarships much larger than $581–see table3.22–it might seem that they are torturingly inefficient; if anything, the vast majority of students would not change their college decisions because of these scholarships. However, the 5% increase is still a powerful effect when one considers that it represents an extra student out of every classroom now going to college, and it is well-documented in the literature. I give table 3.1 as a quick synopsis of the literature that has found them to have a positive, significant impact on the college-matriculation rates of students.

It should be noted that there are two major sources of bias in these results: first, the distinction between luring a promising student who would otherwise leave the state back to the farm [9] and helping a promising but timid and disadvantaged student to take a risk and enroll in higher education by reducing the financial uncertainty caused by credit constraints[Heller, 2006].[10]

Although the above authors and others have expressed a lot of concern for this bias, the focus of the literature on matriculation rates into college has caused them to play little attention to a second avenue through which these scholarships affect educational attainment: increased retention. The Cornwell, et al. study does at least contend with

[9] Note that there is a non-trivial strategic component to this: students who leave the state are 54% more likely not to return to that state once they have finished their degrees[Kodrzycki, 2001]. This may explain the pre-dominance of these programs in the South in that there is nothing special culturally about the South, but the early programs of Georgia and Arkansas forced neighboring states such as Florida, Mississippi, and South Carolina to follow suit or else risk losing promising students.

[10] In preparation for this project, I spent thirty hours in the field at a High School Counseling and Career Center observing students in the process of applying for colleges, scholarships, and financial aid. The counselor herself explained to me that selling college to marginal–usually low-SES–students is done most effectively by attacking the more-easily measured cost side rather than explaining the benefit side, i.e. emphasizing “college costs less than you think it does” over “college can increase your wages by this amount.” For a paper that makes a case that credit constraints are not binding, look no farther than [Cameron and Taber, 2004].

Reference	State	Effect
Cornwell, et al. (2006)	Georgia	5.9% increase in aggregate enrollment
Dynarski (2003)	All Southern	4.7% increase in P(College Enrollment)
Conley and Taber (2005)	Georgia – HOPE	95% confidence interval of 1%-8% increase in P(College Enrollment)
Van der Klaauw, et (2002)	general	10% increase in financial aid leads to 8.6% increase in prob(enrollment)
St. John, et al. (2003)	Indiana – Promise	15% - 24% increase in retention after the first year

Table 3.1. Studies that have found that Broad-Based Merit Aid Programs increase educational attainment.

this, but their range of estimates for the retention increase of the Indiana 21^{st} Century Scholars' Program should serve as an upper bound because of the need-based component of that program. While my cross-sectional data are not ideal for looking at retention, I use years of schooling as my dependent variable to allow for more flexibility in seeing this play out. However, the preponderance of the literature is such that I am confident in the impact of Broad-Based Merit Aid Programs even before I carry out my own estimation.

3.2.2. Economic Framework of Marriage

In order to understand my theoretical approach to the marriage market, consider a pool of single men and women–their education already determined–who converge in order to select spouses out of those whom they find suitable. Under Gary Becker's initial two assumptions–that people will maximize their interests and that the marriage market is in equilibrium[Becker, 1973]–any match that is formed will be one in which the surplus generated by the marriage is positive, and there will not be any person who could make a unilateral switch that would make them better off. There are lots of models that have the same flavor as this[Siow, 2006][Chiappori et al., 2006], so–rather than including cumbersome theoretical details–I will focus on those parts that are most relevant to this study.

Assume for purposes of this illustration that the only characteristic that impacts the utility of a match is each spouse's level of education e_i. If a person remains single, then that education exactly determines their wage, and they get e_i. If they form a match, then the additional surplus generated by a match beyond their reservation utilities will take the form

$$S(e_i, e_j) = \tau(|e_i - e_j|) + \omega(|e_i - e_j|) \tag{3.1}$$

I use τ to represent the gain in the surplus from a similarity in education levels (i.e. the positive assortative mating on traits that Becker [1973] identifies. As the difference in education levels of prospective spouses goes down–note that equation 3.1 expresses it as a function of absolute value–this part of the surplus will increase; in other words, τ is monotonic decreasing.

Meanwhile, ω represents the gains-from-trade part of the match, i.e. the surplus that comes from disparity in labor-market-wages. As that disparity increases, the surplus from an arrangement in which one spouse makes labor market investments while the other makes household investments increases, in that the labor-opportunity cost of the household producer goes down. Therefore ω is monotonic and increasing.

To find the effect of an increase in education on marital surplus, I take the partial derivative of equation 3.1 yielding

$$\frac{\partial S}{\partial e_j} = \frac{\partial \tau}{\partial e_j} + \frac{\partial \omega}{\partial e_j} \tag{3.2}$$

Assuming that $e_i > e_j$, $\frac{\partial \tau}{\partial e_j} < 0$ and $\frac{\partial \omega}{\partial e_j} > 0$, so the sign of the whole derivative will depend on the magnitudes of these two effects. If τ–the trait effect–is stronger, then $\frac{\partial S}{\partial e_j} < 0$, while the reverse will be true if ω is greater in magnitude.

While I conceptualize the surplus in this way, I cannot observe the surplus, so I take an increase in surplus as signifying an increase in the probability of marriage, i.e. new marriages are formed when $\frac{\partial S}{\partial e_j} > 0$. A listing of the interpretations of all possibilities is given in table 3.2.

		Marriages Increase	Marriages Decrease
Education	high	Trait effect τ	Wage Effect ω
of man	low	Wage Effect ω	Trait effect τ

Table 3.2. Interpretations of different marriage results given an increase in women's education for high- and low-education men. Effect given is the one that is implied to be stronger by the given change in marriage rates.

The surprising empirical fact is that–unlike in the generation in which Becker formulated his model–the higher educated of the pair is more likely to be the woman! We have just completed a century of amazing change in which each twentieth century cohort of women has emphasized work more, until in the last generation it shifted from being merely a secondary source of family income to being a career. That these changes would necessitate greater investment in schooling to prepare for the demands of a true career is unsurprising, but it would certainly seem strange to their grandmothers in the 1950's that today's women are actually attending- and graduating from college in greater numbers than men[Goldin, 2004, Jacob, 2002]. This great investment in schooling on the part of women has coincided with a delay in the beginning of their family responsibilities. Their average age at first marriage has increased by 4.3 years to 25.1, and there is no indication of its being reduced anytime soon. This delay coincides with a reduction in fertility: the average woman at the end of her childbearing years now has 1.9 children, compared to 3.0 children in 1980 (facts are from US Census website, available at `http://www.census.gov/Press-Release/www/2003/cb03ff03.html`). For men, 36 year old men in 2000 had the same marriage rates as 30 year olds did in 1980. In table 3.23 I give some tabulations of men and women by education to show just how great this disparity is. In the first two columns, one can compare the aggregate numbers of men and women at each level of schooling attainment.[11]

While comparing education levels of men and women in the same cohort is striking, the relevant comparison is actually between men and their prospective marriage partners. Of great importance is that men are marrying younger women, who (1) will gain more education eventually than their peers who are the same age as the men, and (2) have not had as much time to accumulate that education at the time that I observe them. To aid

[11]This is measured in years. Note that these tabulations are not made using the CPS weights; since my eventual marriage regressions will be unweighted, this should not be troubling, as I have a lot of covariates that I use.

in this comparisons, I look at linked married couples in columns 3-5 of table 3.23 . While men with advanced degrees ($educ \geq 17$) are still more educated than their spouses, men of all other levels are more likely to have a wife who has more schooling than they do.

In fact, in the remaining columns of the table, I compare mens' education levels to the constructed average education level of all prospective spouses (details of how I construct this are in section 3.8). Even when compared to a weighted average of prospective mates, more women than men are expecting to "marry down" when it comes to education.

Given that women are–on average–the higher educated spouse, I expect that an increase in education of the woman out of a particular couple would cause τ to decrease, while ω will increase, i.e. an increase in education coupled with a decrease in marriages indicates the importance of trait-based assortative mating. In this way, seeing what happens to marriage rates allows me to observe which effect is stronger according to the schedule in table 3.2.

Note that highly educated men are still more likely to "marry down"; this means that it is very likely for the effects on τ and ω to be reversed and there to be a different result even with the same surplus functions in place. In this paper, I find that the marriage rates of men with college degrees are significantly reduced, indicating the effect of ω–the wage difference effect–to be the stronger.[12]

On the other hand, for less-educated men, this should not hold up: since the trait similarity would be worsened, but the surplus from wage differential improved, these men being married less would indicate the trait effect to be more important. However, I find

[12] In my dataset the unweighted LFP of women age 18 and over is $\frac{665{,}857}{1{,}102{,}759} = 0.603$, while the LFP for men of the same ages is $\frac{756{,}358}{985{,}047} = 0.768$. A more in-depth look at LFP might be a good way to look at the relative importance of these effects.

that the most educated men are also the most likely to see a decrease in marriage rates when faced with women who have higher levels of education. This supports my claim that ω has a larger magnitude than τ.

3.2.3. Design

Since the key to testing for this effect is my use of State-Level Broad-Based Merit Aid Programs as–effectively–an instrumental variable for the education level of women,I will give some background information about these types of programs. The current generation has seen an enormous increase in the amount of financial aid provided for college students to attend school. The Broad-Based Merit Aid Scholarship Programs that I employ are part of this movement that took place throughout the past thirty years. Unfortunately, each BBMA program is idiosyncratic to its particular state. I am unable to give a single definition, but most of these programs do share the following characteristics:

Characteristic 1. *provides large scholarships to students. These are typically on the order of $1,000 or more per semester, up to full tuition plus some extra expenses at a state university.*

Characteristic 2. *targets a broad portion of high school graduates, based mainly on merit. These are typically students who earn a 'B' average or better in high school, ranging from 25% to 60% of high school graduates [Dynarski, 2004].*

Characteristic 3. *requires that students attend a state public institution in their home state to receive the money.*

Characteristic 4. *requires continued evidence of high performance in college through a minimum gpa requirement in order to provide continued funding.*

Characteristic 5. *is financed through a state lottery or other type of "sin-tax."*

Twenty-two states currently have programs that are of this ilk, and I present a list of these programs in table 3.22 in my appendix. While the chronological pioneer may have been the Indiana 21^{st} Century Scholars Program, the archetype is Georgia's HOPE Scholarship, passed in 1993, which exhibits all of the above characteristics. More thorough histories of this program can be found in several references mentioned below[Dynarski, 2003, Cornwell et al., 2006].

Some justification should be required in claiming that these programs are exogenous. I have investigated whether demographics or urban influence are significant predictors of a program: they are not. In addition, trends on overall education are not either. The reader may wonder what predicts these programs, particularly because the balance of them are in the South. The correlation between education and migration suggests that there is definitely a motivation for colleges to keep their home-grown sages. Ionescu and Polgreen [2008] define the "the skill premium" as the ratio of skilled to unskilled wages. States who see this ratio rise with the ratio of skilled over unskilled labor hours are said to exhibit increasing returns to scale; Florida is one such state. States for whom the opposite happens–an example is New York–exhibit decreasing returns to scale. Ionescu and Polgreen [2008] point out that increasing returns to scale states have an incentive to increase scholarship aid to their students, as they can expect the increase in high skilled workers to be able to find jobs and pay taxes within their states.

More interestingly, however, is the interaction between these states and their neighbors: when a decreasing returns-to-scale "state such as New York" increases its educational spending, it is a best response even of states like "Florida" to increase theirs as well in order to maintain the outflow of human capital from New York to Florida. This is certainly true in the case of bordering states.[13]

3.2.4. The Data

Data for this study are US Census CPS data available from IPUMS. I use the October Supplement for years 1989-2006, giving me six additional years after the close of the datasets used in previous studies[Dynarski, 2003, Conley and Taber, 2005]. I can see education, marriage, age, employment data and demographic characteristics. Limited migration data are also available, which I will exploit in section 3.6[14].

The strength of my data is that I have almost 3 million observations from a representative sample of the United States. I have eighteen cohorts of data with only minimal changes in coding. The weaknesses include not having a true panel, only repeated cross-sections, as well as being unable to observe parental characteristics, foremost of which is parental education.

[13]Although Ionescu and Polgreen are quick to point out that the migration flow between non-contiguous New York and Florida is actually the highest of any state pair.

[14]These data would be of interest to most labor economists if one were to look at whether differently-educated people tended to relocate more frequently or less-. In addition to the obvious relaxation of my assumption that everyone is still living in the state in which they attended high school–tested and found reasonable by Dynarski [2004] who claims that college students are listed in the state of residence of their parents–data on mobility would be useful in identifying possible strategic relocation into a state to exploit the BBMA program for college funding. Finally, they would prove useful in distinguishing between student "creation" and student "redirection" to borrow the vocabulary of international trade. Finally, the use of a sliding scale to award differential scholarship amounts for time of residency–as is practiced by the Kalamazoo PROMISE scholarship–offers the potential of a regression discontinuity study of the effects of additional scholarship aid on educational attainment. While census data includes a state of birth, CPS only includes the country of birth, with a general "United States" code given to all domestic-born people

For this reason, I must make the assumption that my subjects still live in the state in which they attended high school, as well as assume that they all graduate from high school (conditional on attaining at least this level of education) in the year in which they are 18 years old. In addition, there is discussion in the literature about the need to distinguish between diversion and creation of college students; [Cornwell et al., 2006] failing to account for the decrease in enrollments of neighboring states in cases when students have been kept "down on the farm" would cause estimates of effect size to be biased upwards.

In the year 2007, the oldest cohort that had been exposed to broad-based merit aid programs was 35 years old (in Indiana), so using an age range from 18 all the way into the mid-30's or beyond gives me a good mix of treated- and untreated observations, while covering up to an age–36–by which eighty percent of men have already been married even in the marriage-poor years near the end of the twentieth century. Additional details of how I managed, cleaned, and used the data can be found in my data appendix, section 3.11.

3.3. Impact of Broad-Based Merit Aid Programs on educational attainment

In this section, I verify the results of previous work that shows that Broad-Based Merit Aid programs increase the educational attainment of men and women in the states in which they are passed. I will endeavor to compare my own general empirical results to the substantial literature that has appeared that shows that they do. First, I repeat the estimations for the impact of merit scholarships on educational attainment, finding a positive and significant effect. In a later section, I examine the many program states separately to identify a smaller subsample of states whose programs are particularly effective; this subsample is used for robustness checks below.

3.3.1. Description of Method

In order to assess the effects of these laws, I employ a differences-in-differences framework. I realize that this approach may have shortcomings;[15] in accordance with the recommendations of prior authors, I will take care to calculate Huber-White standard errors, as well as employing State-Fixed-Effects Model to account for unobservable differences that vary systematically by state.

In my estimation of the effect of the merit scholarships on educational attainment, it is a useful exercise to first imagine the ideal experiment: Consider an average seventeen-year-old at the start of his- or her senior year in high school. While it is true that the median high school graduate does attend college now, were there only a single normally distributed parameter describing aptitude, the median student would differ only slightly from the student on the threshold between attending college and not-.[16] The perfect experiment would be to take two identical copies of that student and place each in a different state, one of which–determined randomly–could then pass a BBMA program law. Finally, I could observe the subsequent educational attainments of each.

[15]See objections to the diff-in-diff method raised in Bertrand et al. [2002] and Conley and Taber [2005]. I have answered these by clustering standard errors according to state in all estimations.

[16]This reasoning is based on the generous figure of 80% of students attending some form of college, together with the first standard deviation below the mean being at the 16^{th} percentile of the normal distribution.

While the two main differences between this experiment and my study are obvious–that the students are not identical, and that the states' passage of merit aid programs is not random–this mental experiment does serve to clarify who my control group must be: those students who are of similar characteristics (and I can control for those), but in states that do not pass BBMA program laws. By this reasoning, then, a suitable empirical framework for determining a treatment effect is differences-in-differences . Fortunately, the passage of laws at different times by different states gives me a rich set of data to use for this estimation.

In addition to observing states, some of which are implementing policies in between the years of data that I have, I also have the benefit of observing age cohorts.[17] Sorting into cohorts defined by state and age gives me exact identification of whether a person has been exposed to a BBMA program; this means that I can use a differences-in-differences specification when I estimate a regression model for individual educational attainment y–an indicator of at least some college education being attained–of the form:

$$y_i = \alpha \cdot Merit_{s(i),t(i)} + \theta_{s(i)} + \gamma_{t(i)} + +\beta \cdot X_i + \varepsilon_i \tag{3.3}$$

In this equation, the subscript i denotes people; hence $s(i)$ is the state in which a particular person resides, and $t(i)$ refers to the year at which that person is observed; θ_s

[17] I am using these as an imperfect reading of cohorts in school, as I observe age, not grade level or birthday; this means that I must assume that all high school students graduate in the year in which they are eighteen years old. It is a frequent occurrence in these data that many 18-year-olds are still in high school; I treat these students as having matriculated into college already for the purposes of this regression, as this is true for the balance of them.

represents a state fixed effect; γ_t is a time effect; and X represents demographic characteristics, in this case ethnicity/race.[18] The effect of interest–how educational attainment is affected by exposure to Broad-Based Merit Aid Programs–will be the coefficient on the $Merit$ variable, which is a dummy for turning 18 either during- or after the year in which someone's state implemented the program. I take this is a proxy for exposure to a Broad-Based Merit Aid Program.

3.3.2. Results

In table 3.5, I present the estimation results for educational attainment over several different sub-samples of my data. In column one, I give results for all people aged 18-36; in column two, I restrict this sample to only women, and in column three only to African American women. As is pointed out by Dynarski [2004], the CPS data do not contain any indicators of academic performance within secondary school. This means that the program effects that I estimate are a product of two unknowns, the "behavioral response" of potential students to the scholarship, and the share of students who are eligible. The estimates in table 3.5 do not distinguish between these. Note that–for those students who do take up the scholarships–the effects are potentially great, while for many students who never attend college, they are zero.

My results confirm the very important finding that the merit programs increase educational attainments. Most importantly for later, women are seeing a significant increase in education. Men are, too, even though as a group they attain less schooling than do women.

[18] As explained in more detail in the appendix, I use dummies for the principal minority races–Asian and African American–as well as an exclusive dummy for being of hispanic origin.

Sample	All People	All Women	Black Women
merit	0.0703***	0.0759***	0.0668***
	(0.0060)	(0.0073)	(0.0109)
male	-0.0405***	–	–
	(0.0014)		
black	-0.1199***	-0.1219***	–
	(0.0034)	(0.0038)	
hispanic	-0.2951***	-0.2845***	–
	(0.0062)	(0.0067)	
asian	0.0551***	0.0409***	–
	(0.0062)	(0.0062)	
Sample Size	781218	404047	45603

Table 3.3. Estimated effect of merit scholarship programs on college matriculation. Huber White Standard Errors–clustered by state–are given in parentheses.

Unfortunately, these programs are not a homogenous class of program, but rather a varied phenomenon with origins in the idiosyncratic political jockeying within each state. For this reason, I think it prudent to carry out a more detailed examination of the programs, both in terms of their characteristics and their strength; this exploration is carried out in section 3.7.

3.4. Impact of BBMA Programs on Marriage

Next, I estimate the difference in marriage outcomes for those men whose pools of marriage prospects have had their education levels increased according to the effects shown in section 3.3.2. While I use a very similar framework to the educational attainment estimation developed above, I will first briefly remind the reader of my empirical strategy. Then, I will use the joint age distribution of married couples as observed in my data to identify the cohorts of men–in years relative to the passage of the Broad-Based Merit Aid programs–that are most suited to my identification strategy. My main finding will be that the increase in women's education is causing a shift in marriage patterns: less-educated men are more likely to be married, while men with college degrees are less likely.

Recalling that my treatment here provides a shock to the education levels of one's prospectve spouses, I compare the two groups of men indicated in figure 3.1: those who only barely missed out on exposure to the Broad-Based Merit Aid programs passed by their state, and those who are significantly older. In order to properly sort between these two groups, I look at the exact age-distributions of husbands versus wives. While in section 3.4 I am assuming that the age difference between men and women who marry stays constant in those states that implement programs, I will check the veracity of that assumption later in section 3.5.2.

3.4.1. Identifying Cohorts of Interest

First of all, I assume that each state constitutes a distinct marriage market. While this may seem to be a strong assumption, in most cases it does hold.[19] In addition to the state, relative ages determine the degree to which certain men and women interact. While–generally speaking–cohorts of men can be sorted between the three categories given in figure 3.1, my aim here is to provide exact corresponding age cohorts for further analysis:

In order to do so, I look at the age distribution of all linked married couples in table 3.4. The data confirm that most men marry younger women. In particular, almost 20% of couples feature a man who is at least five years older than his wife, only only 3.6% of couples have a wife as much older than her husband. It becomes clear that the median husband is between 2 and 3 years older than his wife, and 35% of husbands are between

[19] Charles and Luoh [2003] find that more than 60% of wives marry a husband born in the same state as they were; I have my own examination of this in section 3.6.

2 and 5 years older. Based on these data, it is safe to assert that women entering the marriage market are more likely to wind up with older men than not; for this reason, the treatment cohorts[20] of interest for male marriage rate will be those cohorts -5 through -1, i.e. men within the five years preceding the passage of a scholarship program. Note that none of these men are directly exposed to the BBMA program, so any effect on their marriage outcomes because of additional education would be moderated through the women's educational attainments.

3.4.2. Marriage Estimation Model

In order to determine the relationship between marriage outcomes and this exposure to educated women, I estimate a logit model for marriage over all men i as follows:

$$y_i^\star = \beta T_{t(i),s(i)} + \alpha A_i + \psi T_{t(i),s(i)} \times A_i + \theta_{s(i)} + \gamma_{t(i)} + \delta P(Age_i) + \zeta X_i + \xi Educ_i + \eta LFP_i + \varepsilon \tag{3.4}$$

In this equation, $y_i^\star$ is an unobservable latent marriage parameter specific to each man. All that is observable to the econometrician is an indicator having value 0 when a man is single–equivalent to $y_i^\star < 0$–and 1 for any other marital status, which is equivalent

[20] *Treatment Cohort* is my short-hand for how a person's age places them in relationship to the timing of the Broad-Based Merit Aid Scholarship Program Implementation. A treatment cohort is essentially the number of years that a person turned eighteen after the law was passed, but I explain fully the construction of the dummies for treatment cohort TC_i here:
If a state did not pass a BBMAP law during my sample period (from 1989-2006), then people in that state are not in any treatment cohort. If a state did pass such a law in year j, then a person born in year k will be in Treatment Cohort $TC = (j - k) - 18$.
In essence, this means that the students who are 18 when the law is passed are in cohort zero, with negative numbers indicating how many years *before* the law a person turned 18, and with positive numbers indicating how many years *after* the law a person turned 18. Recall that I observe age, not birth year, so I must define it as $TC = (yr - j) - (Age - 18)$.

	Cumulative % of All Married Couples 1990	1995	2000	2005
AGE DIFFERENCE:				
Husband 6 years older than wife	0.845	0.850	0.848	0.853
Husband 5 years older than wife	0.800	0.804	0.804	0.807
Husband 4 years older than wife	0.736	0.745	0.744	0.745
Husband 3 years older than wife	0.655	0.666	0.666	0.671
Husband 2 years older than wife	0.552	0.561	0.563	0.576
Husband 1 years older than wife	0.427	0.440	0.441	0.465
Husband and wife same age	0.290	0.307	0.307	0.336
Wife 1 years older than husband	0.171	0.184	0.189	0.211
Wife 2 years older than husband	0.110	0.119	0.123	0.141
Wife 3 years older than husband	0.077	0.084	0.085	0.101
Wife 4 years older than husband	0.540	0.060	0.063	0.072
Wife 5 years older than husband	0.040	0.044	0.044	0.054

Table 3.4. Cumulative Percentages of age differences between spouses in married couples; source CPS data 1989-2007.

to $y_i^\star > 0$. This means that positive coefficients indicate factors that contribute *toward* marriage.

As in the educational attainment estimations, X represents demographic characteristics, such as race; θ_s represents a state fixed effect; and γ_t is a time effect. $P(age)$ represents a polynomial on age.[21] In addition, an education term measured in years has been added to this equation so that I may now control for the income signal that men give to prospective mates when seeking a marriage match; using their labor force participation also helps in this respect as well.

Finally, T is a dummy variable for whether a man is in the treatment cohorts -5 through -1 of interest delineated above, i.e. he graduated from high school in the five

[21]The estimates presented here once again use a squared term for age. While it would be nice to include income explicitly, the causal effect of marriage on the observable income of married men would create endogeneity issues. This concern may still apply to Labor Force Participation, as well.

years before a Broad-Based Merit Aid Program was implemented, and therefore is seeking marriage with women who were affected by the Broad-Based Merit Aid Programs. Note that I estimate this equation over only men who are not themselves exposed to the BBMA Programs, thus removing the younger men only from states that do implement. I count on controlling for age to protect me from any issues that may arise in this, as this removal of men in the treatment states is necessary to preserve the comparison that I indicated in figure 3.1. To test the effect on men who have college degrees, I include the main effect α and interaction ψ parameters.

3.4.3. Results

Estimation results are presented in table **??**. For the sake of comparison, column one gives the cross-sectional effect of education on marriage in a logit specification. Column two gives the specification in I describe above, in which the coefficients on T and its interactions represent the effects of interest. Column three repeats this estimation over the restricted subset of states that implement a Broad-Based Merit Aid Program at some point during the time period.

While most of my predictors are significant in columns 1 and 3, the coefficient on *any_degree* is not significantly different from zero for either of the groups once education–in number of years–is controlled for. For those men who correspond to women who've been exposed to scholarships, however, wedding bells are ringing: they are 2% more likely to be married; when interacting this with education, however, the gains in marriage are being enjoyed entirely by the men without college degrees: having a college degree more than offsets this effect.

Recall equation 3.2, in which I decompose marital surplus into the components that come from trait matching τ and gains-from-exchange ω. For educated men–who are more educated than their average prospective mates[22]–a decrease in marriage rates caused by increasing women's education is driven by a decrease in the gains-to-exchange part of the surplus. Meanwhile, for less-educated men, the positive estimate of T points to the same conclusion, as already-better educated women are widening the wage differential gap even more.

Standard errors that are given are Huber-White standard errors that come from the state groupings because the treatments–scholarship programs–are implemented at the state level. The marriage penalty to older treatment men with college degrees is statistically significant and negative.

Repeating the estimation, except that I am narrowing the sample to only those states that eventually pass BBMA Programs. Because the 22 states that do implement Broad-Based Merit Aid Programs do so at different times throughout the eighteen years of my data, I can still effectively implement a differences-in-differences estimation over this subsample by exploiting the differential timing of cohorts. I present these estimation results, and they are similar to those calculated over the whole sample.

The marriage rates of degreed men are decreasing, but why? Recalling my earlier economic framework from section 3.2.2, that the higher-educated men would see a different impact on their marriage behavior than others is unsurprising. Many men with a college degree are in an environment where their mean prospective spouse is less-educated than they are; since their marriage rates are decreasing, this means that the wage effect ω of

[22]See table 3.23.

the narrowing of their wages with those of prospective wives is overpowering the trait preference effect τ as women's educations approach their own. But if this mechanism is biting, then how can I account for the women?

Sample	All People	Older Men	Men in Changing States
age	0.4539***	0.4756***	0.4834***
	(0.0233)	(0.0276)	(0.0385)
age square	-0.0059***	-0.0059***	-0.0061***
	(0.0003)	(0.0004)	(0.0006)
indicator for any college degree	0.0148	0.0221	0.0217
	(0.0383)	(0.0414)	(0.0558)
years of education	0.0490***	0.0488***	0.0439*
	(0.0128)	(0.0127)	(0.0197)
male	-1.7536***		
	(0.0452)		
labor force participation	1.1554***	1.1827***	1.1932***
	(0.0317)	(0.0337)	(0.0474)
labor force/woman interaction	-1.7538***		
	(0.0438)		
Treatment Indicator		0.1153**	0.0865
		(0.0396)	(0.0475)
Treatment/degree interaction		-0.2316**	-0.1999**
		(0.0765)	(0.0592)
constant	-7.7878***	-10.1412***	-10.3384***
	(0.4333)	(0.5251)	(0.7344)
Sample Size	628335	299572	130502

Table 3.5. Logit estimation: coefficients for impact of education on probability of marriage; dependent variable is indicator for married status. First column is regression over all people; second column uses design for older men; third column restricts sample to men in states that implement a program.

3.5. What about the Women?

In this section, I explore two possible explanations: first, that women affected by the treatment are now marrying younger men–particularly younger men whose education

Sample	All People	Older Men	Men in Changing States
age	0.1044***	0.0977***	0.0992***
	(0.0040)	(0.0033)	(0.0047)
age squared	-0.0013***	-0.0012***	-0.0012***
	(0.0001)	(0.0000)	(0.0001)
indicator for any college degree	0.0034	0.0045	0.0045
	(0.0088)	(0.0086)	(0.0115)
years of education	0.0113***	0.0100***	0.0090*
	(0.0029)	(0.0026)	(0.0039)
male	-0.3842***		
	(0.0105)		
labor force participation	0.2656***	0.2430***	0.2449***
	(0.0086)	(0.0095)	(0.0126)
labor force/woman interaction	-0.4032***		
	(0.0130)		
Treatment Indicator		0.0242**	0.0180
		(0.0082)	(0.0097)
Treatment/degree interaction		-0.0452**	-0.0393***
		(0.0144)	(0.0112)
Sample Size	628335	299572	130502

Table 3.6. Logit models: marginal effects for impact of education on probability of marriage. First column is regression over all people; second column uses design for older men; third column restricts sample to men in states that implement a program.

levels have been enhanced by the scholarship programs–and, second, that women are opting out of the marriage market altogether. I rule out both, leaving the only remaining possibility being that their increase in education is enabling women to substitute from more-educated husbands to less-educated ones.

3.5.1. Are Women Opting out of Marriage?

One concern is that changing educational forces might change the composition of women who are in the marriage pool. An alternative explanation for the above result would be that increasing education allows those women who might appeal to degree-holding me to stay in school longer, delaying marriage. This alternative explanation is consistent with the findings of Blau et al. [2000] that improving labor market conditions of women leads to a decrease in the marriage rates of women. However, I can use the scholarship programs as an exogenous source of variation in education, which is something that they do not have; a second difference between their study and this one is their focus on younger women.

To check the latter, I estimate the equivalent marriage equation that I did for men. However, instead of my treatment T, I now use the *merit* variable, allowing me to test for the direct effect of exposure to a Broad-Based Merit Aid Program on women's propensity to marry. Huber-White standard errors are calculated accounting for the implementation of policies at the state level.

	DV: Is Married
merit	0.0121
	(0.0378)
education in years	0.0072**
	(0.0024)
age	0.7689***
	(0.0306)
age squared	-0.0110***
	(0.0005)
labor force participation	-0.5900***
	(0.0368)
constant	-12.3028***
	(0.5183)
sample size	273788

Table 3.7. Marriage estimation results for effect of scholarship exposure on marriage rates; sample includes all women ages 23-37.

The coefficient on *merit* is very small and not significantly different from zero; women exposed to the laws are marrying no more often than before. No decrease in marriage would imply that women are substituting different husbands rather than opting out of the marriage market altogether. Note that I control for age, meaning that delaying of marriage would look equivalent to opting out of marriage with these cross-sectional data. This evidence is therefore equivalent to evidence that neither effect happens.

3.5.2. Do Women Marry Different-Age Spouses?

My empirical strategy exploits the difference in ages between husbands and wives. In this section, I check the magnitude of the change of this caused by an increase in education. In addition to validating my instrument, this can help identify a mechanism for how college-degreed men's marriage rates decline. If women are substituting in younger men whom they meet in college, then there should be evidence that women who marry are seeing less of an age difference between them and their spouses.

In order to check the age differences of spouses, I first properly match them through the CPS data spousal links.[23] This allows me to then report an age and education of spouse for each person who is married.

In table 3.8 I give the average age differences between members of a couple, grouped by education of each spouse. I sort the couples based on whether the woman was originally in the treatment- or control groups as diagrammed above. It is clear that assortative matching by education is the norm, as the diagonal (same-education) cells have the most

[23]For each household, this requires matching within household via the SPLOC variable.This is made easier by the fact that–with only one exception in the eighteen years of my data–spouses are adjacent to each other; I can follow a nearly identical procedure for spousal education and income, and even race if I should want to do so.

	husband's education					
	$< HS$	HS	Some College	BA Degree	Grad Degree	Total
Education of All Women						
$< HS$	2.94	2.48	2.40	2.69	3.10	2.75
	48917	22651	11038	6648	3037	92291
HS	3.02	2.35	2.47	2.75	3.22	2.55
	30443	91836	34602	15779	5358	178018
Some College	2.86	2.29	2.21	2.61	2.95	2.43
	11786	30928	42598	19405	8757	113474
BA Degree	2.34	2.37	2.04	2.01	2.52	2.20
	4547	11366	15070	37458	20449	88890
Grad Degree	2.23	2.56	2.06	1.86	2.40	2.22
	1480	3102	4312	10758	18264	37916
Education of Treatment Women	husband's education					
$< HS$	4.68	4.85	4.49	8.36	3.85	4.83
	723	406	155	44	7	1335
HS	4.03	3.35	4.02	5.92	12.69	3.79
	346	1032	347	85	13	1823
Some College	3.1	3.09	3.08	5.33	5.84	3.36
	150	436	601	137	25	1349
BA Degree	2.87	3.17	2.30	3.24	4.16	3.10
	39	99	143	308	71	660
Grad Degree	-2.66	3.9	1.83	3.42	2.21	2.77
	03	20	12	47	46	128

Table 3.8. Mean age difference within married couples conditional on education; mean age difference given is husband's age minus wife's age; data are unweighted. n underneath.

total people within their respective rows and columns. This is true both in couples with treated- and untreated women.

Women with graduate degrees have the smallest age gaps with their husbands, while men with graduate degrees have the largest. In addition, there appear large variations in the mean age differences within cells conditional on whether the women have received the treatment or not. This by itself is not conclusive evidence that the treatment has affected marriage patterns of women, as treated women tend to be younger. In order to build in controls for age, I estimate a regression model with age of husband as the dependent variable, using the coefficient for the *merit* variable as my test for whether women affected by the scholarship are marrying men whose age differs from their own in a systematic way. I restrict the analysis to only women. Results are given in table 3.9

	Husband Age	
	Women ages 26-49	ages 26-36
education in years	-0.1032***	-0.1511***
	(0.0098)	(0.0164)
merit	0.2008	0.1419
	(0.2029)	(0.1928)
labor force participation	-0.2038***	-0.2912***
	(0.0316)	(0.0427)
N	273312	102935

Table 3.9. Regression model for husband age minus wife age; in addition to the coefficients reported above, race, time, and state dummies are used, and a polynomial on age is also included among controls; Huber-White standard errors are grouped by state.

I give the estimations for two groups of women, the first large enough to encompass all women of potentially marriageable age, and the second focusing on a younger subset to roughly include only those women who are young enough to compare to those receiving the scholarships.

First, the coefficient on *merit* is not significantly different from zero. At first glance it might seem large, but–considering how I use the age difference in my empirics and the skewness of the husband-wife age matching–it's not terrible: shifting all men to be younger by 0.2 years would mean reducing the percentage of marriages in which the man is older from 66% to 63%.

Of equal importance, however, is the negative coefficient on education: while it is significant and negative–indicating that more educated women marry younger husbands, it is small enough that I am not concerned about the education effect from the scholarship programs changing marriage distributions.[24] Women who get married under the treatment are–on average–not marrying men who are of a different age than those being married by the corresponding control women.

Another way to check for women substituting in younger husbands would be to carry out a regression for men's marriage in which men who were exposed to the treatment are also included. While it would be possible to use the coefficient on the *merit* variable as evidence for the marriage rates, this would be problematic because of the delaying effect of the additional education that can be seen above when I estimate for men ages 18-26. This delay effect would bias my estimates downward and make it very difficult to infer whether there was a marriage substitution on the part of women toward younger men.

As a result of this section, however, I rule out that women are opting out of marriage all-together, as well as that they are marrying men of a different age. The culprits for stealing the brides from the more educated men can only be their less-educated peers, who–once women's earning power has increased from their education–do not suffer a disadvantage in the marriage market from their small wallets.

[24]The Broad-Based Merit Aid programs caused an increase of 0.1 years of educational attainment for women. Multiplying this by the -0.15 coefficient in table 3.9 gives a change in husband's age of 0.15 years, which is less than two months.

3.6. Migration Issues

All of the preceding analysis has assumed that state of current residence is identical to the state in which a person went to high school. This is clearly an oversimplification. In this section, I address the many issues surrounding migration; ultimately, I have little ability to ensure that migration is not playing a role in my results, but the evidence that is available suggests that there is little cause to worry. Some issues that might be of concern:

- The state of observation may be an imperfect proxy for application of the treatment to a subject
- states are an unreasonable approximation for an exogenously determined marriage market
  - married couples are observed in their *ex post* state: unlike age, which cannot be changed, I can only observe where they are living after their marriage, not before[25]
  - singles may respond to changes in the available marriage-ready populations of state

There are some additional simultaneity issues that are possible: older men, anticipating or responding to changes in the marriage market (one cause of these changes could be these very scholarship programs), may move into the state; also, young women may pick their schools based not only on the direct return in terms of human capital, but

[25]An exception to this are those couples who have been married less than five years, and for whom the *migsta*5 variable is available. While identifying which couples have been married for this brief length of time is not explicitly allowed by the data, an upper bound on the percent of them who come from the same state would be obtained by assuming this variable represents their state of origin from when they were single.

also accounting for a change in returns in the marriage market. Evidence and discussion of this phenomenon can be found in the work of Jeanne Lafortune [2007] for men; for women, however, the effect in terms of years of education is small enough that there is no reason to worry.

All of these considerations mean that I must worry about testing for educational- and marriage changes on the proper population. Dynarski [2003] maintains that out-of-state college students are still listed in their home states. She tests only for the college-going rates of 18/19 year-olds (as do Conley and Taber [2005]), so this is less of a concern for her than it is for this study. Nevertheless, there exists a definite tradeoff between the age of the person and the quality of the signal about their state of origin. If there is an outflow of people post-education and pre-marriage from a state that implements a Broad-Based Merit Aid Program, then the true effects would be higher than those estimated, i.e. estimates would be biased downwards, with the bias increasing as the true effect of education on marriage increased.

First, I attempt to set an upper bound on the migration problem. Here, I do this by exploiting the availability of some migration data for a subset of existing respondents[26], in order to check the extent to which young husbands and wives originate in different states.

I show in table 3.6 that most marriages involve a man and a woman who lived in the same state five years previously. One caveat with this table is that I cannot observe if the couple were married five years previously, so I must use age as a proxy for this. Nevertheless, even the vast majority of married couples with a husband aged less than 27 were living in the same state five years previously.

[26]This is accomplished through the use of the *migsta*5 variable–only available in years 1995 and 2005–which gives a previous state of residence five years before the completing of the survey.

husband age	% living in same state	# married couples
18	0.716	100,661
19	0.783	276,317
20	0.819	442,146
21	0.727	635,260
22	0.857	918,021
23	0.836	1,113,891
24	0.874	1,416,798
25	0.890	1,676,078
26	0.900	1,813,273
27	0.917	1,907,102
28	0.918	2,131,389
29	0.919	2,270,372
30	0.928	2,546,039

Table 3.10. Fractions of a husbands and wives having lived in the same state five years previously, sorted by age and reported for individuals; note that data on this are only available for the 1995 and 2005 respondents.

The increase in probabilities over the decade of the twenties reflects two forces: first, the decreasing mobility of people with age, and second, the tendency of those who are older to have bee married longer. Without data on duration of marriage, distinguishing between these two effects is not possible. Nevertheless, for those men ages 27 and up, the chances that they were living in the same state as their spouse are overwhelmingly high; there is little need to consider cross-state mobility when evaluating the results of this study.

3.7. Refining the pool of States

3.7.1. State-By-State Effects of the Aid Programs

Despite my careful mention of their heterogeneity in section 3.2.3–up until this point I treat the Broad-Based Merit Aid Programs as homogenous instruments. In this section, I will relax this assumption by carrying out a state-by-state estimation of the effectiveness of their programs. After identifying a subset of states in which the programs are effective at increasing women's education, I will use this subgroup as a robustness check for additional estimations, relying on only those states to verify my findings about the marriage propensities of men.

In order to do this, I take each Broad-Based Merit Aid Program state in sequence, estimating the effect of its program in comparison to the entirety of my control states, repeating the differences-in-differences estimation carried out with equation 3.3. Since I have 29 control states, I still maintain more than 2/3 of the power that I have with the full regression even though I am only using one treatment state at a time. With the exception of the state of consideration, remaining states that have passed a Broad-Based Merit Aid Program are excluded from the regression. In addition to reporting the treatment effects in table 3.11(which is currently located in the additional tables section), I report the state effect as estimated in comparison to the remaining control states. My expectation is that this state effect would be biased upwards by the exclusion of those other states implementing programs over the same period, but it's a useful consideration, as distinguishing between the state effect and the coefficient on *merit* will inform examination of some states who have anecdotally been cited as having effective programs.

Results for each state are presented in table 3.11. In addition to giving the estimated coefficient of the *merit* indicator, I also give the state effect for that particular state, relative to Maine; the state effect is different from the merit effect in that it is based on the educational of control cohorts within the state compared to all the control states. There is clear heterogeneity in the effects of the programs: four states (New Hampshire,

	AR	FL	GA	LA	NM	DE	MD	TN
merit	0.0374*	0.0346**	0.0248	0.0689**	0.0725***	-0.0138	0.0445***	0.1344*
	(0.0154)	(0.0115)	(0.0159)	(0.0210)	(0.0159)	(0.0076)	(0.0087)	(0.0577)
state effect	-0.1420***	0.0112	-0.0406***	-0.0992***	0.0107	-0.0430***	0.0103	-0.0969***
	(0.0094)	(0.0064)	(0.0077)	(0.0063)	(0.0091)	(0.0093)	(0.0098)	(0.0084)
	WV	AK	CA	IN	KY	MS	MO	
merit	0.1901***	-0.0244	0.1004***	0.0757***	0.0514***	0.0412*	0.0167	
	(0.0396)	(0.0248)	(0.0114)	(0.0214)	(0.0131)	(0.0182)	(0.0175)	
state effect	-0.1842***	-0.0195*	-0.0002	-0.1474***	-0.1231***	-0.0302***	-0.0499***	
	(0.0105)	(0.0091)	(0.0082)	(0.0167)	(0.0083)	(0.0083)	(0.0118)	
	SC	MI	NH	NJ	NV	SD	CO	
merit	0.0749***	0.0986***	0.1588***	0.1693***	0.0297	0.0575	0.0637	
	(0.0190)	(0.0188)	(0.0122)	(0.0320)	(0.0193)	(0.0613)	(0.0426)	
state effect	-0.0827***	-0.0378**	-0.0084	0.0295	-0.1038***	-0.0052	0.0199*	
	(0.0088)	(0.0125)	(0.0209)	(0.0154)	(0.0097)	(0.0107)	(0.0081)	

Table 3.11. Estimated state-by-state effects of merit scholarship programs; merit denotes the effect of the scholarship; state effect gives the coefficient for that particular state's effect

New Jersey, Tennessee, West Virginia) show double-digit increases in number of students in college on account of their laws, although all of these states implemented laws in 2002 or later. Some surprising states show little or no impact of their programs. Of these, Georgia was found to have a positive impact in the 1990's using data from 1989-2000[Dynarski, 2004, Conley and Taber, 2005]; the result here likely comes from omitted variable bias associated with the inclusion of later data, long after the HOPE scholarship was first implemented; estimates for Georgia alone with data stopping at 2000 were in line with the existing literature.

As a check of the effect of the programs, I take a subset of states for whom:

- The estimated effects of the scholarship programs are particularly strong, and
- the programs were implemented long enough in the past that respondents in the data are frequently married, even among college graduates

The four states I named above violate the second criterion; California, Indiana, Louisiana, Michigan, New Mexico, South Carolina do not, so I use them as the only treatment states in a separate estimation. Note that this list of states is evenly split among my classification of strong- versus marginal programs in the list of programs given in table 3.22. That classification is based on the degree to which they meet the criteria given when I define these programs; for identification of a causal effect of education, what matters here is the strength of the impact, and that strength is the one in which these particular states seem to excel. For the use Marginal effects from this restricted estimation are given as column three of table 3.15.

There is a slight shifting of the coefficients observed for these states for whom the scholarship programs are strikingly effective: a small, but significant advantage for men who are degree-holders reappears, but the marginal effect of the interaction between degree and treatment exposure dips to almost -22%. A stronger effect of the marriage disadvantage correlating with a stronger effect of scholarships is further confirmation of a causal effect.

3.7.2. Why do certain states implement these programs?

Using these programs as an instrument does carry the implicit assumption that they are exogenous. While the large scale of the states means they can be taken as exogenous from the perspective of the individual students, it is still interesting to consider characteristics of the states that may increase the likelihood they would implement large-scale scholarship programs.

As mentioned above, Broad-Based Merit Aid programs are products of the legislative process within the states, one that may be anything but transparent. Legislators have their own objective functions, but they also will respond to conditions within the state. Ionescu and Polgreen [2008] suggest that states consider the relationship between the high- and low-skill labor sectors in their states when implementing far-reaching scholarship programs.

In order to classify states, they estimate the equation

$$SP_{s,t} = \beta_{0,s} + \beta_{1,s} \cdot LR_{s,t} + \beta_{2,s} \cdot t + \varepsilon_s$$

where $SP_{s,t}$ is the skill premium, i.e. the ratio of skilled to unskilled wages in the state, and $LR_{s,t}$ is the ratio of skilled over unskilled hours. The define Increasing Returns to Scale (IRS) states as those with $\beta_1 > 0$, and DRS states as those with $\beta_1 < 0$. The intuition is that IRS states can produce additional high-skilled workers without compromising their employment potential within the states; DRS states offer a much more limited set of opportunities to those graduates.

Ionescu and Polgreen posit that IRS states will gain from implementing state-wide scholarship programs, while DRS states will not; to see if this might explain the passage of these laws, I estimate a series of logit models over the fifty states for having implemented a law, with Ionescu and Polgreen's estimated coefficients[27] serving as my independent variable. Table 3.12 gives my results; in column one, I use as a dependent variable having implemented any type of Broad-Based Merit Aid Program, while column two restricts them to those programs classified as strong (i.e.)

While I do not have access to the full panel of data used to carry out their estimations, table 3.12 gives the estimated betas, both for having implemented any type of program at all and for having implemented a "Strong" program that meets nearly all of the characteristics of Broad-Based Merit Aid programs I gave above. The coefficient represents the degree to which Returns-to-scale in education–the ability for a state to produce highly educated workers and that state's economy to hire them–affect the likelihood of state-level scholarship programs. Column three gives a marginal effect calculated for only the strong programs estimate.

The marginal effect for predicting a strong program suggests that doubling the return in wages for additional skilled labor hours corresponds to a 12.7% increase in the probability of passing a strong Broad-Based Merit Aid program, all other things being held equal.

[27]I am very grateful to Linnea Polgreen for providing me with all of these coefficients.

	DV: Any Program	DV: Strong Program
Returns to Scale	0.503	0.728
	(0.0369)	(0.444)
Marginal Effect		[0.127]
constant	-0.236	-1.219
	(0.291)	(0.352)
N	50	50

Table 3.12. Effect of state returns-to-scale in education on scholarship program implementation.

3.8. A Continuous Measure of Marital Exposure

The preceding estimates suggest that differences in education of potential spouses lead to changes in marriage market outcomes as mediated through the gains-from-trade portion of marital surplus. Unfortunately, these estimates are problematic because they inflexibly use relation to the law as a proxy for the exposure of a man's potential marriage partners to scholarship programs. In addition, they fail to account for the competition of younger, more educated men who have also had access to the Broad-Based Merit Aid Programs. While the evidence in section 3.5 provided indirect checks that this competition was not a source of ommitted variable bias, I take a direct approach here to solve both of these problems:

I implement a continuous measure for the marriage pool's ambient education of potential spouses. First, I describe in detail the construction of this measure, and then I will give results from using it to estimate the effect on marriage of changes in women's education levels. Next, I will use the same method to construct a continuous measure that accounts not only for the exposure of women within a man's marriage pool to the scholarships, but also for the competition from men for women in that marriage pool.

After making these changes in specification, the results are strikingly similar to those found above: men with college degrees see their marriage chances decrease when their potential mates become more educated.

3.8.1. Implementing a Measure of Spousal Exposure

My goal is to take a measure of the average education level of the pool of a man's potential wives. In the CPS data, actual realizations can be observed to the extent that currently married spouses are linked in the same household. By using the characteristics of wives in the sample, I am making the implicit assumption that these wives reflect an age-independent sampling of the marriage pool as a whole.[28]

From the set of married couples in my data, I observe the empirical distribution $f_j(i)$, which denotes the probability that a married man of age j is married to a woman of age i; I take this probability directly from the observed joint age distribution; assuming each state to represent a distinct marriage market, my data are a representative sample from this distribution, and can therefore be used to construct a weighted average of potential spouse characteristics from this observed distribution, which I take over the entire 18-year span of my data. Of paramount interest is the proportion of a man's marriage pool that has been exposed to the Broad-Based Merit Aid programs. This is similar to a method implemented by Maria Porter [2007] in her study of the Chinese marriage market.

[28]This assumption could be checked with data that are a panel, as the later-marrying couples could be compared to the earlier ones. I have no doubt that that they are different, but the cross-sectional data here force this assumption to be made. Looking at rates of pre-marital cohabitation might give an insight into this, but it is also not possible with the CPS data from IPUMS, as distinguishing cohabiting households from roommate or relative situations would be based purely on guess-work.

For calculating the empirical probabilities, I included all persons of the opposite sex who are seven years or fewer different in age. A summary of probability densities for men of some ages is given in table 3.14.[29] In the context of this distribution, the proportion of a man's marriage pool exposed to a scholarship program is given by $\Psi_j(i) = \int_{-\infty}^{i} f_j(\xi)d\xi$, which I am approximating by

$$\Psi_j(i) \approx \sum_{n=-6}^{2} f_j(n) \tag{3.5}$$

Here, i represents the age-difference between a man of age j and the first cohort exposed to the scholarship programs. This provides a more continuous representation of the degree to which a man's marriage pool has been affected by the scholarship program in that it weights more heavily those men who graduate from high school immediately before the laws than those who have some intervening years; the cohorts of women immediately below this latter group of men in age would have unaffected education levels. A summary of this measure for the most frequent cohorts of men is given below in table 3.13. One would expect the mean exposure to increase with treatment cohort. Recall that men in treatment cohorts $0-3$ are themselves exposed to the scholarship programs.

I use this measure in a regression for marriage probabilities in place of the T indicator presented in previous sections, allowing me to estimate the effect of an increase in the percent of a man's marriage pool who have been exposed to the scholarships–effectively serving as an instrument for potential mates' education–on the marriage probability of

[29]Note that Porter [2007] uses the years from -6 to +2.

Treatment Cohort	Percent Exposed	Number Men
-7	0	17,578
-6	.013	14,852
-5	.037	14,454
-4	.097	13,559
-3	.139	13,127
-2	.217	12,208
-1	.401	12,039
0	.630	10,990
1	.831	10,168
2	.946	9,628
3	1	8,824

Table 3.13. Mean exposure of a man's marriage pool to scholarship programs, by treatment cohort.

age hus	age of wife 17	18	19	20	21	22	23	24	25	26	27	28	29
18	12.8	33.8	18.0	13.7	3.6	1.4	0.8	0.2	0.0	0.0	0.4	1.4	0.0
19	8.5	17.2	29.5	19.4	8.5	2.6	3.4	1.9	1.4	0.3	0.3	0.1	0.6
20	4.1	10.8	20.8	25.1	14.4	8.4	3.7	3.8	1.6	0.7	1.3	0.9	0.8
21	2.6	8.1	14.3	21.2	21.0	12.0	6.4	4.8	2.4	1.3	1.3	0.8	0.8
22	0.9	3.7	6.5	15.3	20.7	20.4	11.8	6.2	4.1	2.8	1.5	1.2	0.9
23	0.6	1.9	5.2	9.2	14.2	18.1	18.9	13.6	5.8	3.2	2.4	1.7	1.1
24	0.4	0.7	2.5	6.1	9.5	14.7	17.4	18.3	10.9	6.0	3.9	2.4	1.7
25	0.2	0.6	1.6	3.4	6.1	10.5	14.8	16.6	17.2	10.8	5.4	3.8	2.5
26	0.1	0.6	1.1	1.9	3.8	6.2	10.7	14.5	15.8	17.0	9.5	5.3	3.6
27	0.1	0.3	0.6	1.3	2.0	4.7	6.1	10.7	13.7	16.9	16.6	9.3	5.3

Table 3.14. Probability distribution of wife's age, conditional on husband's age, with truncation for extreme age differences.

that man. The dependent variable in these regressions is an indicator for still being married.[30] I give the calculated marginal effects in table 3.15.

[30] This is in opposition to the indicator I use above for ever having been married. Results are similar for each: I provide a table in the additional tables appendix so that the reader can see typical results for that dependent variable.

Sample	All People	People in Changing States	People in Effective States
age	0.0910***	0.0922***	0.0928***
	(0.0042)	(0.0064)	(0.0044)
age squared	-0.0011***	-0.0011***	-0.0011***
	(0.0001)	(0.0001)	(0.0001)
% of women exposed to merit scholarships	0.0785***	0.0616**	0.0783***
	(0.0162)	(0.0179)	(0.0197)
any college degree	0.0091	0.0067	0.0174*
	(0.0087)	(0.0124)	(0.0077)
% women merit/degree interaction	-0.1254	-0.1036	-0.1954*
	(0.0656)	(0.0524)	(0.0884)
education in years	0.0110***	0.0101*	0.0087**
	(0.0028)	(0.0044)	(0.0025)
labor force participation	0.2574***	0.2580***	0.2569***
	(0.0109)	(0.0159)	(0.0126)
Sample Size	628335	299572	130502

Table 3.15. Marginal effects from estimation of effect of continuous scholarship exposure measure on men's marriage rates in three different groups of states.

The three columns in this table are differentiated by the subsample over which the estimation was carried out. The first column is all states, the second column is only those states that eventually implement Broad-Based Merit Aid Programs (with time variation entirely driving the differences in cohort exposure), and the third column–discussed separately in section 3.7–includes all control states, together with a selected group of treatment states .

The coefficients on *pctplmerit*, my measure of potential spouse' education, are positive and significant; this measure is a finer indication of a man's dating environment that the indicator I used for results above. Under this specification, however, the indicator for a man having a college degree loses significance. I interpret this as meaning that omitted variable bias was a factor in the results with a binary cohort exposure indicator; once the education of prospective spouses is accounted for, any marriage advantage to men of having a college degree vanishes.

Among those degreed men, however, the ones whose spouses are exposed to scholarship programs are still seeing a large decrease in their marriage probability to the tune of $11\% - 13\%$. While these effects are only borderline significant, they are within a standard error of the estimates in section 3.4.

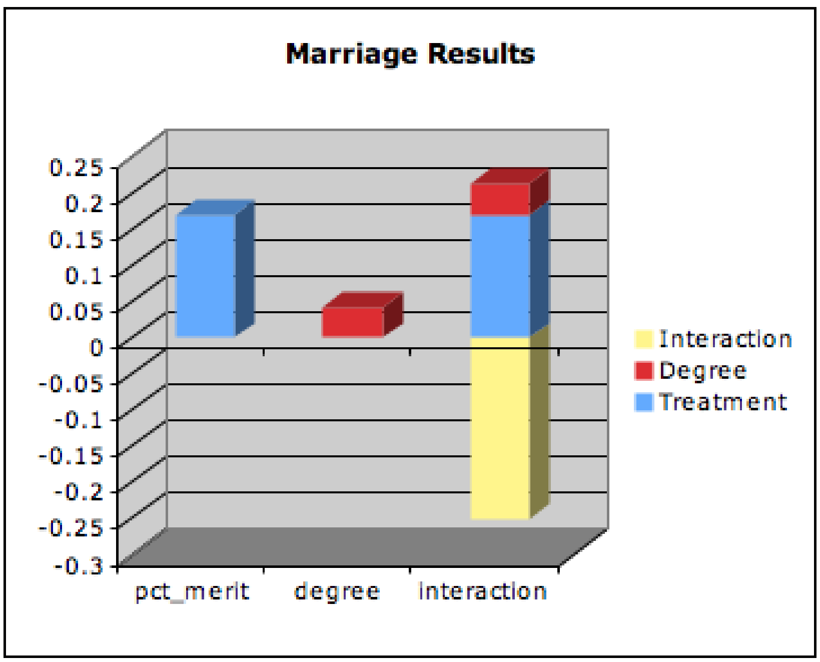

Figure 3.2. Estimated coefficients of a logit model; dependent variable is marriage indicator for men; independent variable is a continuous measure of potential wives' scholarship program exposure, with controls for college degree and interactions included.

3.8.2. A Continuous Marriage Measure that Accounts for Competition

However, this measure of marriage probability only accounts for a portion of the story, as those men who have increasing portions of their marriage pool filled with scholarship-eligible women must also compete with younger men who are receiving the scholarships as well. Up until now, I have been dealing with the competition problem by excluding these men, estimating my marriage equations only over those men. While this has served to hold their own education constant, accounting for the changing education levels of their competitors is necessary; I do so in this section.

This aspect of marital competition allows for second-order effects to cause interactions between marital decisions of quite disparate cohorts of men. While I track marriages between all couples with ages within seven years of each other, the balance of these marriages involve wives who are two years or less older, or husbands who are 6 years or less older. Considering only these eight years, a woman born in 1975 has an interest in the availability of men born in years 1969 to 1977. However, the man born in 1977 can realistically marry a woman born as late as 1983, while the man born in 1967 will be considering wives born even earlier: 1965. This means that a woman or man is competing with like who are born up to eight years earlier or later than she is[Porter, 2007].

In equation 3.5, I give an empirical probability of marrying someone exposed to a Broad-Based Merit Aid Program. Note that this probability is a function of sex, age, year, and state (the last three variables are subsumed by treatment cohort when I give the calculations above.) By taking these calculated probabilities for women, I can weight them using the wife age density $f_j(n)$ for a man of a given age, in a given state, in a given year, allowing me to compute the percentage of a man's competitors who are exposed to the scholarship program.

I can compute a single measure that accounts for both the "demand" shock in a man's marriage market (the fraction of women who are exposed to the treatment) together with the competition from other men, which is effectively a "supply" shock. I do so by computing the ratio $\frac{W_s}{M_s}$ of percent of women affected by the programs divided by percent of competing men affected by the programs.

The latent model for marriage propensity that I estimate is:

$$y_i^\star = \beta \cdot \frac{PP_{t,s}}{PC_{t,s}} + \theta_s + \gamma_t + \delta \cdot P(Age_i) + \zeta \cdot X_i + \xi \cdot Educ_i + \eta \cdot LFP_i + \varepsilon \tag{3.6}$$

In which the term $\beta \cdot \frac{PP_{t,s}}{PC_{t,s}}$ represents the estimated effect of a change in ratio; intuitively, this ratio increases either because a man's potential wives increase their education, or because the proportion of other men competing for those wives who have been exposed to the scholarship decreases. Marginal effects are given in table 3.16 across the same three samples for whom the above continuous outcomes are given in table 3.15, with the results largely unchanged.

3.9. Conclusion

Above I show that increased education for women–while holding mens' education constant–compromised either the ability or desire of educated men to find spouses through the marriage market. The initial estimation involved establishing cohorts of men in relation to the implementation of Broad-Based Merit Aid Programs, sources of exogenous

Sample	All People	People in Changing States	People in Effective States
age	0.0819***	0.0893***	0.0839***
	(0.0104)	(0.0123)	(0.0056)
age squared	-0.0011***	-0.0012***	-0.0011***
	(0.0001)	(0.0001)	(0.0000)
Ratio of schol women to schol men	-0.0037	-0.0040	0.0088
	(0.0096)	(0.0104)	(0.0076)
indicator for any college degree	0.0083	0.0090	0.0193
	(0.0149)	(0.0163)	(0.0104)
Ratio schol women/schol men interact	-0.0177***	-0.0193***	-0.0175***
with degree	(0.0036)	(0.0041)	(0.0017)
education in years	0.0065	0.0070	0.0019**
	(0.0035)	(0.0038)	(0.0006)
labor force participation	0.1792***	0.1953***	0.1664***
	(0.0207)	(0.0205)	(0.0095)
Sample Size	628335	299572	130502

Table 3.16. Marginal Effects for logit estimation of impact of change in ratio of scholarship-exposed women over scholarship-exposed men on men's marriage rates; subset of states used in estimation is given for each model.

increase in the education levels of women; because men most often marry younger women, there were a significant number of men whose own education levels had not changed, but who were facing a marriage market in which many of their potential mates were 7% more likely to have attended college. While these men were 2% more likely to be married, this increase was more than offset for those men with college degrees, who were 4% less likely to be married when their corresponding female cohorts had been exposed to the scholarship programs.

Once the increases in women's education of the past thirty years are taken into account, the differential effect for men with college degrees versus less-educated men suggests that the gains-from-trade effect of marriage, seen through negative assortative mating on wages, is more powerful than the positive assortative mating holds for most other observable traits. All evidence I present is consistent with the substitution of less educated-husbands for more-educated, suggesting that these educated women still value marriage, but their higher earning power gives them more freedom in deciding whom to marry; I examine the women who have been exposed to the scholarship programs to rule out two alternative explanations: that woman could be opting out of the marriage market completely, and that women could be marrying younger men. The evidence based on women respondents rejects both of these explanations.

Next, I construct a continuous measure for the average education level of a man's prospective spouses. Measuring the treatment in this more direct way confirms my results: I find an increase in the marriage rate of those men who see an increase in the average education level of their potential spouses, although this effect is still more than negated for those men with college degrees. This is true both over all the states and when I consider only a collection of those states with the strongest educational increases.

Indeed, women who are more educated are not opting out of marriage. Rather, their education benefits the less-educated men, who now have more educated spouses available to them; increasingly, educated women are able to afford to choose these men as marriage partners.

3.10. Appendix – Robustness Checks

The results that I present above are surprising and do represent some weighty assumptions: in this section I carry out a number of checks to verify the positive relationship between women's education and men's marriage rates.

3.10.1. Are Broad-Based Merit Aid States Systematically Different?

I now consider the similarity of these two groups of states: in order to treat the laws as exogenous, I would like to be able to show that the groups of states are similar on a variety of demographic measures, including income, employment, race, urban influence, and–of course–education. Note that showing merely a similarity of observable factors cannot rule out a host of others–unobservable to the economist, but perhaps observable to state legislatures–that may precipitate implementation of these laws.

I have sorted the states into groups depending on whether they pass a Broad-Based Merit Aid program law before the 2006 CPS survey, the last in my data. Assignment of state programs to strong- or marginal status can be found in the table 3.22, although in what follows I have lumped marginal- and strong states together into a single group of the twenty-three treated states. Note that the estimates which follow in table 3.17 come from taking a weighted average over all people in either group (i.e. people in the states, rather than an aggregated average of people. I treat California separately because it is so large and because it's scholarship program is one of the weaker fits for the characteristics I listed earlier.

Unfortunately, the two groups do not look very similar in this comparison, having significant differences in every category shown. I suspect that this is the case because of the predominance of Broad-Based Merit Aid programs in the South, as Southern States Typify all of the treated group's characteristics, except the percent urban, which will later on be the most problematic. The difference in education measures is particularly troubling, as it could signify that Broad-Based Merit Aid programs are passed as a response to poor

	Schol	Schol w/o CA	Non_Schol
% with College	0.415	0.413	0.431
	(0.002)	(0.002)	(0.001)
Household Income	50,012	48,798	50,296
	(155.178)	(173.382)	(137.453)
% in Labor Force	0.702	0.719	0.716
	(0.002)	(0.002)	(0.001)
% Urban	0.815	0.752	0.776
	(0.001)	(0.002)	(0.001)
% Black	0.163	0.200	0.128
	(0.001)	(0.002)	(0.001)

Table 3.17. Summary statistics for women ages 18-31, sorted by state scholarship program status; middle column removes California from Sample.

matriculation rates of high school students into post-secondary education. This response bias could overstate actual effects, which would be troublesome indeed.

Restricting attention to African Americans, the two groups of states become more similar: Focusing on the marriage patterns of African Americans would be of interest for several reasons: first, many of them live in the South, where most Broad-Based Merit Aid programs have been enacted. Second, they tend to be less well-off–suggesting a greater responsiveness to scholarships and financial aid–which makes them more sought after by colleges; and finally, there is a greater tendency among educated black women to foresake marriage[Bennett et al., 1989], so black men are a subgroup for whom there's a high chance of marriage market foreclosure. In table 3.18, I give summary statistics for blacks only:

With this subgroup, the state groups seem much more similar; note however that there are two difficulties: first, these data shown are observable factors, so there is still no guarantee of the unobservables being in sync between treated and non-treated states.

	Schol	Non_Schol
% with College	0.431	0.438
	(0.004)	(0.004)
Household Income	\$34,352	\$33,914
	(310)	(296)
% in Labor Force	0.686	0.676
	(0.004)	(0.004)
% Urban	0.825	0.882
	(0.003)	(0.003)

Table 3.18. Summary statistics for African American populations, sorted by state scholarship program exposure.

Robustness checks are still in progress for different groupings of the merit programs, as well as ages.[31]

Unfortunately, there is another difficulty with the above tabulations: I do not make any time distinctions. In order to account for time effects for a particular indicator y (of which % in College, LFP rate, etc. are each one), I must estimate regression equations of the form

$$y = \sum_{i=-4}^{-1} \alpha_i * \gamma_i + \theta_s + \tau_t \tag{3.7}$$

[31]Curiously, while I do not show time trends in this table, the education numbers for Blacks generally decline throughout the 1990's, in keeping with my earlier education estimation for black women. I can only speculate as to the cause of this, but my biggest fear is that it has to do with non-reporting. In fact, a casual inspection of the data shows about 20% fewer reported people in the college age cohorts (18-24) for all races than there are in older ones. If this is under-reporting, then I am deeply disturbed by it, and looking into a way to deal with it should be a high priority. My original intuition for this was that African Americans tend to live in more urban areas, and these are more similar in both the South and the North, while the rural areas are really different. However, this did not appear to be true when I looked at the data, so investigating whether urban influence might be as effective a proxy as this use of race I will leave for another paper.

Where γ is a dummy for treatment cohort; θ and τ are state- and time effects. Significant αcoefficients represent the degree to which being in a treatment cohort affects the level of y. Significant differences mean that the levels of the characteristics are significantly different when states differ by the predictor, i.e. for people who are in a particular treatment cohort when compared with the others.

By using the dummies for the treatment cohorts immediately proceeding the passage of the laws, I am able to test for a statistically significant relationship between *levels* of the demographic characteristic and passage of the law. I present two sets of estimates, one weighted using the CPS weights, and the other employing clustering by state on unweighted data. There are not great differences between the two sets of estimates.

	Dependent Variable			
	College	Income	Labor Force	Urban
tc is -1	0.004	-2072.759**	0.011	-0.011
	(0.007)	(700.431)	(0.007)	(0.006)
tc is -2	0.015*	-1402.775*	0.007	0.010
	(0.007)	(676.373)	(0.007)	(0.005)
tc is -3	0.029***	-2377.542***	0.017**	0.017***
	(0.007)	(604.040)	(0.006)	(0.005)
tc is -4	-0.004	703.628	-0.006	0.024***
	(0.007)	(674.400)	(0.006)	(0.005)

Table 3.19. Regression coefficients of treatment cohort indicators; using each of the indicated demographic measures allows testing for significant differences for people in the cohorts exposed to scholarship programs; weighted.

Unfortunately, there are indeed statistically significant differences in these indicators between the treatment- and control states. The cohort three years before the passage of the law in particular seems to have a strong influence on the passage of laws, which is in line with a legislative process that might take several years in the case of these programs.

	Dependent Variable				
	College	Income	Labor Force	Urban	Black
tc is -1	-0.014*	-1647.595*	-0.016*	-0.009	0.008
	(0.007)	(720.101)	(0.007)	(0.006)	(0.005)
tc is -2	-0.014	-239.191	-0.011	-0.017**	0.005
	(0.007)	(767.685)	(0.007)	(0.006)	(0.005)
tc is -3	-0.018*	-1498.574	-0.012	-0.024***	0.007
	(0.008)	(818.601)	(0.007)	(0.007)	(0.006)
tc is -4	-0.042***	-617.403	-0.015*	-0.032***	0.010
	(0.008)	(841.988)	(0.007)	(0.007)	(0.006)

Table 3.20. Regression coefficients of treatment cohort indicators; using each of the indicated demographic measures allows testing for significant differences for people in the cohorts exposed to scholarship programs; unweighted, but clustered by state.

Note that this is happening with observable factors, so there would also be the problem of unobservables, as well. For this reason, I have already employed State Fixed Effects in the estimations in 3.4.

3.11. Appendix – Treatment of Data

The data that I use for this study are CPS Data, October Education Supplement from IPUMS USA, available on the world wide web at `http://usa.ipums.org/usa/` . IPUMS has a web-based interface that allows selection and extraction of variables of interest; then a customized dataset is e-mailed to the user along with a .do file for Stata 10, which can be run in order to create a Stata 10 dataset with the data.

After this dataset is created, there are still numerous decisions that must be made concerning how to transform the data into a form usable for my analyses described above. I am happy to provide data and .do files upon request, although–because of their size–the logistics of doing this may cause some delays. There are some coding decisions that I must make based on my particular interest.

The CPS data marriage variable that comes through IPUMS is coded to reflect the following states:

(1) Married, spouse present

(2) Married, spouse absent

(3) Separated

(4) Divorced

(5) Widowed

(6) Never married/single

I treat codes 1 - 5 as the state of having entered marriage, while I treat the sixth as never having entered marriage. Technically, this would then constitute a study of the transition into first marriage. Note that I do not use a cohabitation outcome. While there is a concern that this may bias the marriage estimates upward[32], I did try the estimations counting only #1 as married, and results were not very different.

One issue that arises is a differential coding of the education variable between the 1989-1991 data and subsequent years. Data from 1992 on have a finer coding of education, particularly with respect to levels of education beyond college. In order to make these more comparable, I created a variable for years of schooling that mapped as follows:

Note that even the education recode still accurately captures the boundary between HS diploma and at least some college.

[32]Intuitively, counting a divorced person as a married person may make a lot of single people increase my effect size. This is less of a worry for me than the young age of a good many of the scholarship-exposed subjects.

Ed. Level	*education*	*educ*99	*educrec*
K or less	0	1	1
1-4	2.5	4	2
5-8	6.5	5	3
9	9	6	4
10	10	7	5
11	11	8	6
HS grad	12	10	7
Some college	13	11	
AA degree	14	12	8
Bachelors Degree	16	14	9
Masters Degree	17	15	9
Prof. Degree	19	16	9
Doctorate Degree	21	17	9
years in data	–	1992-	1989-

Table 3.21. Correspondence between *education* variable and the two principle educational attainment variables reported in CPS: note the finer detail for more educated people provided by *educ*99.

In addition, race and ethnic origin are treated separately in the census data. I account for this be creating mutually exclusive dummies for African Americans, Asian Americans, Native Americans, and those of Hispanic origin separately, with caucasians serving as the base category. I am not concerned with more specific ethnic groups.

3.12. Appendix – Additional Tables

strength	state	Program	year	annual amount
Marginal	AK		1999	$2,750
Strong	AR	Academic Challenge Scholarship	1991	$2000-$3000
Marginal	CA	Cal Grant Program	1996	$9,400
Marginal	CO	College in Colorado Scholarship	2005	
Strong	DE	SEED Scholarship	2006	
Strong	FL	Bright Futures Program	1997	75% of all tuition/fees
Strong	GA	HOPE Scholarship	1993	tuition/fees/books
Marginal	IN	Twenty-First Century Scholars Program	1990	tuition/fees
Marginal	KY	Kentucky Educational Excellence Scholarship	1999	$1,000
Strong	LA	TOPS program	1998	tuition/fees
Strong	MD	see Dynarski	2002	
Marginal	MI	Michigan Merit Award Scholarship	2000	"one time $2500, or $1000 for out-of-state"
Marginal	MS	Mississippi Resident Tuition Assistance Grant	1996	$500 - $1000
Marginal	MO	Bright Flight Scholarship	1997	$2,000
Marginal	NV	(Gov. Guinn) Milennium Scholarship Program	2000	$40-$80 per credit hour
Marginal	NH	21st century scholars fund	2006	
Marginal	NJ	Governor's Stars Scholarship	2004	
Strong	NM	Lottery Success Scholarship	1997	full tuition/fees
Strong	SC	HOPE Scholarship (LIFE Scholarship)	1998	full tuition + $300
Marginal	SD	Opportunity Scholarship	2004	
Strong	TN	HOPE/Education Lottery Scholarship	2004	
Strong	WV	PROMISE	2002	full tuition

Table 3.22. List of State-level Broad-Based Merit Aid Programs and some details about them; based on tables in Heller [2006] and Dynarski [2004]. Strong/Marginal provides my own heuristic assessment of how many characteristics the programs exhibit out of my list in section 3.2.3.

Age	Treatment Women Hus	Neither	wife	Total	Control Women Hus	Neither	wife	Total
18	62	48	26	136	205	174	70	449
19	89	86	63	238	303	401	179	883
20	107	169	92	368	431	740	409	1,580
21	125	200	149	474	636	1,032	609	2,277
22	145	220	146	511	961	1,450	903	3,314
23	162	281	173	616	1,206	1,876	1,154	4,236
24	138	260	176	574	1,415	2,392	1,492	5,299
25	148	245	154	547	1,714	2,759	1,849	6,322
26	124	202	125	451	2,016	3,284	2,191	7,491
27	103	151	111	365	2,344	3,672	2,474	8,490
28	91	108	101	300	2,673	4,012	2,827	9,512
29	44	75	68	187	2,972	4,348	3,080	10,400
30	40	52	58	150	3,276	4,735	3,473	11,484
Total	1,378	2,097	1,442	4,917	20,152	30,875	20,710	71,737

Table 3.23. Educational Difference Tabulation among Spouses: conditional on the age of the wife, I show number of couples in which the husband, the wife, or neither is more educated. Women are divided between treatment and control groups based on the exposure of their cohort to the scholarship programs.

		All Married Couples Number	Percent	Cum.
DIFFERENCE:				
AGE				
	Husband 20+ years older than wife	490	0.8	100.0
	Husband 15-19 years older than wife	903	1.6	99.2
	Husband 10-14 years older than wife	2,960	5.1	97.6
	Husband 6-9 years older than wife	6,979	12.1	92.5
	Husband 4-5 years older than wife	7,911	13.7	80.4
	Husband 2-3 years older than wife	12,736	22.0	66.7
	Husband and wife within 1 year	18,582	32.1	44.7
	Wife 2-3 years older than husband	3,552	6.1	12.6
	Wife 4-5 years older than husband	1,663	2.9	6.5
	Wife 6-9 years older than husband	1,374	2.4	3.6
	Wife 10-14 years older than husband	459	0.8	1.2
	Wife 15-19 years older than husband	112	0.2	0.4
	Wife 20+ years older than husband	118	0.2	0.2
RACE				
	Both White	43,175	74.6	
	Both Black	3,875	6.7	
	Both Hispanic	4,939	8.5	
	Both Other	2,545	4.4	
	Husband White, wife Black	95	0.2	
	Husband White, wife Hispanic	918	1.6	
	Husband White, wife Other	688	1.2	
	Husband Black, wife White	209	0.4	
	Husband Black, wife Hispanic	66	0.1	
	Husband Black, wife Other	42	0.1	
	Husband Hispanic, wife White	751	1.3	
	Husband Hispanic, wife Black	41	0.1	
	Husband Hispanic, wife Other	74	0.1	
	Husband Other, wife White	360	0.6	
	Husband Other, wife Black	17	-	
	Husband Other, wife Hispanic	44	0.1	

Table 3.24. All numbers are in thousands; source is Table FG3. Married Couple Family Groups, by Presence of Own Children Under 18, and Age, Earnings, Education, and Race and Hispanic Origin of Both Spouses: March 2001; U.S. Census Bureau, Population Division. Internet Release Date: July 17, 2003

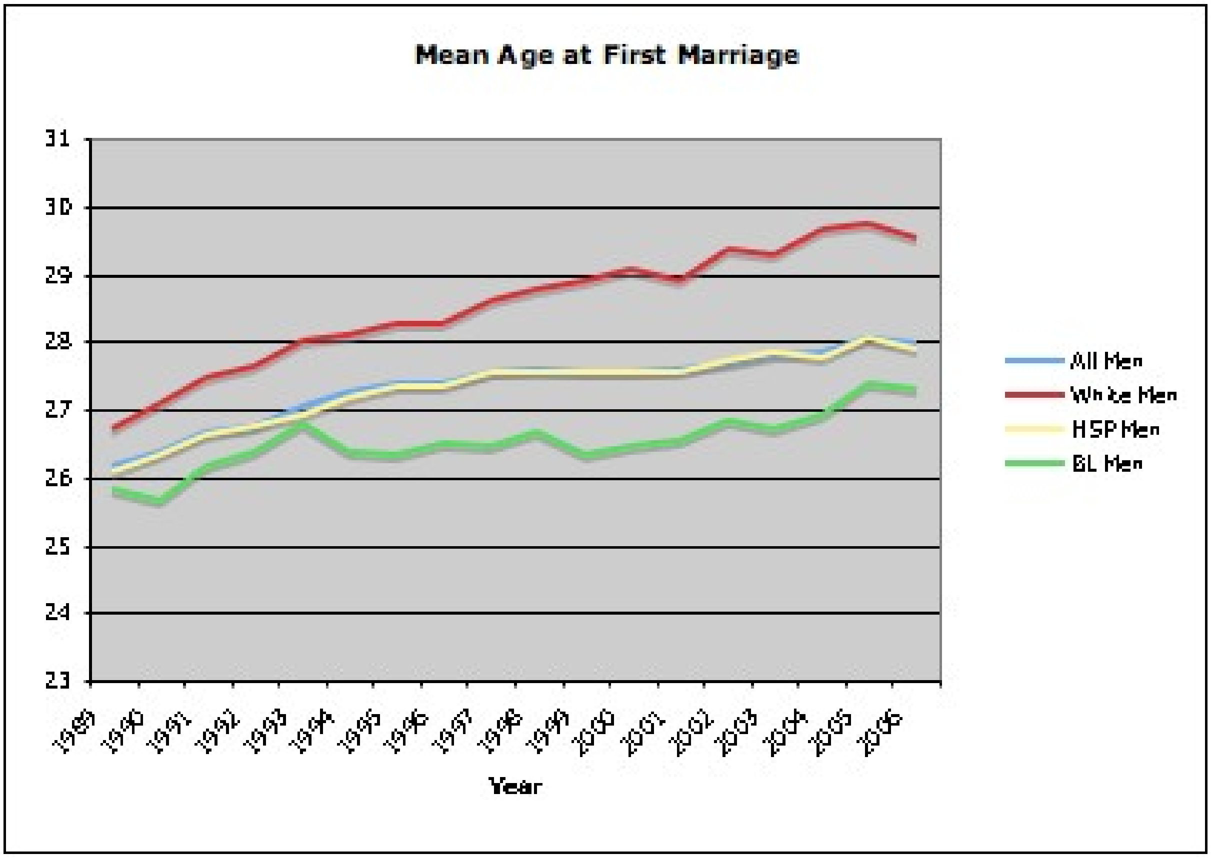

Figure 3.3. Mean Age-at-First-Marriage over Time for Men; source: IPUMS CPS Data, October Supplements

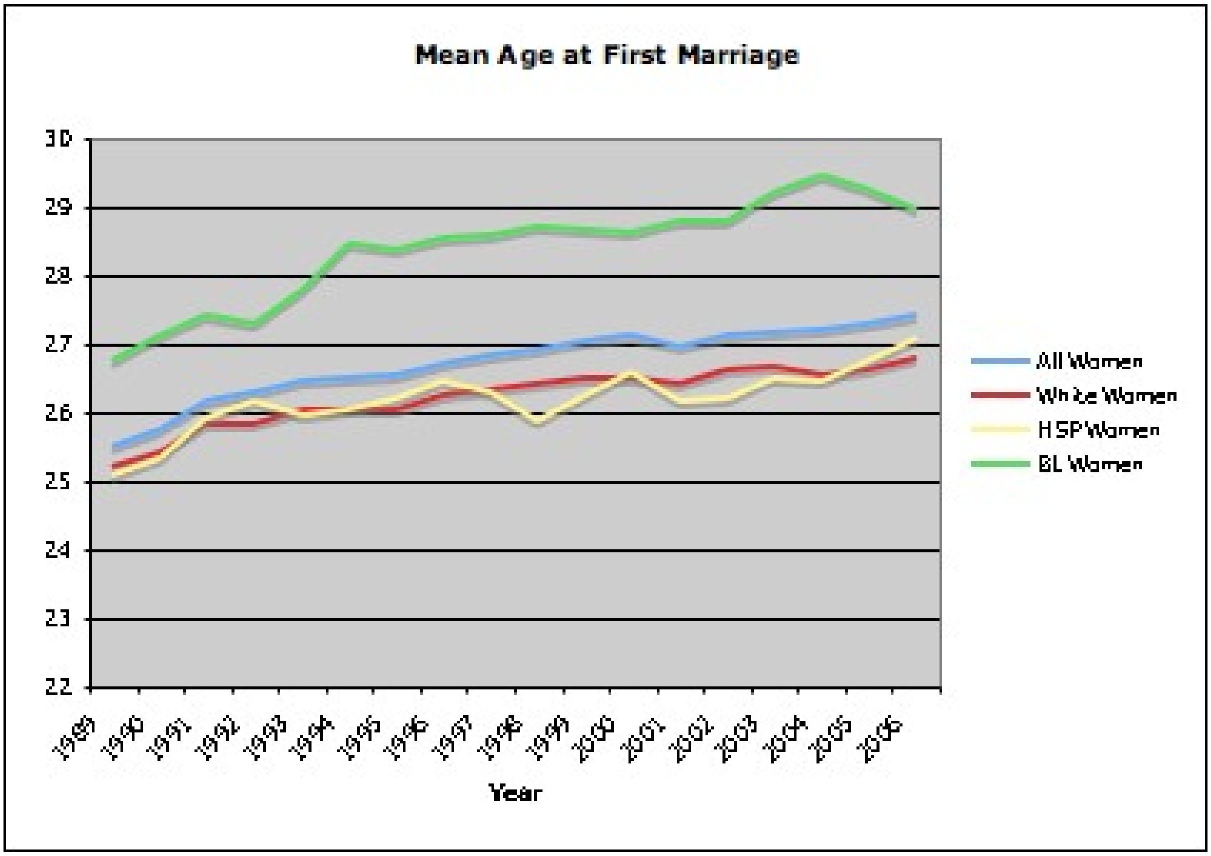

Figure 3.4. Mean Age-at-First-Marriage over Time for Men; source: IPUMS CPS Data, October Supplements

age	Prob(spouse's age is X)																	
	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34
18	12.8	33.8	18.0	13.7	3.6	1.4	0.8	0.2	0.0	0.0	0.4	1.4	0.0	0.0	0.6	0.6	0.0	1.2
19	8.5	17.2	29.5	19.4	8.5	2.6	3.4	1.9	1.4	0.3	0.3	0.1	0.6	0.6	0.0	0.0	0.2	0.1
20	4.1	10.8	20.8	25.1	14.4	8.4	3.7	3.8	1.6	0.7	1.3	0.9	0.8	0.4	0.2	0.1	0.0	0.5
21	2.6	8.1	14.3	21.2	21.0	12.0	6.4	4.8	2.4	1.3	1.3	0.8	0.8	0.6	0.3	0.1	0.0	0.2
22	0.9	3.7	6.5	15.3	20.7	20.4	11.8	6.2	4.1	2.8	1.5	1.2	0.9	0.9	0.4	0.3	0.4	0.2
23	0.6	1.9	5.2	9.2	14.2	18.1	18.9	13.6	5.8	3.2	2.4	1.7	1.1	0.8	0.5	0.6	0.5	0.3
24	0.4	0.7	2.5	6.1	9.5	14.7	17.4	18.3	10.9	6.0	3.9	2.4	1.7	1.2	0.8	0.9	0.6	0.2
25	0.2	0.6	1.6	3.4	6.1	10.5	14.8	16.6	17.2	10.8	5.4	3.8	2.5	1.4	1.2	0.7	0.6	0.6
26	0.1	0.6	1.1	1.9	3.8	6.2	10.7	14.5	15.8	17.0	9.5	5.3	3.6	2.6	1.8	1.4	1.0	0.7
27	0.1	0.3	0.6	1.3	2.0	4.7	6.1	10.7	13.7	16.9	16.6	9.3	5.3	3.3	2.6	1.8	1.0	0.8
28	0.1	0.2	0.3	0.8	1.6	2.6	4.7	6.7	10.0	13.4	16.4	16.3	9.2	5.2	3.0	2.6	1.7	1.3
29	0.0	0.1	0.2	0.6	1.2	1.7	3.2	4.3	6.6	9.9	13.4	16.1	15.5	9.0	5.4	3.4	2.4	1.6

Table 3.25. Probability Age Distribution of wives conditional on husband's age

age	Prob(spouse's age is X)																	
	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40
30	1.8	3.1	4.8	7.2	9.7	13.4	15.7	15.6	8.8	4.5	3.1	2.4	1.6	1.2	0.9	0.7	0.6	0.4
31	1.3	2.2	3.2	5.2	6.7	10.2	13.7	15.3	14.2	8.7	4.8	3.3	2.3	1.7	1.1	0.9	0.8	0.5
32	1.1	1.7	2.6	3.8	5.0	7.6	10.0	13.1	14.7	13.9	8.4	5.0	3.2	2.2	1.5	1.1	0.9	0.8
33	0.7	1.0	1.7	2.5	4.0	5.2	7.0	9.7	13.0	14.3	13.8	8.4	5.4	3.3	2.2	1.5	1.2	0.9
34	0.5	0.7	1.2	2.1	2.7	3.8	5.5	7.3	10.0	12.5	14.2	13.7	8.3	4.5	3.1	2.3	1.6	1.3
35	0.3	0.6	1.1	1.5	2.1	2.8	4.4	5.9	7.2	9.4	12.4	13.8	13.6	8.1	4.4	3.1	2.1	1.7
36	0.3	0.5	0.7	1.1	1.5	2.2	2.9	4.5	5.6	7.3	9.9	12.2	14.5	12.2	8.1	4.6	2.9	2.1
37	0.3	0.4	0.6	0.8	1.3	1.6	2.1	3.1	4.1	5.4	7.7	10.1	12.3	14.1	12.6	7.9	4.2	2.6
38	0.2	0.3	0.3	0.6	0.9	1.2	1.6	2.1	3.3	4.3	5.8	7.8	9.9	11.8	13.5	13.3	7.4	4.2
39	0.1	0.2	0.3	0.4	0.6	0.8	1.2	1.7	2.7	3.5	4.2	5.8	7.7	9.9	11.7	13.6	13.0	7.2
40	0.2	0.2	0.3	0.3	0.6	0.7	1.1	1.6	1.7	2.6	3.2	4.4	6.1	7.6	9.5	11.4	13.0	13.4

Table 3.26. Probability Age Distribution of wives conditional on husband's age

	tratio_reg_mfx b/se
age	0.1261***
	(0.0099)
age_sq	-0.0015***
	(0.0002)
schol_marr_ratio	0.0083
	(0.0066)
any_degree	-0.0275
	(0.0172)
sm_ratioxdegree	-0.0269***
	(0.0045)
education	0.0032
	(0.0039)
inlabforce	0.1958***
	(0.0172)
N	1519404

Table 3.27. Estimated coefficients for effect of women's education on men's marriage rates, accounting for competition from younger men. *schol_marr* is ratio of women who have experienced treatment divided by competing men who have within the context of each man's marriage market. Dependent variable here is ever having been married.

CHAPTER 4

Effects of No Fault Divorce Laws: Evidence from Border Counties (Co-authored with Mark Surdutovich)

4.1. Introduction

Differences in policies adopted by the American states provide researchers with a "laboratory" for studying the effects of policies implemented by subsets of those states. Two common methods employed in doing this are the use of cross-sectional comparisons, and differences-in-differences estimation. Unfortunately, neither of these methods is perfectly suited to data collected on the state level, as both are prone to suffer from the same two shortcomings: law endogeneity of policies and unobserved heterogeneity between states.

In this paper, we present a method to minimize the ill effects of both: exploiting state-level variation by using data that are at the county level. By disaggregating the data, we can select a subset of the counties for which the above-mentioned problems are mitigated. The subset that we choose is those counties that border counties in another state.

While this method is not original to this paper, ours is the first to apply it to a non-business application, as well as the first to seriously consider this method as a solution to law endogeneity. In addition, we propose a method to separate effective borders from non-effective ones.[1]

In order to test our method, we focus on the question of whether Unilateral No-Fault Divorce Laws affect divorce rates. This question is ideal for examination because Unilateral Divorce Laws are similar across states, and the outcome can be readily observed and measured. In a marked departure from other issues such as crime, the primary direction of policy is determined at the state level, rather than being confronted at more local levels through the hiring of police or other personnel.

It is also a question that is rich economically: from 1970 to 1975, the annual rate of divorces per thousand people climbed 33% from 3.5 to 4.8. During this narrow window of time, half of all states in the Union implemented their Unilateral divorce laws. Questions of which caused the other are inescapable, and these make the use of a method such as our border county method all the more crucial.

In this paper, after giving background information, we will confirm the existing results concerning Unilateral Divorce that have been found by literature at the state level. Then, we disaggregate the data from state level to county level: we compare border counties as a group to interior counties, before repeating the above estimations and finding that there is no significant effect of these laws on divorce rates. This is significant as a test of the Coase Theorem, in that we find that parties under contract are able to reach the same efficient outcomes no matter which is given initial rights under law. Next, we confirm the finding of Justin Wolfers [2006] that–while they did initially release a stock of poor

[1]For example, Chicago, Minneapolis-St. Paul, Philadelphia, and New York City are all in border counties, and yet also the principal cities in their respective states, thereby making the use of these areas as a solution to law endogeneity seem questionable.

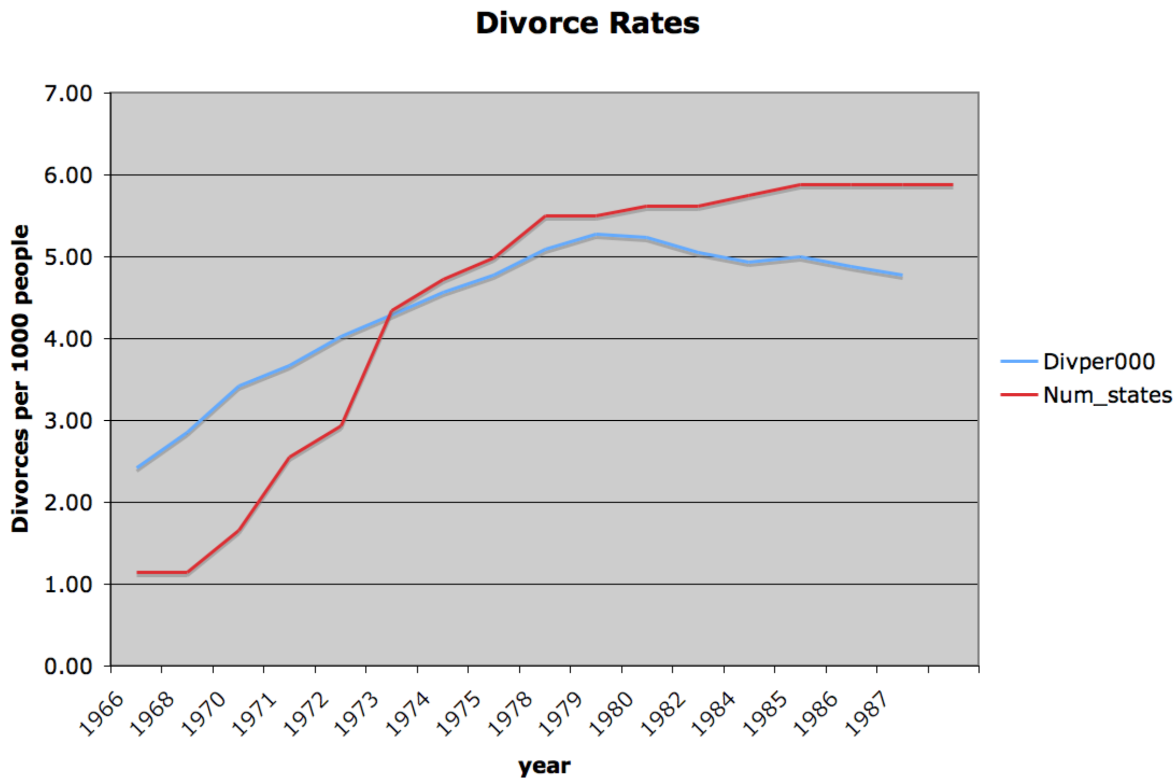

Figure 4.1. Divorce rate for entire United States (with Indicator of number of states with no-fault divorce laws).

marriages–these laws had no long-term effects on divorce rates; the Coase theorem passes this test, too. We also test for temporal differences in the effects of laws, finding that there are no evidence of longer term effects of the laws.

Finally, we provide a discussion of the quality of borders, including an illuminating example of how these borders can be evaluated.

4.2. Background

4.2.1. Policy Effect Estimations

When using cross-sectional state level data to estimate a policy effect, the estimating equation used is typically

$$Y_s = X_s\beta + \gamma_s + \varepsilon_s \tag{4.1}$$

in which β represents the effect of policy–the implementation of which is represented by X_s–while γ_s represents state fixed effects. ε_s represents error and unobserved variance from a variety of sources. Unfortunately, this implementation is not exogenously determined by random assignment or other method of the experimenter; rather, these laws are passed by state legislatures, whose members have complex objective functions and often respond to conditions within the state.

Note that this is a phenomenon that appears in many discussions of policy, not just in those about Unilateral Divorce Law: In the case of crime, this comes in the form of states with higher crime passing tougher crime laws[Besley and Case, 2000]. Environmental law [Sunstein, 1993] and Tiebout sorting [Bayer et al., 2004] are other fields where it has greatly concerned researchers. In the case of divorce, we give table 4.1 (included in the appendix) showing the correlation between state divorce rates and those states which have passed no-fault divorce laws. While it is difficult to see an overall trend in the data when presented in this form, one stylized fact deserves mention: of the nine states that instituted their laws in 1967 or sooner, most are in the bottom half in terms of growth rate of divorces. Since these laws make divorce easier, their association with a relative (in comparison to other states) decrease is noteworthy, and a sign of endogeneity of policy.

In estimating equation 4.1, this endogeneity takes the form of non-zero correlation between X_s and both γ_s and ε_s. While the former correlation is between observables, and therefore not a problem, the latter is much more of a concern, particularly if ε includes a component that is observable to the law makers and not to the econometrician.

A second difficulty in estimating equation 4.1 is the occurrence of unobserved heterogeneity between the states. There are a whole host of forms this can take, from differences in culture and institutions, attitudes of the residents, and latent differences in values and attitudes. A common practice for dealing with both of these difficulties is to use state-level fixed effects. This effectively allows for γ_s to absorb all of the variance between states, and means that the estimation of β depends entirely on differences within states.

Unfortunately, this makes identification of β impossible without additional years of data, and having data that are not a pure cross-section or short panel may be impossible for some applications. In the case of divorce, a lengthy panel of data at the county level are available, meaning that we can use our data to test this method for use in other applications. Accounting for the panel structure of the data means we are now estimating a different equation:

$$Y_{s,t} = X_{s,t}\beta + \gamma_s + \varepsilon_{s,t} \tag{4.2}$$

A second problem now becomes apparent, which is that these fixed effects account for different levels, but they will not identify divergent dynamics among states. It is entirely

	State	1966	1968	Year Law	Growth		State	1966	1968	Year Law	Growth
	Rhode Island	1273	971	1976	-.135		Michigan	21727	25405	1972	.078
	New Mexico	3054	2585	1973	-.083		Maine	2653	3105	1973	.078
	Massachusetts	10140	9993	1975	-.007	★	Oklahoma	12252	14367	1967	.079
	Nevada	9733	10103	1973	.018		Colorado	7253	8552	1971	.082
	Idaho	3062	3278	1971	.034		Dist. of Colum.	1867	2204	1977	.082
★	West Virginia	3901	4243	1967	.042	★	Alaska	1112	1315	1967	.083
	California	69127	75416	1970	.043		Missouri	13758	16475	1973	.090
★	North Carolina	11414	12461	1967	.043		Pennsylvania	16940	20326	1980	.091
★	Utah	3090	3390	1967	.046		Georgia	12921	15590	1973	.093
	Wyoming	1461	1610	1977	.048		Minnesota	5507	6704	1974	.098
	New Jersey	7195	7944	1971	.049		New Hampshire	1702	2073	1971	.098
★	Louisiana	3452	3826	1967	.051		Oregon	6757	8258	1973	.100
	Mississippi	6306	7020		.053	★	Vermont	621	760	1967	.100
★	Virginia	9512	10677	1967	.057		Florida	25801	31620	1971	.101
	Ohio	27914	31655	1974	.062		Montana	2087	2598	1975	.109
	Texas	43046	48852	1974	.063		Connecticut	4169	5351	1973	.124
	North Dakota	759	868	1971	.067		Delaware	797	1023		.124
	Nebraska	2627	3010	1972	.068		South Dakota	949	1245	1985	.135
	Alabama	11256	12918	1971	.068		Wisconsin	5293	6967	1977	.137
	Tennessee	11701	13472		.070		Indiana	17743	23593	1973	.142
★	Maryland	7072	8151	1967	.070		Washington	11307	15789	1973	.166
	Illinois	27862	32119	1984	.071		Kentucky	6840	9821	1972	.180
	Arkansas	7354	8483		.071		South Carolina	3123	4518	1969	.184
	Iowa	5571	6464	1970	.074		New York	7317	14861		.354
	Kansas	6144	7144	1969	.075		Hawaii	897	1865	1973	.365
	Arizona	9186	10701	1973	.076						

Table 4.1. State tabulation of divorce rates and laws; those states with divorce laws prior to 1968 are indicated with ★; Year Law indicates the year in which unilateral divorce law was implemented in each state.

likely that using fixed effects can make things worse rather than better. Relaxing the assumption further–as in [Friedberg, 1998]–to state-time trends still imposes a linearity assumption in the difference in trajectories.

A very nice example of the heterogeneity between states can be observed in the comparison of Indiana and Washington. Both states passed their laws in 1973, and–while they exhibit similar general trends–the Indiana data show more volatility, both upward and downward.[2] For informed readers who protest that Indiana–a conservative, agrarian state with many rural areas–is different from Washington, with its hip cultural core in Seattle, this difference is the very point that we are making: nearly $\frac{1}{3}$ of Indianans live in rural areas, while less than $\frac{1}{5}$ of Washingtonians do. In this case, we are lucky to have an observable variable that so readily differentiates Indiana and Washington, but this is often not the case.[3]

Rather than attempting to tweak these assumptions further, we offer a different method, which is an adjustment of standard differences-in-differences estimation particularly suited to cross-sections and short panels.

4.2.2. The Border Counties Method

Our method is to estimate the policy effects by disaggregating and choosing a subsample–counties that border other states–over which the data will be less-biased than otherwise. It is most precisely described in the context of a two state problem:

[2]While there are some issues with reporting of the data in Indiana, this graph characterizes the balanced panel of counties well, too.

[3]We are particularly concerned with the extent to which this heterogeneity is an expression of liberal attitudes, particularly toward the proper conduct of marriage and divorce: if divorce rates are lower in liberal states because marriage is delayed, and these states also implement unilateral divorce sooner, then it may appear as though the laws have a larger effect than they really do.

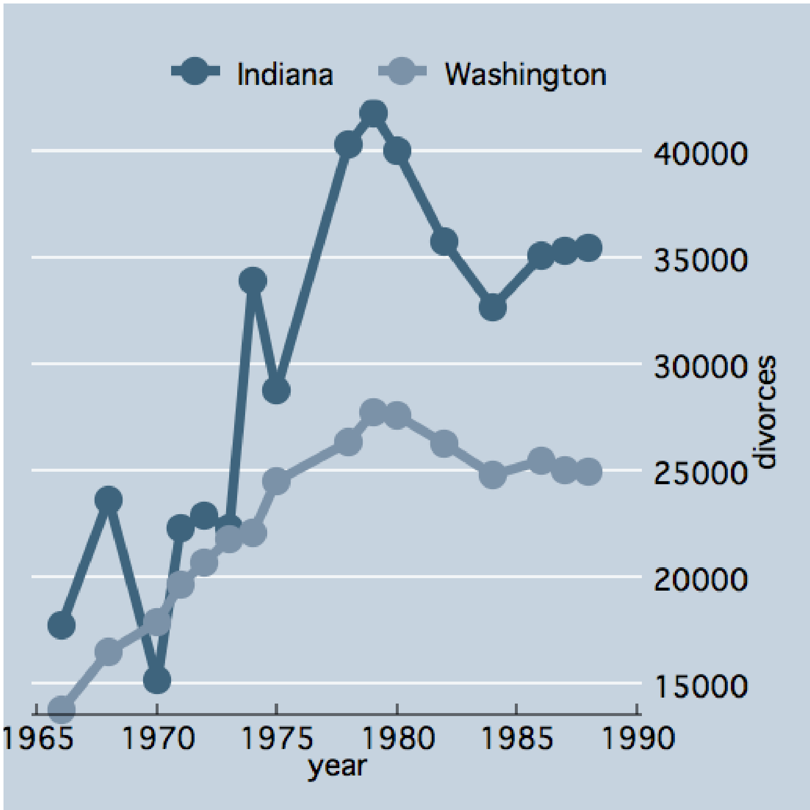

Figure 4.2. Divorce Dynamics

Suppose that A and B are contiguous states, with A being the treatment state state. Our procedure is:

- disaggregate the data into county-level data, so that data are now for counties in A: $A_1, A_2, \ldots$ and counties in B: $B_1, B_2, \ldots$
- using only the data for those counties that border the other state,
- perform differences-in-differences estimation on those counties, with the counties in state B serving as the control group
- because the treatment is still applied at the level of the state, Huber-White Standard Errors–grouped by state–need be calculated

There are several scholars who have used this method or variants already:

As described with the two states, this method is similar to that used by Card and Krueger [1994] in their examination of the effects of a New Jersey minimum wage increase to test for its impact on unemployment in fast food restaurants along the Pennsylvania/New Jersey border. A second paper that uses a bi-state comparison is that of Sloan et al. [1988], who use the US-Canadian border to create a policy experiment in gun laws to examine their effect on crime. An oversight of this paper is that difference in minority crime drove their results, and Seattle and Vancouver have very different compositions of their minority populations. Fox [1986] uses three metropolitan areas that straddle state borders to find that increases in sales tax rates decrease the level of retail activity.

While there is a large literature that identifies controls in this way, [Shadish et al., 2002] we mention Isserman and Rephann [1995] in particularly for using a strategy of identifying a twin county outside of Appalachia for each Appalachian county–based on demographics–to study the effects of the Appalachian Regional Commission.

Thomas J. Holmes [1998] separates states into pro- and anti-business categories based on whether they have closed shop laws as well as the ratings of a consulting firm. He then compares rates of employment in manufacturing between the border counties, finding that manufacturing employment jumps by an average of $\frac{1}{3}$ when crossing from the anti-business side of a borer to the pro-business side. Stephen Billings [2007] also looks at business, focusing specifically on the establishment of enterprise zones. In another paper, the entire US-Canadian border is considered as a unit, with exchange rate fluctuations taken to be exogenous shocks to demand from the perspective of consumers living on the border.[Campbell and Lapham, 2004]

At a smaller scale than county-level, Sandy Black [1999] analyzes the willingness of parents to pay more for houses that correspond to better schools. By comparing houses that are identical except for the fact that they are on different sides of a school district boundary, she finds that parents are willing to pay 2.5% more for a school with average test scores of 5% higher.

However, none of these other papers propose that this method may counteract law endogeneity, while this one does. In addition, we make a first attempt to weed out ineffective borders in section 4.6, and we are the first to systematically apply this border effects methodology to a non-business-related policy.

Intuitively, this method addresses the above problems for two reasons: first, the counties in both states are a small subset of the treated state population. One can readily see that this method is unlikely to solve the problem of law endogeneity completely, and we hypothesize that the importance of border residents' concerns depends upon the presence of major cities or voting blocks that are centralized within the state. However, introducing a measure of regional importance in determining policy is a way to use an observable exogenous measure to deal with the endogeneity problem.

There are some potential biases that concern us: first, the border counties could differ systematically because they are border counties: for example, border county voters might be more supportive of tougher state-level crime policy because they are the most exposed to a rival states' crime policies, i.e. the crackdown of a neighboring state would affect them immediately, and the interior of the state only late. While divorce is not a phenomenon that can be relocated as easily as a business (there is a certain air of implausibility surrounding a couple who relocate from St. Louis, MO, to East St. Louis, IL, in order to give themselves the joint outlet of unilateral divorce), this is a concern if border-county residents are more clamorous than the average state residence. While we expect this effect to make our estimated policy effect sizes be smaller than the actual, we are able to test for this in section 4.4.

Second, both demographic variables and our divorce outcome co-move more closely between contiguous counties that touch along a state border than they do between a pair of arbitrary counties from two different states. Unfortunately, the effect of the border may prove to be much stronger than just in divorce law. If there are many sets of state policies–besides divorce law–that are different, this would bias our effects upwards. While we would like to test for the effect of divorce law on divorce rates, independent of minimum wage policy, we can only rely on the geographic similarity of the border counties to reduce, not eliminate, this upward bias.

One concern that does not worry us is that of discarding data. While there is some sacrifice in terms of efficiency, discarding data that are flawed can still be productive, as estimators based on the remaining data can still be consistent.[Levitt, 1994]

A second concern is the degree to which laws are driven by the conservative- or liberal tendencies of state cultures. If these are static, then state effects should control for them adequately; unfortunately, the long length of time makes this assumption seem overly strong; since our border method includes very few coastal counties, we are concerned that it is omitting many places from the most liberal states.

4.2.3. No-Fault Divorce Law

The movement of states to implement no-fault-divorce laws happened swiftly and thoroughly. A table of every state is provided in the appendix. While all but 5 states have implemented them, half did so between 1970 and 1975.[4] This rush in the 1970's meant that the phenomenon attracted the attention of famous scholars: representing the Chicago school, Gary Becker [1991] suggested that unilateral divorce merely redistributes the surplus between the two parties, which–according to the Coase Theorem[5]–should not lead to an identifiable change in behavior; both parties will still bargain to the efficient outcome regardless of the initial assignment of property rights.

Two particular recent studies support the alternative view that a change in laws should lead to a change in behavior; while this may seem intuitive to the lay person, it is also a very reasonable conclusion, in that Unilateral divorce laws reduce the price of a good: divorce. Nakonezny et al. [1995] find that no-fault divorce laws have a significant positive impact upon divorce rates; Leora Friedberg [1998] accounts for both time- and state specific trends to verify this finding as well. However, her result fails to explain the even greater increase in the divorce rates in states that–before 1988–did not adopt no-fault divorce laws.

By more carefully thinking about the stock/flow relationships surrounding marriage, Justin Wolfers [2006] suggests that–while marriages are released by no-fault divorce during the earlier years of its enactment–the long-term effects are much more negligible as many

[4]A strength of this paper is that we have brought to bear annual data during this period for a richer analysis of short-term effects than was possible in earlier papers.

[5]The obvious caveat is the absence of transaction costs. Perhaps out of their naïveté the unmarried authors assume that there could be no smaller transaction costs than those associated with inter-household bargaining.

divorcees are likely to remarry quickly. While our results support Wolfers' claim, there is an important distinction: Wolfers' finds that the effect of Unilateral Divorce law changes depending on the time frame examined, while our method is intended to be used for those applications where time frame cannot be readily identified, i.e. short panels or cross-sections.

4.3. Similarity of Counties

Data on divorce are taken from the US Census Bureau, as well as the US Vital Statistics. Further details on our data can be found in our Data Appendix, section 4.8. In table 4.3, we show that–on average–border counties are very similar demographically to the interior counties.

Bordering States	Number of Counties
1	962
2	189
3	12
4	1

Table 4.2. Distribution of border counties and the number of additional states they border

While they do not differ systematically in age or wealth-related factors, there is some variation in the proportions of ethnic/race variables (an average of 20%-30% in the case of minority proportions), which provides more than sufficient motivation for us to control for those in the regressions in our empirical work below.

Here, we examine whether border counties are truly more similar to each other than are counties in the same state that are far away from each other. We do this by examining

year		All Counties	Border Counties
1990	Population	79000	81000
1990	Male pop 15-24	5976	6028
1990	Male pop 25-34	6866	6978
1992	White	60691	62756
1992	Black	10073	12152
1992	Latino	7749	6357
1992	Asian	2641	2093
1990	Med Home Value	\$52,403	\$54,814
1990	Per Capita Income	\$14,908	\$15,354

Table 4.3. Demographic Summary Statistics of Counties; unweighted means of sample.

all pairs of contiguous counties, indexed i and j. For any county-level outcomes, we can then estimate the equation

$$|Y_1 - Y_2| = DS \cdot \alpha + \varepsilon \tag{4.3}$$

in which DS is a dummy variable for whether the pair of counties are from the different states. The results of this estimation for unemployment rates are given in table 4.4, and–while the coefficient on DS is significant–suggesting an increased difference in unemployment rates for those counties divided by a state border–its magnitude is only 10% of the constant, suggesting only a small difference.

Next, we assign the counties to random partners out of the entire sample; these random partners could come from any location in the country; here, the effect of being in a different state is significant and greater. this means that the effect of a state border is to create differences between counties that are *far away* from each other, but geographic proximity lessens this. Counties that are close together are more similar. While table 4.4 only uses

pairs	Contiguous	Random
Dif State	0.212	0.88
	(0.051)	(0.24)
Constant	1.88	2.21
	(0.021)	(0.24)

Table 4.4. Regression Results over county pairs for effect of being in a different state on difference in unemployment rates; first column uses only contiguous pairs, while the second column results assign random pairs of counties; data are from 1990.

unemployment data, this result is robust across a series of other demographic measures, as can be seen in the additional tables in section 4.9.

4.4. Testing for Influence of Border Counties on State Policy

In this section, we look for evidence that certain areas of states drive policy. As mentioned above, this is of grave concern for policy analysis; although Nakonezny et al. [1995] find no evidence of endogeneity, their method fails to employ the temporal aspect of the data, meaning that we are obligated to verify this for ourselves. First, we examine how state characteristics affect the passage of no-fault-divorce laws. In the following table, we regress the year of passage of the law on the predictors shown, treating each state as a single observation; this is the method employed in [Bailey, 2005]. Unfortunately, because we only have one observation for each state, we do not employ the use of fixed effects; results are presented in table 4.4.

The only significant demographic predictors of the year no-fault-divorce-laws are passed are the proportions of the population that are either young or African American. In the

	(1)	(2)	(3)	(4)
	Dep Var: Year NFD law passed			
% male 1960	-3.489			
	(3.740)			
% black female 1960	0.897			
	(3.101)			
% non-white female 1960 > 21	1.941			
	(2.939)			
% rural population 1960	-0.817			
	(1.934)			
% race black 1970		3.390		17.050**
		(2.455)		(6.222)
% race other 1970		2.193		2.763
		(2.740)		(2.698)
% farm 1970		-0.504		-2.086
		(1.989)		(2.834)
% young 1970		-4.752		-7.050*
		(2.860)		(2.910)
wage ratio 1970		0.122		5.005
		(2.640)		(3.341)
divorce rate 1970		0.66		1.52
		(1.660)		(1.65)
change in % black, 1960-1970			-7.424	33.356*
			(5.467)	(14.473)
change in % rural, 1960-1970			0.070	1.561
			(1.025)	(1.871)
constant	1975**	1976**	1975**	1980**
	(1.603)	(1.565)	(1.660)	(2.405)
observations	49	50	49	49
R-squared	0.09	0.12	0.04	0.25

Table 4.5. OLS Regression for year of passage of unilateral divorce laws; independent variables vary by specification, but they include the indicated standardized state demographic characteristics; wage ratio denotes $\frac{w_f/F}{w_m/M}$; % young denotes percent who are of ages 20-45; standard errors in parentheses; * significant at 5%; **significant at 1%; note that Alaska and the District of Columnbia are omitted from some regressions.

case of youth, passing these laws earlier indicates a more progressive culture, with appropriate response by the legislature toward encouraging early marriage by making its dissolution easier. The African American marriage market is characterized by much more cautious entry into marriage, particularly by educated women.[Bennett et al., 1989]

4.5. Impact of No-Fault Divorce Laws on Divorce

In this section, we present various estimations culminating in the presentation of our border county method estimation results.

4.5.1. Instantaneous Effects

First, we give the standard OLS results using data at the county level. The equation that we estimate will be variants of equation 4.4.

$$y_{it} = \alpha + \beta U_{it} + \tau_t + \gamma_i + \psi_{i,t} + \xi_{i,t} \tag{4.4}$$

Here, y_{it} represents the average divorce rate, measured in divorces per 1000 people. The advantage of using a dependent variable that is scaled by population is that it is directly comparable between areas of different population; ultimately, however, we are interested in measuring the effect in terms of a number of people. When a large state–such as Texas–implements unilateral divorce, the treatment is being applied to a different number of people than when a small state–such as Vermont–does so. For this reason, all estimations in this section are carried out using weighted least squares, with weights determined by the contemporary population of the counties.

	(1)	(2)	(3)	(4)	(5)
nofault	1.5135***	0.3295***	0.1848	0.1996*	0.1980*
	(0.0431)	(0.0312)	(0.1359)	(0.0933)	(0.0928)
year effects	N	Y	Y	Y	Y
state effects	N	N	Y	Y	Y
state trends	N	N	N	Linear	Quadratic
Sample Size	50463	50462	50451	50457	50457
R-squared	0.010	0.018	0.056	0.062	0.062

Table 4.6. Estimation results; dependent variable is Divorce Rate per thousand people; unit of observation is county, with data from many years between 1966-1988. First column is OLS; year- and state effects are added in the next two columns; column four includes linear state trends, and the fifth column includes state quadratic trends. $nofault$ is an indicator for whether a county is subject to Unilateral/No-Fault divorce laws.

In table 4.6, we add the following additional specifications to the OLS estimates: year effects (for accounting for time trends), state effects, and both linear and quadratic state trends. These estimations parallel those of Friedberg [1998], with the final two accounting for divergent long-term paths in states.

Note that these state/time trends are not the same as state-year interaction effects, which are obtained by interacting the state effects with the year, so as to create a linear trend over time for the state; interacting the state effects with year effects would create a non-parametric trend that would absorb all divorce law effects, since their presence is exactly determined by state-year pairs. Robust standard errors are calculated.

The effect sizes that we find through this method are lower than those found through using this model at the state level [Friedberg, 1998, Wolfers, 2006], in which effects of 0.44 divorces per 1000 people are estimated, and these differences are significant. One contributing factor to this difference may be that some years of data are omitted from our analysis.[6] Another reason might be the change in size distribution among the units of analysis. While the weighting of state- (and county-) level observations correctly accounts for the aggregation of individuals, the distribution of county-level populations is more extreme: with reference to populations in 1980, the highest (Los Angeles County) has more than 7000 times the population of many of the lowest. In states the difference is two orders of magnitude smaller; a greater impact of Unilateral Divorce laws in low-population areas would be sufficient for this decrease in our estimates.

Lest our readers think that an effect of 0.2 divorces per 1000 people is a small effect, consider that this represents between 4% and 7% of all divorces in the US, depending on the year. Note that in these cross-sections, we can only address the instantaneous effect of the law; Wolfers [2006] raises dynamic concerns,[7] and to deal with these, we introduce a new measure of the length of time that one county in a pair was under a no-fault regime while the other was not.

Next, we tweak the above estimations by proceeding with our border-county structure. First, we present parallel estimations to those in table 4.6, except that the analysis is performed only over border counties. By restricting our sample in this way, identification is driven exclusively by the fringe of counties in the states. Other than for determining divorce law exposure, the estimates in table 4.7 ignore the state level of the data entirely; we merely substitute the more local notion of a state border, with border fixed effects and border trends. Therefore, effects presented are those that control for the idiosyncratic culture of nearby states that share a border.

---

[6] While both of the above-mentioned authors have a complete panel of states from 1968-1988, we only use a selected group of years out of these for our county-level panel; see the data appendix for more details.

[7] Increasing divorce initially creates a pool of newly single people who are available for re-marriage, and potentially additional divorce.

Observations are still weighted by population, although we must contend with the fact that approximately $\frac{1}{6}$ of the border counties border more than one other state.[8] We do this by splitting them by border, assigning equal fractions of the county's population to each border-county unit. Because we are weighting observations in this regression, this approach applies no more weight in determining effect sizes, while allowing us the flexibility to account for each of the multiple border interactions.

	(1)	(2)	(3)	(4)
nofault	1.0139**	-0.1521	-0.2975	0.0081
	(0.3590)	(0.5560)	(0.2302)	(0.1354)
year effects	N	Y	Y	Y
border effects	N	N	Y	Y
border trends	N	N	N	Linear
Sample Size	22471	22471	22471	22471
R-squared	0.030	0.123	0.521	0.542

Table 4.7. Estimation results; dependent variable is Divorce Rate per thousand people; unit of observation is county, with data from many years between 1966-1988. Only counties on the borders are included. First column is OLS; year- and state effects are added in the next two columns; column four includes linear state trends. $nofault$ is an indicator for whether a county is subject to Unilateral/No-Fault divorce laws.

The OLS estimates for the border counties alone match those for all counties fairly closely. However, once we put in time effects and border-based effects, the estimated effect of unilateral divorce laws on divorce rates rapidly diminishes; note that these estimates treat the borders as the only geographic constructs of relevance; without accounting for state heterogeneity in any way, the effect of Unilateral Divorce appears to be negative, with some slight significance. The authors make no claim that this is the true effect:

[8]Note that there are separate observations for each of these counties, but the weighting by population means that they are not over-represented in the sample.

rather, we present this table to impress upon the reader the importance of state effects, which we implement in a variety of ways in table 4.8.

	(1)	(2)	(3)
nofault	-0.0520	0.1105	-0.0697
	(0.0992)	(0.0722)	(0.0578)
border effects	trend	Y	fixed effects
state effects	Y	trend	Random Effects
Sample Size	22471	22471	22897

Table 4.8. Estimation results; dependent variable is Divorce Rate per thousand people; unit of observation is county, with data from many years between 1966-1988. Only counties on the borders are included, with effects for border regions and states as indicated. $nofault$ is an indicator for whether a county is subject to Unilateral/No-Fault divorce laws.

The first column includes state effects as well as trends for each individual border-region. For column two, the trends are over states rather than borders. Columns three and four are two attempts at relaxing state fixed effects to a state random effect–through maximum likelihood and then through a mixed effects estimation. While there is one instance of a marginally significant coefficient, the economic significance of unilateral divorce hovers around zero for all of these estimations.

4.5.2. Temporal Effects

For all intents and purposes, using our border county method shows little evidence that Unilateral divorce has any serious effect on divorce rates. However, Justin Wolfers [2006] points out that a temporal dimension is very important in assessing the effects of these laws. While we include time effects in the above estimations, we do not explicitly test for dynamics in the path of divorce rates after the passage of a law. Here, we do so: we use the number of years during which a pair of counties had different laws as an independent variable; our dependent variable is the difference in divorce rates per thousand people.

To do so, we divide our border counties them by length of time for which they've been exposed to the laws, running separate estimations over subgroups of county pairs which are sorted by the length of time the pairs have been under different regimes with respect to divorce laws. We include difference in per-capita income as a demographic control.

	Dep Var: difference in $\frac{Div}{000}$			
	(1)	(2)	(3)	(4)
	$d < 5$	$5 < d < 10$	$10 < d$	all
Years of law difference	-0.019	-0.009	-0.025	0.020
	(0.011)	(0.022)	(0.131)	(0.030)
constant	-0.278	-0.009	-0.015	-0.217
	(0.165)	(0.111)	(0.117)	(0.148)
sample size (pairs)	80767	2096	2804	85667
Number of state-borders	159	55	32	159

Table 4.9. Fixed Effects Panel Regressions for divorce rate differences over pairs of counties, with duration of difference in divorce laws given by d in (1) - (3). Standard Errors in parentheses.

In the long run, this means that the effects of the no-fault-divorce laws are negligible, which confirms the findings of Wolfers [2006]. Note that for those pairs which had a difference in laws for longer than ten years, there is a slight *decrease* in divorce rates, although it's significance is weak. Recalling the result from Friedberg [1998], in which she shows that divorce rates actually increase more for those states that do not implement no-fault-divorce laws, this is what we would expect.

4.6. Assessing Borders Individually for Estimation of Policy Effects

There are various reasons that some borders might prove more effective than others for assessing policy effects using this method. When grappling with the issue of endogeneity, four states have their mega cities located in border counties. These states (and their cities) are Illinois (Chicago), Minnesota (Minneapolis-St. Paul twin cities area), New York (New York City), and Pennsylvania (Philadelphia).

A quick way to verify the results would be to repeat the above estimations without these counties.

However, the more detailed procedure would be to characterize each border[9] in terms of its similarity to corresponding counties as well as its influence on policy. Intuitively, when a state's hinterlands co-move with the capital, then it is impossible to distinguish whether policy responses are being driven by conditions in one region or the other. Therefore, we seek to identify borders of similar counties that exist between very dissimilar states.

In order to do this, all interior counties will be tracked, with the correlation of the sums of these interiors determined. In table 4.6, we give the correlation matrix for change in divorce levels over the interiors of all Southern States. For studying policy effects, the most desirable borders would be those with high co-movement who fall between neighboring states whose interiors have low co-movement.

An initial examination of the southern states suggests that bordering states have more highly correlated interiors. Nevertheless, we find a gem almost immediately from the state of West Virginia: changes in divorce levels in its interior are actually negatively correlated with those in the interiors of both Kentucky and Maryland.

[9]Here, we use the term "border" to refer to the collection of counties in one state that are contiguous with those of a single adjacent state, i.e. Texas-New Mexio.

	MD	VA	WV	NC	SC	GA	FL	KY	TN	AL	MS
Maryland	1.0000										
Virginia	0.0168	1.0000									
WestVirginia	-0.0762	0.5764	1.0000								
NorthCarolina	0.2688	0.6322	0.5947	1.0000							
SouthCarolina	0.7050	0.4177	0.2778	0.5166	1.0000						
Georgia	0.4984	0.4889	0.5629	0.6502	0.6707	1.0000					
Florida	0.6075	0.3958	0.2761	0.6334	0.8051	0.7924	1.0000				
Kentucky	0.6440	0.4404	0.0301	0.2066	0.7662	0.5570	0.7066	1.0000			
Tennessee	0.4570	0.5757	0.5013	0.7330	0.6927	0.8952	0.8325	0.5382	1.0000		
Alabama	0.5377	0.5231	0.4962	0.8217	0.7637	0.8105	0.9121	0.5801	0.8354	1.0000	
Mississippi	0.5224	0.2196	0.4181	0.6157	0.6681	0.6576	0.7018	0.3181	0.7425	0.6984	1.0000

Table 4.10. Correlations in divorce rates for the interiors of all southern states. Geographically neighboring states' correlations are underlined.

To follow this up, we investigate the correlation of the border counties in these three states further: while the Maryland-West Virginia border counties co-move with each other to some degree ($\rho = 0.294$), the contiguous border counties along the West Virginia-Kentucky border co-move quite closely, with a correlation coefficient of 0.781. Note that West Virginia begins my sample having already instituted no-fault divorce, while Kentucky implemented it in 1972.

	year	Counties in West Virginia	Counties in Kentucky
	1966	2.33	2.38
	1968	3.16	3.18
	1970	4.49	4.17
	1971	4.15	3.77
KY	1972	4.25	4.13
passes	1973	4.03	4.33
law	1974	4.76	4.58
	1975	5.31	4.96
	1978	5.43	5.22
	1979	5.41	5.58
	1980	6.39	6.23
	1982	5.87	6.61
	1984	5.53	6.87
	1986	5.53	6.88
	1987	5.70	7.14
	1988	5.69	7.10

Table 4.11. Divorce rates per thousand people in counties along the WV-KY border, reported separately by state groupings; note that West Virginia had already implemented unilateral divorce before the beginning of this series.

In table 4.11, we present levels in divorce, aggregated over all of the border counties within each state; Kentucky's numbers have been normalized so that 1966 is equal for both states.

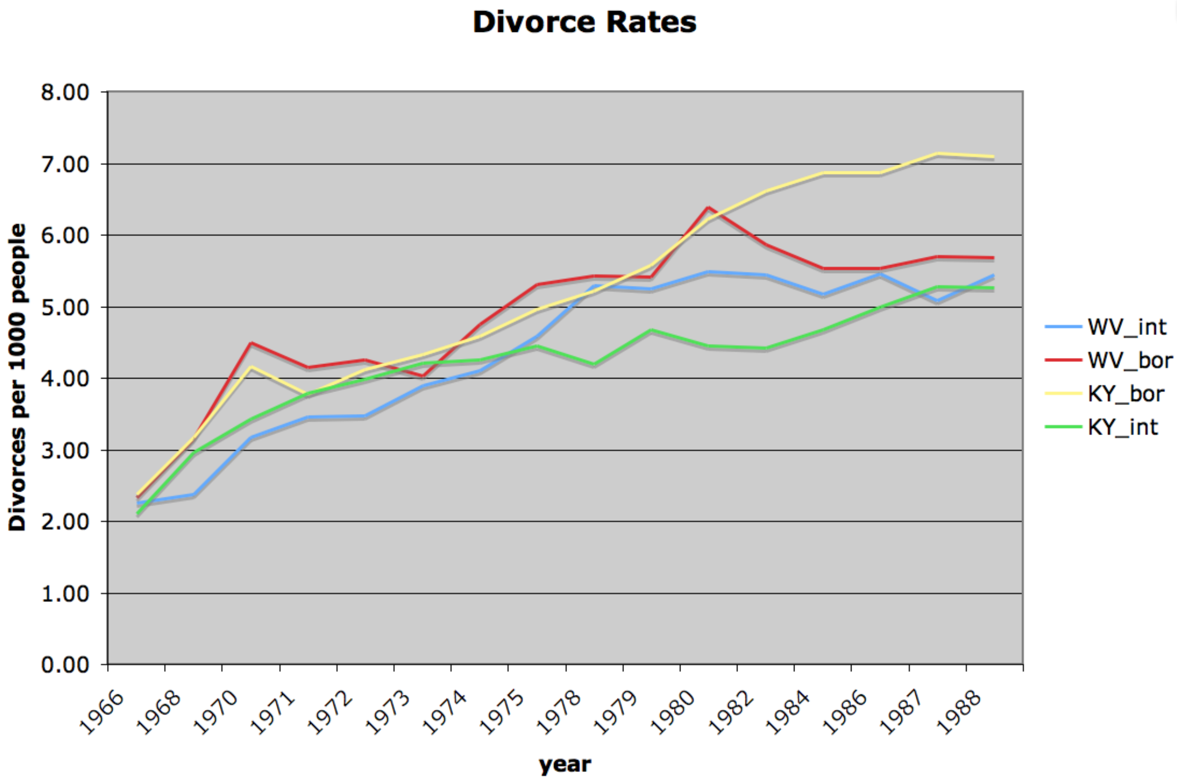

Figure 4.3. Border/interior comparisons of divorce rates for counties in West Virginia and Kentucky.

These divorces represent approximately 5% of the total of West Virginia's interior and 8% of Kentucky's interior, and these are fractions that stay constant throughout the sample period. With no-fault divorce in place only in West Virginia, there is an initial outstripping of the growth in divorce on the Kentucky side; however, once Kentucky implements their law, the the two regions maintain similar levels all the way until 1982, when Kentucky seems to pull away from WV permanently to a level approximately 25% higher. While this behavior matches well a story where the release of the stock of bad marriages is a gradual one–lasting perhaps as long as 15 years–the obvious problems with sample size and time effects prevent us from making a certain inference from this example.

There is no "smoking gun" here for those who would hope to find evidence of no-fault divorce laws increasing divorce rates.

Nevertheless, the illustration of how to find a quality border has been made clear; we wish to extend this method, with a most obvious extension testing borders of states that are split by regional boundaries. One shortcoming of this method is that we use divorce-levels to identify the border, which are in-turn affected by the outcome we examine. A more suitable method of identifying this border would be to use an outcome that is not correlated with no-fault divorce laws in either direction.

4.7. Conclusion

In this paper, we apply a border-county-based methodology to estimate the effect of no-fault-divorce laws on the rate of divorce. This is the first application of this method to a non-business policy application; the availability of panel data on divorce allows an evaluation of the method, which is ideally suited to situations in which only a short panel or a cross section is available.

Our results confirm the findings already existing in the literature that–while in the short term no-fault divorce causes a release in the stock of bad marriages, the long-term effect of these laws is negligible. While many readers might expect a change in laws to cause a change in behavior, this result is actually consistent with the Coase Theorem, in that the change in rights accorded by the law does not prevent husbands and wives from continuing to bargain to the efficient outcome.

In addition, we propose a method for evaluating the quality of borders by examining the co-movement of divorce levels in states' interiors versus their border regions. We illustrate the example of West Virginia and Kentucky to explore this method, which shows promise.

4.8. Appendix – Treatment of Data

The divorce data in years 1966, 1968, 1971-1974 come from the US Vital Statistics. Data for the remaining years come from a proprietary publication of US Census Data distributed by Geolytics, distributed in compact disk form.[Geolytics, 1998] The counties are linked using a file that gives contiguous pairs of counties that is available from Thomas J. Holmes [1998]

After this dataset is created, there are still numerous decisions that must be made concerning how to transform the data into a form usable for my analyses described above. One issue was the transformation of the raw data from US Vital Statistics, which are reported in aggregate numbers of divorces for each place into divorce rates. From the *Geolytics* disk, we were able to obtain county-level population data for the years 1970 and 1975. Unfortunately, because of the preponderance of laws passed between those two years, we were acutely interested in changes in divorce rates during that time. In order to construct the rates for those years in between, we used linear combinations of the populations that were available for the two boundary years according to the following schedule:

$$\delta_{1970+i} = \frac{D_{1970+i}}{\frac{i}{5}Pop_{1975} + \frac{5-i}{5}Pop_{1970}}$$

We are happy to provide data and .do files upon request, although–because of their size–the logistics of doing this may cause some delays.

4.9. Appendix – Additional Tables

Year	state	FIPS	Year	state	FIPS	Year	state	FIPS
1971	Alabama	1	1972	Kentucky	21	1971	North_Dakota	38
1967	Alaska	2	1967	Louisiana	22	1974	Ohio	39
1973	Arizona	4	1973	Maine	23	1967	Oklahoma	40
0	Arkansas	5	1967	Maryland	24	1973	Oregon	41
1970	California	6	1975	Massachusetts	25	1980	Pennsylvania	42
1971	Colorado	8	1972	Michigan	26	1976	Rhode_Island	44
1973	Connecticut	9	1974	Minnesota	27	1969	South_Carolina	45
0	Delaware	10	0	Mississippi	28	1985	South_Dakota	46
1977	DC	11	1973	Missouri	29	0	Tennessee	47
1971	Florida	12	1975	Montana	30	1974	Texas	48
1973	Georgia	13	1972	Nebraska	31	1967	Utah	49
1973	Hawaii	15	1973	Nevada	32	1967	Vermont	50
1971	Idaho	16	1971	New_Hampshire	33	1967	Virginia	51
1984	Illinois	17	1971	New_Jersey	34	1973	Washington	53
1973	Indiana	18	1973	New_Mexico	35	1967	West_Virginia	54
1970	Iowa	19	0	New_York	36	1977	Wisconsin	55
1969	Kansas	20	1967	North_Carolina	37	1977	Wyoming	56

Table 4.12. States and Years they implemented no-fault-divorce; '0' indicates that no-fault divorce laws have never been passed in that state; 1967 denotes a law already being in place before 1968.

pairs	Contiguous	Random
Dif State	0.0038	0.03
	(0.0012)	(0.006)
Constant	0.025	0.038
	(0.0005)	(0.006)

Table 4.13. Difference in % Black Females, 1990

pairs	Contiguous	Random
Dif State	0.033	0.020
	(0.031)	(0.012)
Constant	0.081	0.066
	(0.0013)	(0.013)

Table 4.14. Difference in Doctors per 1000 people, 1990

References

Office of Admissions. The number of texas high schools represented in the entering freshman classes of the university of texas at austin, 1996-2004. Technical report, The University of Texas at Austin, 2005. URL `http://www.utwatch.org/oldnews/toptencap_5_13_05.html`.

Q. Natoya Alee. The florida bright futures program: An analysis of cost saving policy options. Technical report, Florida State University, 2004. Master's Thesis.

Peter Arcidiacono. Ability sorting and the returns to college major. Technical report, Duke University, 2003.

Matha J. Bailey. More power to the pill: the impact of contraceptive freedom on women's life cycle labor supply. Technical report, University of Michigan, September 2005.

Patrick Bayer, Fernando Ferreira, and Robert McMillan. Tiebout sorting, social multipliers and the demand for school quality. Working Paper 10871, National Bureau of Economic Research, November 2004. URL `http://www.nber.org/papers/w10871`.

Gary S. Becker. A theory of marriage: Part i. *Journal of Political Economy*, 81(4):813–46, July-Aug. 1973. URL `http://links.jstor.org/sici?sici=0022-3808%28197307%2F08%2981%3A4%3C813%3AATOMPI%3E2.0.CO%3B2-4`.

Gary S. Becker. *A Treatise on the Family*. Harvard University Press, 1991.

Neil G. Bennett, David E. Bloom, and Patricia H. Craig. The divergence of black and white marriage patterns. *The American Journal of Sociology*, 95:692–722, 1989.

Mark C. Berger. Predicted future earnings and choice of college major. *Industrial and Labor Relations Review*, 41(3):418–429, April 1988.

Marianne Bertrand, Esther Duflo, and Sendhil Mullainanthan. How much should we trust differences-in-differences estimates? Technical Report 8841, National Bureau of Economic Research, 2002.

Timothy Besley and Anne Case. Unnatural experiments? estimating the incidence of endogenous policies. *Economic Journal*, 110(127):F672–94, 2000.

Stephen Billings. Do enterprise zones work? an analysis at the borders. working paper, Social Science Research Network, 2007.

Dan Black and Jeffrey Smith. How robust is the evidence on the effects of college quality? evidence from matching. *Journal of Econometrics*, 121(1-2):99–124, July-Aug. 2004.

Sandra Black. Do better schools matter? parental valuation of elementary education. *Quarterly Journal of Economics*, 114(2):577–600, 1999.

Francine Blau, Lawrence Kahn, and Jane Waldfogel. Understanding young women's marriage decisions: The role of labor and marriage market conditions. *Industrial and Labor Relations Review*, 53(4):624–647, 2000.

David E. Bloom and Neil G. Bennett. Marriage patterns in the united states. working paper 1701, NBER, September 1985.

H. P. Blossfeld and J. Huinik. Human capital investments or norms of role transition? how women's schooling and career affect the process of family formation. *American Journal of Sociology*, 97(1):143–168, 1991.

H. P. Blossfeld and Ursula Jaenichen. Educational expansion and changes in women's entry into marriage and motherhood in the federal republic of germany. *Journal of Marriage and the Family*, 54(2):302–315, 1992.

Brahim Boudarbat and Claude Montmarquette. Choice of university major in canada. Technical report, Universite de Montreal, http://www.econ.ubc.ca/ine/target/paper/Boudarbat-Montmarquette.pdf, April 2006.

Dominic J. Brewer, Eric R. Eide, and Ronald G. Ehrenberg. Does it pay to attend an elite private college? cross-cohort evidence on the effects of college type on earnings. *Journal of Human Resources*, 34:104–23, 1999.

Brian Bucks. The effect of texas' top ten percent plan on college choice. Technical report, University of Texas at Dallas, 2004.

Stephen V. Cameron and Christopher Taber. Estimation of educational borrowing constraints using returns to schooling. *Journal of Political Economy*, 112(1):132–182, 2004.

Jeffrey R. Campbell and Beverly Lapham. Real exchange rate fluctuations and the dynamics of retail trade industries on the u. s.-canada border. *American Economic Review*, 94(4):1194–1206, 2004.

David Card and Alan Krueger. Minimum wages and employment: A case study of the fast-food industry in new jersey and pennsylvania. *American Economic Review*, 84(4): 772–93, 1994.

David Card and Alan B. Krueger. Would the elimination of affirmative action affect highly qualified minority applicants? evidence from california and texas. NBER Working Papers 10366, National Bureau of Economic Research, Inc, March 2005. available at http://ideas.repec.org/p/nbr/nberwo/10366.html.

Kerwin Kofi Charles and Ming Ching Luoh. Male incarceration, the marriage market, and female outcomes. University of Michigan and NBER, 2003.

Pierre-Andre Chiappori, Murat Iyigun, and Yoram Weiss. Investment in schooling and the marriage market. Technical Report 2454, IZA Discussion Paper, 2006.

Eugene Choo and Aloysius Siow. Who marries whom and why. *Journal of Political Economy*, 114:175–201, 2006.

Mary Sue Coleman. Statement by university of michigan president mary sue coleman to u-m board of regents. Technical report, University of Michigan, 2003.

Timothy Conley and Christopher Taber. Inference with 'difference in differences' with a small number of policy changes. Technical Report T0312, NBER, July 2005.

Christopher Cornwell, David B. Mustard, and Deepa J. Sridhar. The enrollment effects of merit-based financial aid: Evidence from georgia's hope program. *Journal of Labor Economics*, 24(4):761–786, 2006.

Julie Cullen, Mark Long, and Randall Reback. Jockeying for position: High school mobility and texas' top ten percent rule. Technical report, UC San Diego, 2005.

Janet Currie and Enrico Moretti. Mother's education and the intergenerational transmission of human capital: Evidence from college openings and longitudinal data. NBER Working Papers 9360, National Bureau of Economic Research, Inc, December 2002. URL `http://ideas.repec.org/p/nbr/nberwo/9360.html`. see also QJE 2003.

Stacy Berg Dale and Alan B. Krueger. Estimating the payoff to attending a more selective college: An application of selection on observables and unobservables. *Quarterly Journal of Economics*, 117(4):1591–1527, 2002.

Lisa M. Dickson. The changing accessibility, affordability, and quality of higher education in texas. In *CHERI Policy Research Conference Assessing Public Higher Education in the* 21^{st} *Century*, 2005.

John DiNardo and David S. Lee. The impact of unionization on establishment closure: A regression discontinuity analysis of representation elections. NBER Working Papers 8993, National Bureau of Economic Research, Inc, June 2002. available at http://ideas.repec.org/p/nbr/nberwo/8993.html.

Maureen Dowd. What's a modern girl to do? *New York Times*, 2005. URL `http://www.truthout.org/issues_05/103105WA.shtml`.

Susan M. Dynarski. Does aid matter? measuring the effect of student aid on college attendance and completion. *American Economic Review*, 93(1):279–288, March 2003. available at http://ideas.repec.org/a/aea/aecrev/v93y2003i1p279-288.html.

Susan M. Dynarski. The new merit aid. In Caroline M. Hoxby, editor, *College Choices: The Economics of Where to Go, When to Go, and How to Pay for It*, pages 63–100. The University of Chicago Press, 2004.

Jianquing Fan. Design adaptive nonparametric regression. *Journal of the American Statistical Association*, 87:998–1004, 1992.

William F. Fox. Tax structure and location of economic activity along state borders. *National Tax Journal*, pages 387–401, 1986.

Leora Friedberg. Did unilateral divorce raise divorce rates? evidence from panel data. *American Economic Review*, 88(3), 1998.

Suqin Ge. Women's college choice: How much does marriage matter? Technical report, University of Minnesota and Federal Reserve Bank of Minneapolis, 2007.

Geolytics. Censuscd blocks: complete US block data and maps. electronic resource, 1998.

Arthur Goldberger. Selection bias in evaluating treatment effects: Some formal illustrations. Technical report, Madison IRP, 1972. Discussion paper 123-72.

Claudia Goldin. The meaning of college in the lives of american women: the past one-hundred years. working paper 4099, National Bureau of Economic Research, June 1992. URL `http://www.nber.org/papers/w4099`.

Claudia Goldin. The long road to the fast track: Career and family. Technical Report 10331, NBER, 2004.

Joshua Goldstein and Catherine Kenney. Marriage delayed or marriage foregone? new cohort forecasts of first marriage for u.s. women. *American Sociological Review*, 66: 506–519, 2001.

Jinyong Hahn, Petra Todd, and Wilbert Van der Klaauw. Identification and estimation of treatment effects with a regression-discontinuity design. *Econometrica*, 69(1):201–209, 2001.

Daniel S. Hamermesh and Stephen G. Donald. The effect of college curriculum on earnings: An affinity identifier for non-ignorable non-response bias. Technical report, The University of Texas at Austin, 2006.

Donald E. Heller. Early commitment of financial aid eligibility. *American Behavioral Scientist*, 49:1719–1738, 2006.

Danielle Holley and Delia Spencer. The texas ten percent plan. *Harvard Civil Rights-Civil Liberties Law Review*, 34:264–296, 1999.

Thomas J. Holmes. The effects of state policies on the location of industry: Evidence from state borders. *Journal of Political Economy*, 106(6), 1998.

Catherine L. Horn and Stella M. Flores. Percent plans in college admissions: A comparative analysis of three states' experiences. Technical report, The Civil Rights Project at Harvard University, 2003.

Jessica Howell. Assessing the impact of eliminating affirmative action in higher education. Technical report, University of California, Sacramento, 2004.

Caroline M. Hoxby. The return to attending a more selective college: 1960 to the present. Technical report, Harvard University, 1998.

Felicia Ionescu and Linnea Polgreen. Brain drain and public funding for higher education in the us. Technical report, University of Iowa, 2008.

Andrew Isserman and Terance Rephann. The economic effects of the appalachian regional commission: An empirical assessment of 26 years of regional development planning. *Journal of the American Planning Association*, 61(3):345–364, 1995.

Brian A. Jacob. Where the boys aren't: Non-cognitive skills, returns to school and the gender gap in higher education. *Economics of Education Review*, 21:589–598, 2002.

Brian A. Jacob and Lars Lefgren. Remedial education and student achievement: A regression-discontinuity analysis. *Review of Economics and Statistics*, 86(1):226–244, 2004.

John F. Kain and Daniel M. O'Brien. Hopwood and the top 10 percent law: How they have affected thecollege enrollment decisions of texas high school graduates. In *Mellon Foundation, Diversity in Higher Education Conference*, 2001.

Thomas J. Kane. Rising public college tuition and college entry. Technical Report 5164, National Bureau of Economic Research, Inc, October 1995. available at http://papers.nber.org/papers/W5164.

Changhui Kang. University prestige and choice of major field: Evidence from south korea. Technical report, National University of Singapore, 2004.

Yolanda K. Kodrzycki. Issues in economics: Retaining college graduates in the workforce: How well is new england doing? *Regional Review*, 2001(2), 2001. URL `http://www.bos.frb.org/economic/nerr/rr2001/q2/issues.htm`.

Oystein Kravdal. Does marriage require a stronger economic underpinning than informal cohabitation? *Population Studies*, 53(1):63–80, 1999.

Jeanne Lafortune. Making yourself attractive: Pre-marital investments and the returns to education in the marriage market. Job market paper, MIT, 2007.

Lars Lefgren and Frank McIntyre. The relationship between women's education and marriage outcomes. *Journal of Labor Economics*, 24:787–830, 2006.

Kevin Leicht and Teresa Sullivan. Minority student pipelines before and after the challenges to affirmative action. Technical report, The University of Texas at Austin, 2000.

Steven Levitt. Using repeat challengers to estimate the effect of campaign spending on election outcomes in the u.s. house. *Journal of Political Economy*, 102(4):777–98, 1994.

Rachel Proctor May. Scrap it or cap it? what to do with texas' top ten percent law. *Austin Chronicle*, 2005.

Justin McCrary. Manipulation of the running variable in the regression discontinuity design. Technical report, University of Michigan, 2005.

David Mitch. How did illiterates fare as literacy became almost universal? evidence from nineteenth and early twentieth century liverpool. *Interchange*, 34:313–335, 2003.

James Monks. The returns to individual and college characteristics: Evidence from the national longitudinal survey of youth. *Economics of Education Review*, 19:279–290, 2000.

Claude Montmarquette, Kathy Cannings, and Sophie Mahseredjian. How do young people choose college majors? Technical report, Universite de Montreal, 1997.

Paul A. Nakonezny, Robert D. Shull, and Joseph Lee Rodgers. The effect of no-fault divorce law on the divorce rate across the 50 states and its relation to income, education, and religiosity. *Journal of Marriage and the Family*, 57(2):477–488, May 1995.

Sunny Niu, Marta Tienda, and Kalena Cortes. College selectivity and the texas top 10% law. In *Annual Meetings of the Population Association of America*, 2004.

Gerald Oettinger. Parents' financial support, students' employment, and academic performance in college. Technical report, The University of Texas at Austin, 2005.

Valerie K. Oppenheimer. Women's employment and the gain to marriage: The specialization and trading model. *Annual Review of Sociology*, 23:431–453, 1997.

Maria Porter. The effects of sex ratio imbalance in china on marriage and household decisions. Technical report, University of Chicago, 2007.

Elaina Rose. Education and hypergamy, and the success gap. Technical report, University of Washington, 2005.

William R. Shadish, Thomas D. Cook, and Donald T. Campbell. *Experimental and quasi-experimental designs for generalized causal inference*. Houghton Mifflin, 2002.

Rachna Sheth. Hearing weighs merits of texas top 10 rule. *Daily Texan (U. Texas-Austin)*, 2004.

Aloysius Siow. Economic theories of union formation. Technical report, University of Toronto, 2006.

John H. Sloan, Arthur L. Kellermann, Donald T. Reay, James A. Ferris, Thomas Koepsell, Frederick P. Rivara, Charles Rice, Laurel Gray, and James LoGerfo. Handgun regulations, crime, assaults, and homicide: A tale of two cities. *New England Journal of Medicine*, 319:256–262, 1988.

Edward P. St. John, Glenda D. Musoba, and Ada B. Simmons. Keeping the promise: The impact of indiana's twenty-first century scholars program. *The Review of Higher Education*, 27(1):103–123, 2003.

Cass R. Sunstein. Endogenous preferences, environmental law. *Journal of Legal Studies*, 22:217, 1993.

J. Phillip Thompson and Sarah Tobias. The texas ten percent plan. *American Behavioral Scientist*, 43(7):1121–1138, 2000.

Arland Thornton, William G. Axinn, and Jay D. Teachman. The influence of school enrollment and accumulation on cohabitation and marriage in early adulthood. *American Sociological Review*, 60(5):762–774, October 1995. URL `http://links.jstor.org/sici?sici=0003-1224%28199510%2960%3A5%3C762%3ATIOSEA%3E2.0.CO%3B2-0`.

Marta Tienda and Sunny Niu. Flagships, feeders, and the texas top 10% plan: a test of the "brain drain" hypothesis. Technical report, Princeton University, 2004.

Marta Tienda and Sunny Niu. Capitalizing on segregation, pretending neutrality: College admissions and the texas top 10% law. Technical report, Princeton University, 2003.

Marta Tienda, Kevin T. Leicht, Teresa Sullivan, Michael Maltese, and Kim Lloyd. Closing the gap? admissions & enrollments at the texas public flagships before and after affirmative action. Technical report, Princeton University, 2003.

W. Trochim. *Research Design for Program Evaluation: The Regression Discontinuity Approach*. Sage Publications, 1984.

Sarah Turner and William Bowen. Choice of major: The changing (unchanging) gender gap. *Industrial and Labor Relations Review*, 52:289–313, 1999.

Wilbert Van der Klaaw. Estimating the effect of financial aid offers on college enrollment: A regression–discontinuity approach. *International Economic Review*, 43:1249, 2002.

David Vincent. *Literacy and Popular Culture: England 1750-1914*. Cambridge University Press, 1989.

B. Weisbrod and P. Karpoff. Monetary returns to college education, students' ability, and college quality. *Review of Economics and Statistics*, 50:491–497, 1968.

Justin Wolfers. Did unilateral divoce laws raise divorce rates? a reconciliation and new results. *American Economic Review*, 96(5):1802–1820, December 2006.

Zheng Wu. Economic circumstances and the stability of nonmarital cohabitation. *Journal of Family Issues*, 21(3):303–328, 2000.

Yu Xie, James M. Raymo, Kimberly Goyette, and Arland Thornton. Economic potential and entry into marriage and cohabitation. *Demography*, 40:351–367, 2003.

Basit Zafar. College major choice and the gender gap. Job market paper, Northwestern University, 2007.

Liang Zhang. Do measures of college quality matter? the effect of college quality on graduates' earnings. *The Review of Higher Education*, 28(4):571–596, 2005.