ESSAYS ON THE EFFECTS OF FAMILY AND SCHOOLING ON STUDENT OUTCOMES

By

MARK HOEKSTRA

A DISSERTATION PRESENTED TO THE GRADUATE SCHOOL
OF THE UNIVERSITY OF FLORIDA IN PARTIAL FULFILLMENT
OF THE REQUIREMENTS FOR THE DEGREE OF
DOCTOR OF PHILOSOPHY

UNIVERSITY OF FLORIDA

2006

Copyright 2006

by

Mark Hoekstra

ACKNOWLEDGMENTS

This work has benefited tremendously from the instruction and encouragement of David Figlio. I would also like to thank Larry Kenny, Rich Romano, Mark Rush, Damon Clark, Francisco Martorell, Steve Slutsky and countless others who provided helpful advice and comments throughout all of the stages of this research.

I would like to thank the School Board of Alachua County for providing school data and the people at the Office of the Clerk of the Circuit Court at the Alachua County Courthouse for help in acquiring the divorce data used in this analysis. I would also like to thank an anonymous state university for sharing admissions data and a state office for sharing earnings data.

TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

Abstract of Dissertation Presented to the Graduate School
of the University of Florida in Partial Fulfillment of the
Requirements for the Degree of Doctor of Philosophy

ESSAYS ON THE EFFECTS OF FAMILY AND SCHOOLING ON STUDENT OUTCOMES

By

Mark Hoekstra

August 2006

Chair: David Figlio
Major Department: Economics

This dissertation examines the effect of changes in family structure and university selectivity on the outcomes for students. In the first paper, I examine the effect of parental divorce. Previous research has identified the effects of parental divorce primarily by comparing the outcomes of children whose parents divorced to those of children in intact families, conditional on observable characteristics. In contrast, this paper identifies the effect of parental divorce on educational outcomes by comparing the outcomes of children whose parents divorced to those of children whose parents filed for divorce but later had the cases dismissed.

Using a panel of child-level administrative data on reading and mathematics standardized test scores and disciplinary records for a large Florida school district, I find no evidence that parental divorce negatively affects children overall. In contrast, I find that experiencing parental divorce 6 years earlier causes girls to score over 14 percentile points higher on tests of reading achievement, relative to the alternative. I also find some

evidence for a positive effect on the mathematics achievement of girls and a negative effect on the reading achievement of boys, although those results are less precisely estimated and less robust.

In the second paper, I estimate the effect of attending a state's flagship university on earnings from ages 28-33. Doing so is typically difficult because those who are accepted at and choose to attend more selective schools typically would have higher earnings later on due to other factors such as higher ability, motivation, or family support. To solve that problem, I use a regression discontinuity design that effectively compares the earnings of students who were barely accepted at the flagship to those of students who barely missed the cutoff. I find suggestive evidence of a positive effect on the earnings of white men ranging from 1% to 25%, the magnitude and statistical significance of which depend on the functional form used. I find no consistent evidence of an effect on the earnings of women generally, although I do find that white women with a strong attachment to the labor force have significantly higher earnings as a result of being accepted at the state flagship university.

CHAPTER 1
"JUST KIDDING, DEAR": USING DISMISSED DIVORCE CASES TO IDENTIFY THE EFFECT OF PARENTAL DIVORCE ON STUDENT PERFORMANCE

1.1 Introduction

The increased incidence of divorce in American families was undoubtedly one of the most significant social trends of the 20th century. Although divorce rates have declined slightly recently, the number of divorces per 1,000 married women aged 15 and older more than doubled from 9.2 in 1960 to 19.5 in 1996, and demographers project that if current rates of divorce continue, approximately 50% of recent first marriages will end in divorce. The impact of this trend on children is clear: over 1 million children are affected annually by divorce.

In a rare example of unity between liberals and conservatives, concern regarding the implications of parental divorce for children's well-being has been expressed by politicians across the political spectrum. President George W. Bush has stated that "the most effective, direct way to improve the lives of children is to encourage the stability of American families" (2002a). Senator and former First Lady Hillary Rodham Clinton has also shown concern regarding the effect divorce has on children, saying, "The instability of American households poses great risks to the healthy development of children" (1998, p. 39). These concerns have resulted in several political movements toward divorce reform. Three states have passed "covenant marriage" laws, which introduce a second tier of marriage that offers more limited grounds for divorce and require pre-divorce counseling. Perhaps more importantly, there has also been a political movement toward

legislation that would change no-fault and unilateral divorce laws to make it more difficult to get a divorce when children are involved; at least eight states have considered such legislation since 1996 (Friedberg, 1998).

The common belief underlying nearly all policy statements on this issue is that divorce causes children to be worse off than they would be if their parents had stayed together. An equally common view in policy circles is that this belief is not motivated by ideological agenda but is rather a well-documented empirical fact. For example, in remarks made to the Chamber of Commerce in Charlotte, North Carolina, President George W. Bush stated

> Research shows that two-parent families are more likely to raise a child that is going to go to high school or college, that a child in a two-parent family is less likely to get addicted to drugs. Now, I understand there are some families that just simply aren't meant to be. I know that. I'm not—I'm wise about that. On the other hand, we ought to aim for a goal, a goal that recognizes the power and importance of two-parent families in America." (2002b)

Similarly, in *It Takes a Village*, Hillary Rodham Clinton wrote, "Recent studies demonstrate convincingly that while many adults claim to have benefited from divorce and single parenthood, most children have not" (1998, p. 39).

It seems, however, that this is one more area in which the causal link has been made in the political arena before it has been convincingly demonstrated in academia. Despite all of the interest in divorce and its effects on children, serious methodological issues limit the extent to which academic researchers have been able to determine the causal link. While there is clearly a strong association between family structure and child well-being, the central problem is that it is difficult to determine what child outcomes would result if troubled marriages that might otherwise end in divorce were to continue. Put differently, the challenge at hand is to separate the underlying causes of divorce and

their effects on children from the effects of the divorce itself. Determining the nature of these causal relationships is very important for public policy, since the scope of public policy is largely limited to increasing the costs to the parents of getting a divorce and forcing the couple to reconcile, rather than solving the underlying problems directly.

While researchers have made some progress toward meeting this challenge, data limitations have continued to impair efforts at determining whether the effects captured are those of the divorce or of some unobserved variable that is in fact causing both the divorce and the child outcomes. Such limitations have caused McLanahan and Sandefur to lament the fact that because no randomized experiment is feasible, analysts will never be able to agree on the causal role of family structure in child outcomes (p. 11, 1994), while Gruber states that the evidence "has yet to convincingly address potential selection biases associated with the decision to divorce" (p. 2, 2000).

Clearly such pessimism is not the result of a lack of interest or effort by academics. Indeed, as Gruber (2000) has noted, there are vast literatures in economics, sociology, and developmental psychology that examine the consequences of divorce. Although the early studies were cross-sectional, more recently the trend has been toward doing event analyses of divorce that control for as many pre-divorce student and family characteristics as possible. These studies essentially identify the effects of divorce by comparing the outcomes of children who experienced parental divorce to those of children in intact families, conditional on pre-divorce observables. However, this identification strategy only works to the extent one is able to control for every conceivable difference between families that divorce and families that do not, which is a difficult if not impossible standard for any data set to meet. This problem is exacerbated

by the fact that the outcome variables used (such as high school graduation, years of education, and earnings) are not observed prior to the parental divorce, so one cannot even control for the pre-divorce level of the outcome measure used. Consequently, the conditional outcomes of children of intact families may not represent the correct counterfactual of how well off children of parental divorce would have been had their parents stayed together for an exogenous reason. To estimate the true effect of divorce, one must then be able to separate the effects of the unobserved process evident prior to the filing of the divorce from the effects of the marriage dissolution itself, something that is difficult to do by comparing children from intact families to children from families that experienced divorce. A better approach would be to utilize data not only on children whose parents divorce, but also on children whose parents file for a divorce that is later dismissed, the latter of which would effectively form the control group. This is precisely the identification strategy that I propose.

I am able to do this by combining two exceptional data sets. The first consists of an eight-year panel of detailed data on every student in grades 1 through 12 in the Alachua County school district, which is the 194th largest district in terms of enrollment among the more than 16,000 school districts in the United States. The second data set consists of public records on divorces filed in Alachua County from 1993 - 2003. I merge these into one data set matching parent names, child names, and child birth dates found in both data sets. By constructing a data set in this way, I can examine the effects of parental divorce on children's standardized reading and mathematics test scores as well as discipline problems at the micro level of the children themselves. This is the first research that uses individual-level data for which the educational outcomes of students

are observed prior to parental divorce, which enables me to use student-specific fixed effects. Furthermore, this is the first research that utilizes a data set that identifies the children of parents who file for divorces that are eventually dismissed rather than granted, in addition to children whose parents did divorce. By comparing children whose parents dismissed divorce cases to the children of parents who actually divorced, I can distinguish the effects of divorce from those of the underlying causes of divorce, the latter of which are evident in both families.

The results lend little support to the idea that parental divorce negatively affects the academic achievement of students overall. Although I find that students who experienced parental divorce have lower reading and math scores and more disciplinary problems afterward relative to children from intact families, I find a similar (and stronger) result when comparing the outcomes of children whose parents filed for divorce but later decided against it to those of children from intact families. This suggests that the so-called consequences of divorce found by comparing children of divorce to children of intact families are likely consequences of the factors that caused the parents to divorce rather than of the divorce itself. Indeed, when comparing the outcomes of children whose parents divorced to children whose parents filed for and dismissed a divorce case, I find no negative effect of divorce overall. In fact, I find that experiencing a parental divorce six years ago causes girls to score 14.68 percentile points higher on reading tests than they would have had their parents stayed together, a result that is both statistically significant and robust. I find somewhat weaker and less robust evidence that parental divorce has a positive effect on the mathematics achievement of girls and a negative effect on the reading achievement of boys. Finally, the results suggest that experiencing

parental divorce causes an increase in disciplinary problems immediately after the divorce, but that there is no effect 4 years after the divorce.

1.2 Theoretical Considerations, Literature Review, and Identification Strategy

1.2.1 How Divorce Affects Student Achievement

There are several mechanisms through which divorce may affect the academic achievement of children. A child whose parents divorce may experience less parental attention and assistance with school work at home, thus reducing child learning. Parental absence may also reduce the average *quality* of the assistance received at home, further lowering school performance. For example, the custodial parent may now be the one to assist a child in a subject area in which the absent parent would have been more able to help. The child may also experience less parental guidance as a result of divorce, allowing the student to lose focus in school. The overall trauma of the divorce and family structure change may also distract a child from school activities, at least temporarily. A divorce may lower the level of economic resources available for the child, leading to a reduction in the quantity and quality of non-school educational inputs purchased for the child, causing the child to be worse off. Finally, a child may have to move as a result of the divorce, forcing the child to adapt to a new house, neighborhood, and perhaps even school. Some research has suggested that moving itself negatively affects academic achievement (e.g., Haveman, Wolfe, and Spaulding, 1991).

It is important to note, however, that not all of the ways in which divorce can affect children are negative. For example, although the loss of contact with a divorced parent is typically assumed to have negative consequences for the child, divorce may be beneficial if the divorced parent is abusive or alcoholic. Furthermore, parental conflict itself may lead to a reduction in the quantity or quality of parental inputs for the child's education as

well as distract the student from focusing on school. To the extent that divorce reduces parental conflict, children may perform better academically. Consequently, while divorce may affect child outcomes though any of several mechanisms, the net effect of divorce is theoretically ambiguous.

The effect of divorce need not be permanent, either. Depending on the extent to which parent and child overcome the trauma from the change in family structure and successfully adapt to the new circumstances, it is quite possible that the effect of divorce may change over time.

1.2.2 A Review of the Literature

The early research on divorce consisted mainly of cross-sectional studies that compared the outcomes of children from intact families to those of children from divorced families (e.g., Keith and Finlay, 1988). Several researchers have noted, however, that there are significant differences in the pre-divorce observable characteristics of families that experience divorce relative to those that do not. In an effort to control for pre-divorce characteristics of families, some studies have used retrospective data on variables such as self-reported parental conflict (e.g., Amato and Booth, 1991). More recently, the trend throughout the literature is to use longitudinal data to accurately control for some pre-divorce student and family characteristics such as the student's cognitive test scores, family income, and parent's education (e.g., Cherlin, Kiernan, and Chase-Lansdale, 1995; Borgess, 1998; Lang and Zagorsky, 2000; Painter and Levine, 2000; Ermisch and Francesconi, 2001a and 2001b; Fronstin, Hill, Yeung, and Duncan, 2001; Greenberg, and Robins, 2001; Deleire and Kalil, 2002;). With the exception of Lang and Zagorsky (2000), these studies have found that divorce has negative consequences for children.

Although this research does illustrate that it is important to control for pre-existing differences in divorced and intact families, significant problems remain. First, several studies rely on self-reported measures of parental conflict. For such measures to be properly used, it must not only be the case that the measures accurately capture the parental conflict that may impact children's outcomes, but it must also hold that such measures of conflict are comparable across households. Similar problems exist when using measures of children's psychological well-being.

Second, the primary child outcomes that researchers have used, such as marital status, educational attainment, and earnings, are observable only when the child reaches adulthood. Consequently, researchers examining those outcomes can only control for pre-divorce differences in family characteristics and not in child outcomes themselves.

Third, and most importantly, there is significant reason to believe that controlling for pre-existing *levels* of family characteristics and child outcomes may be inadequate. For example, it is easy to conceive of an unobserved variable that determines both a child's outcome and the family structure. This unobserved variable may be a process that causes parental relations to worsen to the point that parents select into divorce, while at the same time negatively affecting the children. Since this is a process and not merely a level of family or child characteristics (after all, by definition the parents had not yet divorced at the time of the pre-divorce observation), controlling for pre-divorce characteristics or outcomes will not adequately measure how well off the children would be if the parents did not divorce. Consequently, even conditional on pre-divorce observables, the outcomes of children of intact families may not be the correct counterfactual for children who experienced parental divorce. In contrast, by using the

outcomes of children whose parents file for a divorce that is later dismissed as an estimate of the counterfactual, my identification strategy is able to separate the effect of the divorce from the effects of the processes that caused the divorce.

There have been other attempts at overcoming the selection effects associated with parental divorce. Sandefur and Wells (1999) used sibling information to identify the effects of divorce by comparing siblings with varying exposures to single-parent families or changes in family structure to each other. Ermisch and Francesconi (2001a) used a similar sibling strategy. However, within-family comparisons will be flawed if siblings are affected differently by divorce, whether due to age or other reasons. More importantly, sibling strategies will also lead to downward-biased estimated effects of divorce if there is an underlying family trend over time that determines both family structure and child outcomes. Furstenberg and Kiernan (2001) used a slightly different strategy and compared the outcomes of children who experienced divorce to those of children whose parents divorced after the children had grown up. That approach, however, will fail when there are significant pre-divorce trends or when there are unobserved differences in the intensities of the underlying problems leading to the two types of divorce, both of which are very plausible possibilities.

Gruber (2000) approached the selection problem by exploiting variation in the unilateral divorce laws across states and over time. That approach, however, may not overcome the selection problem if there were underlying trends that led states to pass unilateral divorce laws at different times or if, as Gruber noted, those laws had an effect on children in ways other than through divorce. Indeed, the basic problem is that in addition to being concerned with unobserved variables in families that experience

divorce, since the variation used to identify the effects of divorce in Gruber's approach occurred at the macro (state) level (instead of the individual level), one must also be concerned about unobserved characteristics at the state level that might lead to worse outcomes through means other than an increased propensity for divorce.

Other research on the consequences of divorce has focused on examining the effects of separation by parental death (Fronstin, Greenberg, and Robins, 1999; Lang and Zagorsky, 1999; Corak, 2001). This allows researchers to identify the effects of the complete loss of parental contact and supervision as well as of economic support.[1] However, in nearly all cases the underlying processes that cause divorce are substantially different from those that cause spousal death.[2] Consequently, an examination of the effects of parental separation by death does little to shed light on the issue of what would happen to children whose parents divorced if an exogenous force such as increased costs of divorce had prevented them from doing so.

1.2.3 Identification Strategy

The identification strategy employed in this paper is most similar in spirit to that used by Bound (1989), who estimated the disincentive effects of Social Security Disability Benefits by comparing the labor outcomes of rejected disability applicants to those of accepted disability applicants. Bound concluded that disability benefits accounted for substantially less than half of the postwar decline in the labor force participation rates of older men and that previous cross-sectional strategies had exaggerated the causal disincentive effects. The purpose of this paper is to determine

---

[1] Unobserved life insurance payouts may complicate the issue of economic support, however.

[2] A notable but rare possible exception is spousal homicide.

whether or not a similar story is true with respect to the causal effects of parental divorce on student outcomes.

To do that, I compare the annual standardized test scores and disciplinary records (number of infractions per year and days suspended per year) of children whose parents dismissed divorce cases to those of children whose parents divorced. Since I observe outcomes for each student in every year, I am able to utilize student-specific fixed effects, allowing for both a time-invariant shock effect of parental divorce as well as an effect that changes over time. By identifying the effects of parental divorce by comparing not the achievement levels but the *changes* in the test scores of children whose parents divorce to those of children whose parents dismiss divorce cases, I lessen the possibility that the effect I estimate is really an effect of an unobserved variable correlated with divorce and not of the divorce itself. Still, my data allow me to test the validity of this identification strategy in two ways:

- Do the observable characteristics of the two groups look similar prior to the parents filing for divorce? To the extent that there are differences, one might be concerned that these differences may cause one group to trend differently in the post-divorce period regardless of whether the couples divorced or not. I address this question in Section 1.4.3

- Are there differences in the trends of the outcomes prior to the filing of divorce? Again, to the extent that there are, one might be concerned that these trends may continue in the post-divorce period regardless of whether the marriages ended in divorce or not. I address this question in Section 1.4.5.

Although the variation I exploit in this paper is not exogenous, there is reason to believe that the selection at work would imply that the estimated effects of parental divorce on student test scores will be biased downward, if at all. For example, those couples who decide to have the divorce case dismissed may do so in part out of concern that the divorce may adversely affect their children, which for reasons discussed earlier is

a widely held belief. To the extent that the increased concern for children in this group (the control) might have caused the post-divorce case scores of their children to trend upward over time relative to the divorce group even had they divorced, my estimates will be downward biased. Second, it could also be the case that the parents who dismiss their divorce cases do so because they experienced a positive shock to their marriage during the divorce process that may cause them to rationally expect their marriage to improve significantly in the future, whereas those couples who divorce do not. Again, to the extent that this would cause the test scores of children whose parents dismissed divorce cases to trend up relative to those of children whose parents divorced, my estimates will be downward biased. Finally, those parents who choose not to divorce may do so simply because their marriages are not as bad as those of parents who divorce.[3] If this implies that the children whose parents dismissed divorce cases would learn at a faster rate afterward than would the appropriate control group ideally characterized by families that would have experienced divorce if not for some exogenous force, then this too would cause my estimates of the parental divorce effect on test scores to be biased downward. Together, these possibilities suggest that the selection at work in my identification strategy should, if anything, bias my estimates of the effect on test scores downward, implying that any positive effect would be a lower bound.

[3] This would be similar to the problem with the traditional identification strategy of comparing children whose parents divorce to children whose parents stay together, although one might reasonably expect that the extent of the bias would be smaller here since both groups of couples filed for divorce.

1.3 Parental Divorce and Student Test Scores

1.3.1 School Data

To address the effects of divorce, I use a confidential student-level data set provided by the School Board of Alachua County in the state of Florida. This data set consists of observations of students in the first through twelfth grades for the academic years of 1993-94 through 2002-2003. The Alachua County School District is large relative to school districts nationwide; in the 2000-2001 school year, there were on average 2,200 students in each grade, making it the 194th largest school district among the more than 16,000 districts nationwide. The student population was approximately 56% white, 36% African-American, 4% Hispanic, and 2% Asian. Forty-four percent of students were eligible for subsidized lunches.

For each first- through tenth-grader I observe norm-referenced standardized test scores in reading and mathematics. The test scores reflect the percentile ranking on one of two national tests relative to all test-takers nationwide. Prior to the 1999-2000 academic year, nearly every student in the third through ninth grades took the Iowa Test of Basic Skills (ITBS). In addition, at the discretion of the school principal, many first and second-graders also took these exams. Starting in the 1999-2000 school year all first- through tenth-graders were tested using the Stanford 9 test. This change was made because the Stanford 9 test is used for the Florida Comprehensive Assessment Test (FCAT) that was introduced in the 2000-01 school year to enable the state of Florida to evaluate schools. Both the ITBS and the Stanford 9 are exams used by schools nationwide to test mathematics and reading. Except for some first and second graders prior to 2000, almost all students took the tests in a given year. As described later,

however, observations on some students were dropped in order to ensure a clean comparison.

In addition, student records also contain the names and addresses of the parents of each student for each year. This information is gathered primarily during August of each year during registration, although it is updated continually throughout the year. The data on names are crucial because that is the information used to match divorce information to the student records. Discipline records are also observed for every first- through twelfth-grader, beginning in 1993 (prior to when the standardized testing records begin). Finally, I observe information on each student's race, sex, school lunch status, disability status, and gifted status.

In the following analysis, I use four dependent variables from these school data. The primary outcomes that are used are the mathematics and reading scores on the Iowa Test of Basic Skills or Stanford 9 examinations. In addition, I also look at two outcomes from the disciplinary records for each year, including the total number of days each student was suspended and the total number of disciplinary infractions each student committed.

1.3.2 Divorce Data

The divorce data used in this study were gathered from public records information at the Alachua County Courthouse. This information includes the names of every husband and wife who filed for a divorce at the Alachua County Courthouse between January 1, 1993 and March 12, 2003. For each filing, I retrieved the filing date, the final judgment date, and the final judgment type. In addition, I also obtained child names and birth dates for certain divorce cases by personally examining the files at the Alachua County Courthouse, as described in Section 1.3.4.

1.3.3 Divorces in Alachua County, Florida

In order to file for a divorce in Florida, at least one of the parties in the marriage must have resided in Florida for at least six months. There are then two filing types. If there are no minor or dependent children of the marriage parties and if the marriage parties agree about how to divide property and that the marriage is irretrievably broken, they may file for a Simplified Dissolution. However, if there are children involved, the couple must file for a Dissolution of Marriage. All divorce information used in this study is obtained for couples who have filed for the general Dissolution of Marriage.[4]

In order for the court to grant the dissolution of the marriage, the court must either rule that the marriage is irretrievably broken or that one of the marriage parties has been judged mentally incapacitated for a minimum of three years.[5] The court may then choose to do any of several things seen as in the best interests of the marriage parties and dependent children, such as ordering that either or both marriage parties consult with a person deemed qualified by the court (e.g., a marriage counselor) and found accepby the ordered party or parties, or extending the proceedings no more than three months to enable the parties themselves to effect a reconciliation. During any period of continuance, the court can make orders regarding alimony and support for the parties, child custody and visitation rights, property division, and so on. Although there is no mandated pre-divorce waiting period in Florida, if there are minor children of the

---

[4] This includes nearly all divorce-seeking couples with children. Although having children violates a condition for filing for a Simplified Dissolution, some such couples may exist. For example, if a couple files for a divorce and mistakenly claims that the wife is not pregnant when she in fact is, they may file for a Simplified Dissolution. These sorts of exceptions are probably very rare, however, and to the extent young children are involved, my data set would not be changed anyway.

[5] Not surprisingly, the "irretrievably broken" clause is the path most commonly tread by those seeking divorce in Alachua County, Florida.

marriage, then prior to obtaining a final hearing each parent is required to attend one of seven four-hour parenting education classes approved by the 8th Judicial Circuit Court. Finally, if after the final hearing the court finds that the marriage is irretrievably broken, a final order of dissolution of marriage is given. Alternatively, if the parties work out the problems, the petitioner may have the case voluntarily dismissed. The judge may also notify the parties of intent to dismiss if they have not fulfilled their obligations to the court. Within a month after this intent to dismiss is issued, the judge may order that the case be dismissed. Within days of the resolution, the case is closed. For reasons discussed in the next section and seen in 9, this study uses only those dismissed divorce cases in which the petitioner specifically requested that the case be dismissed.

1.3.4 Merging the Divorce Data with the School Data

Since my primary identification strategy depends crucially on correctly matching children in the school data to divorces filed in Alachua County, every effort was made to ensure that those matches that were made were correctly made. Consequently, divorces were matched to students' parents using a created variable: $\text{Firstname}_{\text{parent1}}\text{Lastname}_{\text{parent1}}\text{Firstname}_{\text{parent2}}\text{Lastname}_{\text{parent2}}$.

Only unique couple-name combinations were used. Consequently, if John and Mary Smith were observed to have filed for more than one divorce case from January 1, 1993 through March 12, 2003, those divorce cases were not matched to students.[6] Similarly, if in the school district in any given year from 1993 through 2003 there were two or more children who were not siblings but who had parents with identical names,

[6] In reality, the uniqueness standard was applied more strictly than this. In the divorce data I observe up to nine names for both the husband and the wife, due to the fact than any address or name changes must be disclosed to the court. If *any* first-last name couple combination for a given divorce was identical to that in another divorce case, that divorce case was not matched.

those children were not matched to any divorce. Siblings were defined as children who shared the same last name and lived at the same residential address.

Divorces were matched to students on a year-by-year basis. Since the parental name information from the school district is from fall registration in August of each year, these parental names were matched to divorces filed from August 1st of that year through July 31st of the following year. This was done to increase the likelihood that the parent names from the school district used to match to divorces were both present. In contrast, if one were to try to match August names to a divorce filed in January of that same year, the parent names in the school data may not both be present or may have changed since the divorce was filed.

Table 1 shows how many divorces have been filed in Alachua County, Florida, from January 1, 1993, through March 12, 2003. The also shows how the number of total divorce cases varies from the number of divorce cases expected to be associated with children in the public school system. For example, in the year 2000 there were 1,123 divorce cases filed, of which 974 were General Dissolutions (a necessary but not sufficient condition for the case to have children involved.) Of those, 904 had unique parent name combinations. A random check of 100 General Dissolutions from 1993-2003 indicated that 54% of the marriages had minor children of that marriage, implying that an estimated 488 of those divorce cases may be expected to have minor children of the relevant marriage involved. Since I match divorces only to children in grades one through twelve and approximately 10% of students in the county attend private schools, there were approximately 293 divorces in 2000 that I could reasonably expect to match. Given that about 10% of the parent name combinations in the school data were

nonunique, there remained approximately 264 divorces filed in the year 2000 that I could expect to match. In all, I could reasonably expect to have matched at most 2,512 divorces. While I do not claim that this is the exact number of matchable divorces, it is my best guess as to how many I could expect to match.

As shown in Table 2, I matched 724 divorce cases to names in the school data using the parent name identifier[7], for a match rate of 28.8%. Of those 724 divorce cases, 583 were matched to a student for whom I observed at least one test score. Of those matches made to children observed with at least one test score, a random check of 100 children matched to divorces suggested that an estimated 97 percent of the matches made were made correctly.[8]

However, only 66 of those 724 matched divorce cases had been dismissed. In order to increase the sample size of dismissed divorce cases, I went to the Alachua County Courthouse and looked up all dismissed divorce cases with unique parent name combinations that were filed from January 1, 1993 – March 12, 2003. I then matched these dismissed divorce cases to children in the school data for which the first and last name of the child matched along with at least one of the following two identifiers (and none contradicted significantly[9])

---

[7] The matches in Table 2 include only matches made to children whose parents were believed to be the natural or adoptive parents of that child. I defined parents as the natural or adoptive parents of a child when the child shared his or her last name with at least one of the parents listed by the school district in the year before or in the year in which the divorce case was filed.

[8] This was done by manually looking up the divorce judgment papers for each of 100 randomly selected matches made and comparing the child's name from my matched data to the names of the children in the divorce papers. All observations matched to the three cases that were incorrectly matched were dropped from the data set.

[9] For example, if the date of birth in one file said 8/16/1985 and the date of birth in the other file said 8/16/1986, I made the match provided that the child name and parent names matched.

- child's date of birth
- parents' names

Furthermore, only dismissed divorce cases in which one spouse was not found to be deceased were matched. At this point, some adjustments were made to the matched set of students matched to a dismissed divorce case in order to ensure a proper comparison, the impact of which is shown in Table 3. First, all observations matched to a divorce case that had been dismissed by the judge (as opposed to ones in which the petitioner requested the dismissal directly) were dropped from the data set. Although it may not at first seem intuitive why one would want to eliminate those dismissed cases from the data set, it becomes evident from looking at the characteristics of both groups prior to filing for divorce, as shown in columns C and D of Table 9. For example, the average reading score of children whose parents later filed for a divorce that was dismissed due to something other than a direct request by the petitioner was 33.3, while the average reading score of children whose parents later filed for and specifically requested the dismissal of a divorce case was 57.3. Similar differences are evident between these two groups with respect to math scores, subsidized lunch status, and the percent black.

In addition, since I want to ensure that the dismissed divorce cases in the data set were not caused by a threat of violence by one spouse to the other, I acquired data on domestic violence cases filed from 1993-2003. I then matched domestic violence cases to the school data by matching the parent name combinations in the domestic violence cases to parent name combinations in the school data set. The observations of students who were matched to a domestic violence case were then dropped from the data set.

Finally, all observations matched to students for whom only one parent name was listed by the school district in the year prior to that in which the divorce was filed were dropped from the data set, since those children could not have been matched to a case that ended in divorce due to the nature of the matching algorithm described above. The absence of a parent in the school records could reflect unobserved negative family characteristics. In addition, it is unclear exactly what a divorce means for a family for which only one parent name is listed by the school district.

1.3.5 The Final Data Set Used in the Analysis

Although my primary identification strategy is to compare the outcomes of children of actual parental divorces to those of dismissed parental divorces, in order to replicate the methodology of other papers in the literature, I need to be able to identify children who did not experience parental divorce from 1993 through March of 2003. Unfortunately, the data do not contain this information. Consequently, I try to identify these children in two ways. First, in the less restrictive method, I define two groups by trying to eliminate those children who a) could not have been matched to a divorce, or b) were likely to be in a single-parent family. Specifically, I removed from the data set all observations of any child who met one of the following conditions:

1. Was observed with at most one parent's name for one year and was not matched to a divorce in another year.
2. Was observed with parents whose names were not unique for any year from 1993-2003 (after accounting for siblings) and thus could not have been matched to a divorce case in that year.
3. Was observed with parents whose names were the same as those associated with more than one divorce from 1993-2003.

Although the primary cross-sectional results excluded students only on the basis of the above conditions, as a check I also performed the cross-sectional analysis using data in which I also excluded each student that met the following condition as well:

4. Was observed to have a different last name than at least one of the parents listed by the school district.

The purpose of these deletions is to ensure that those students who remained did not experience parental divorce and form the counterfactual used in previous researchers' identification strategies. In addition, I drop all observations of students for whom the first and last names of both parents changed over time.

The result of leaving out these students based on conditions 1 through 3 is to reduce the overall sample size from 1,500-2,000 students/grade/year to 400-700 students/grade/year. In all, the data used to compare children of divorce to children whose parents did not divorce consist of 60,196 observations on 17,241 children from 1993-2003 (35,055 observations on 9,654 children when conditions 1-4 are used). The descriptive statistics of these students in the year 2000 can be seen in columns A and B of Table 9. It is clear from these columns that the more restrictive sample (column B) does appear to eliminate students whose parents are not married or for whom a grandparent is listed as a parent, even though it may also eliminate children whose parents are in fact married.

In the main analysis in which only children whose parents filed for divorce are included, there are 6,761 observations on 1,028 children whose parents filed for one of 716 divorce cases. A total of 93 of those divorce cases were dismissed, affecting 156 children. The first row of Table 4 shows how observations of these two groups of

students are distributed over time. It shows that approximately 75% of the children linked to a divorce case are observed after the divorce. This proportion of children declines steadily; approximately 20-25% of students linked to a divorce are observed at least five years after their parents' divorce case was closed.

When only observations linked to at least one test score are included, there are 27,102 observations on 9,388 children in the data set that includes children whose parents never filed for divorce and those whose parents did file for divorce. In the data set used in the main analysis in which only children whose parents filed for divorce were included, there are 3,525 observations on 801 children, representing 580 divorce cases. There are 111 children linked to 93 dismissed divorce cases. The second row of Table 4 shows how observations on students matched to parental divorce cases were distributed over time. Approximately 75-80% of the students matched in each group are observed with a test score after the divorce case was closed. As shown in Table 4, approximately 35% of students matched to divorce are observed 3 to 5 years after the case is closed, while approximately 20-30% are observed with a test score more than 5 years after the divorce case was closed.[10]

Overall, the distribution for the group of children whose parents divorced is quite similar to the distribution for the group of children whose parents filed for a divorce that was later dismissed, with the exception that more than 5 years after the divorce or dismissal, I tend to observe relatively more (5 to 10 percentage points) students who

[10] For the entire sample of observations (some of which do not have a test score), the time after the divorce is defined as the calendar year of the observation minus the year in which the divorce case was closed. For the subset of observations for which there is at least one test score, the time after the divorce is defined as the number of years between the date the divorce case was closed and the date of the test and thus is not necessarily an integer.

experienced a dismissed divorce than those who experienced parental divorce. While this could be due to the fact that children whose parents later divorce are on average slightly older (1.6 years) than their peers whose parents file and dismiss a divorce case, later in the paper I nevertheless examine the sensitivity of the results by reestimating the results after excluding all observations 5 or more years after the closure of the divorce case. However, the overall similarities in the distributions of the observations in the two groups is important because one might be concerned that children who are negatively affected by divorce leave the county and thus the sample. The overall similarities in the distributions indicates, however, that for attrition to bias the results, it must not only be the case that children whose parents divorced do so at the same rate as those whose parents dismissed a divorce case (at least for the first five years afterwards) but also that those in the two groups who did leave were affected by the closure of the case in different ways. While not impossible, such a scenario does seem unlikely.

1.4 The Effects of Divorce on Student Performance

1.4.1 Comparing the Test Scores of Children of Divorce to Those of Children in Intact Families

A common finding in the divorce literature is that children of divorced parents experience poorer outcomes than do children who are brought up in two-parent households. Even though this approach has serious flaws, it is still constructive to test whether my data appear to be qualitatively similar to data used in previous research. To test for the unconditional cross-sectional effects of divorce, I estimated a regression using pooled data in which I control only for year and grade effects to remove the effect of any trend over time in percentile test scores in the school district (whether caused by a change in the test used or something else.) The general regression equation was

$$test_{it} = b_0 + b_1 X + b_2 PostDivorce_{it} + \varepsilon_{it}$$

where $test_{it}$ is the test score of student *i* at year *t* and *X* is a vector of covariates one expects to affect test scores. The variable *PostDivorce* is equal to one if the test was taken after the child's parents finalized their divorce.

The results for reading and math test scores are given in Tables 5 and 6, respectively. The p-values are given in the second row of each cell, which were calculated using standard errors clustered at the family level.[11] When only student grade and year effects are included as covariates, parental divorce is associated with reductions of 1.97 and 1.31 percentile points in reading and math, respectively, although neither is statistically significant at conventional levels. As other researchers have noted, however, there are significant differences in the observable characteristics of children whose parents divorce compared to those of children whose parents remain married. My data allow me to condition on several important variables, including race, sex, school lunch status, and zip code median family income, and squared zip code income. Although including these variables may cause an upward bias in the test score estimates due to the fact that some could themselves change as a consequence of divorce (e.g., subsidized lunch status), it is still worthwhile to include them to see if their inclusion explains the differences between children whose parents divorce and children whose parents do not divorce. The top sections of row (b) in Tables 5 and 6 contain these results and suggest that parental divorce is associated with reductions of 2.19 and 1.32 percentile points in reading and math, respectively, the former of which is statistically significant at the 10%

[11] The school district does not identify families, so although I identify families for children whose parents filed for divorce, for the other children I assumed each was in a separate family.

level. The top section of row (c) also includes school fixed effects.[12] There, the result indicates that parental divorce is associated with reading and math scores that are 0.78 and 0.48 percentile points lower, respectively, although neither estimate is statistically significant at the 10% level.

These data also allow me to examine the extent to which the impact of divorce affects children differentially based on the grade at which they experienced parental divorce, represented by *DivorceGrade* variable in the equation below. In addition, I can examine how the effects of divorce grow or diminish over time by including an interaction term measuring the number of years after the divorce when the test was taken. Consequently, the general form of the regression estimated is given by

$$test_{it} = b_0 + b_1 X + b_2\, PostDivorce_{it} + b_3\, YearsAfter{*}PostDivorce$$
$$+ b_4\, DivorceGrade{*}PostDivorce + \varepsilon_{it.}$$

The results given in the bottom of row (b) in Tables 5 and 6 suggest that the association between having experienced parental divorce and lower test scores grows over time. I find that reading and math scores fall by approximately 0.79 and 1.26 statistically significant percentile points, respectively, in every year after the divorce. In the case of reading achievement, the statistically significant coefficient of 1.23 on the grade of the student at the time of the divorce indicates that divorce is less negative for the child when it occurs when the child is older. As shown in the far right column, experiencing a parental divorce while in the 4th grade is associated with statistically significant declines of 3.87 and 5.25 percentile points on math and reading tests 6 years later.

[12] The student's school was only observed for students for whom I observed at least one test score in that year. In addition, prior to the 1999-2000 school year, I observed the school only for 3rd – 5th graders.

In the bottom of row (c), the results are shown when school fixed effects are included in the model. While the association between lower math scores and parental divorce is not changed much by the inclusion of the school effects, the association between parental divorce and worsening reading scores over time is smaller than when school effects are not included, reducing that effect for the hypothetical 10th grader whose parents divorced 6 years prior to a statistically insignificant -1.79 percentile points.[13]

Tables 7 and 8 contain similar analyses using the number of days suspended per year and the number of disciplinary infractions per year as outcome variables. From these tables, it is clear that having experienced parental divorce is associated with statistically significant increases in both the number of days suspended per year and the number of disciplinary infractions committed. Experiencing parental divorce is associated with 0.79 more days suspended per year and 0.39 more disciplinary infractions per year, both of which are statistically significant at the 1% level. By comparison, the average student in the data set is suspended for 1.2 days and commits 1 infraction per year, implying that the cross-sectional correlations found are quite large. When the effect of divorce was allowed to vary over time, each year after the divorce is associated with increases of 0.16 days suspended and 0.07 infractions per year, although only the former is statistically significant at the 10% level. As shown in the far right column of each table, experiencing a parental divorce as a 4th grader is associated with committing 0.54 more disciplinary infractions and being suspended for 1.16 more days when in the 10th grade, both of which are statistically significant at the 5% level. While the inclusion of

[13] Just as with the income measures, it is possible that the school fixed effects pick up some of the effect of parental divorce as well, which would cause these estimates to be biased upward. This is due to the fact that moving to an area with a lower quality school may itself be a consequence of parental divorce.

school fixed effects does reduce these correlations to marginal statistical significance, on the whole it does appear that experiencing parental divorce is correlated with more disciplinary problems.

These cross-sectional correlations grow even stronger when using the more restrictive definition of children whose parents were and remained married over the time period. For example, although unreported, the negative cross-sectional effect of divorce on reading scores for a 10th-grader 6 years goes from -3.87 (p=0.059) to -6.20 (p=0.004) when students whose last name differs from that of a reported parent are not defined as children whose parents were and remained married. Similarly, the negative cross-sectional effect on days suspended increases to 1.46 (p=0.000) from 1.16 (p=0.004).

Given the potential problems with estimating the effects of divorce by comparing children of parental divorce to children of intact families conditional on observable characteristics, the important thing to note from the results in Tables 5 - 8 is not that divorce has a negative effect. Rather, the point is that there is a cross-sectional correlation between having experienced parental divorce, lower academic achievement, and higher rates of disciplinary problems, even conditional on observable characteristics. Whether or not this is indeed the true causal effect remains to be seen, and is the focus of the remainder of the paper.

1.4.2 Do We Observe the Same Correlation when Comparing Children of Dismissed Divorce to Children of Intact Families?

It is worth asking, however, whether or not similar associations are seen when comparing the outcomes of children whose parents filed for divorce but did not divorce to the outcomes of children in intact families. If experiencing a dismissed divorce case is associated with worsening outcomes relative to children whose parents do not file for

divorce, it suggests that the correlations observed in the previous section are consequences of the factors that caused the parents to file for divorce rather than of the divorce itself. Results are contained in Tables 9 through 13.

The results are striking. As seen in the top of row b of Tables 9 and 10, a tenth grade student whose parents filed for divorce 6 years earlier scores 6.42 and 3.78 percentile points lower on reading and math tests than his or her counterparts in intact families, although only the effect on reading scores is statistically significant at the 5% level. Similarly, the results in Tables 11 and 12 show that a 10th grade student whose parents filed for divorce 6 years earlier commits a statistically significant 1.21 more infractions/year and is suspended for a statistically significant 2.20 more days/year than a student whose parents never filed for divorce.

As one would expect, the correlation between worse outcomes and experiencing a dismissed divorce are even stronger when the more restrictive definition of a child in an intact family is used. Although unreported, the so-called "effect" of a dismissed divorce on the reading achievement of a 10th grader whose parents divorced six years prior changes from -5.49 (p=0.170) to -8.03 (p=0.046) while the "effect" on math achievement goes from -6.08 (p=0.141) to -7.33 (0.080).

Of course, these differences in the achievement and disciplinary behavior of these children whose parents filed for divorce cannot be a consequence of divorce since the parents did not in fact divorce. Again, this suggests that the correlations observed by comparing children whose parents divorced to children whose parents did not are likely not the effects of divorce itself but rather of the underlying reasons that caused the parents to file for divorce.

1.4.3 How Similar are Families That Experience Divorce to Those That Experienced a Dismissed Divorce?

Since my primary identification strategy uses children whose parents file for and dismiss divorce cases as the "control" group against which to compare the children who experience parental divorce, it is important that I compare the characteristics of these two groups prior to the filing of the divorce cases. Table 13 presents descriptive statistics for these two groups 0 to 3 years prior to filing the divorce in columns B and C. When a child was observed more than once in this time period, I calculated the average value of each variable from all observations of that child in that category. The numbers indicate that these two groups appear similar to each other among observable characteristics, with three exceptions. The first is that there are relatively more boys whose parents later filed and dismissed a divorce case (60.0%) than whose parents later divorced (47.3%). The second is that children whose parents later filed and dismissed a divorce case tend to have more disciplinary problems than children whose parents later divorced. The third is that children whose parents later filed and dismissed a divorce case are on average 1.6 years younger than children whose parents later divorce. Despite the overall similarities between these two groups, to ensure that the results are not driven by unobserved differences between the two groups, I use individual student fixed effects to control for any time-invariant differences in the family backgrounds of these children.

1.4.4 The Effect of Parental Divorce on Family Income

Before examining how parental divorce affects the academic achievement and disciplinary problems of children, it is beneficial to ensure that my data show what one would expect regarding the effect of parental divorce on family income. Unfortunately, the only measure of family income recorded by the school district is school lunch status.

Although there is a consensus that school lunch status is a good measure of family income for children in elementary school, the social stigma associated with free or reduced lunch for children in middle and high school that lowers take-up rates makes it much less reliable, particularly for my data set in which the vast majority of post-divorce observations are for middle and high school students. This might especially be a concern if students whose parents divorced are particularly unlikely to want to receive federally subsidized school lunch. For this reason and because only a small percentage of children are eligible for free or reduced lunches, I instead use the measure of family income at the zip code level, which has been used as a proxy for family income by others (e.g., Fryer and Levitt, 2004). The model estimated, which is the same as that estimated to determine the effect of parental divorce on test scores and disciplinary problems, was

$$FamilyIncome_{it} = \theta_i + b_0\, Grade_{it} + b_1\, Grade^2_{it} + b_2\, Year_t + b_3\, PostDivorceCase_{it} + b_4\, PostDivorceCase_{it}*Divorce_{it} + b_5\, YearsAfterCaseClosure_{it} + b_6\, YearsAfterCaseClosure_{it}*Divorce_{it} + \varepsilon_{it}$$

where $FamilyIncome_i$ is the median family income in the zip code of student i, θ_i is a student fixed effect, $Grade_{it}$ is the grade of student i at year t, and $Year_t$ is a year fixed effect. The variable *PostDivorceCase* is a dummy variable equal to one if the test was taken after the parental divorce case was closed (whether due to a judgment of dissolution or a dismissal) while the variable *PostDivorceCase*Divorce* is the interaction between *PostDivorceCase* and a dummy variable equal to one if the parents' divorce case ended in divorce (as opposed to a dismissal). The variable *YearsAfterCaseClosure* is the number of years after the divorce case was closed (including dismissed cases) while the variable *YearsAfterCaseClosure*Divorce* is the interaction between the number of years after the

divorce case was closed and a dummy variable equal to one if the divorce case ended in a judgment of divorce.

The results are given in Table 14 and indicate that every year after the divorce, the family income of children whose parents divorce falls by $288 for every year afterwards (p=0.465). Six years after the divorce case ended, average zip code family income fell by $1,223 relative to their dismissed divorce counterparts, although that is not statistically significant at the 10% level. Still, this result is comforting to the extent that one would expect children whose parents divorce to move to lower-income neighborhoods relative to children whose parents dismissed divorce cases.

1.4.5 The Pre-Divorce Trends of Children Whose Parents Later File for Divorce

One might also be concerned that the student fixed effects approach may be insufficient if the pre-divorce *trends* of these two groups are different. In order to test whether there is a statistical difference between the pre-divorce trends of these two groups, I estimated the following equation similar to that which will be estimated to find the effects of divorce

$$Outcome_{it} = \theta_i + b_0\, Grade_{it} + b_1\, Grade^2_{it} + b_2\, Year_t + b_3\, PreDivorceCase_{it} + b_4\, PreDivorceCase_{it}{*}Divorce_{it} + b_5\, YearsBeforeFiling_{it} + b_6\, YearsBeforeFiling_{it}{*}Divorce_{it} + \varepsilon$$

where $Outcome_{it}$ is the outcome variable for student i at time t, θ_i is a student fixed effect, *Grade* is the student's grade, and *Year* is a student fixed effect. The variable *PreDivorceCase* is a dummy variable equal to one if the observation is prior to filing a divorce case, while $PreDivorceCase_{it}{*}Divorce$ is a dummy variable equal to one only if the observation was for a child whose parents would later file a divorce case and get divorced. The variable *YearsBeforeFiling* is the number of years prior to filing a divorce

case, while the variable $YearsBeforeFiling_{it}*Divorce$ is equal to the number of years prior to filing a divorce case that would end in divorce.

The coefficient of interest is thus b_6, which essentially captures the difference in the pre-divorce trends of children whose parents would file for and then dismiss a divorce case relative to those of children whose parents would later divorce. If $b_6<0$, it means that as one goes back in time from the time of the divorce, children whose parents later divorce get worse off relative to those who will experience a dismissed parental divorce. Equivalently (and perhaps more intuitively), to the extent that $b_6<0$, it implies that as the time of the divorce filing approaches, the divorce group is gaining relative to the dismissal group. Conversely, to the extent that $b_6>0$, as the time of the divorce filing approaches, the divorce group is dropping relative to the dismissal group.

The equation was estimated on a sample that excluded observations more than 3 years prior to the filing of the divorce in an attempt to capture trend differences that occur relatively close to the decision to file for divorce. The results given in Table 15 show that for neither test scores nor disciplinary problems was there a statistically significant difference in the trends of these two groups prior to divorce.[14]

1.4.6 The Causal Time-Invariant Effect of Parental Divorce

I now turn to the main question of how these outcomes compare *after* the divorce case has closed. By comparing the outcomes of children whose parents divorced to those of children whose parents filed divorce cases that were later dismissed, I can effectively

[14] When observations that occurred more than 3 years prior to the divorce filing are included, the differences between the pre-divorce trends in reading and math scores as well as days suspended remain statistically insignificant. However, there is a statistically significant difference in the pre-divorce trends for the number of disciplinary infractions, suggesting that as the time of divorce approaches, children whose parents later dismiss a divorce case commit 0.09 more infractions per year (p=0.095) than are children whose parents later divorce.

separate out the effect of the divorce itself from the effects of the underlying causes of the divorce. First I examine whether or not experiencing parental divorce has a time-invariant effect on student test scores and disciplinary problems. The regression equation estimated in order to address these issues was

$$Outcome_{it} = \theta_i + b_0\, Grade_{it} + b_1\, Grade^2_{it} + b_2\, Year_t + b_3\, PostDivorceCase_{it} + b_4\, PostDivorceCase_{it}*Divorce_{it} + \varepsilon_{it.}$$

The variable *PostDivorceCase* is a dummy variable equal to one if the test was taken after the parental divorce case was closed (whether due to a judgment of dissolution or a dismissal) while the variable *PostDivorceCase*Divorce* is the interaction between *PostDivorceCase* and a dummy variable equal to one if the parents' divorce case ended in divorce (as opposed to a dismissal). The standard errors used to calculate the p-values reported in the tables were clustered at the family-year level.

The coefficient of interest in this equation is b_3, which effectively captures the effect of having experienced a parental divorce relative to having experienced the dismissal of a parental divorce case. The results given in 16 indicate that parental divorce does not have a statistically significant effect on reading or math test scores. Finally, the results suggest that parental divorce causes children to be suspended 0.75 more days per year and to commit 0.33 more disciplinary infractions per year, both of which are statistically significant at the 5% level.

1.4.7 The Causal Effects of Parental Divorce Over Time

It may be, however, that the effect of parental divorce is a cumulative effect that increases over time. My data allow me to examine how the outcomes of these two groups change over time and whether or not they diverge from each other. The regression equation estimated to address these issues was

$$Outcome_{it} = \theta_i + b_0\, Grade_{it} + b_1\, Grade^2_{it} + b_2\, Year_t + b_3\, PostDivorceCase_{it} + b_4\, PostDivorceCase_{it}*Divorce_{it} + b_5\, YearsAfterCaseClosure_{it} + b_6\, YearsAfterCaseClosure_{it}*Divorce_{it} + \varepsilon_{it.}$$

The variable *YearsAfterCaseClosure* is the number of years after the divorce case was closed (including dismissed cases) while the variable *YearsAfterCaseClosure*Divorce* is the interaction between the number of years after the divorce case was closed and a dummy variable equal to one if the divorce case ended in a judgment of divorce.

The coefficients of interest are b_3 and b_5, which estimate the time-invariant effect of parental divorce and the time-varying effect of parental divorce, respectively. By utilizing data on children whose parents filed for but dismissed divorce cases, both coefficients capture the effect of parental divorce relative to the effect of dismissed divorce.

The estimated coefficients are given in the first several columns of Table 17, while the estimated effects of divorce after 1, 2, 4, and 6 years are calculated in the last 4 columns. The effect of parental divorce on reading and math scores after 6 years is positive at 3.98 and 5.01 percentile points, respectively, although neither effect is statistically significant at the 10% level.

The results also show that although there is an initial statistically significant spike in disciplinary problems immediately after the divorce, parental divorce causes a reduction in disciplinary problems after 6 years. For example, a student who experienced parental divorce gets suspended 2.13 more days per year thereafter but gets suspended 0.58 fewer days for every year after the divorce, both of which are statistically significant

at the 1% level. The net effect is that 1 year after the divorce was finalized, parental divorce causes statistically significant increases of 0.67 infractions and 1.56 days suspended per year, while there is no statistically significant effect after 4 years. After 6 years the student who experienced divorce is suspended 1.32 fewer days on average than the student whose parents dismissed the divorce case, an effect that is not quite statistically significant at the 10% level. These results suggest that although experiencing a parental divorce causes more disciplinary problems for children in the short term, after an initial adjustment period children are no worse off and perhaps better off in terms of disciplinary problems at school as a consequence of the divorce.

Given that the sample size here is relatively small, as a sensitivity check I examined whether the results were changed when any given divorce case was excluded from the sample. For the case of the disciplinary results, although the magnitude of the effect on days suspended and disciplinary infractions after 6 years when any given divorce case is dropped is never closer to zero than -0.56 and -0.29, respectively. Consequently, while it seems likely that parental divorce causes a short-lived initial increase in disciplinary problems, the notion that disciplinary problems for children overall are reduced in the long term is less certain.

1.4.8 Are the Effects of Parental Divorce Different for Boys than for Girls?

It may be, however, that the consequences of parental divorce are different for boys than for girls. In order to address that question, the regression equations were estimated separately for boys and for girls. The results are shown in Table 18.

The most striking result is that the effect of parental divorce on reading test scores is very different for boys than for girls. Although the effect for boys after 6 years is a statistically insignificant -2.75 percentile points, girls score a statistically significant and

large 14.68 percentile points higher on reading as a result of parental divorce. Again, due to the small sample size I examined the extent to which these results were sensitive to any given divorce case. Although dropping any one divorce case does not cause the effect for the girls to be lower than 12.45 percentile points (p=0.018), the results for the boys are more sensitive. For the boys, dropping one divorce case can cause the results to range from -0.08 after 6 years to -5.26 (p=0.093) after 2 years.[15] Consequently, while the estimates show that daughters typically thrive in terms of reading comprehension as a result of divorce, there is only weak evidence to suggest that the reading scores of boys are affected in a negative way.

The effect of divorce on the mathematics test scores of girls was estimated as 6.68 percentile points after 6 years, although it is not statistically significant (p=0.279). Dropping any one divorce case caused the effect for girls to range from 14.03 percentile points (p=0.016) to 2.16 percentile points (p=0.743), while the result for the boys ranged from 0.89 to 7.18 (p=0.328).

The results also show that although both boys and girls appear to have more disciplinary problems immediately following the divorce, the effect for boys is larger. For example, the effect of parental divorce on days suspended and disciplinary infractions for boys after one year is 2.27 days and 1.04 infractions, both of which are statistically significant. However, after 4-6 years, there is no statistically significant effect for boys. Girls, while experiencing a smaller initial increase in disciplinary problems immediately following the divorce, if anything they appear to benefit from the parental divorce in the long term by committing 0.73 fewer infractions and being

[15] The effect after six years in that case is -6.47 percentile points and has a p-value of 0.204.

suspended 1.67 fewer days 6 years after the divorce, although neither is statistically significant at the 10% level.

1.4.9 Does the Effect of Parental Divorce Depend on the Age of the Student at the Time of Divorce?

It is also possible that the effect of parental divorce on a child depends on how old the child was at the time the parents filed for divorce. In order to examine that possibility, I included a variable equal to the grade of the student at the time the divorce was filed for all observations after parents divorced (or zero if there was no divorce). The third column from the right of Table 19 gives the estimated coefficient of this variable while the last two columns estimate the effect of parental divorce 4 years after the fact. The first of those columns does so for a parental divorce that occurred when the child was in the 1st grade while that second column estimates the effect of a divorce that occurred when the child was in the 6th grade. The results suggest that any age effects are small, at best, as the coefficient on the grade-divorce interaction is never statistically significant at the 10% level. This suggests that although the age of the student at the time of the divorce does not seem to be an important factor in the effect of divorce, at least for the range of ages examined in this paper.

1.5 Robustness of Results

A frequent concern regarding studies that utilize relatively small data sets is that the results may be driven by a small number of outliers. In order to address the concern that a subset of dismissed divorce cases is in fact driving my result, I estimated the effects of parental divorce again after making four adjustments to the data set:

- Excluding dismissed divorce cases in which at least one parent's name was changed or dropped in the school district records. These cases were dropped due to the concern that the dropping or change of a parent name in a family not observed

to experience divorce may be correlated with negative family unobserved characteristics.

- Excluding dismissed divorce cases in which a motion for default was entered. A motion for default is entered by a petitioner so that he or she can proceed with the divorce without the other spouse being present. As near as I can tell, however, the other spouse did eventually respond in court in all of the cases in my data. Still, one might be concerned that such a motion may be correlated with negative unobserved family characteristics.

- Including all children whose parents were married but did not file for divorce. Although the results are identified by comparing the dismissed divorce group to the divorce group, these children were included to ensure that their absence did not influence the results.[16]

- Excluding all observations more than 5 years after the divorce case was closed. As discussed earlier, although 4 shows that the rate of attrition in the data set is approximately equal for children whose parents divorce compared to those whose parents file for and dismiss a divorce case, more than five years after the case is closed there are relatively more observations for the dismissal group than the divorce group. To ensure that the results are not sensitive to observations more than five years after the case was closed, those observations were excluded.

The results are shown in Tables 20 – 23. In row (a) of each table, the main result presented earlier from using the full data set is presented for comparison purposes. The results for reading scores presented in Table 20 show that the positive and statistically significant effect of parental divorce on girls is indeed robust to the changes mentioned above. Similarly, the result that there may be a small negative effect on the reading achievement of boys is also consistent.[17]

---

[16] The main reason for including the children whose parents did not divorce, besides allowing the cross-sectional comparisons presented earlier in the paper, was to help identify the year fixed effects and grade effects.

[17] One might also be concerned that although the pre-divorce test scores of children whose parents later divorce are similar to those whose parents later file and dismiss a divorce case, there may be differences between the groups when they are separated by gender. To some extent, this seems to be the case; the pre-divorce average reading and math scores for girls in the divorce group are 7.4 and 4.9 percentile points lower than those of the girls in the dismissal group. In contrast, the pre-divorce average reading and math scores for boys in the divorce group are 5.4 and 7.8 percentile points higher than those of boys in the dismissal group. To the extent that one would expect students with higher (lower) percentile test scores to see future gains (declines) in their scores *relative to their peers nationwide*, this would cause my results for

Table 21 shows the sensitivity results for mathematics scores. Again, the results show that the effect estimated for girls is not affected much by the various groups discussed above, although the effect still never reaches the point of statistical significance. Similarly, the effect of parental divorce for 5th and 10th grade boys four years after their parents divorced is never observed to be negative.

Table 22 shows the sensitivity results for days suspended per year. Consistent with the results presented earlier, after 4 years there does not appear to be a statistically significant effect of parental divorce on days suspended per year, with one exception. As shown in row (e), when all observations more than 5 years after the divorce case was closed were excluded, the effect of divorce on girls after four years is a statistically significant 1.06 additional day suspension per year. However, this result should be interpreted with caution since relatively few girls get suspended and the sample gets quite small after the deletion of all observations more than 5 years after the closure of the divorce cases.

Table 23 shows the results using the number of disciplinary infractions per year as the outcome variable. Again, although there is consistency to the idea that parental divorce causes a temporary spike in disciplinary problems—especially for boys—after 4 years the results are consistent in showing that there is not a statistically significant effect.

---

girls to be a lower bound while suggesting that the true effect for boys is more negative than my estimations show. However, clearly all students with high (or low) percentile scores in the country cannot move up (or down) relative to their peers over time, so it's unclear that this should be a concern at all.

1.6 Conclusions

Previous research has used the conditional outcomes of children from two-parent families as an estimate of the counterfactual that would be observed if parents who divorced were instead to stay together for an exogenous reason. In this paper, I argue that the performance of children whose parents filed for divorce but did not divorce is a much more realistic estimate of the appropriate counterfactual. Consequently, this paper has identified the effects of parental divorce on student performance by comparing the outcomes of children who experienced parental divorce to those whose parents filed for divorce but did not divorce, conditional on student fixed effects. The results indicate that parental divorce does not negatively affect academic achievement. In contrast, I find that parental divorce *positively* affects the reading scores of girls in a statistically significant and robust way; six years after the fact, girls score 14.68 percentile points higher as a result of the parental divorce. There is also somewhat weaker evidence to suggest that parental divorce positively affects the mathematics scores of girls, especially those who are older at the time of the divorce.

The evidence on the effect of parental divorce on the academic achievement of boys is somewhat less clear. Although I find some evidence that parental divorce negatively affects the reading achievement of boys, the effects are considerably smaller and are never statistically significant. I find no evidence that the mathematics achievement of boys is affected in a negative way by parental divorce.

I also show that although experiencing parental divorce may increase disciplinary problems for children overall in the short term, I find little evidence that it does so after 4 to 6 years and find that it may even reduce disciplinary problems in the long run. However, this result should be interpreted with caution since children whose parents later

dismiss a divorce case tend to have more disciplinary problems than children whose parents later divorce. Although the inclusion of student fixed effects in the estimation and the fact that I found no statistically significant different in the pre-divorce trends of the two groups in the three years prior to filing for divorce should reduce this problem, one might still be concerned that higher *levels* of disciplinary problems might be associated with higher future *trends* after the divorce, which would cause the estimated effects of parental divorce on disciplinary behavior to be biased downward.

Collectively, these results suggest that children overall are not harmed and that girls stand to benefit significantly from their parents ending a troubled marriage relative to the alternative. Although my data force me to remain agnostic regarding the exact mechanism through which this occurs, potential explanations consistent with my results include a reduction in parental conflict after the divorce or the refocusing of parental time from the marriage to the children.

These results differ significantly from those of previous research that has consistently found negative effects of parental divorce. The difference between these results and the results in previous research is most likely a direct consequence of the respective identification strategies used. Indeed, I show in this paper that although there is a strong correlation between having experienced a parental divorce and having lower outcomes, there is an even stronger correlation between having one's parents file and dismiss a divorce case and having lower outcomes. This suggests that the worse outcomes observed after parental divorce are largely not a consequence of the divorce itself but rather of the underlying problems that caused the couples to file for divorce. The appropriate conclusion is thus not that family problems do not negatively affect the

achievement and behavior of children, but rather that, conditional on having rather family problems significant enough for a parent to file for divorce, on average children overall are made no worse off by divorce and daughters are made significantly better off.

Although this paper did not examine unilateral divorce laws directly, the findings presented here may shed some light on that policy issue. The fact that I find no evidence that the academic achievement of children overall is negatively affected by parental divorce lends no support to the notion that policy-makers should make divorce more difficult in order to make children better off. Furthermore, my finding that divorce has large *positive* effects on the academic achievement of girls 6 years after the divorce suggests that there may well be significant social costs associated with using divorce laws to make divorce more difficult when children are involved.

Table 1-1: Matchable Divorces in Alachua County, Florida

General Divorce Cases Only (excludes Simplified Dissolutions)	Cases with unique husband-wife name combinations	Cases assuming that children are involved in 54% of General Divorces	Cases with children in grades 1-12 (12/18=66.67%)	Cases with children in the public school system (given 10% private enrollment)	Cases after excluding nonunique parent names in school file (10%)
924	886	478	319	287	258
863	802	433	289	260	234
875	816	441	294	264	238
876	820	443	295	266	239
965	900	486	324	292	262
900	840	454	302	272	245
909	843	455	303	273	246
974	904	488	325	293	264
878	816	441	294	264	238
868	817	441	294	265	238
180	170	92	61	55	50
9,212	8,614	4,652	3,101	2,791	2,512

Table 1-2: Families Matched to Unique Divorces

Year	Matchable divorces given nonunique names in school data	Divorce cases matched to school names	Divorce cases linked to at least one test score	Children with test scores linked to a divorce
1993	258	95	51	65
1994	234	68	47	56
1995	238	78	56	76
1996	239	61	45	64
1997	262	70	63	89
1998	245	58	53	74
1999	246	62	58	80
2000	264	83	75	114
2001	238	69	68	93
2002	238	66	65	88
2003	50	14	14	25
Total	2,512	724	595	824

Table 1-3: Families Matched to Unique Divorces

Sample	Cases Ending in Divorce Matched to Student Records	Cases Ending in Divorce Matched to Student Test Scores	Dismissed Divorce Cases Matched to Student Records	Dismissed Divorce Cases Matched to Student Test Scores
(a) All Cases Matched Using Unique Parent Names	658	542	-	-
(b) All cases matched using student names and birth dates retrieved from all dismissed divorce cases filed from 1993 - 2003	0	0	164	132
(c) Both (a) and (b)	658	542	164	132
(d) Same as (c), but excluding dismissed divorce cases not explicitly known to be dismissed voluntarily	658	542	131	99
(e) Same as (d), but excluding dismissed divorce cases in which one parent name was matched to a domestic violence case	623	511	123	92
(f) Same as (e), but excluding dismissed divorce cases matched to student records in which only one parent name was listed prior to the divorce	623	511	93	69

Table 1-4: Distribution of Observations of Students Matched to a Parental Divorce Case

			Students Observed in Post-Divorce Time Periods			
Data	Group	Total	Post-Divorce	1 - 3 Years	3 - 5 Years	5+ Years
All students	Children who Experience Parental Divorce	872 100%	667 76.5%	663 76.0%	314 36.0%	185 21.2%
	Children who Experience a Dismissed Parental Divorce	156 100%	114 73.1%	103 66.0%	53 34.0%	41 26.3%
Students observed with at least one test score	Children who Experience Parental Divorce	690 100%	530 76.8%	399 57.8%	226 32.8%	137 19.9%
	Children who Experience a Dismissed Parental Divorce	111 100%	90 81.1%	64 57.7%	41 36.9%	32 28.8%

Table 1-5: The Cross-Sectional Effects of Parental Divorce on Reading Test Scores

	Obs.	Parents Divorced Prior to Test	Divorce-years after divorce interaction	Divorce-grade of student interaction	Effect of divorce on 10th grader whose parents divorced 6 years prior
(a) includes year dummy variables and grade	26,252	-1.97			-
		0.192			
(b) Also includes race, sex, free lunch, zip code income, income squared, and year dummies	26,252	**-2.19**			-
		0.099			
		-4.03	**-0.79**	**1.23**	**-3.87**
		0.157	**0.057**	**0.030**	**0.059**
(c) Same as (b), but also includes school dummy variables	18,976	-0.78			-
		0.582			
		-2.51	-0.64	**1.15**	-1.79
		0.408	0.131	**0.046**	0.363

Coefficients and estimates that are statistically significant at the 10% level are in bold. Each row represents a different regression. Test scores are percentile rankings in the Iowa Test of Basic Skills and the Stanford 9 (FCAT) achievement tests. The first row of each cell presents the estimated coefficient, while the second row contains p-values calculated using robust standard errors.

Table 1-6: The Cross-Sectional Effects of Parental Divorce on Mathematics Test Scores

	Obs.	Parents Divorced Prior to Test	Divorce-years after divorce interaction	Divorce-grade of student interaction	Effect of divorce on 10th grader whose parents divorced 6 years prior
(a) includes year dummy variables and grade	23,228	-1.31			-
		0.380			
(b) Also includes race, sex, free lunch, zip code income, income squared, and year dummies	23,228	-1.32			-
		0.319			
		1.32	**-1.26**	0.25	**-5.25**
		0.670	**0.006**	0.674	**0.015**
(c) Same as (b), but also includes school dummy variables	18,962	-0.48			-
		0.719			
		2.51	**-1.25**	0.17	**-4.27**
		0.420	**0.005**	0.762	**0.034**

Coefficients and estimates that are statistically significant at the 10% level are in bold. Each row represents a different regression. Test scores are percentile rankings in the Iowa Test of Basic Skills and the Stanford 9 (FCAT) achievement tests. The first row of each cell presents the estimated coefficient, while the second row contains p-values calculated using robust standard errors.

Table 1-7: The Cross-Sectional Effects of Parental Divorce on Days Suspended Per Year

	Obs.	Parents Divorced Prior to Test	Divorce-years after divorce interaction	Divorce-grade of student interaction	Effect of divorce on 10th grader whose parents divorced 6 years prior
(a) includes year dummy variables and grade	60,196	**0.79**			-
		0.001			
(b) Also includes race, sex, free lunch, zip code income, income squared, and year dummies	60,196	**0.72**			-
		0.001			
		0.15	**0.16**	0.02	**1.16**
		0.553	**0.053**	0.791	**0.004**
(c) Same as (b), but also includes school dummy variables	19,841	0.21			-
		0.295			
		0.06	0.11	-0.06	0.49
		0.839	0.261	0.441	0.266

Coefficients and estimates that are statistically significant at the 10% level are in bold. Each row represents a different regression. The first row of each cell presents the estimated coefficient, while the second row contains p-values calculated using robust standard errors.

Table 1-8: The Cross-Sectional Effects of Parental Divorce on Disciplinary Infractions Per Year

	Obs.	Parents Divorced Prior to Test	Divorce-years after divorce interaction	Divorce-grade of student interaction	Effect of divorce on 10th grader whose parents divorced 6 years prior
(a) includes year dummy variables and grade	60,196	**0.39**			-
		0.004			
(b) Also includes race, sex, free lunch, zip code income, income squared, and year dummies	60,196	**0.35**			-
		0.005			
		0.28	0.07	-0.03	**0.54**
		0.109	0.151	0.333	**0.016**
(c) Same as (b), but also includes school dummy variables	19,841	0.10			-
		0.398			
		0.02	0.07	-0.04	0.27
		0.900	0.172	0.316	0.254

Coefficients and estimates that are statistically significant at the 10% level are in bold. Each row represents a different regression. The first row of each cell presents the estimated coefficient, while the second row contains p-values calculated using robust standard errors.

Table 1-9: The Cross-Sectional "Effects" of Dismissed Divorce on Reading Test Scores

	Obs.	Parents dismissed case prior to test	Dismissal-years after dismissal interaction	Dismissal-grade of student interaction	"Effect" of dismissal on 10th grader whose parents dismissed case 6 years prior
(a) includes year dummy variables and grade	23,305	-3.33			-
		0.362			
(b) Also includes race, sex, free lunch, zip code income, income squared, and year dummies	23,305	**-6.42**			-
		0.023			
		-7.53	-0.73	1.60	-5.49
		0.178	0.373	0.152	0.170
(c) Same as (b), but also includes school dummy variables	17,068	-3.40			-
		0.250			
		0.10	**-1.76**	1.48	-4.57
		0.989	**0.058**	0.289	0.326

Coefficients and estimates that are statistically significant at the 10% level are in bold. Each row represents a different regression. Test scores are percentile rankings in the Iowa Test of Basic Skills and the Stanford 9 (FCAT) achievement tests. The first row of each cell presents the estimated coefficient, while the second row contains p-values calculated using robust standard errors.

Table 1-10: The Cross-Sectional "Effects" of Dismissed Divorce on Mathematics Test Scores

	Obs.	Parents dismissed case prior to test	Dismissal-years after dismissal interaction	Dismissal-grade of student interaction	"Effect" of dismissal on 10th grader whose parents dismissed case 6 years prior
(a) includes year dummy variables and grade	20,533	-4.24			-
		0.297			
(b) Also includes race, sex, free lunch, zip code income, income squared, and year dummies	20,533	-3.78			-
		0.219			
		-1.79	-1.40	1.03	-6.08
		0.765	0.124	0.331	0.141
(c) Same as (b), but also includes school dummy variables	17,029	-3.52			-
		0.256			
		-3.51	-1.07	1.68	-3.20
		0.630	0.288	0.184	0.452

Coefficients and estimates that are statistically significant at the 10% level are in bold. Each row represents a different regression. Test scores are percentile rankings in the Iowa Test of Basic Skills and the Stanford 9 (FCAT) achievement tests. The first row of each cell presents the estimated coefficient, while the second row contains p-values calculated using robust standard errors.

Table 1-11: The Cross-Sectional "Effects" of Dismissed Divorce on Days Suspended Per Year

	Obs.	Parents dismissed case prior to test	Dismissal-years after dismissal interaction	Dismissal-grade of student interaction	"Effect" of dismissal on 10th grader whose parents dismissed case 6 years prior
(a) includes year dummy variables and grade	54,225	0.52			-
		0.214			
(b) Also includes race, sex, free lunch, zip code income, income squared, and year dummies	54,225	0.44			-
		0.208			
		-1.93	**0.66**	0.04	**2.20**
		0.009	**0.013**	0.588	**0.028**
(c) Same as (b), but also includes school dummy variables	17,798	0.99			-
		0.127			
		-3.21	**0.96**	0.18	**3.22**
		0.019	**0.017**	0.309	**0.033**

Coefficients and estimates that are statistically significant at the 10% level are in bold. Each row represents a different regression. The first row of each cell presents the estimated coefficient, while the second row contains p-values calculated using robust standard errors.

Table 1-12: The Cross-Sectional "Effects" of Dismissed Divorce on Disciplinary Infractions Per Year

	Obs.	Parents dismissed case prior to test	Dismissal-years after dismissal interaction	Dismissal-grade of student interaction	"Effect" of dismissal on 10th grader whose parents dismissed case 6 years prior
(a) includes year dummy variables and grade	54,225	0.45			-
		0.181			
(b) Also includes race, sex, free lunch, zip code income, income squared, and year dummies	54,225	0.38			-
		0.190			
		-0.53	**0.33**	-0.06	**1.21**
		0.257	**0.016**	0.421	**0.036**
(c) Same as (b), but also includes school dummy variables	17,798	0.73			-
		0.129			
		-1.02	**0.49**	-0.07	**1.64**
		0.143	**0.021**	0.440	**0.051**

Coefficients and estimates that are statistically significant at the 10% level are in bold. Each row represents a different regression. The first row of each cell presents the estimated coefficient, while the second row contains p-values calculated using robust standard errors.

Table 1-13: Descriptive Statistics

	(A) -	(B) -	(C) (Not Used in Analysis)	(D) (Used in Main Analysis)	(E) (Used in Main Analysis)	(D) - (E)
	Children in 2000 whose parents I think are married but never filed for divorce (met conditions 1-3 in Section 3.5)	Children in 2000 whose parents I think are married but never filed for divorce-more restrictive (met conditions 1-4 in Section 3.5)	Children whose parents later file for a divorce case that was dismissed but for which the petitioner did not specifically request the dismissal	Children whose parents later file for and specifically request the dismissal of a divorce case	Children whose parents later file a divorce case that ends in divorce	Difference between Column B and Column C
Age	-		-	10.3	11.9	**-1.6**
				(4.3)	(3.3)	**p=0.000**
% Black	28.4	14.5	54.7	18.2	19.6	-1.5
	(45.1)	(35.2)	(50.3)	(38.9)	(39.7)	p=0.798
% Male	50.1	51.4	49.1	60.0	47.3	**12.7**
	(50.0)	(50.0)	(50.5)	(49.4)	(50.0)	**p=0.075**
% Subsidized Lunch	33.6	17.3	77.7	39.4	32.4	7.0
	(47.2)	(37.8)	(40.4)	(48.0)	(44.8)	p=0.279
% Disabled	13.3	11.1	32.1	18.2	17.1	1.1
	(34.0)	(31.4)	(47.1)	(38.9)	(37.7)	p=0.845
% Gifted	9.6	10.6	5.7	12.7	10.0	2.7
	(29.5)	(30.8)	(23.3)	(33.6)	(30.1)	p=0.539
Average Zip Code Median	45,923	48,143	39,944	45,887	47,405	-1,518
Family Income	(12,407)	(11,703)	(11,409)	(11,422)	(12,707)	p=0.399
% Committed Disciplinary	21.8	17.7	37.1	36.4	21.5	**14.9**
Infraction in a year	(41.30)	(38.1)	(42.2)	(43.3)	(36.1)	**p=0.005**
Average number of times	0.78	0.60	1.38	1.27	0.65	**0.62**
disciplined per year	(2.50)	(2.2)	(2.16)	(2.8)	(1.8)	**p=0.023**
Average number of days	1.02	0.76	1.36	1.89	0.83	**1.06**
suspended per year	(4.18)	(3.5)	(2.56)	(5.2)	(3.2)	**p=0.033**
Reading Score	58.4	64.1	33.3	57.3	58.4	-1.1
	(29.8)	(28.1)	(28.03)	(29.1)	(27.5)	p=0.781
Math Score	60.0	66.0	38.6	56.5	59.8	-3.3
	(29.4)	(27.8)	(27.58)	(30.9)	(28.1)	p=0.459

If children are observed more than once in each category, the average was used. Standard errors are in parentheses. Differences reported may not be equal to differences in the numbers in the table due to rounding. Statistically significant differences at the 10% level are in bold.

Table 1-14: Estimated Effects of Parental Divorce on Student Family Income Using Student Fixed Effects

Outcome	Independent Variable				Effect of Divorce after:			
	Dummy Variable		Years After Variable					
	Post-Divorce Case (including dismissed cases)	Post-Divorce Case * Divorce (interaction)	Years after Divorce Case (including dismissed cases)	Years after Divorce Case * Divorce (interaction, finalized divorces only)	1 Year	2 Years	4 Years	6 Years
Zip Code Family Income	-84	508	395	-288	219	-69	-646	-1,223
	0.947	0.69	0.323	0.465	0.851	0.954	0.679	0.574

Coefficients and estimates that are statistically significant at the 10% level are in bold. Each regression includes student fixed effects, grade, grade squared, and year dummy variables as covariates. Each row represents a different regression. Estimated coefficients are given in the first row of each cell while p-values are given in the second row.

Table 1-15: Estimated Pre-Divorce Trends

Outcome	Independent Variable				Coefficient of Interest	Hypothesis Test
	Dummy Variable		Years Before Variable			
	Pre-Divorce Case (including dismissed cases)	Pre-Divorce Case * Divorce (interaction)	Years before Divorce Case was filed (including dismissed cases)	Years before Divorce Case * Divorce (Difference in Pre-Divorce Trends)	Difference in Pre-Divorce Trends	Reject null hypothesis that both groups have the same pre-divorce trend?
Reading	-2.52	1.32	1.03	0.16	0.16	No
	0.533	0.751	0.633	0.943	0.943	
Math	-1.81	2.08	2.21	-0.95	-0.95	No
	0.655	0.620	0.357	0.703	0.703	
Days Suspended	**0.49**	**-0.95**	0.19	-0.14	-0.14	No
	0.161	**0.013**	0.173	0.341	0.341	
Disciplinary Infractions	0.30	**-0.55**	0.04	0.01	0.01	No
	0.150	**0.012**	0.678	0.892	0.892	

Each regression includes student fixed effects, grade, grade squared, and year dummy variables as covariates. Each row represents a different regression. Estimated coefficients are given in the first row of each cell while p-values are given in the second row. Observations more than 3 years prior to filing were excluded from the sample.

Table 1-16: Estimated Time-Invariant Effects of Parental Divorce on Student Test Scores and Behavior

Outcome	Post-Divorce Case (including dismissed cases)	Post-Divorce Case * Divorce (interaction)	Effect of Divorce
Reading Score	0.39	-0.18	-0.18
	0.880	0.943	0.943
Mathematics Score	-3.41	2.99	2.99
	0.242	0.320	0.320
Days Suspended	**-0.56**	**0.75**	**0.75**
	0.034	**0.008**	**0.008**
Disciplinary Infractions	**-0.33**	**0.33**	**0.33**
	0.042	**0.048**	**0.048**

Each regression includes student fixed effects, grade, grade squared, and year dummy variables as covariates. Each row represents a different regression. Estimated coefficients are given in the first row of each cell while p-values are given in the second row.

Table 1-17: Estimated Effects of Parental Divorce on Student Test Scores and Behavior

Outcome	Independent Variable				Effect of Divorce after:			
	Dummy Variable		Years After Variable					
	Post-Divorce Case (including dismissed cases)	Post-Divorce Case * Divorce (interaction)	Years after Divorce Case (including dismissed cases)	Years after Divorce Case * Divorce (interaction, finalized divorces only)	1 Year	2 Years	4 Years	6 Years
Reading	1.73	-1.62	-0.15	0.93	-0.68	0.25	2.11	3.98
	0.525	0.567	0.831	0.176	0.795	0.925	0.498	0.324
Math	-2.69	2.35	-0.01	0.44	2.80	3.24	4.13	5.01
	0.376	0.455	0.992	0.605	0.351	0.294	0.286	0.328
Days	**-1.86**	**2.13**	**0.62**	**-0.58**	**1.56**	**0.98**	-0.17	-1.32
Suspended	**0.000**	**0.000**	**0.003**	**0.005**	**0.000**	**0.001**	0.717	0.110
Disciplinary	**-0.88**	**0.92**	**0.25**	**-0.25**	**0.67**	**0.43**	-0.07	-0.56
Infractions	**0.003**	**0.003**	**0.022**	**0.026**	**0.002**	**0.011**	0.784	0.202

Coefficients and estimates that are statistically significant at the 10% level are in bold. Each regression includes student fixed effects, grade, grade squared, and year dummy variables as covariates. Each row represents a different regression. Estimated coefficients are given in the first row of each cell while p-values are given in the second row. Test scores are percentile rankings on the Iowa Test of Basic Skills and Stanford 9 Exams.

Table 1-18: Estimated Effects of Parental Divorce on Student Test Scores and Behavior

Outcome	Sex	Independent Variable				Effect of Divorce after:			
		Dummy Variable		Years After Variable					
		Post-Divorce Case (including dismissed cases)	Post-Divorce Case * Divorce (interaction)	Years after Divorce Case (including dismissed cases)	Years after Divorce Case * Divorce (interaction, finalized divorces only)	1 Year	2 Years	4 Years	6 Years
Reading Score	Boys	3.82	-3.73	0.01	0.16	-3.57	-3.41	-3.08	-2.75
		0.237	0.277	0.990	0.867	0.257	0.280	0.438	0.610
	Girls	-2.04	2.41	-0.63	**2.04**	4.46	**6.50**	**10.59**	**14.68**
		0.569	0.517	0.437	**0.013**	0.206	**0.064**	**0.008**	**0.004**
Math Score	Boys	-1.59	2.45	-0.08	0.36	2.81	3.17	3.90	4.63
		0.707	0.583	0.945	0.754	0.507	0.462	0.462	0.506
	Girls	-4.35	2.56	-0.26	0.69	3.24	3.94	5.31	6.68
		0.186	0.459	0.819	0.543	0.303	0.224	0.226	0.279
Days Suspended	Boys	**-2.43**	**2.88**	**0.52**	**-0.61**	**2.27**	**1.66**	0.43	-0.79
		0.000	**0.000**	**0.062**	**0.027**	**0.000**	**0.000**	0.492	0.476
	Girls	-1.07	**1.20**	**0.64**	**-0.48**	0.72	0.24	-0.72	-1.67
		0.124	**0.095**	**0.025**	**0.096**	0.146	0.518	0.275	0.154
Disciplinary Infractions	Boys	**-1.28**	**1.31**	0.22	**-0.27**	**1.04**	**0.77**	0.22	-0.32
		0.001	**0.002**	0.146	**0.074**	**0.001**	**0.001**	0.487	0.584
	Girls	-0.34	0.41	**0.25**	-0.19	0.22	0.03	-0.35	-0.73
		0.359	0.297	**0.091**	0.193	0.459	0.913	0.352	0.244

Coefficients and estimates that are statistically significant at the 10% level are in bold. Each regression includes grade, grade squared, student fixed effects, and year dummy variables as covariates. Each row represents a different regression. Estimated coefficients are given in the first row of each cell while p-values are given in the second row. Test scores are percentile rankings on the Iowa Test of Basic Skills and Stanford 9 exams.

Table 1-19: Estimated Effects of Parental Divorce on Student Test Scores and Behavior

Outcome	Sex	Independent Variable					Effect after 4 Years	
		Dummy Variable		Years After Variable			Child Grade at Filing	
		Post-Divorce Case (including dismissed cases)	Post-Divorce Case * Divorce (interaction)	Years after Divorce Case (including dismissed cases)	Years after Divorce Case * Divorce (interaction, finalized divorces only)	Divorce * Student Grade at Time of Divorce (finalized divorces only)	Grade 1	Grade 6
(a) Reading Score	Boys	3.81	-3.41	0.01	0.15	-0.06	-2.87	-3.19
		0.240	0.432	0.993	0.879	0.903	0.500	0.438
	Girls	-1.95	1.10	-0.59	**2.09**	0.26	**9.73**	**11.01**
		0.587	0.809	0.471	**0.011**	0.602	**0.029**	**0.007**
(b) Math Score	Boys	-1.60	2.48	-0.08	0.36	-0.01	3.93	3.90
		0.707	0.655	0.944	0.754	0.991	0.505	0.464
	Girls	-4.19	0.05	-0.20	0.73	0.45	3.42	5.67
		0.201	0.991	0.860	0.519	0.398	0.477	0.199
(c) Days Suspended	Boys	**-2.39**	**1.95**	**0.54**	**-0.57**	0.16	-0.16	0.63
		0.001	**0.043**	**0.052**	**0.040**	0.206	0.837	0.337
	Girls	-1.04	0.95	**0.64**	-0.47	0.04	-0.87	-0.68
		0.131	0.232	**0.024**	0.107	0.387	0.200	0.303
(d) Disciplinary Infractions	Boys	**-1.27**	**1.09**	0.22	**-0.26**	0.04	0.08	0.27
		0.001	**0.025**	0.138	**0.084**	0.497	0.825	0.410
	Girls	-0.34	0.41	**0.25**	-0.19	0.00	-0.35	-0.35
		0.360	0.359	**0.092**	0.193	0.996	0.390	0.350

Coefficients and estimates that are statistically significant at the 10% level are in bold. Each regression includes grade, grade squared, student fixed effects, and year dummy variables as covariates. Each row represents a different regression. Estimated coefficients are given in the first row of each cell while p-values are given in the second row.

Table 1-20: Estimated Effects of Parental Divorce on Student Reading Test Scores

Restrictions	Sex	Independent Variable					Effect after 4 Years	
		Dummy Variable		Years After Variable		Grade Interaction	Student Grade at Filing	
		Post-Divorce Case (including dismissed cases)	Post-Divorce Case * Divorce (interaction)	Years after Divorce Case (including dismissed cases)	Years after Divorce Case * Divorce (interaction, finalized divorces only)	Divorce * Student Grade at Time of Divorce (finalized divorces only)	Grade 1	Grade 6
(a) None; Same data and specification as row (a) of Table 15	Boys	3.81	-3.41	0.01	0.15	-0.06	-2.87	-3.19
		0.240	0.432	0.993	0.879	0.903	0.500	0.438
	Girls	-1.95	1.10	-0.59	**2.09**	0.26	**9.73**	**11.01**
		0.587	0.809	0.471	**0.011**	0.602	**0.029**	**0.007**
(b) Excludes dismissed divorce cases in which at least one parent's name was changed or dropped	Boys	4.14	-3.82	0.03	0.17	-0.05	-3.20	-3.43
		0.218	0.386	0.977	0.874	0.931	0.464	0.420
	Girls	-1.89	1.09	-0.63	**2.13**	0.25	**9.88**	**11.11**
		0.598	0.810	0.437	**0.010**	0.614	**0.027**	**0.006**
(c) Excludes dismissed divorce cases in which a motion for default was filed	Boys	4.84	-4.31	-0.11	0.26	-0.09	-3.33	-3.76
		0.173	0.347	0.916	0.800	0.870	0.463	0.396
	Girls	-2.13	1.34	0.59	**2.09**	0.24	**9.96**	**11.18**
		0.568	0.773	0.469	**0.012**	0.617	**0.027**	**0.007**
(d) Includes all children whose parents did not file for divorce	Boys	3.74	0.24	0.67	-0.41	-0.58	-1.97	-4.86
		0.300	0.958	0.513	0.708	0.263	0.680	0.282
	Girls	-2.89	3.34	**-1.28**	**2.00**	-0.20	**11.19**	**10.21**
		0.435	0.470	**0.086**	**0.018**	0.692	**0.015**	**0.016**
(e) Excludes all observations more than 5 years after the divorce case was closed	Boys	5.21	-4.70	-0.84	1.24	-0.25	-0.01	-1.27
		0.145	0.317	0.576	0.428	0.641	0.998	0.811
	Girls	-0.94	0.78	-1.27	**2.54**	0.17	**11.11**	**11.96**
		0.802	0.870	0.161	**0.011**	0.736	**0.015**	**0.004**

Coefficients and estimates that are statistically significant at the 10% level are in bold. Each regression includes grade, grade squared, student fixed effects, and year dummy variables as covariates. Each row represents a different regression. Estimated coefficients are given in the first row of each cell while p-values are given in the second row.

Table 1-21: Estimated Effects of Parental Divorce on Student Mathematics Test Scores

Restrictions	Sex	Independent Variable					Effect after 4 Years	
		Dummy Variable		Years After Variable		Grade Interaction	Student Grade at Filing	
		Post-Divorce Case (including dismissed cases)	Post-Divorce Case * Divorce (interaction)	Years after Divorce Case (including dismissed cases)	Years after Divorce Case * Divorce (interaction, finalized divorces only)	Divorce * Student Grade at Time of Divorce (finalized divorces only)	Grade 1	Grade 6
(a) None; Same data and specification as row (b) of Table 15	Boys	-1.60	2.48	-0.08	0.36	-0.01	3.93	3.90
		0.707	0.655	0.944	0.754	0.991	0.505	0.464
	Girls	-4.19	0.05	-0.20	0.73	0.45	3.42	5.67
		0.201	0.991	0.860	0.519	0.398	0.477	0.199
(b) Excludes dismissed divorce cases in which at least one parent's name was changed or dropped	Boys	-2.30	3.02	-0.05	0.34	0.02	4.42	4.52
		0.615	0.600	0.966	0.774	0.974	0.470	0.419
	Girls	-4.19	0.07	-0.23	0.75	0.44	3.50	5.72
		0.201	0.987	0.843	0.511	0.405	0.467	0.195
(c) Excludes dismissed divorce cases in which a motion for default was filed	Boys	0.31	0.10	0.16	0.24	0.09	1.15	1.59
		0.945	0.985	0.900	0.853	0.877	0.851	0.780
	Girls	-4.42	0.25	-0.28	0.79	0.45	3.87	6.11
		0.195	0.958	0.814	0.504	0.399	0.430	0.178
(d) Includes all children whose parents did not file for divorce	Boys	-2.73	1.66	-0.66	0.41	0.13	3.44	4.07
		0.543	0.768	0.571	0.742	0.832	0.585	0.485
	Girls	-4.53	2.11	-0.03	0.57	0.21	4.59	5.62
		0.197	0.656	0.977	0.644	0.696	0.385	0.241
(e) Excludes all observations more than 5 years after the divorce case was closed	Boys	-2.34	2.03	0.09	0.77	-0.06	5.06	4.79
		0.610	0.729	0.962	0.678	0.924	0.496	0.490
	Girls	-3.62	0.34	-0.62	0.58	0.45	3.09	5.33
		0.289	0.942	0.652	0.677	0.419	0.576	0.280

Coefficients and estimates that are statistically significant at the 10% level are in bold. Each regression includes grade, grade squared, student fixed effects, and year dummy variables as covariates. Each row represents a different regression. Estimated coefficients are given in the first row of each cell while p-values are given in the second row.

Table 1-22: Estimated Effects of Parental Divorce on Days Suspended per Year

Restrictions	Sex	Independent Variable					Effect after 4 Years	
		Dummy Variable		Years After Variable		Grade Interaction	Student Grade at Filing	
		Post-Divorce Case (including dismissed cases)	Post-Divorce Case * Divorce (interaction)	Years after Divorce Case (including dismissed cases)	Years after Divorce Case * Divorce (interaction, finalized divorces only)	Divorce * Student Grade at Time of Divorce (finalized divorces only)	Grade 1	Grade 6
(a) None; Same data and specification as row (c) of Table 15	Boys	**-2.39**	**1.95**	**0.54**	**-0.57**	0.16	-0.16	0.63
		0.001	**0.043**	**0.052**	**0.040**	0.206	0.837	0.337
	Girls	-1.04	0.95	**0.64**	-0.47	0.04	-0.87	-0.68
		0.131	0.232	**0.024**	0.107	0.387	0.200	0.303
(b) Excludes dismissed divorce cases in which at least one parent's name was changed or dropped	Boys	**-1.88**	**1.91**	**0.50**	**-0.51**	0.11	-0.01	0.53
		0.005	**0.044**	**0.073**	**0.066**	0.387	0.987	0.431
	Girls	**-1.01**	1.00	**0.64**	**-0.45**	0.03	-0.76	-0.59
		0.147	0.214	**0.025**	**0.125**	0.457	0.275	0.380
(c) Excludes dismissed divorce cases in which a motion for default was filed	Boys	**-2.55**	**2.59**	**0.82**	**-0.83**	0.11	-0.61	-0.05
		0.001	**0.014**	**0.014**	**0.013**	0.382	0.473	0.945
	Girls	-1.11	1.12	**0.68**	-0.48	0.03	-0.78	-0.63
		0.128	0.179	**0.023**	0.112	0.498	0.289	0.381
(d) Includes all children whose parents did not file for divorce	Boys	**-2.27**	**2.02**	**0.61**	**-0.59**	0.15	-0.18	0.57
		0.003	**0.037**	**0.047**	**0.063**	0.176	0.824	0.430
	Girls	-1.12	1.23	**0.53**	-0.44	-0.01	-0.56	-0.61
		0.131	0.124	**0.083**	0.157	0.784	0.458	0.402
(e) Excludes all observations more than 5 years after the divorce case was closed	Boys	**-1.51**	1.56	0.33	-0.25	0.05	0.60	0.87
		0.002	0.062	0.149	0.326	0.678	0.445	0.213
	Girls	0.44	-0.35	-0.14	**0.36**	-0.01	**1.09**	**1.05**
		0.324	0.535	0.232	**0.013**	0.818	**0.002**	**0.002**

Coefficients and estimates that are statistically significant at the 10% level are in bold. Each regression includes grade, grade squared, student fixed effects, and year dummy variables as covariates. Each row represents a different regression. Estimated coefficients are given in the first row of each cell while p-values are given in the second row.

Table 1-23: Estimated Effects of Parental Divorce on Disciplinary Infractions per Year

Restrictions	Sex	Independent Variable					Effect after 4 Years	
		Dummy Variable		Years After Variable		Grade Interaction	Student Grade at Filing	
		Post-Divorce Case (including dismissed cases)	Post-Divorce Case * Divorce (interaction)	Years after Divorce Case (including dismissed cases)	Years after Divorce Case * Divorce (interaction, finalized divorces only)	Divorce * Student Grade at Time of Divorce (finalized divorces only)	Grade 1	Grade 6
(a) None; Same data and specification as row (d) of Table 15	Boys	**-1.27**	**1.09**	0.22	**-0.26**	0.04	0.08	0.27
		0.001	**0.025**	0.138	**0.084**	0.497	0.825	0.410
	Girls	-0.34	0.41	**0.25**	-0.19	0.00	-0.35	-0.35
		0.360	0.359	**0.092**	0.193	0.996	0.390	0.350
(b) Excludes dismissed divorce cases in which at least one parent's name was changed or dropped	Boys	**-0.99**	**1.07**	0.16	-0.19	0.01	0.32	0.38
		0.010	**0.027**	0.277	0.200	0.839	0.400	0.257
	Girls	-0.33	0.43	**0.25**	**-0.18**	0.00	-0.30	-0.31
		0.389	0.330	**0.096**	**0.218**	0.929	0.475	0.415
(c) Excludes dismissed divorce cases in which a motion for default was filed	Boys	**-1.40**	**1.49**	**0.37**	**-0.39**	0.01	-0.08	-0.01
		0.002	**0.007**	**0.041**	**0.029**	0.821	0.854	0.970
	Girls	-0.38	0.48	0.20	-0.13	0.00	-0.05	-0.06
		0.328	0.287	0.197	0.386	0.925	0.910	0.873
(d) Includes all children whose parents did not file for divorce	Boys	**-1.15**	**1.04**	0.27	-0.27	0.04	0.03	0.25
		0.010	**0.049**	0.104	0.123	0.383	0.942	0.496
	Girls	-0.34	0.42	0.21	-0.17	-0.01	-0.27	-0.30
		0.402	0.342	0.171	0.281	0.809	0.532	0.469
(e) Excludes all observations more than 5 years after the divorce case was closed	Boys	**-0.69**	**0.90**	0.08	-0.10	0.05	0.48	0.35
		0.026	**0.034**	0.498	0.453	0.294	0.206	0.298
	Girls	0.29	-0.05	-0.10	0.16	-0.03	0.56	0.39
		0.489	0.909	0.534	0.334	0.230	0.157	0.300

Coefficients and estimates that are statistically significant at the 10% level are in bold. Each regression includes grade, grade squared, student fixed effects, and year dummy variables as covariates. Each row represents a different regression. Estimated coefficients are given in the first row of each cell while p-values are given in the second row.

CHAPTER 2
THE EFFECT OF ATTENDING THE FLAGSHIP STATE UNIVERSITY ON EARNINGS: A REGRESSION DISCONTINUITY APPROACH

2.1 Introduction

The question of the extent to which attending a more selective college affects subsequent earnings is of interest for several reasons. In addition to being of obvious significance to the students and parents making decisions regarding whether to incur costs associated with either getting admitted and/or attending a more selective university, it is also tied to the question regarding peer effects in education—whether attending college with more highly qualified students increases learning and ultimately productivity in the labor force.

The issue also raises policy questions, especially with respect to affirmative action in admissions policies at flagship state universities. In the past, flagship state universities have used such policies in order to admit more black and Hispanic students. However, beginning in the 1990's, these policies were challenged both legally and politically. For example, between 1996 and 1998 Texas and California eliminated affirmative action in college and university admissions; Florida followed suit in 2000. Finally, in 2003 the Supreme Court ruled in *Gratz v. Bollinger* that the University of Michigan's undergraduate admissions policy violated the Equal Protection Clause of the Fourteenth Amendment because it was not "narrowly tailored to achieve [the university's] asserted

compelling interest in diversity."[12] In addition, states may well be interested in how large the flagship university should become.

The empirical difficulty in estimating the effect of university selectivity on earnings is that attendance at more selective universities is likely correlated with unobserved characteristics that themselves will affect future earnings. Such biases could arise for two reasons. First, bias could arise if certain student abilities or characteristics are observed by college admissions committees by examining the student's applications but not by the econometrician. Second, bias could arise if, conditional on all observable student and family characteristics and admission to the more selective university, the decision to attend that university is correlated with unobserved student or family characteristics that would themselves affect subsequent earnings. For example, if the student chooses to attend the more selective university because she is more motivated than an observationally equivalent student who chose to attend a less selective school, the effect of selectivity on earnings will be overstated. On the other hand, if the student chooses to attend the more selective university because she received more need-based financial aid at the selective school relative to an observationally equivalent student who chose a less selective school, the effect of selectivity on earnings will likely be understated.

Researchers have taken several approaches to answering this question. Black and Smith (2004) describe problems that can arise for much of this literature that relies on the

---

[1] Taken from the majority opinion written by Chief Justice Rehnquist and accessed at http://www.supremecourtus.gov/opinions/02pdf/02-516.pdf.

[2] This is not to say that states may not legally engage in any affirmative action, however. Indeed, in the same ruling the court upheld the affirmative action practices of the University of Michigan Law School.

assumption of "selection on observables." Several other approaches have been used. Dale and Krueger (2002) compare the earnings of one group of students to another group who were accepted at similarly selective colleges but who chose to attend less selective colleges and find that attending more selective colleges has a positive effect on earnings only for students from low-income families. Brewer, Eide, and Ehrenberg (1999) estimate the payoff by explicitly modeling high school students' choice of college type and find significant returns to attending an elite private institution for all students. Behrman, Rozenzweig, and Taubman (1996) identify the effect by comparing female twin pairs and find evidence of a positive payoff for attending Ph.D.-granting private universities with well-paid senior faculty; Lindahl and Regner (2005) use Swedish sibling data and show that cross-sectional estimates of the selective college wage premium are twice the within-family estimates. Finally, in a related debate, Sander (2004) found negative effects of attending more selective law schools for black student beneficiaries of affirmative action, a conclusion that has been vigorously challenged by others (e.g., Ayres and Brooks, 2005).

In contrast to previous research, this paper identifies the effect of school selectivity on earnings by comparing the earnings of those just below the cutoff for admission to the flagship state university to those of applicants who were barely above the cutoff for admission. To do so, we combined confidential administrative records from a large flagship state university with earnings records collected by the state through the Unemployment Insurance program.

The unique data set used allows this paper to make two primary contributions to the existing literature. First, by using the application data from a large flagship state

university, this paper addresses the question of how college selectivity affects earnings in the context in which the public policy decision is made. Indeed, although determining the effect of attending an elite private college over a less selective one is interesting for several reasons, the public policy question is largely confined to the extent to which admission at flagship state universities affects the subsequent earnings of various subgroups.

Second, because we have actual admissions data from the university, we can use a regression discontinuity design to detect whether or not there is a discontinuity in earnings at the point of the admission cutoff. In this way we estimate an "intent-to-treat" effect—that is, the effect of admission to the flagship state university. Doing so overcomes any biases that might arise due to the correlation of the decision to enroll at the flagship university with other unobserved factors that may themselves affect earnings, so long as the assumptions for the regression discontinuity design are met.

By combining confidential admissions data from a large university to earnings records collected by the state through the Unemployment Insurance program, we find suggestive evidence of positive discontinuities in the earnings of white men that correspond to 1% - 27% higher earnings as a result of being admitted to the state flagship university, the magnitude and statistical significance of which depends largely on model specification. Furthermore, although there does not appear to be a consistent earnings effect for women overall, we do find that there is a positive flagship earnings effect for the subset of women with strong attachment to the labor force. Finally, we find no evidence that admission to the flagship causes applicants to be more or less likely to be observed in the labor force 10 – 15 years later.

2.2 Data

The data used in this study are from two sources. First, we acquired administrative data on admissions from a large flagship state university. As part of the agreement in acquiring the data, we agreed not to disclose the name of the institution involved. The university was able to retrieve the following information for every student that applied for admission to the university from 1986 – 1989: Social Security Number, race, sex, term for which the student was applying for admission, ACT score, SAT math score, SAT verbal score, whether or not the student subsequently enrolled, year of birth, and whether or not the student subsequently graduated from the university. Finally, we also observe each student's high school GPA, a discrete (to the nearest tenth of a point) number recalculated by the university after excluding certain courses and adjusting for different scales used by high schools.

These data were then sent to a state office to which employers submit Unemployment Insurance tax reports. Using the provided Social Security Numbers, quarterly earnings records from 1998 through the second quarter of 2005 were matched to the university records. All nominal wages were adjusted using the CPI so as to be measured in 2005 dollars.

One advantage of these earnings data is that they allow us to look at earnings well after nearly all applicants have completed their educations. The primary results in the paper are based on earnings observed 15 years after high school graduation—or when the individual is approximately 33 years old. These earnings are much more likely to be predictive of lifetime earnings than are earnings observed for people in their early and mid-twenties who are still finishing their educations and sorting themselves in the job market.

Another advantage of these administrative data over survey data is that they likely contain less measurement error. A limitation, however, relates to the fact that an individual's earnings will not be observed if he or she is employed in a job not covered by the UI system or has moved out of state. The latter of these may be a particularly significant concern to the extent that working in state is endogenous to whether or not the student was admitted to the flagship state university. Fortunately, the data also allow us to examine if there is a discontinuity in whether or not an applicant is observed with earnings.

There were 38,719 high school graduates who applied for admission in the summer or fall of the 1985-86, 1986-87, 1987-88, 1988-89, or 1989-90 school years. Of those, there were 7,024 for whom we did not observe either a high school GPA or an ACT or SAT score. In addition, 992 applicants were excluded because their high school GPA was lower than 2.0 or higher than 4.0. Two hundred fourteen more applicants were excluded because they cancelled their application prior to the admission decision. Finally, 1,674 applicants were deleted because they did not meet the minimum GPA required for admission in that term/year.[3] Thus, the final data set contains observations on 28,815 applicants.

2.3 Identification Strategy

This paper uses a regression discontinuity design to estimate the causal effect of admission at a state's flagship university on earnings. This design will distinguish the effect of admission to the flagship university from other confounding factors so long as the unobservable determinants of earnings (e.g., motivation, parental support, etc.) are

[3] In some of the years, a 2.0 was the minimum GPA required. Later on, however, this minimum was increased and there was no SAT score observed that would ensure admission for someone with that GPA.

continuous at the admission cutoff. As long as this condition holds, any discontinuous jump in earnings at the admission cutoff is properly interpreted as the causal effect of admission to the flagship university on earnings.

This condition will fail in this context if either applicants or the university can manipulate the side of the cutoff on which applicants fall. For applicants, this would be a problem if those who would barely miss the cutoff were to retake the SAT until they surpassed the cutoff. In reality, such a scenario is unlikely for the simple reason that the admission rule was never published or revealed by the university and, in fact, was changed from year to year. Consequently, it is very unlikely that the applicant would know, prior to applying, whether or not she was just above the cutoff or just below it. Furthermore, although there was an appeals process for rejected applicants, it affected relatively few students and was described by one admissions officer on the committee at the time as "very noisy".

Applying a regression discontinuity design in this context is somewhat different, however. The reason is that the admission cutoff rule is two dimensional rather than one dimensional since it depends on both the SAT score and the high school GPA and took the form of a nonlinear sliding scale. To address this issue and convert the two-dimensional sliding scale rule into a one-dimensional rule, we created an adjusted SAT score for each student. We did so by subtracting the SAT score required for admission, given the student's high school GPA, from each student's actual SAT score. For example, if an SAT score of 1100 was necessary for admission given a student's high school GPA and that student scored an 1150 on the SAT, that student was assigned a score of 50. As a result, all students assigned scores of 0 or higher were predicted to be

accepted to the flagship university. In case students with similar adjusted SAT scores have different earnings potentials, we directly control for actual SAT and high school GPA when estimating the earnings effects.

One approach to estimate the discontinuity at the cutoff is to compare the earnings of those who barely were admitted (e.g., those with adjusted SAT scores of 0 or 10) to those who were barely rejected (e.g., those which adjusted SAT scores of -20 or -10). However, if earnings increase with adjusted SAT scores as is likely the case, this will overstate the effect of admission to the flagship university on earnings.

The alternative approach is to estimate an equation for the outcome as a function of the adjusted SAT score. Specifically, we estimate the following equation using least squares regression:

$$Outcome_i = \beta_0 + \ + \ \beta_1 X \ + \beta_2(Admit_i) + \ \gamma(f(Adjusted\ SAT\ Score_i)) + \varepsilon_i$$

where $Admit_i = 1$ if $(Adjusted\ SAT\ Score_i) \geq 0$, $f(Adjusted\ SAT\ Score_i)$ is a flexible polynomial function of the adjusted SAT score, and X is a vector of control variables, which included year-by-term of admission dummies, actual SAT score and high school grade point average.[4] I then estimate this equation using various functions f(·).

2.4 The Admission Rule

2.4.1 Estimating the Admission Rule

Because the admissions records are nearly 20 years old, the university did not have records of the exact rules used to determine admissions. During the time in question, however, admissions decisions were made using a discrete sliding scale of high school

[4] By controlling directly for SAT score and high school GPA, we control for the fact that the earnings ability of an individual with a high SAT score and low GPA may be different from that of an individual with a high GPA and a low SAT score, even if both have the same adjusted SAT score.

GPA (as adjusted by the university to account for course content and differences in high school GPA scales) and SAT score.[5] That is, for a given high school GPA, the student was admitted if her SAT score met or exceeded the cutoff SAT score. Higher high school GPAs implied lower minimum SAT scores necessary for admission. For example, to compensate for a high school GPA that was one tenth of a point lower, a student may have to have an SAT score that is 20 points higher.[6]

In order to estimate the admission cutoff, the data were first partitioned by race and term of application (either summer or fall). The data were then partitioned further by high school GPA, after which the following equation was estimated using Ordinary Least Squares:

$$Acceptance = \beta_0 + \beta_1(SAT\_Cutoff) + \epsilon$$

where *Acceptance* is a dummy variable equal to one if the student was accepted and *SAT_Cutoff* was a dummy variable equal to one if the SAT score was greater than or equal to a given SAT score.

For example, the SAT cutoff for the fall of 1986 for white males with a high school GPA of 3.5 was determined by estimating this equation separately using all possible SAT scores (e.g., from 800 to 1400) as the cutoff. The SAT score that resulted in the estimation with the highest R^2 was the cutoff that was then used.[7] This process was

---

[5] For some students, ACT scores were used instead. In those cases, we converted these to equivalent SAT scores using the university formula.

[6] To assure the confidentiality of the university that provided the data, we cannot reveal the admission standards as we estimated them. However, we can note that the tradeoff between SAT score and high school GPA was nonlinear.

[7] The "winning" R^2 was typically around 0.50.

repeated for all cohorts. For example, it was repeated for the fall of 1986 for white males with a high school GPA of 3.6, and then 3.7, etc.

2.4.2 Does the Admission Cutoff Predict Which Students Are Accepted and Which Are Rejected?

After estimating the admission cutoff, the obvious question is whether or not the probability of acceptance at the university is discontinuous at the admission cutoff. This can be seen in Figure 1, which shows the probability of being accepted (the outcome) on the vertical axis and the number of SAT points above or below the cutoff *given* the student's high school GPA on the horizontal axis. This figure takes the same form as others presented after it. The open circles represent local averages. For example, at an adjusted SAT score of zero (i.e., for students who barely made the estimated admissions cutoff), the open circle is the percentage of those applicants who were accepted at the flagship.

The estimates of the discontinuity shown in Figure 1 are reported in Table 1. Row (1) reports estimates of the discontinuity without controlling for either a function of the adjusted SAT score or the actual SAT score and high school GPA. In contrast, the regression equations that yielded the discontinuity estimates reported in rows (2) – (4) included polynomials of order 3 or higher in the adjusted SAT score. For example, the equation estimated in row (2) includes the variables (Adjusted SAT Score), (Adjusted SAT Score)^2, and (Adjusted SAT Score)^3 as well as those same variables interacted with a dummy equal to one if the applicant was predicted to have been admitted by the flagship (i.e., those for whom the Adjusted SAT Score $\geq$ 0). Specifications (2) and (4) also control for each applicant's actual SAT score and adjusted High School GPA.

The results are consistent in showing that there is a large and statistically significant discontinuity in the likelihood of acceptance at the admission cutoff on the order of 68 – 69 percentage points. For example, in the preferred specification in row (4) that allows for fourth-order polynomials in Adjusted SAT Score on both sides of the admission cutoff, the estimated discontinuity is 68.6 percentage points, which is statistically significant at all traditional levels. Furthermore, a look at the underlying data in Figure 1 shows that this discontinuity is not simply the consequence of an incorrect functional form.

2.4.3 Potential Causes of the 'Fuzziness' of the Estimated Admission Discontinuity

The fact that this estimated discontinuity is less than one means that this design utilizes a 'fuzzy' discontinuity. Several factors may be responsible for this, all of which must be considered when interpreting the results on earnings. First, a handful of high schools had reputations for giving lower grades than average. While this was not taken into account by the university in calculating adjusted high school grade point averages, it was something taken into account during the admissions process. Other exceptions were made for student-athletes and perhaps for the occasional son or daughter of donors. This, however, does not invalidate the regression-discontinuity design. Rather, any estimate of the effect of admission on earnings will be valid only for those who are affected by the admission guideline. While this is the majority of applicants, it would not include, for example, student-athletes or those who attended one of the handful of high schools treated differently by the admissions committee. Indeed, the discontinuity shown in Figure 1 shows clearly that the estimated admission rule was the defining factor in admissions for the majority of applicants.

There are other sources of noise that may cause the estimated discontinuity in the probability of being admitted to be less than one. As in any process with human involvement, errors in either the decision-making process or the reporting process almost certainly exist to some degree. Even more significantly, in any given term the university aimed to enroll a certain number of students. Given uncertainty about yields, the university would often change the admission rule slightly during the process to accept more or fewer students, thereby introducing noise into the admission rule.[8,9] Finally, and perhaps most importantly, given the presence of any noise in the admission process such as that caused the factors mentioned above, the estimation of the rule is itself probably characterized by some degree of error, especially for cells in which there were relatively few observations.

However, there are less innocuous explanations of why the estimated discontinuity is less than one. Perhaps the most problematic of these would be that the university observes something about the student unobserved to the researcher, which is the type of bias about which Dale and Krueger (2002) were primarily concerned. The best evidence against this possibility comes from the application form itself, which reveals the information about the student that was disclosed to the university. Contrary to what one might expect based on the application process for universities at the current time, the application in the late 1980's was very simple. On it, students were required to include

---

[8] According to one admission official who served in the late 1980's, the university aimed to err on the side of a policy that was too low at first, then toughened it slightly if more students chose to attend than expected. In this way the university didn't reject applicants who it would have accepted had the university processed the application somewhat more quickly.

[9] This noise will not bias the earnings estimates so long as the applicants who were admitted did not have different earnings potential relative to those with identical SAT and high school GPAs who were not admitted. This seems, at least to us, like a reasonable assumption given the applicant and bureaucratic noise that determines the processing order of applications.

their race, age, nation of birth, address, high school name and address, emergency contact information, planned major, and current semester high school courses. In addition, applicants were asked if they were found guilty of any non-minor offenses or to have interfered with the operation of any educational institution. Each applicant was also asked whether family members attended the university. Finally, the applicant was required to have official high school transcripts and an SAT or ACT score sent to the university. Notably absent from the required application materials were letters of recommendation and essays.

As stated earlier, of the information contained in the application, the only parts used in making the application decision were the SAT score and the (adjusted) high school grade point average. More important, however, is that the simplicity of the admission rule consisting of a sliding scale using adjusted high school GPA and SAT score that the university asserts to have used in this time period is reflected in the application itself.

As a result of the 'fuzzy' admissions cutoff, all wage discontinuity estimates must be reweighted by the estimated admission discontinuity in order to determine the treatment effect.

2.4.4 Do Applicants Who Just Meet the Admission Cutoff Subsequently Attend and Graduate from the Flagship State University?

Although we estimate an intent-to-treat effect in order to avoid the selection bias that might arise from choosing whether to attend the flagship state university conditional on acceptance, clearly the receipt of an acceptance letter by itself could not affect earnings later on. Rather, the mechanism through which the effect could occur would be attending the flagship university.

The data show that there are large differences in the likelihood of subsequent attendance of the flagship university between those who barely met the admission cutoff and those who barely missed it. For example, among those who just met the admission cutoff (those with adjusted SAT scores of 0), 51.0% of them subsequently attended the flagship university compared to 10.4 % of those students who missed the cutoff by 10 SAT points, given their high school GPA. Among those who met or exceed the admission cutoff by no more than 20 SAT points (again, conditional on high school GPA), 53.3% subsequently attended the flagship university compared to only 11.5% of those who missed the cutoff by no more than 30 SAT points.

As one might expect, there are also significant differences in the likelihood of graduating from the flagship university. Only 7.2% of those with adjusted SAT scores of -10 subsequently graduate from the flagship university compared to 32.5% of those who barely were accepted and had adjusted SAT scores of 0. Similarly, only 8.0% of applicants who missed the cutoff by no more than 30 SAT points (conditional on high school GPA) graduated from the flagship, compared to 36.3% of those who exceeded the cutoff by no more than 20 SAT points.

2.4.5 Do Admitted Applicants above the Admission Cutoff Enroll and Graduate from the Flagship at Different Rates than Applicants Just Below the Cutoff?

One test of whether applicants on either side of the discontinuity are similar is to see if, conditional on acceptance (or enrollment), those who barely met the estimated admission cutoff were more or less likely to enroll (or graduate) than those barely missed the estimated cutoff.[10] This can best be seen by Figures 2 and 3. Figure 1 shows the

[10] Unfortunately, this is one of the only tests we can perform to see if students on either side of the cutoff were similar, since we were unable to get additional background information on the applicants. Later in the paper we will show that controlling for SAT score and adjusted high school GPA does not change the

likelihood of choosing to enroll at the flagship university, conditional on being accepted to the flagship university.[11] Figure 2 shows the likelihood of graduating from the flagship university, conditional on enrolling at the flagship university. To the extent that the graduation rates were discontinuous at the admission cutoff, one might be concerned that the groups on either side of the admission cutoff were not in fact similar.

There does appear to be a difference in the likelihood of enrolling at the flagship university, conditional on admission, at the admission cutoff, as shown by Figure 1. There does not, however, appear to be a discontinuity in the likelihood of graduating from the flagship university, conditional on enrollment, as shown in Figure 2. Although the results on enrollment may raise some questions, the fact that the graduation rates are no different for those who barely exceeded the cutoff than those who barely missed it supports the assumption that those on either side of the admission cutoff are very similar except for their likelihood of being admitted.

2.5 Attrition from the Earnings Data

As discussed earlier, one drawback to using these state-level wage data is that some applicants may not be observed with positive earnings 10 - 15 years after high school graduation because they moved to a different state. To the extent that the probability of working in-state is endogenous to admission and attendance at the flagship university, the discontinuity estimates may be biased. The data allow us to examine the extent to which this is the case by comparing the probability of observing applicants who were barely

---

discontinuity estimates, which shows that, even on the margin, the university was not selecting high GPA (or high SAT) students among others with equivalent adjusted SAT scores.

[11] Since there are relatively few students below the cutoff who were admitted to the flagship (as shown in Figure 1), these data were restricted to those who missed the cutoff by fewer than 100 points. There were 58 applicants who missed the cutoff by 10 SAT points but still were accepted; 42 of those students enrolled.

accepted to that of observing the earnings of applicants who were barely rejected. Furthermore, we do so separately for men than women due to the differences in their respective labor force participation rates. We also do so using two definitions of being observed with positive earnings. For example, we examine whether there is a discontinuity in the likelihood of being observed with 4 consecutive quarters of earnings in the 10th year after high school graduation as well as in the likelihood of being observed with any positive quarter of earnings in that year.

2.5.1 The Attrition of White Males

The best evidence of whether there is a discontinuity in the attrition of men in the earnings data is visual. Specifically, Figures 4a – 4i show the likelihood of being observed with 4 consecutive quarters of earnings in each of the 7th – 15th years after high school graduation. Shown on each figure are the local averages as well as the predicted probability based on a cubic polynomial of adjusted SAT score on either side of the admission cutoff. These discontinuity estimates are summarized in Table 2. Of the estimated discontinuities shown in Figures 4a – 4i, the estimated discontinuity is statistically significant at the 10% level only for 14 years (Figure 4h, estimate = 0.048, p=0.061). More importantly, perhaps, is that it is difficult to see a distinct discontinuity in the underlying data in any of the nine figures.

Figures 5a – 5i show the likelihood of being observed with any positive earnings in the years following high school graduation, beginning in the 7th year when the applicants were approximately 25 years old. Here, none of the estimates is statistically significant at the 10%, although the estimate for 11 years comes close (p = 0.105). Thus, of the eighteen estimations shown on the first two columns of Table 2 (and in Figures 4 and 5), only one estimate is statistically significant at the 5% level, which coincidentally is what

one would expect by chance alone. In addition, the underlying data shown in the figures hardly reveal compelling evidence of a discontinuity in any of the graphs. Consequently, it seems unlikely that the wage discontinuity estimates should be biased because those who were barely accepted at the flagship state university were more or less likely to show up in the earnings data 7 – 15 years after entering college.

2.5.2 The Attrition of White Females

The story is remarkably similar for white women. Figures 6a – 6i show the probability of being observed with 4 consecutive quarters of earnings 7 – 15 years after high school graduation. Similarly, Figures 7a – 7i show the probability of being observed with any positive earnings 7 – 15 years after graduating from high school. Of all the discontinuity estimates from those graphs, only the discontinuity for being observed with any positive earnings after 9 years (Figure 7c, estimate = -0.067) was statistically significant at either the 10% or 5% level. Coincidentally, out of 18 estimates summarized in the last two columns of Table 2, one would expect roughly one statistically significant estimate by chance alone. And again, no compelling discontinuities seem evident in the underlying data graphed in Figures 6a – 6i and Figures 7a – 7i.

2.5.3 The Admission Discontinuity for Those Observed with Positive Earnings

Even though there does not appear to be compelling evidence of attrition in the data caused by admission to the flagship state university, since the wage estimate results are based on the sample of applicants observed with positive earnings, it is instructive to ensure that the admission discontinuity is also observed with this group. The underlying data and estimated admission discontinuity is shown on Figure 8a and 8b for that group of individuals for whom we observe 25 consecutive quarters of earnings in the 12th and 15th year following high school graduation, respectively. Figures 9a and 9b show the

underlying data and regression discontinuity estimates for the applicants observed with any positive earnings in the 12th and 15th years following high school graduation, respectively.

The result is clear, if unsurprising: There is still a statistically significant discontinuity of approximately 0.70 even for those who stayed in the sample and were observed with positive earnings 12 and 15 years after high school graduation.

2.6 The Effect of Admission at the Flagship University on Labor Market Outcomes

2.6.1 The Earnings of White Males

Since the effect of attending a flagship state university may very well vary by race and sex, I first examine the effect of admission at the flagship university on the subsequent earnings of white males. To do so, I used two definitions of earnings. The first was the natural log of the sum of four quarters of consecutive real earnings in the 10th – 15th years following high school graduation, or when the individuals were approximately 28 - 33 years old.[12] Consequently, for those who applied for admission in the fall of 1986, I used earnings received from the 3rd quarter of 2001 through the 2nd quarter of 2002. Similarly, for those who applied for admission in the fall of 1987, I used earnings received from the 3rd quarter of 2002 through the 2nd quarter of 2003, and so on.

[12] The advantage of examining earnings in this time period was shown by Mincer (1974), who showed that the return to schooling can be underestimated if earnings prior to the "year of overtaking" are used. Assuming that the cost of investment is constant over time, that year is equal to (1+1/r) years after the completion of formal education, where r is the interest rate. Thus, assuming r=0.09 and an applicant finishes schooling at age 22, the year of overtaking is 22 + 12.1 = 34.1, which is approximately the age examined in this paper. This matters to the extent that attending the flagship university causes differences in post-schooling investment.

The second measure of earnings is the natural log of the annualized average earnings in the four quarters of consecutive earnings in each of the 10th – 15th years following high school graduation.[13]

The results for the 10th – 15th years following high school graduation are shown in Figures 10a – 10f for the consecutive quarter earnings measure and in Figures 11a – 11f for the annualized earnings measure. Plotted on each figure are the local averages for each adjusted SAT score along with two fitted lines. Since the underlying data appear—at least to us—to be linear in the adjusted SAT score, the first fitted line of predicted earnings is from an OLS regression in which we control for adjusted test score in a linear fashion allowing for a different slope on either side of the admission cutoff. The second fitted line of predicted earnings is from an OLS regression in which we control for a cubic polynomial of adjusted test score as well as a cubic interacted with a dummy variable equal to one for those with an adjusted SAT score greater than or equal to 0.

The discontinuity estimates from both approaches and for both measures of earnings are shown on the figures themselves as well as in Table 3. In the linear case, they range from 1% to 8%, which corresponds to an increase in wages due to admission at the flagship university on the order of 1.5% to 11%. However, only 4 of the 12 estimates are statistically significant at the 10% level, and only 2 of those are statistically significant at the 5% level.

The linearity assumption in the functional form is important, however, as is evident from the discontinuity estimates using the cubic functional form. Those discontinuity

[13] It appears from the data that when positive earnings are observed in one quarter, they tend to be observed for many consecutive quarters. Still, to the extent that some individuals move out of (or in) state during, say, the 10th year after high school graduation, this second earnings measure will allow them to remain in the sample.

estimates range from 0.123 to 0.193, which corresponds to an increase in wages due to admission at the flagship university on the order of 18% - 28%. All but 3 of the 12 estimates shown in Figures 10a – 10f and Figures 11a – 11f are statistically significant at the 5% level and all but 1 are statistically significant at the 10% level.

This difference caused by the regression specification is largely driven by the fact that the earnings of those who just missed the admission cutoff by 40 or fewer SAT points are lower than those of applicants who missed the cutoff by 50 – 70 points. The reason for this is not immediately clear; one would certainly expect earnings to rise as ability rises. One potential explanation is that those who barely were rejected still attended another 4-year university, while those who missed the cutoff by more either did not attend college or attended a community college first. Consequently, those who just missed may have lower earnings because they have less work experience. However, this explanation is not entirely satisfactory both because one would expect college graduates to catch up with high school graduates by age 33. Furthermore, one would expect the difference to decline from age 28 – 33 as the college graduates catch up on the earnings scale. It should be noted, however, that this downward slope in the fitted regression line cannot be driven by the selective admission of students on the left-hand side of the cutoff. Even if the university did selectively admit students who just missed the cutoff on the basis of some unobserved (to us) factor that is correlated with higher earnings potential, those admitted students remain on the left-hand side of the cutoff in the earnings graphs. Still, since the functional form assumptions do seem to be important, more sensitivity analysis will be performed later in the paper.

One pattern that does become apparent from the discontinuity estimates is that they are quite similar across the two measures of earnings. For example, the discontinuity estimate using the linear functional form is 6.9% after 15 years for the measure that uses four consecutive quarters of earnings and 6.3% using the annualized measure of earnings.

2.6.2 White Females

2.6.2.1 The effect of admission on subsequent earnings

The underlying data and fitted regression lines for women are shown in Figures 12a – 12f for the consecutive earnings measure and Figures 13a – 13f for the annualized earnings measure. The summary of the estimates shown on these figures is given in Table 4. As shown there, the linear regression discontinuity estimates on earnings 10 – 15 years after high school graduation for the two earnings measures are all negative and range from -0.018 to -0.095, which implies a flagship earnings effect of -2% to -13%. Only 3 of the 12 estimates are statistically significant at the 10% level, 2 of which are also statistically significant at the 5% level.

However, once again there is significant sensitivity to the functional form assumption used. Allowing for a cubic functional form of adjusted test score causes the regression discontinuity estimates to range from -0.108 to 0.136, although only 3 positive estimates are statistically significant at conventional levels. The only statistically significant estimates are the estimates for the discontinuity in 4 consecutive quarters of earnings 11, 13, and 14 years after high school graduation which range from 0.118 to 0.136. Once again, this difference in the discontinuity estimates appears to be driven largely by the fact that those who miss the admission cutoff by 20 or fewer points tend to have lower earnings than those who missed the cutoff by 30 – 40 points, causing the cubic regression line to trend downward as it approaches the cutoff.

There is also less consistency in the estimates across the two measures of earnings, which could be a consequence of the fact that women are more likely to work part-time or leave the labor force than are men.

It does seem difficult, at least to us, to see any distinctive discontinuity in the underlying data themselves that are shown in Figures 12a – 12f and Figures 13a – 13f. Thus, it seems difficult to pin down the flagship university earnings effect for women.

2.6.2.2 The effect of admission on the labor market attachment of white women

It is possible that the wide range of estimated discontinuities in the earnings of women is due in part because women who are accepted at and subsequently attend the flagship state university are more or less likely to leave the labor force. Such a difference could occur if the marriage market at the flagship university differed from that of the next-best-alternative university. To analyze this, we restrict the data to include only women for whom positive earnings are observed in the 15th year following high school graduation. We then examine the degree of labor force attachment by calculating the percentage of quarters in the 5 years prior to that in which the women were observed with positive earnings. The resulting outcome gives us a measure of labor force attachment for women from age 28 to age 33.[14]

The local averages of this measure of labor force participation are graphed in Figure 14. There is no discernible discontinuity in labor force participation at the admission cutoff, which is reassuring in that it suggests that the earnings estimates for

[14] This age was chosen since by age 28 almost all women will have completed their educations. In addition, we include only those who are observed with earnings 15 years after high school graduation in order to distinguish labor force participation from the propensity to move out of state.

women presented earlier are unlikely to have been driven by differences in labor force participation.

2.7 The Sensitivity of the Earnings Estimates

2.7.1 White Men

Given the sensitivity of the discontinuity estimates for white men to functional form, here I examine the sensitivity more fully to both functional form and specification. Since the results were very similar across both earnings measures, I only use the four consecutive quarter measure of earnings. Similarly, we only examine earnings 12 and 15 years after high school graduation. The results from these robustness checks are reported in Table 5.

The first five rows examine the sensitivity of the estimates to both the functional form of the adjusted SAT score and to the inclusion of control variables. As for the latter, it is evident from comparing the estimates in specification (1) to (2) and comparing specification (4) to (5) that the inclusion of control variables (year/term dummy variables and actual SAT score and GPA) does not affect the discontinuity estimates in a substantial way. This is consistent with the assumption underlying the regression discontinuity design that all other variables that affect earnings (such as high school GPA) vary continuously at the admission cutoff.

However, it is also clear that the choice of functional form of adjusted SAT score matters significantly. Specifically, the inclusion of a quadratic or higher order term allows the fitted regression line to slope downward as it approaches the admission cutoff from the left, as seen earlier in the earnings figures. This in turn results in estimates that are approximately twice as large as those resulting from the linear specification.

In specifications number (6) and (7), the sample is restricted to only those applicants who were observed with positive quarterly earnings in every quarter for 6 straight years starting in the 10th year after high school graduation. Thus, these estimates can be interpreted as the flagship effect for those applicants who are particularly attached to the labor market. Although the estimates using the linear functional form are similar, the discontinuity estimates using a cubic functional form increase from approximately 0.08 to 0.25. Thus, although functional form matters here as well, if anything it appears that the flagship earnings effect is larger for applicants with a strong attachment to the (in-state) labor force.

Specification (8) restricts the sample to only those applicants who missed or exceeded the admission cutoff by no more than 100 SAT points. As one might expect from looking at the data in Figures 10 and 11, the statistically significant regression discontinuity estimates of 0.167 and 0.157 are very similar to those using a polynomial of order 2 or higher using the full data set.

Finally, we examine the extent to which there is a discontinuity in earnings for the median earner in the sample. Here, the estimated discontinuities range from 0.034 to 0.098, none of which are statistically significant at conventional levels and which are lower than the OLS estimates. This suggests that although attending the state's flagship state university may increase earnings on average relative to the alternative, there is less compelling evidence that it does so for the median earner.

2.7.2 White Women

We also perform a similar set of robustness checks for white women, the results of which are shown in Table 6. The results are consistent with those for the men in that the inclusion of the control variables does not affect the estimates in a meaningful way.

Similarly, the functional form of adjusted SAT score does seem to matter somewhat; the quadratic and cubic specifications result in estimates that are less negative or even positive relative to the linear specification.

However, the most striking result concerns the effect of attending the flagship state university for women who have strong attachment to the labor force, as shown in specifications (6) and (7). For these women, the estimates are positive and, for three of the four estimates, are statistically significant at the 10% level. As shown in Table 6, the discontinuity estimates are 0.144 and 0.217 for 12 and 15 years after high school graduation using the cubic specification, both of which are statistically significant at the 1% level. Using the linear functional form, the discontinuity estimates are 0.77 and 0.53 for 12 and 15 years after high school graduation, only the former of which is statistically significant at the 10% level.

As shown in row (8), restricting the sample to only those applicants within 100 SAT points of the admission cutoff does not substantially affect the estimates. And finally, the median regression discontinuity estimates reported in rows (9) and (10) confirm the result that the cubic specification results in more positive discontinuity estimates, although only the estimate for 12 years is statistically significant at the 10% level.

2.8 Conclusion

In this paper, we identify the causal effect of attending the flagship state university by utilizing a regression discontinuity design that compares the earnings of those who were just accepted by the flagship to the earnings of those who just missed the admission cutoff. We do so by combining confidential student applicant records from a large flagship state university to earnings data collected by the state through the

Unemployment Insurance Program. After linking these two data sets together, we estimate the admission cutoff at the flagship and find that this estimated cutoff based on applicants SAT score and adjusted high school GPA coincides with a very large, distinct discontinuity in the likelihood of being admitted to the university.

We then examine whether or not there is a discontinuity in the likelihood of being observed with positive earnings 7 – 15 years after high school graduation. We find little evidence that admission to the flagship causes men or women to more or less likely to work in-state than their counterparts who barely missed the admission cutoff.

Finally, we estimate the intent-to-treat effect of attending the flagship state university on total earnings. For white men, we find evidence of positive discontinuities that translate to increases in earnings from 1% to 27%, although the discontinuities are not estimated precisely in all specifications. The size of the coefficients and their statistical significance depend largely on the functional form; polynomials of adjusted SAT score of order 2 or higher result in larger, statistically significant earnings discontinuities. There does not appear to be an effect on the median earnings of those who are admitted to the flagship state university, however.

For women overall, we find little consistent evidence of either a positive or negative effect of attending the flagship state university on earnings. However, we do find evidence of a large and statistically significant effect on the earnings of the subset of women with strong attachment to the labor force; the estimates of the effect on earnings range from 8% to 32%.

The results provide some suggestive evidence that being accepted by and attending the flagship state university may indeed cause an increase in subsequent earnings.

Consequently, the higher earnings that result from attending the flagship state university may justify, at least to some extent, costs (i.e., SAT preparation courses) undertaken by students and parents to gain admission to and/or to attend the top university in the state, at least for men and for women with a strong attachment to the labor force. Perhaps more importantly, although this paper did not (yet) examine the effect of attending the flagship university on minority applicants, the results for whites may yield insight nonetheless into the potential consequences of the elimination of affirmative action and the subsequent reduction in enrollment rates for minorities at the top state schools. To the extent that the effect for minorities is similar to that of white applicants, one may well expect that the earnings of minorities may fall as a result of the elimination of affirmative action in the admissions of flagship state universities.

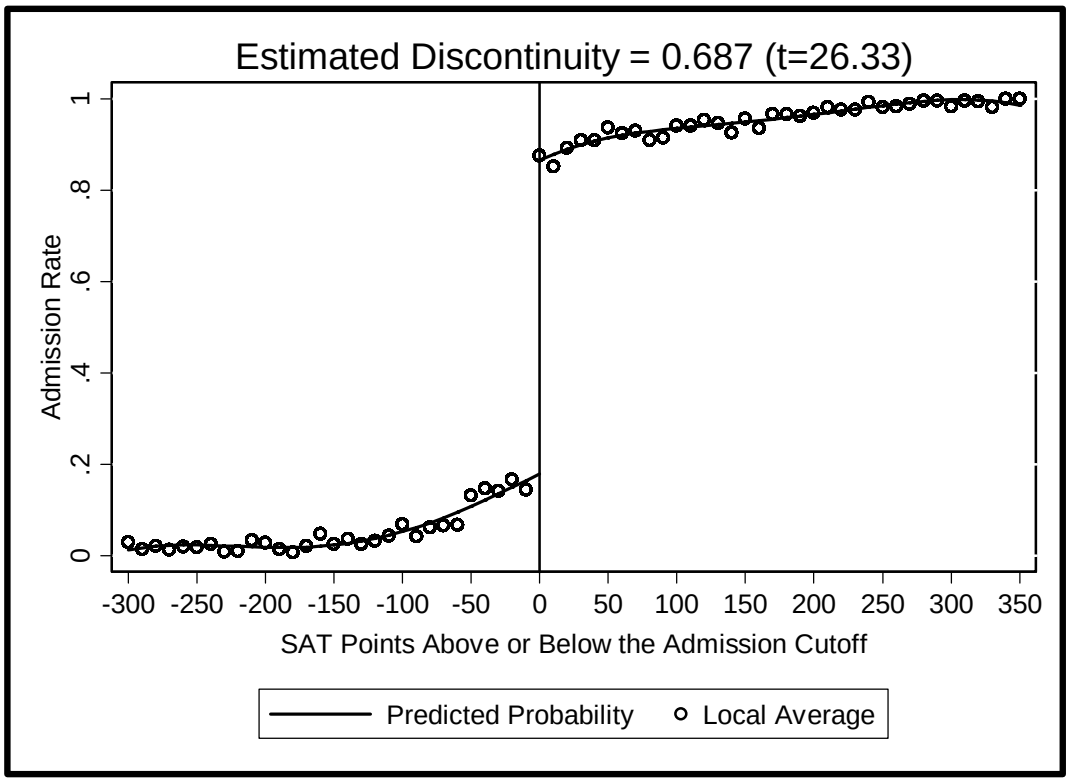

Figure 2-1: Fraction Admitted to the Flagship State University

Table 2-1: Regression Discontinuity Estimates for the Admission Rate of White Applicants

Regression	Estimated Discontinuity	Function of Adjusted SAT	Controls
(1)	**0.876** **(0.013)** **[0.000]**	none	No
(2)	**0.685** **(0.021)** **[0.000]**	cubic, cubic*Admit	Yes
(3) (Plotted in Figure 1)	**0.687** **(0.026)** **[0.000]**	4th order, 4th order*Admit	No
(4)	**0.686** **(0.025)** **[0.000]**	4th order, 4th order*Admit	Yes

Notes: Each row represents a different OLS regression. Robust standard errors are in parentheses; p-values are given in brackets. Controls include a dummy variable for each year/term of application as well as actual SAT score and high school GPA. Estimates in bold are statistically significant at the 10% level.

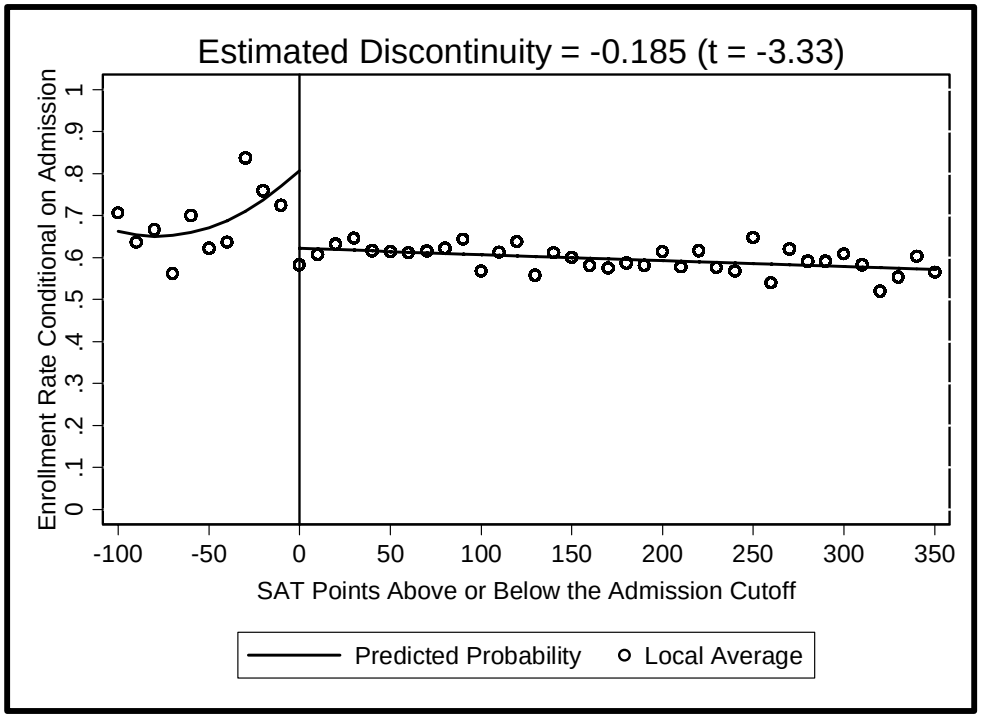

Figure 2-2: Enrollment Rates for Admitted White Applicants

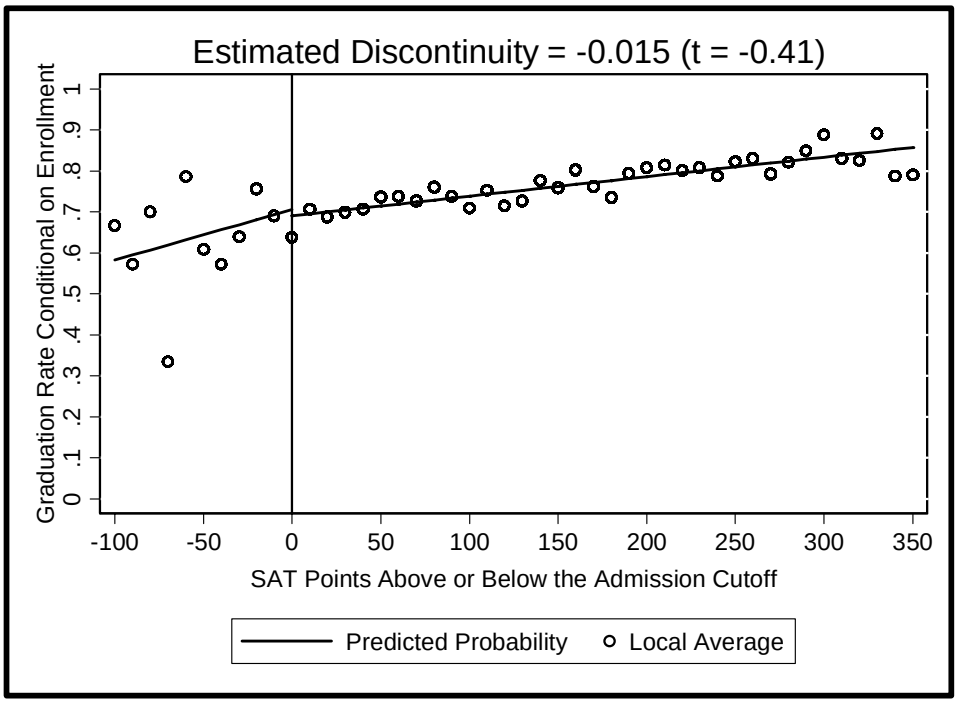

Figure 2-3: Graduation Rates for Enrolling White Applicants

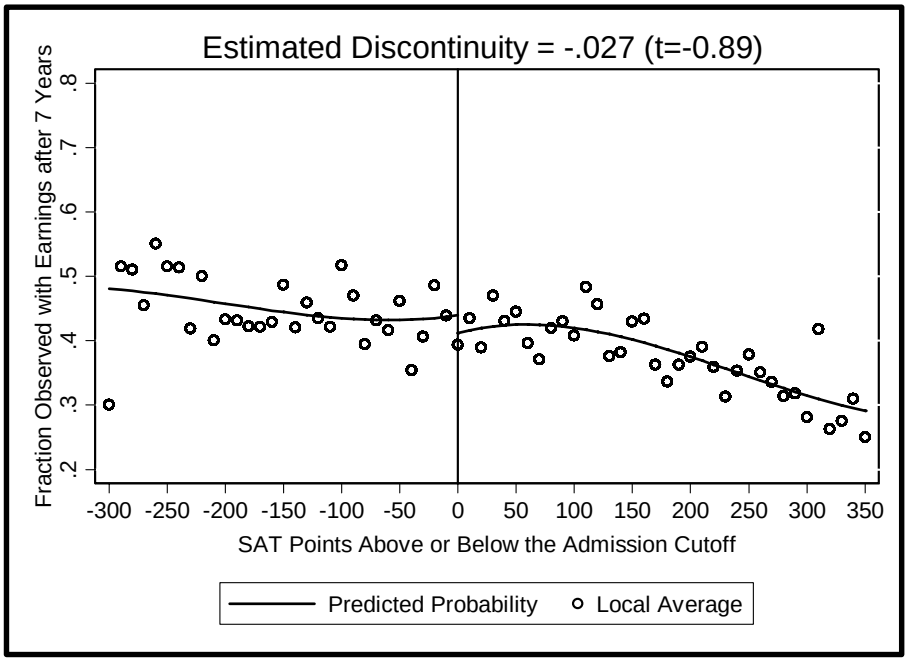

Figure 2-4a: The Likelihood of Being Observed with 4 Consecutive Quarters of Positive Earnings 7 Years after High School Graduation for White Men

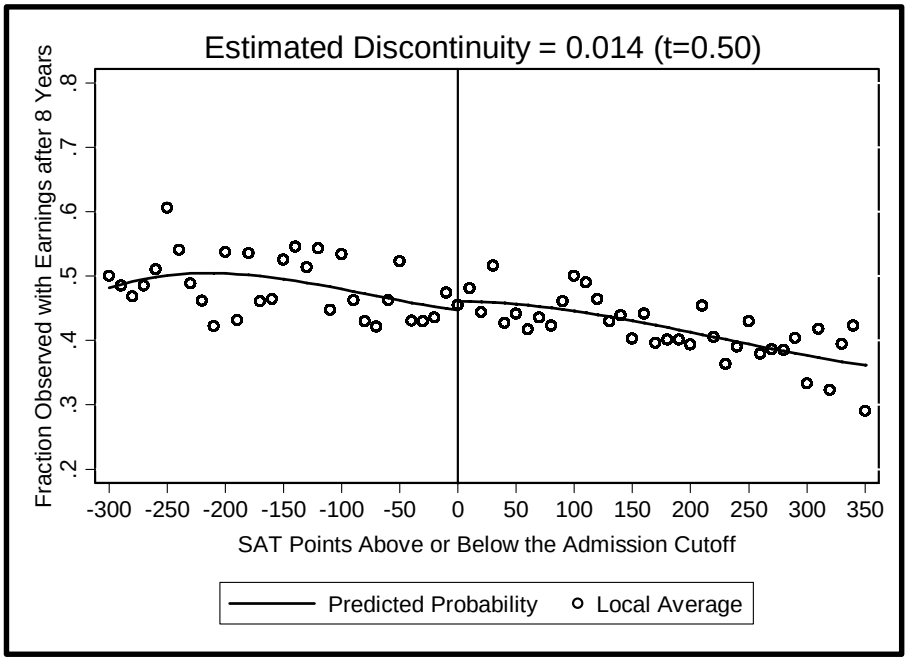

Figure 2-4b: The Likelihood of Being Observed with 4 Consecutive Quarters of Positive Earnings 8 Years after High School Graduation for White Men

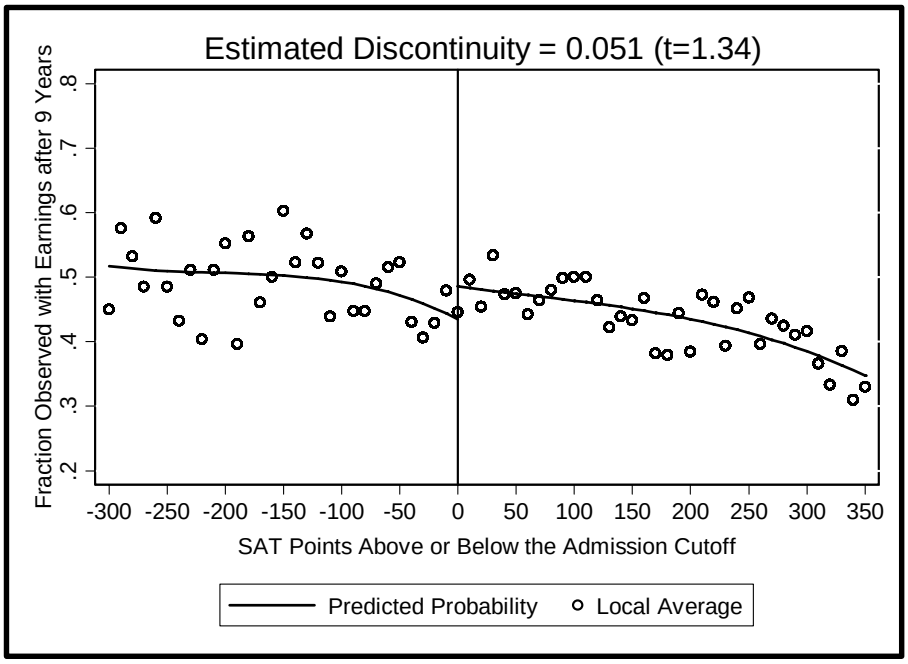

Figure 2-4c: The Likelihood of Being Observed with 4 Consecutive Quarters of Positive Earnings 9 Years after High School Graduation for White Men

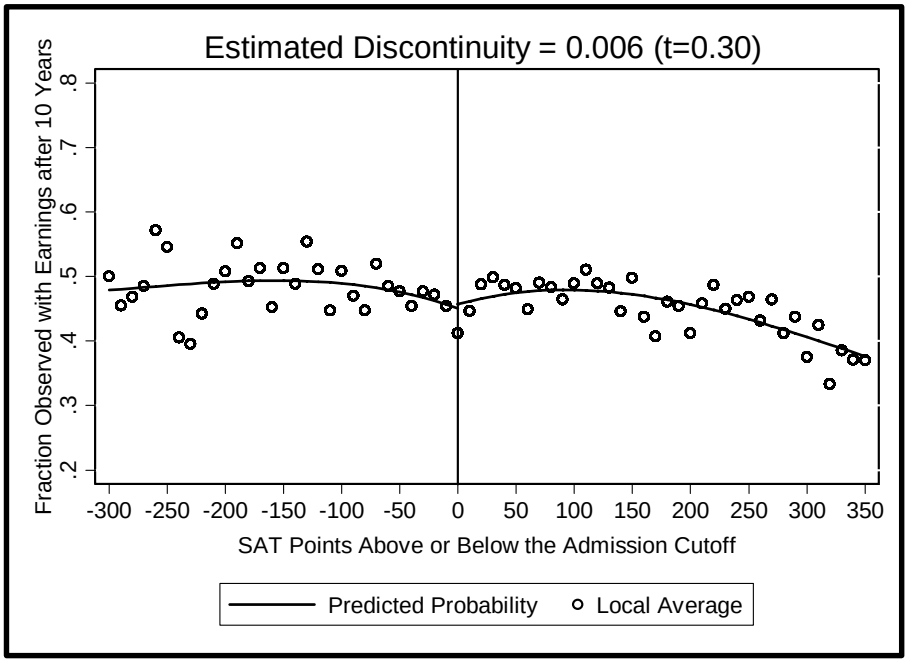

Figure 2-4d: The Likelihood of Being Observed with 4 Consecutive Quarters of Positive Earnings 10 Years after High School Graduation for White Men

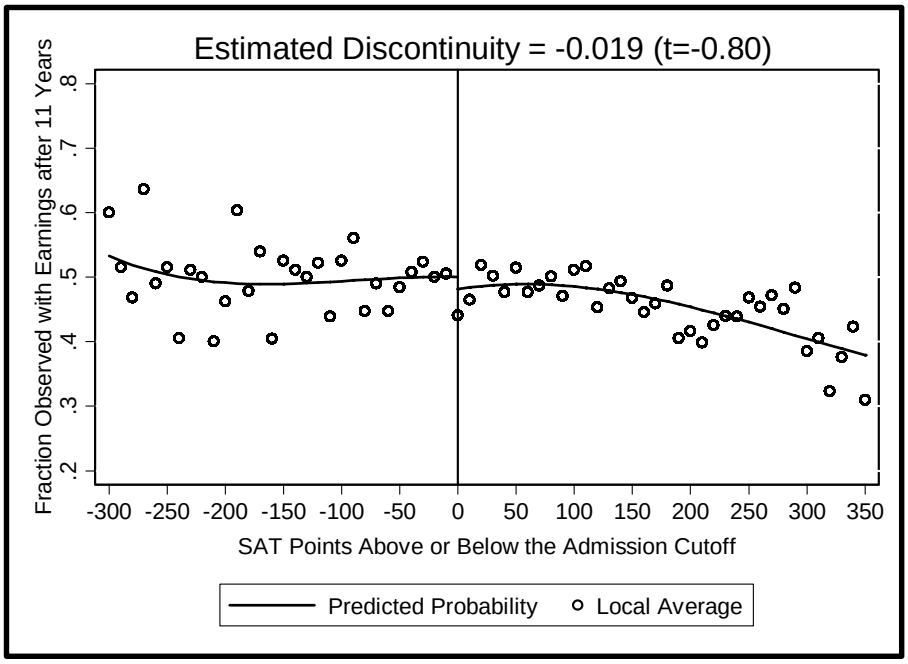

Figure 2-4e: The Likelihood of Being Observed with 4 Consecutive Quarters of Positive Earnings 11 Years after High School Graduation for White Men

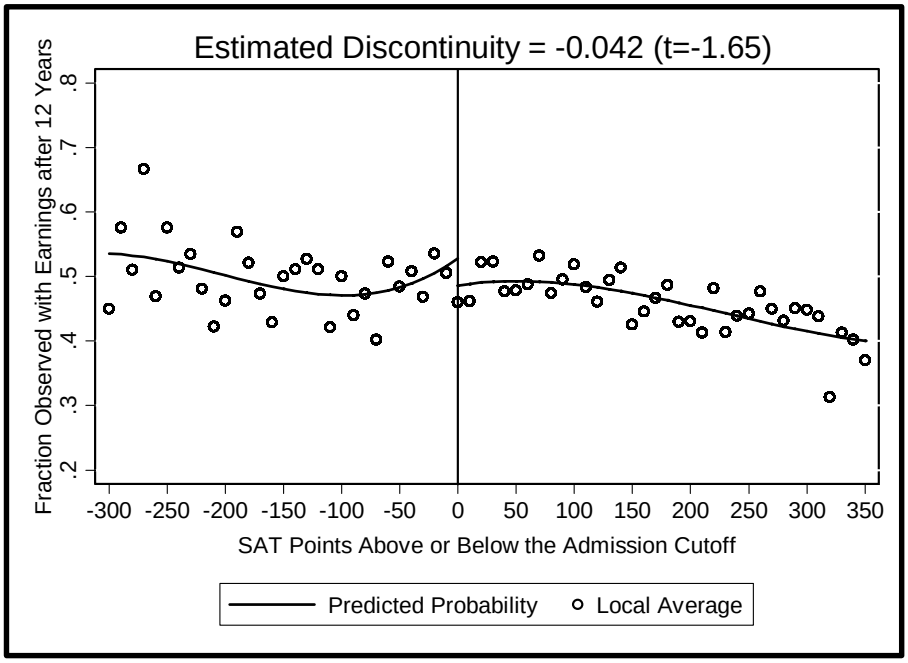

Figure 2-4f: The Likelihood of Being Observed with 4 Consecutive Quarters of Positive Earnings 12 Years after High School Graduation for White Men

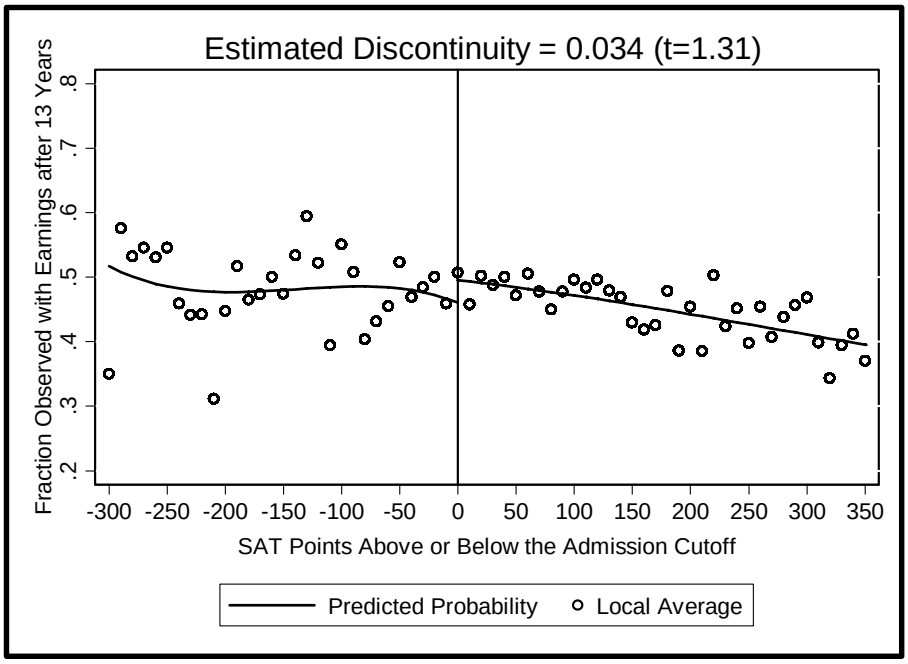

Figure 2-4g: The Likelihood of Being Observed with 4 Consecutive Quarters of Positive Earnings 13 Years after High School Graduation for White Men

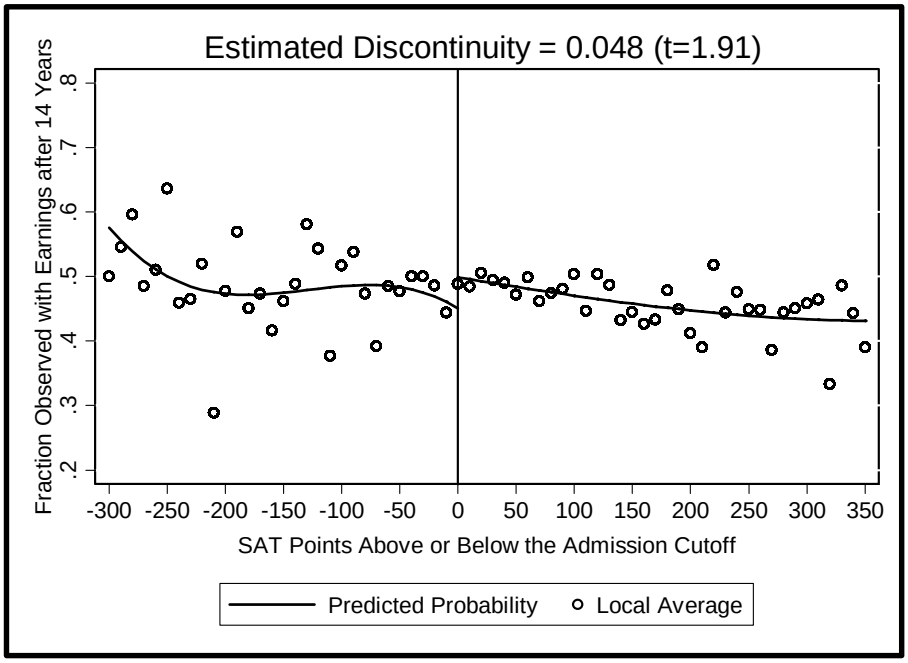

Figure 2-4h: The Likelihood of Being Observed with 4 Consecutive Quarters of Positive Earnings 14 Years after High School Graduation for White Men

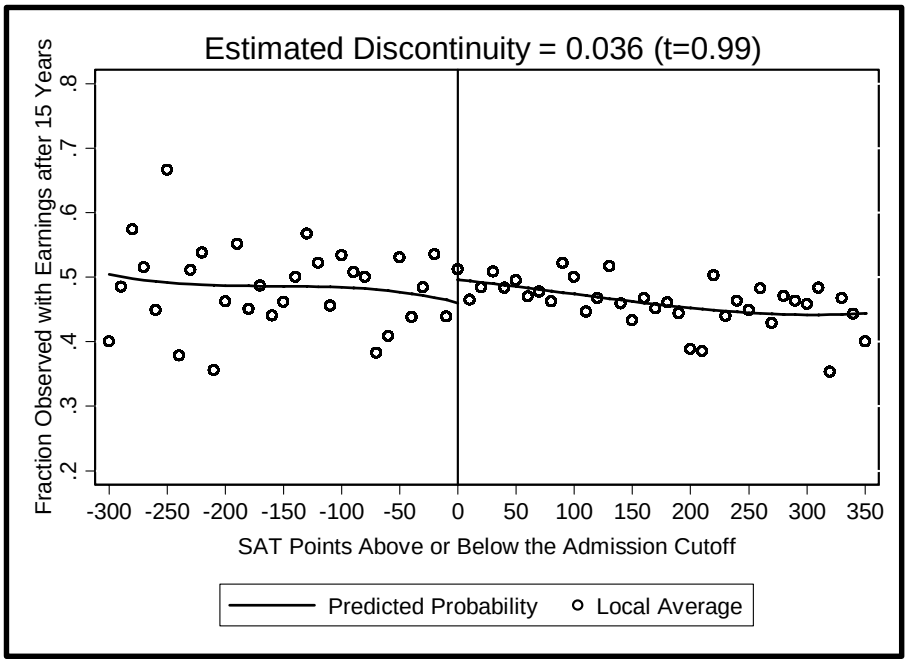

Figure 2-4i: The Likelihood of Being Observed with 4 Consecutive Quarters of Positive Earnings 15 Years after High School Graduation for White Men

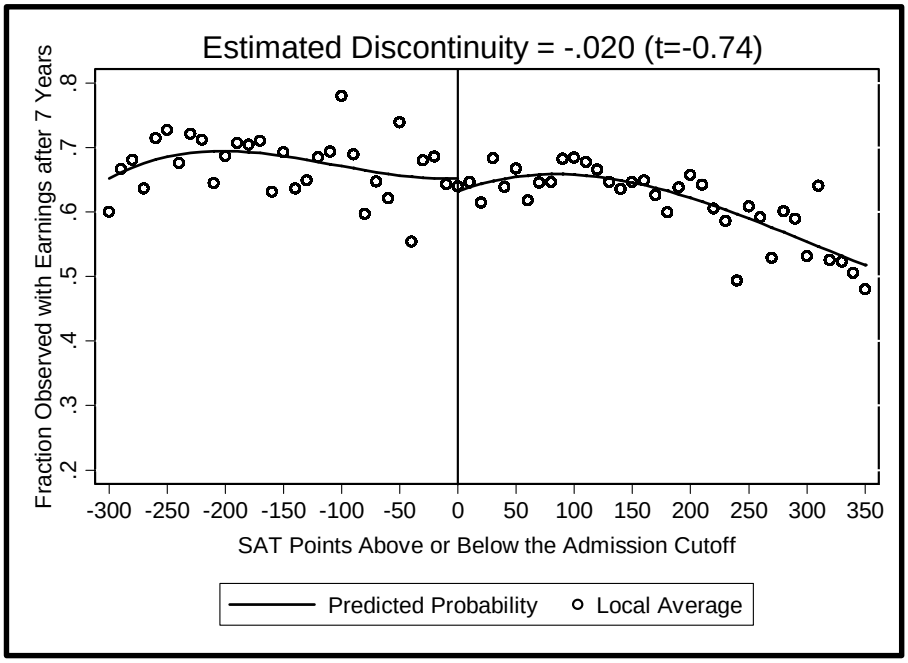

Figure 2-5a: The Likelihood of Being Observed with Positive Earnings in the 7th Year after High School Graduation for White Men

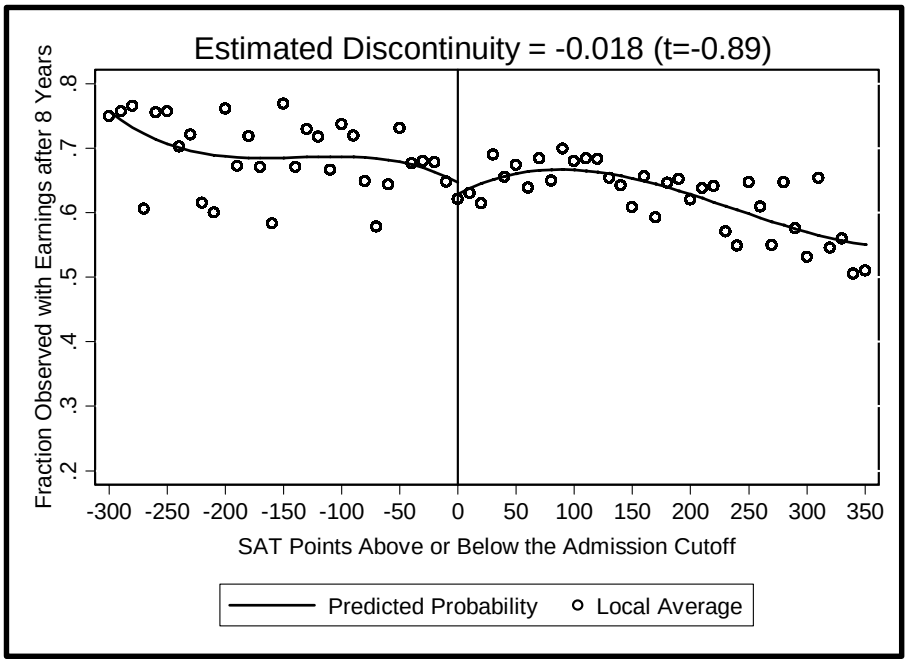

Figure 2-5b: The Likelihood of Being Observed with Positive Earnings in the 8th Year after High School Graduation for White Men

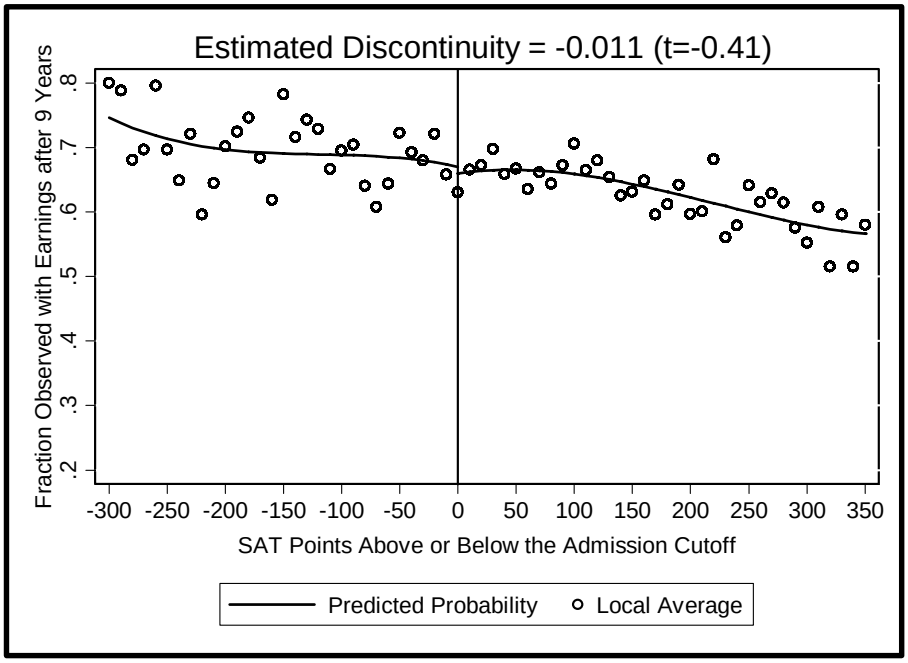

Figure 2-5c: The Likelihood of Being Observed with Positive Earnings in the 9th Year after High School Graduation for White Men

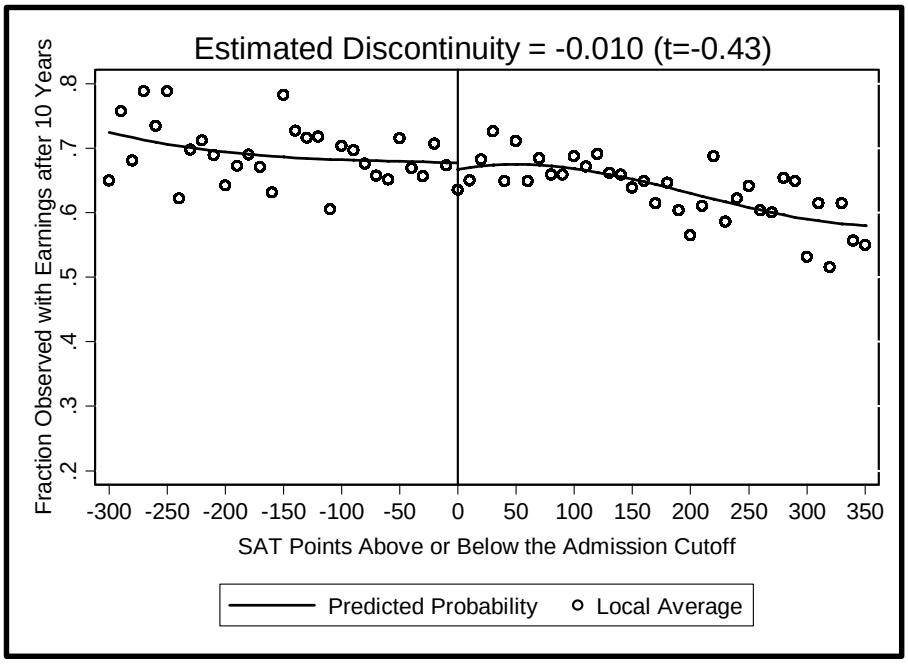

Figure 2-5d: The Likelihood of Being Observed with Positive Earnings in the 10th Year after High School Graduation for White Men

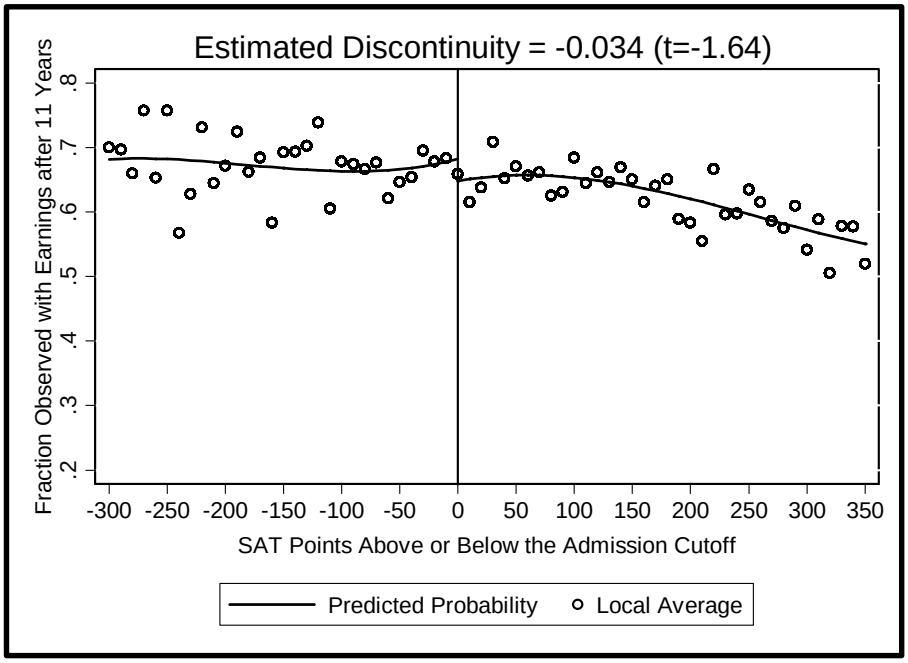

Figure 2-5e: The Likelihood of Being Observed with Positive Earnings in the 11th Year after High School Graduation for White Men

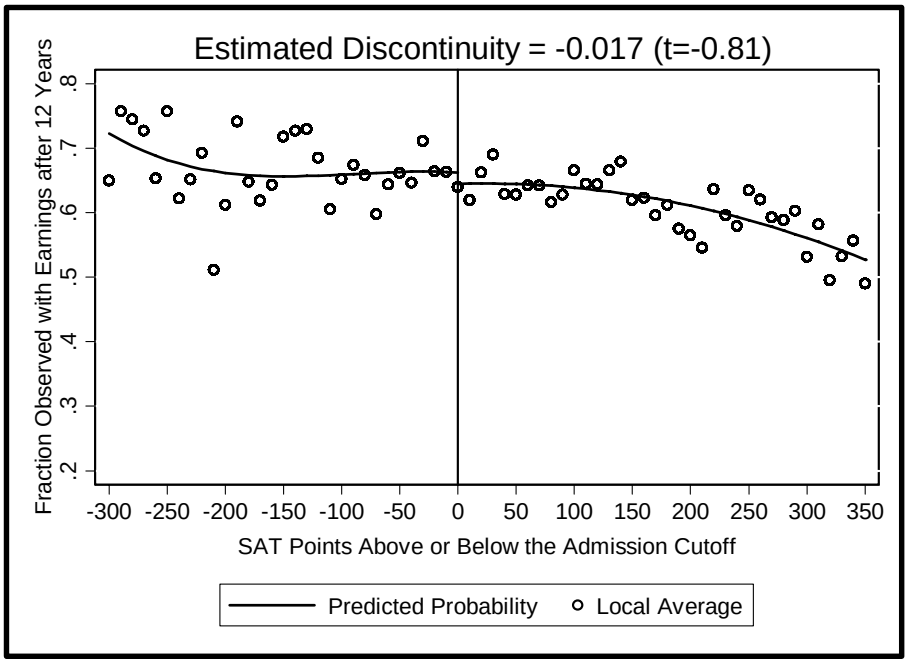

Figure 2-5f: The Likelihood of Being Observed with Positive Earnings in the 12th Year after High School Graduation for White Men

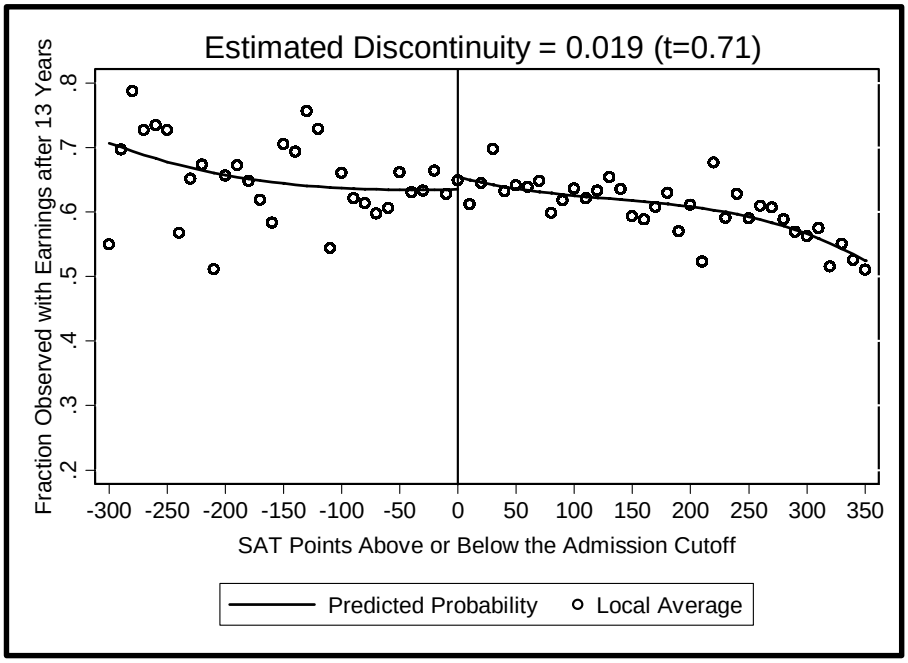

Figure 2-5g: The Likelihood of Being Observed with Positive Earnings in the 13th Year after High School Graduation for White Men

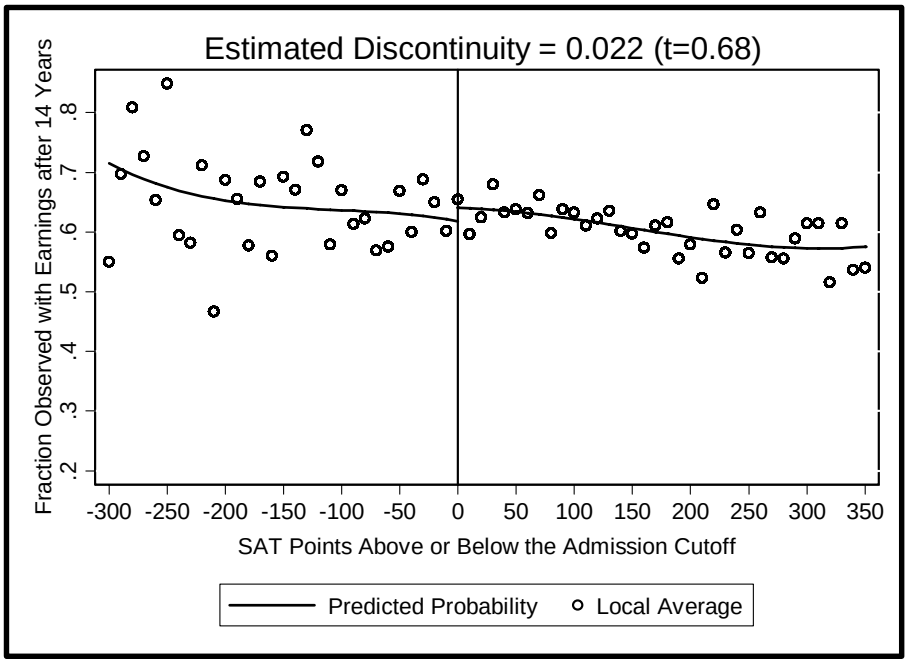

Figure 2-5h: The Likelihood of Being Observed with Positive Earnings in the 14th Year after High School Graduation for White Men

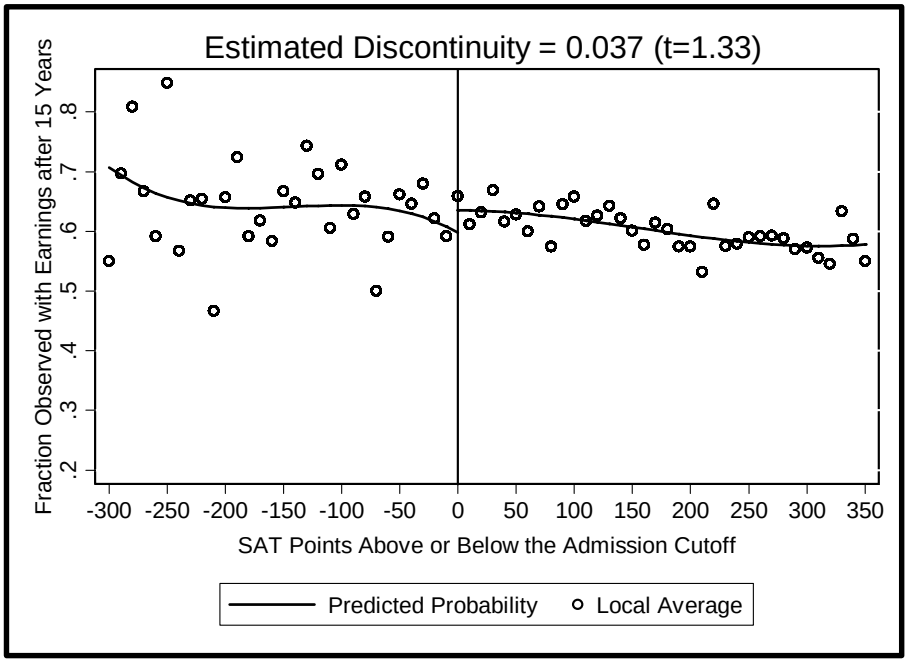

Figure 2-5i: The Likelihood of Being Observed with Positive Earnings in the 15th Year after High School Graduation for White Men

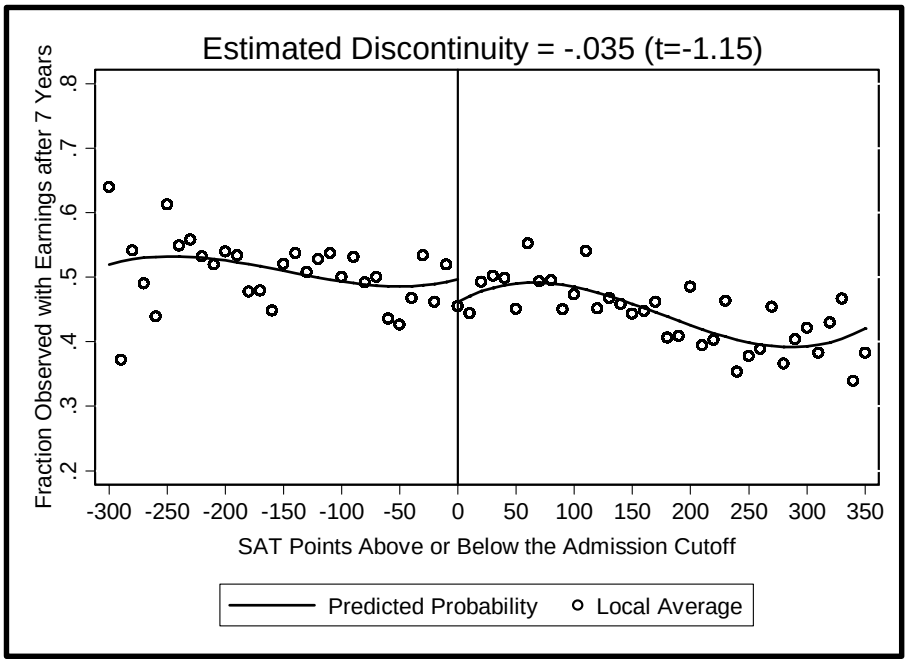

Figure 2-6a: The Likelihood of Being Observed with 4 Consecutive Quarters of Positive Earnings 7 Years after High School Graduation for White Women

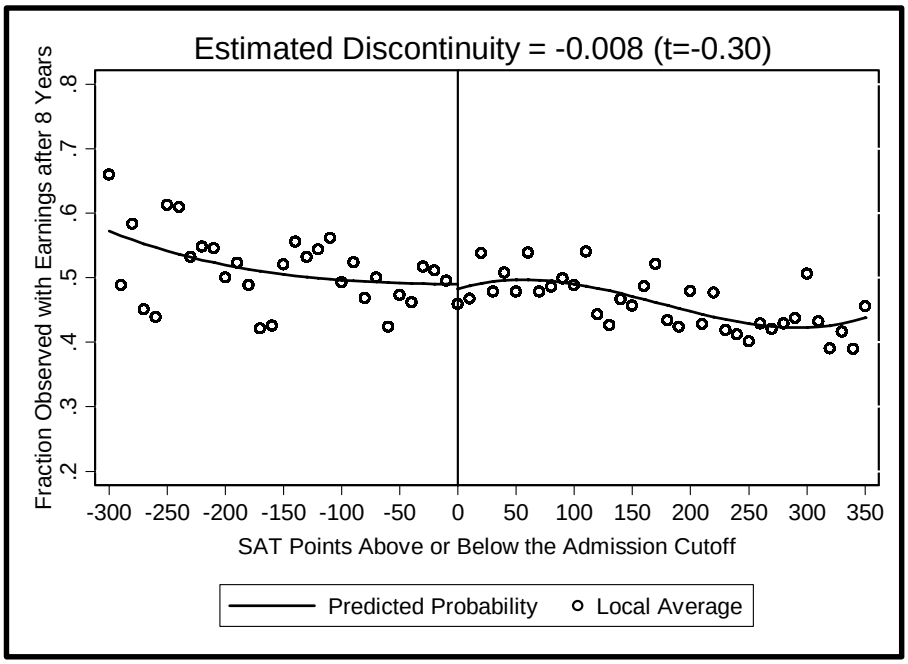

Figure 2-6b: The Likelihood of Being Observed with 4 Consecutive Quarters of Positive Earnings 8 Years after High School Graduation for White Women

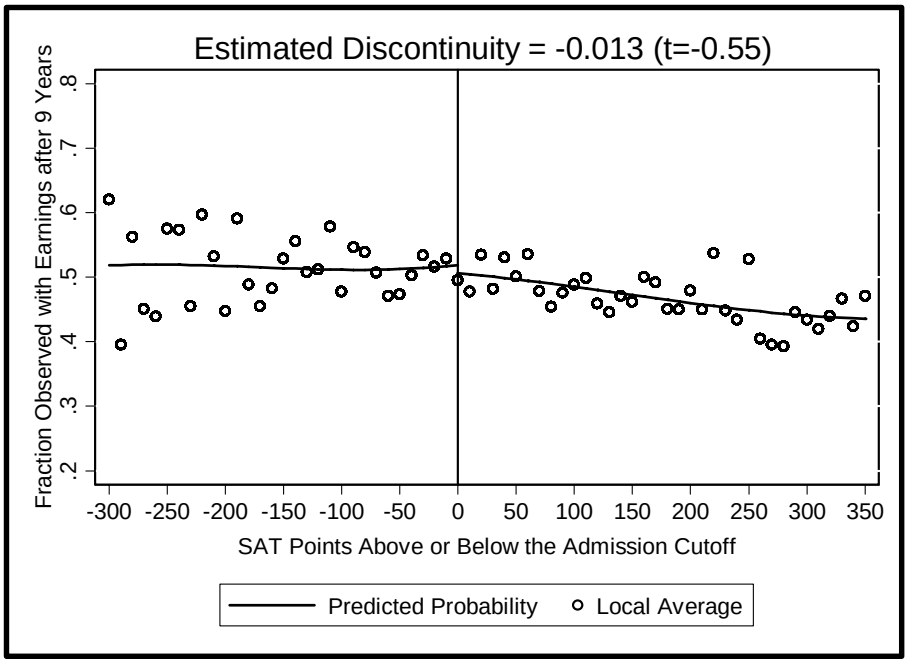

Figure 2-6c: The Likelihood of Being Observed with 4 Consecutive Quarters of Positive Earnings 9 Years after High School Graduation for White Women

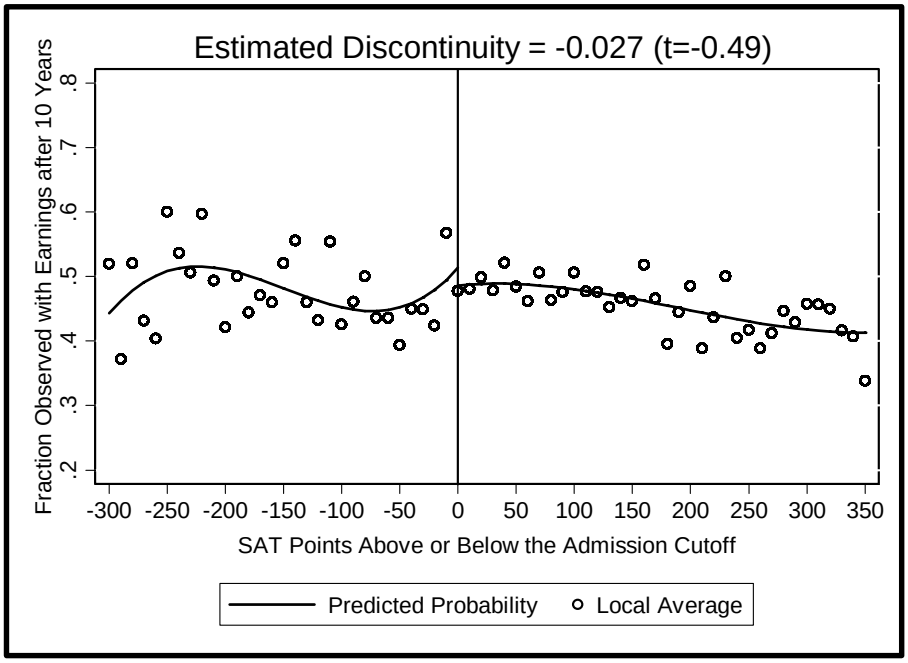

Figure 2-6d: The Likelihood of Being Observed with 4 Consecutive Quarters of Positive Earnings 10 Years after High School Graduation for White Women

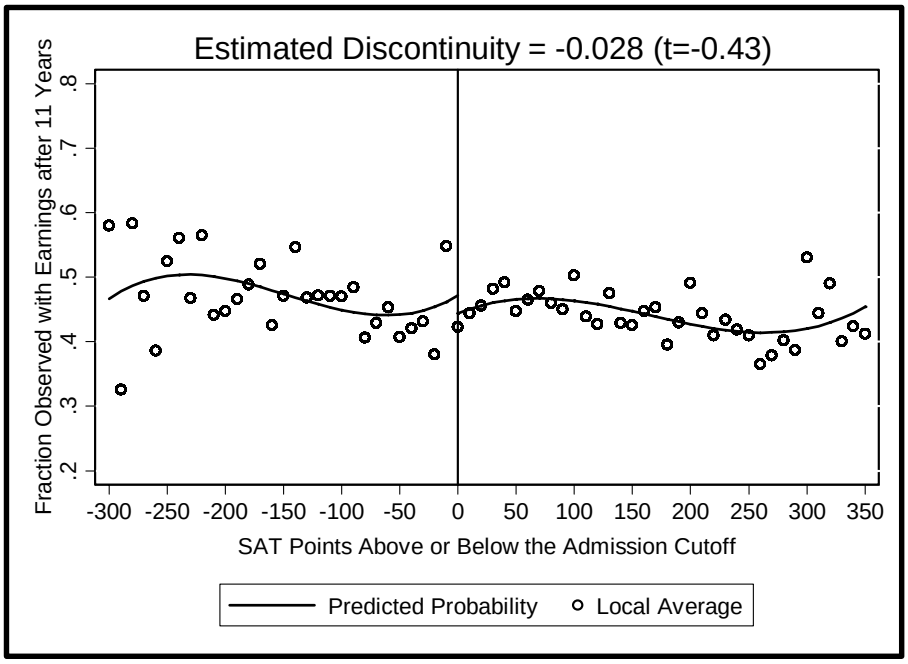

Figure 2-6e: The Likelihood of Being Observed with 4 Consecutive Quarters of Positive Earnings 11 Years after High School Graduation for White Women

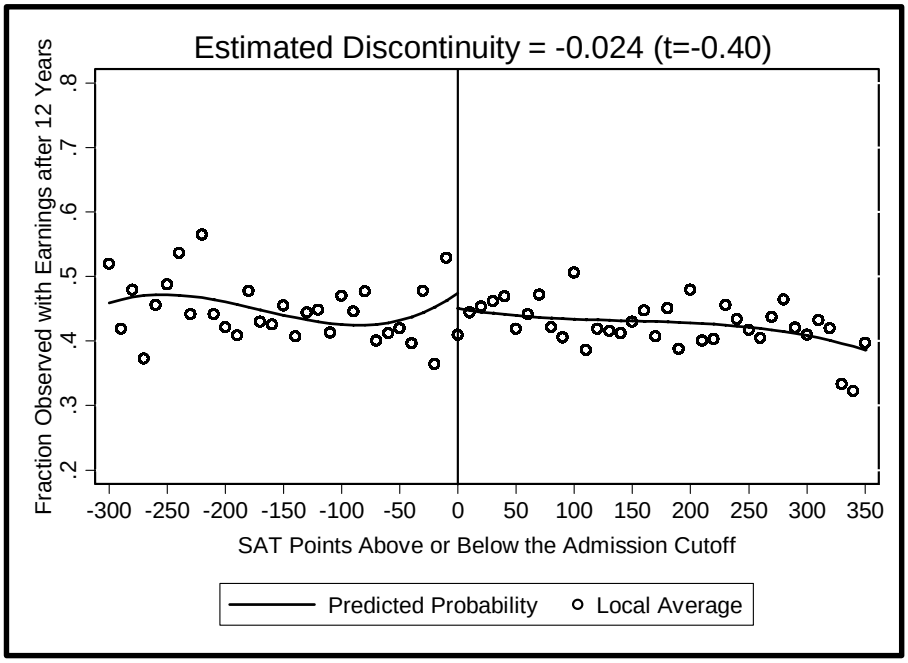

Figure 2-6f: The Likelihood of Being Observed with 4 Consecutive Quarters of Positive Earnings 12 Years after High School Graduation for White Women

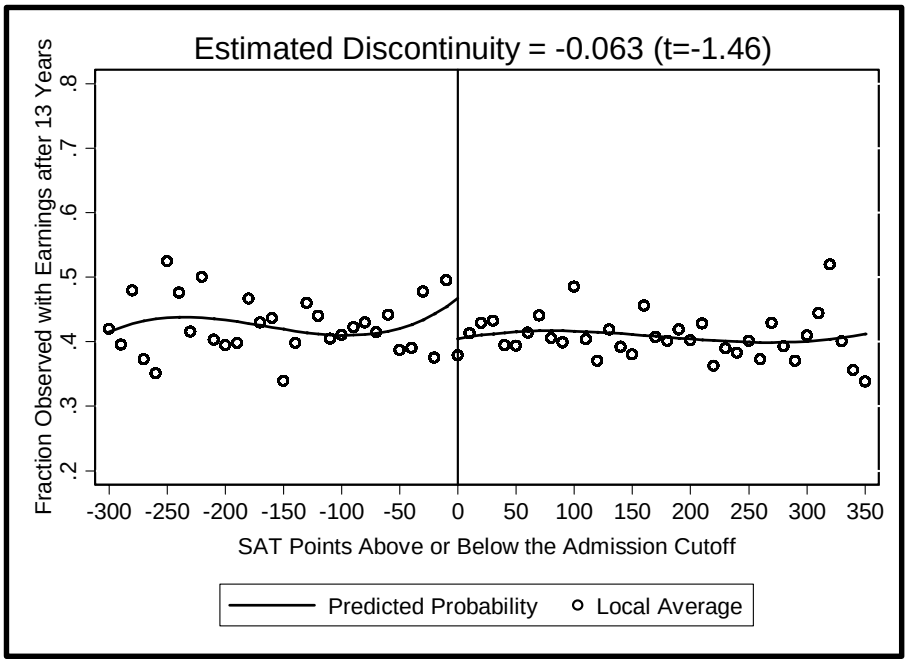

Figure 2-6g: The Likelihood of Being Observed with 4 Consecutive Quarters of Positive Earnings 13 Years after High School Graduation for White Women

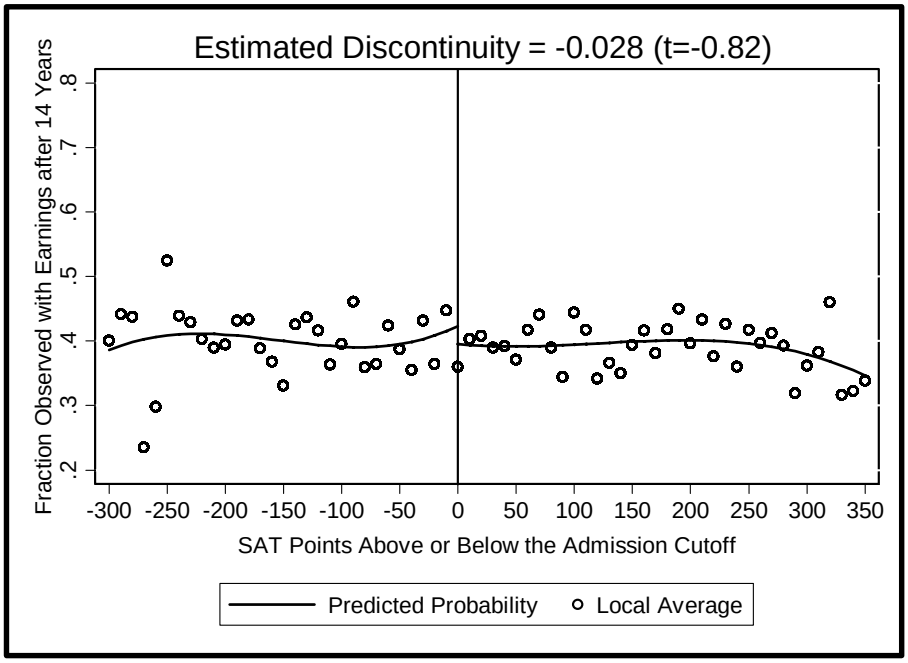

Figure 2-6h: The Likelihood of Being Observed with 4 Consecutive Quarters of Positive Earnings 14 Years after High School Graduation for White Women

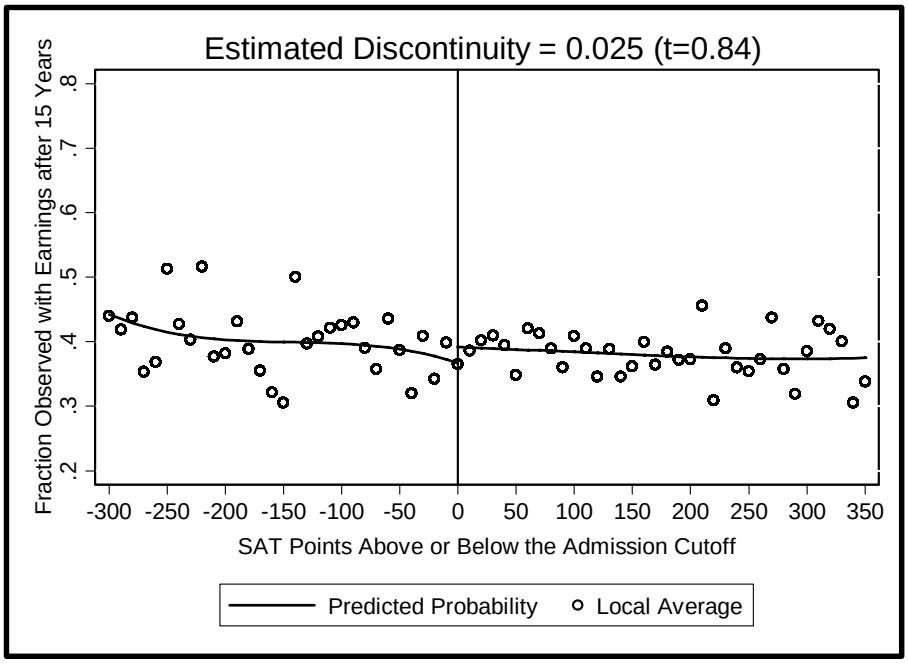

Figure 2-6i: The Likelihood of Being Observed with 4 Consecutive Quarters of Positive Earnings 15 Years after High School Graduation for White Women

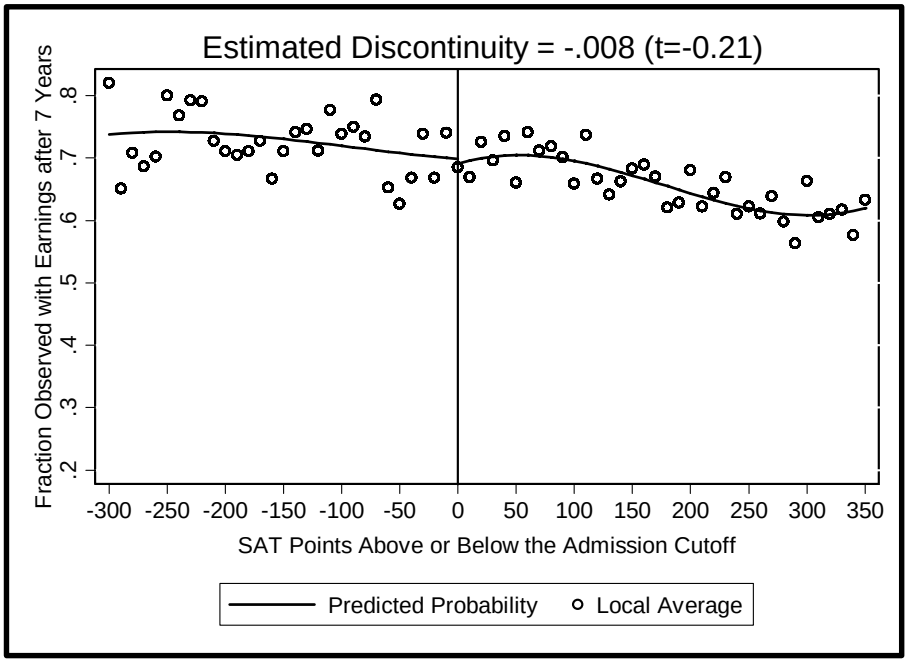

Figure 2-7a: The Likelihood of Being Observed with Positive Earnings in the 7th Year after High School Graduation for White Women

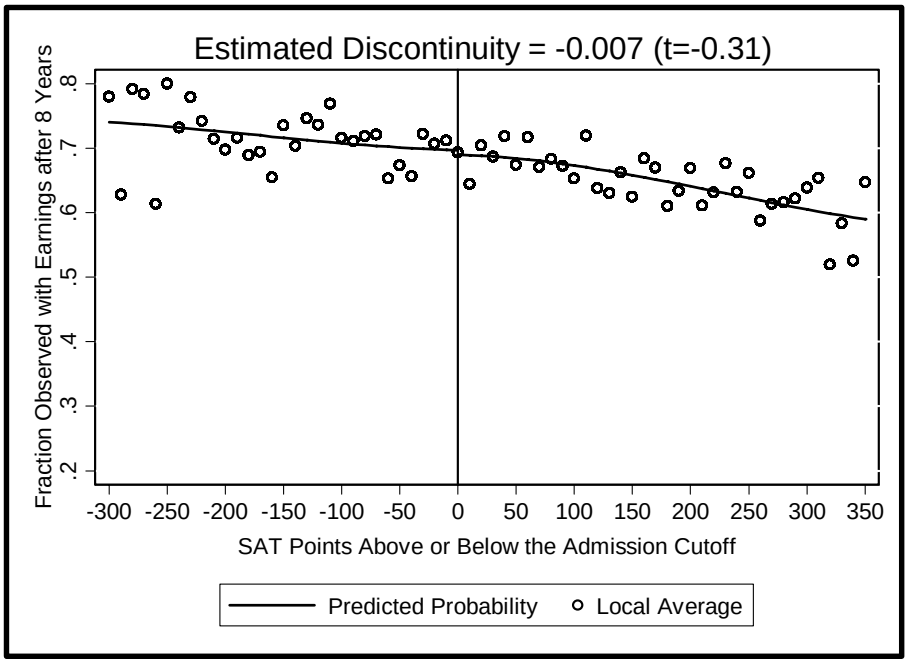

Figure 2-7b: The Likelihood of Being Observed with Positive Earnings in the 8th Year after High School Graduation for White Women

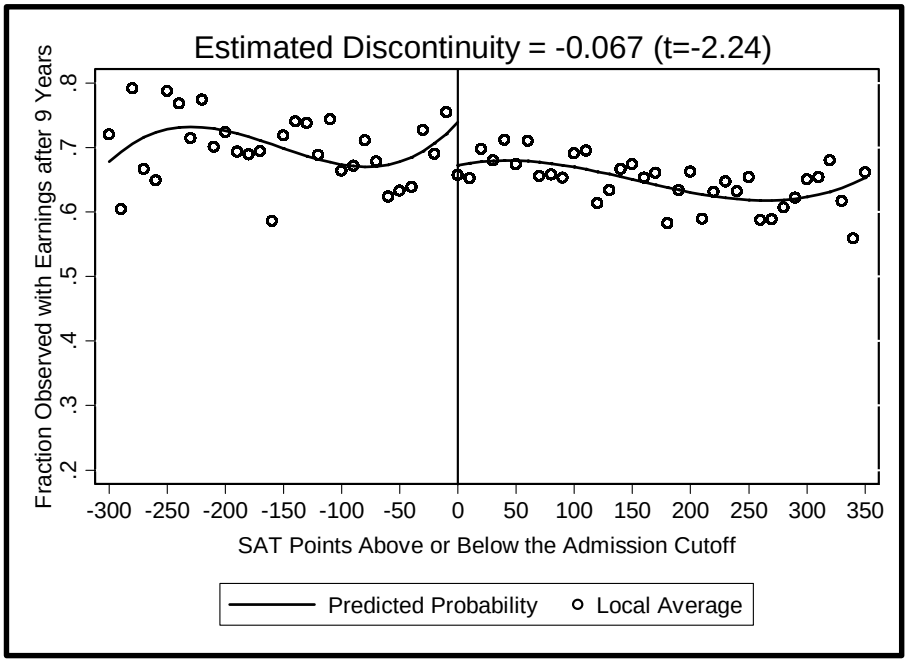

Figure 2-7c: The Likelihood of Being Observed with Positive Earnings in the 9th Year after High School Graduation for White Women

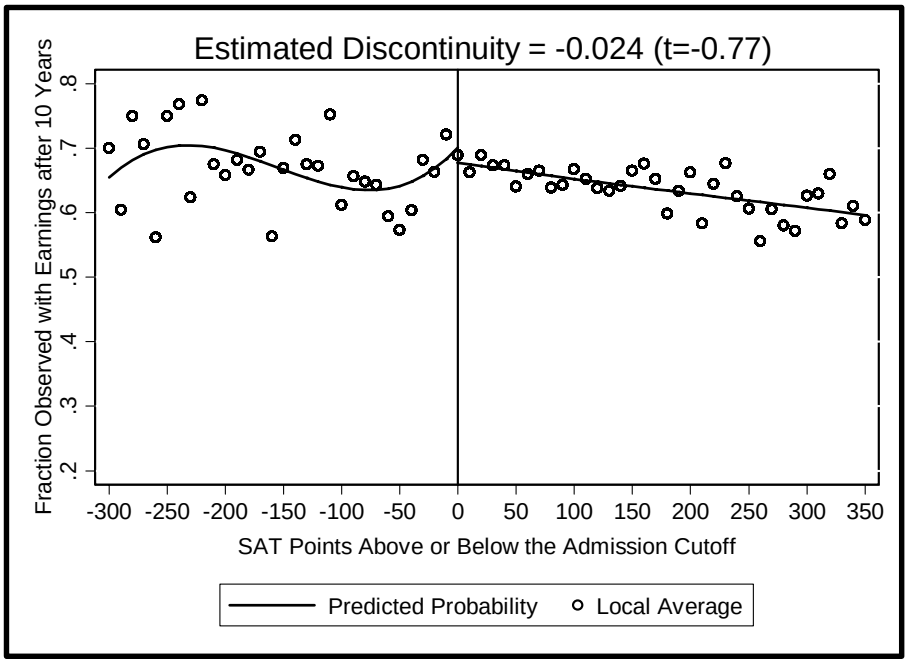

Figure 2-7d: The Likelihood of Being Observed with Positive Earnings in the 10th Year after High School Graduation for White Women

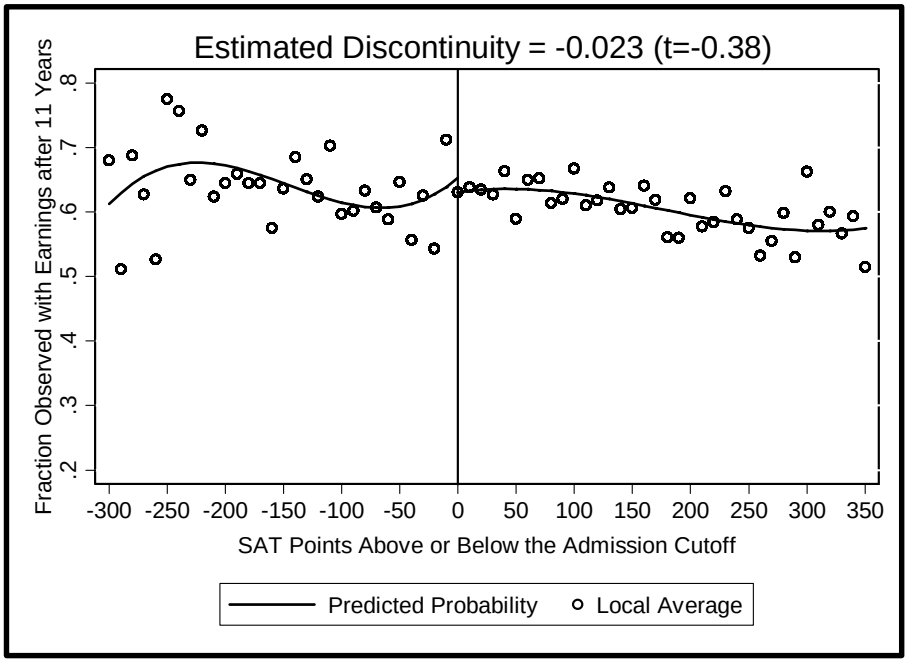

Figure 2-7e: The Likelihood of Being Observed with Positive Earnings in the 11th Year after High School Graduation for White Women

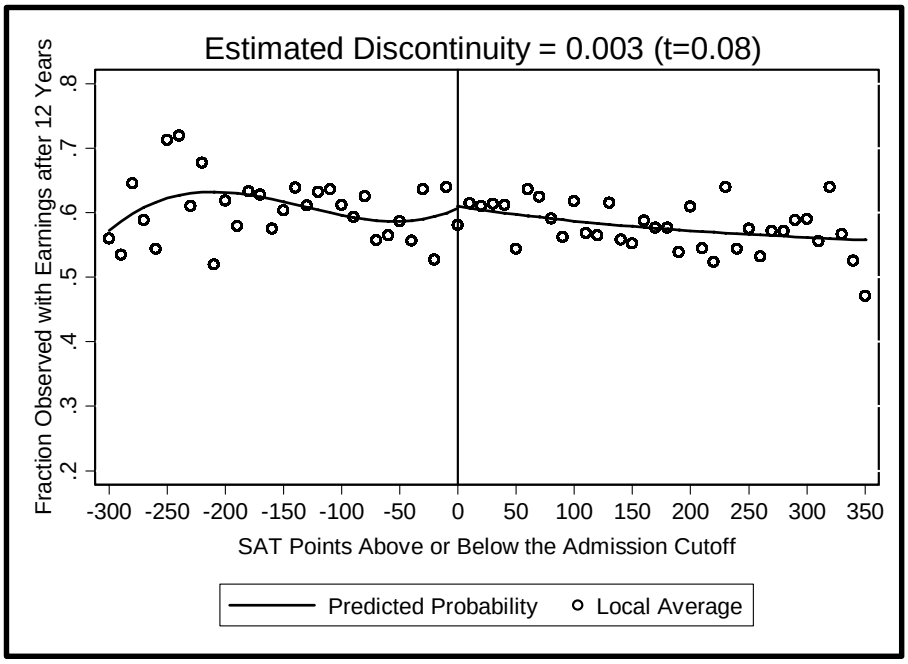

Figure 2-7f: The Likelihood of Being Observed with Positive Earnings in the 12th Year after High School Graduation for White Women

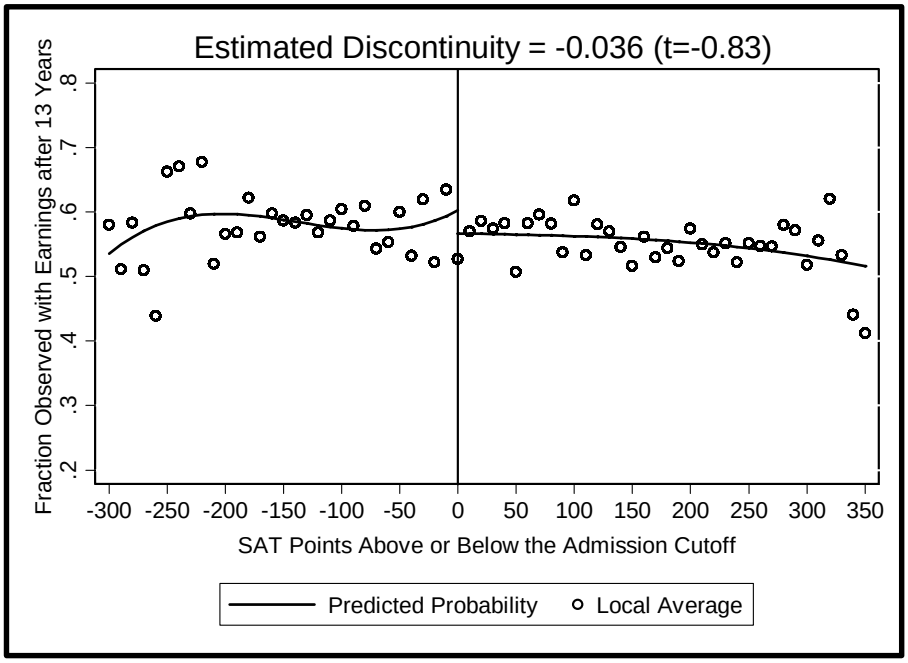

Figure 2-7g: The Likelihood of Being Observed with Positive Earnings in the 13th Year after High School Graduation for White Women

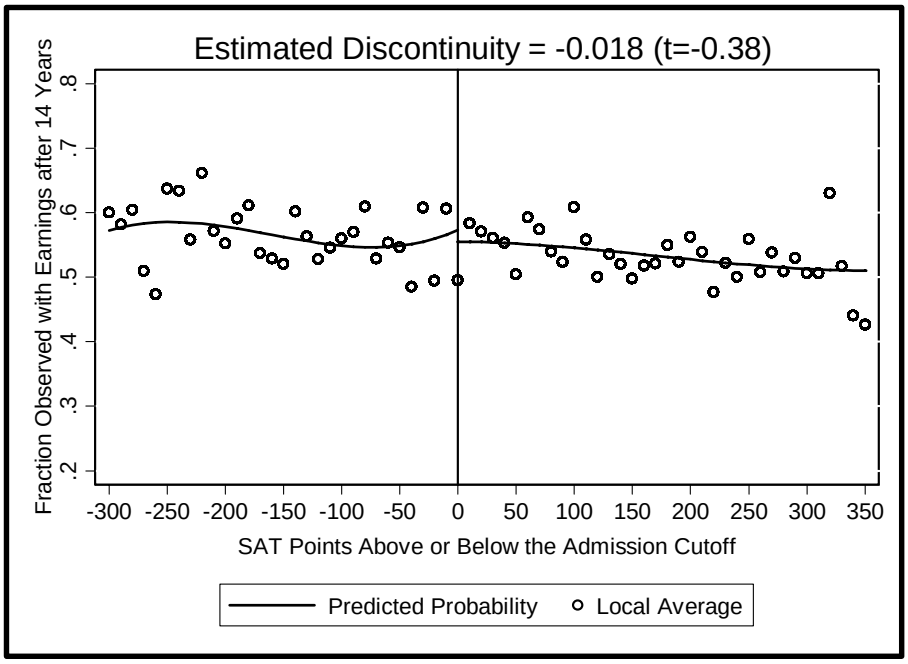

Figure 2-7h: The Likelihood of Being Observed with Positive Earnings in the 14th Year after High School Graduation for White Women

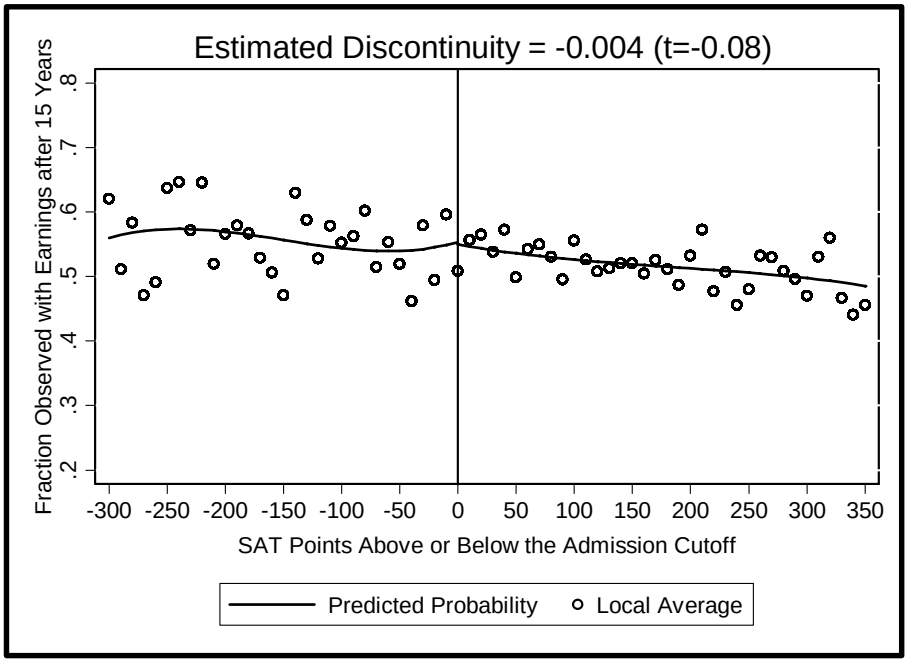

Figure 2-7i: The Likelihood of Being Observed with Positive Earnings in the 15th Year after High School Graduation for White Women

Table 2-2: Regression Discontinuity Estimates for the Likelihood of Being Observed with Earnings 7 – 15 Years after High School Graduation (a summary of estimates presented in Figures 2-4a-f, 2-5a-f, 2-6a-f, and 2-7a-f)

Year After High School Graduation	Men		Women	
	Earnings Measure		Earnings Measure	
	4 Consecutive Qtrs.	Annualized	4 Consecutive Qtrs.	Annualized
7	-0.027	-0.020	-0.035	-0.008
	(0.031)	(0.027)	(0.030)	(0.036)
	[0.376]	[0.459]	[0.253]	[0.834]
8	0.014	-0.018	-0.008	-0.007
	(0.028)	(0.020)	(0.025)	(0.023)
	[0.620]	[0.377]	[0.764]	[0.758]
9	0.051	-0.011	-0.126	**-0.067**
	(0.038)	(0.026)	(0.023)	**(0.030)**
	[0.186]	[0.683]	[0.587]	**[0.028]**
10	0.006	-0.010	-0.027	-0.024
	(0.021)	(0.024)	(0.055)	(0.031)
	[0.762]	[0.666]	[0.625]	[0.446]
11	-0.019	-0.034	-0.028	-0.023
	(0.0240)	(0.021)	(0.066)	(0.060)
	[0.426]	[0.105]	[0.671]	[0.708]
12	-0.042	-0.017	-0.024	0.003
	(0.025)	(0.021)	(0.060)	(0.043)
	[0.104]	[0.424]	[0.688]	[0.936]
13	0.034	0.019	-0.063	-0.036
	(0.026)	(0.027)	(0.043)	(0.043)
	[0.193]	[0.479]	[0.148]	[0.411]
14	**0.048**	0.022	-0.028	-0.018
	(0.025)	(0.033)	(0.034)	(0.047)
	[0.061]	[0.502]	[0.416]	[0.703]
15	0.036	0.037	0.025	-0.004
	(0.037)	(0.028)	(0.030)	(0.044)
	[0.328]	[0.189]	[0.402]	[0.933]

Notes: Robust standard errors clustered at the adjusted SAT score level are in parentheses; p-values are in brackets. Estimates in bold are statistically significant at the 10% level. All estimates are from regressions controlling for a cubic of adjusted SAT score

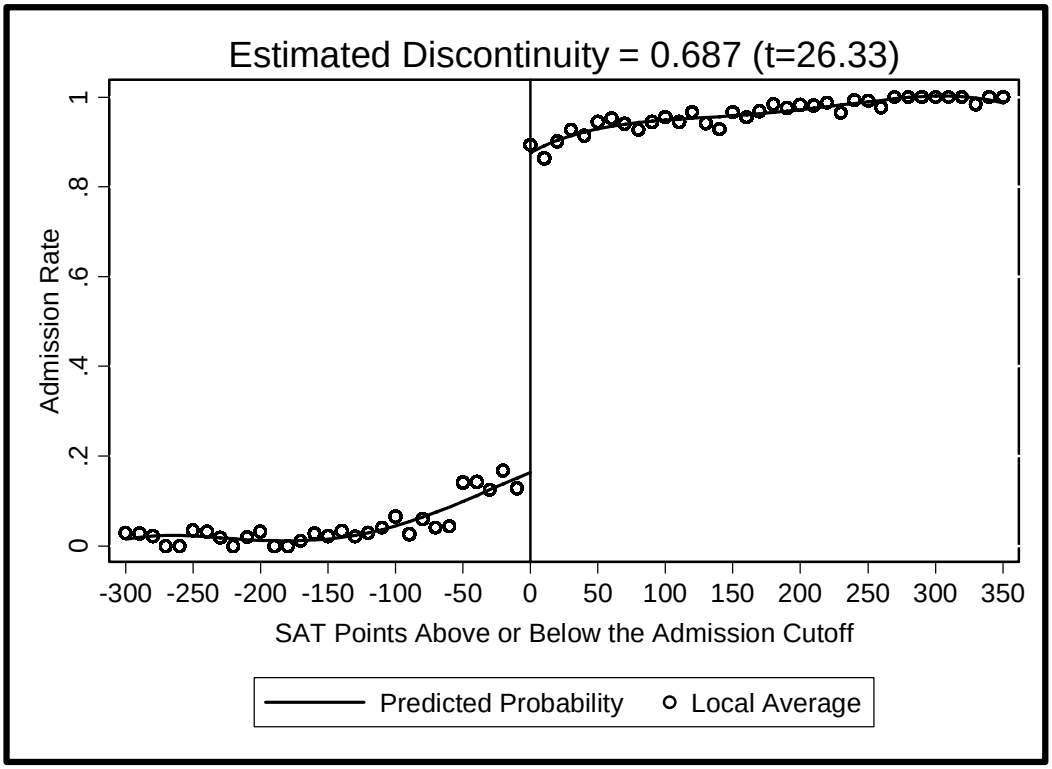

Figure 2-8a: Regression Discontinuity Estimates for the Admission Rate of White Applicants Observed with 4 Consecutive Quarters of Earnings in the 12th Year after High School Graduation

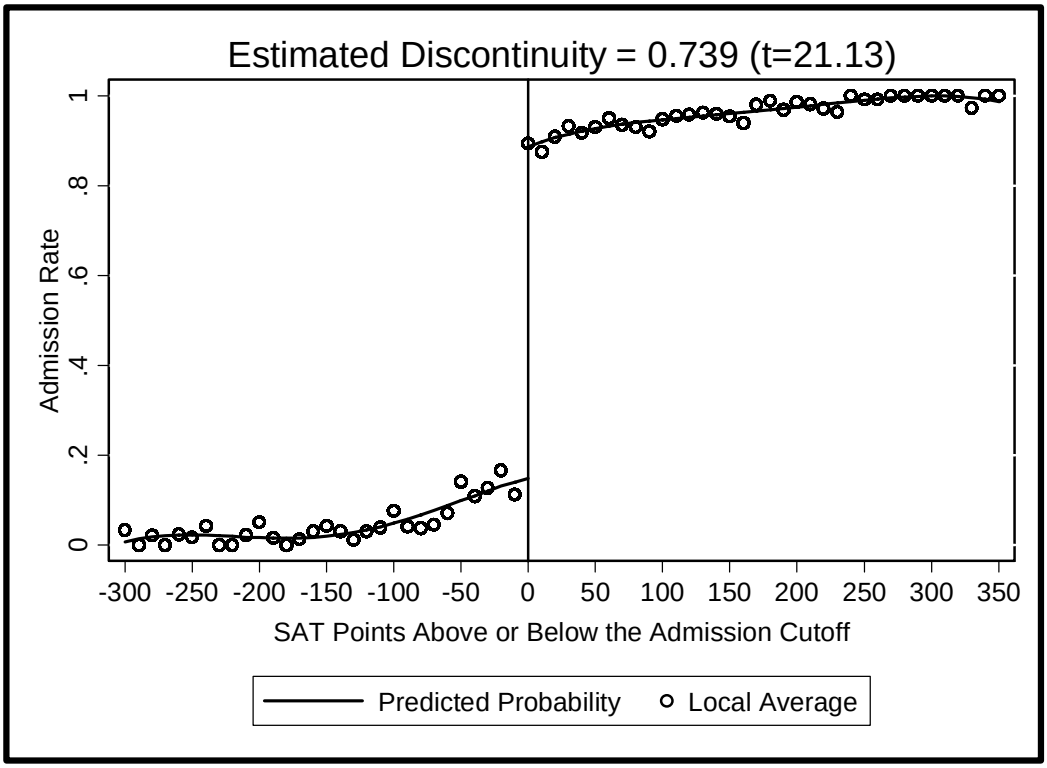

Figure 2-8b: Regression Discontinuity Estimates for the Admission Rate of White Applicants Observed with 4 Consecutive Quarters of Earnings in the 15th Year after High School Graduation

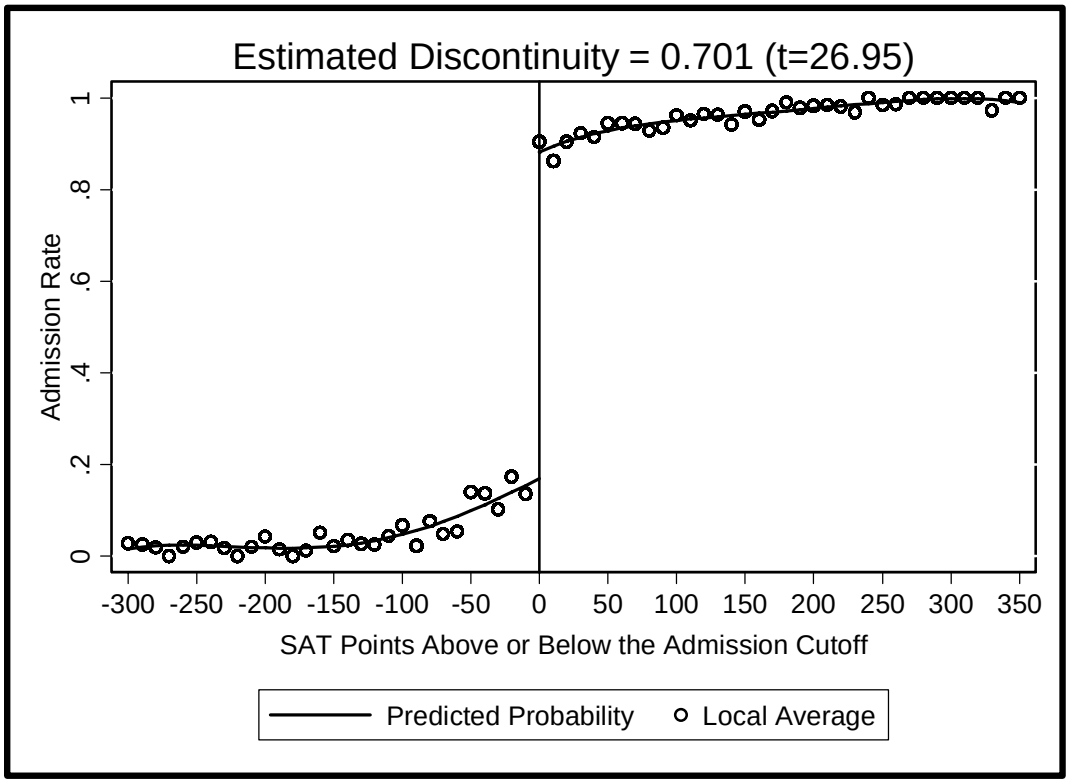

Figure 2-9a: Regression Discontinuity Estimates for the Admission Rate of White Applicants Observed with Positive Earnings in the 12th Year after High School Graduation

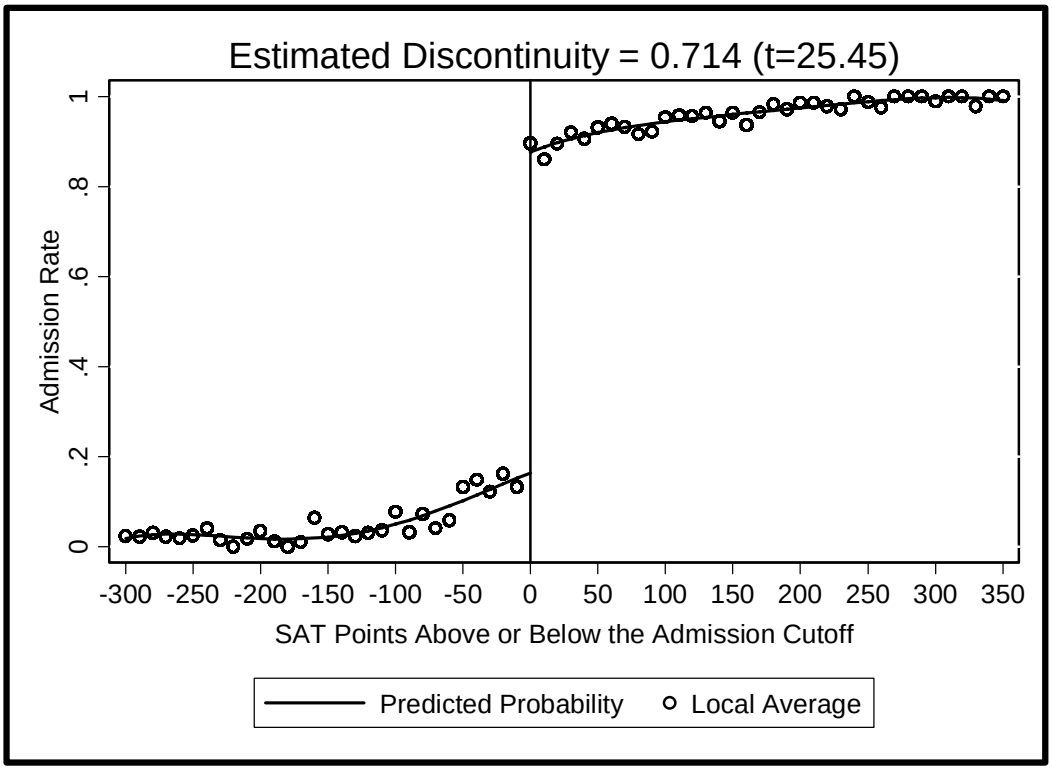

Figure 2-9b: Regression Discontinuity Estimates for the Admission Rate of White Applicants Observed with Positive Earnings in the 15th Year after High School Graduation

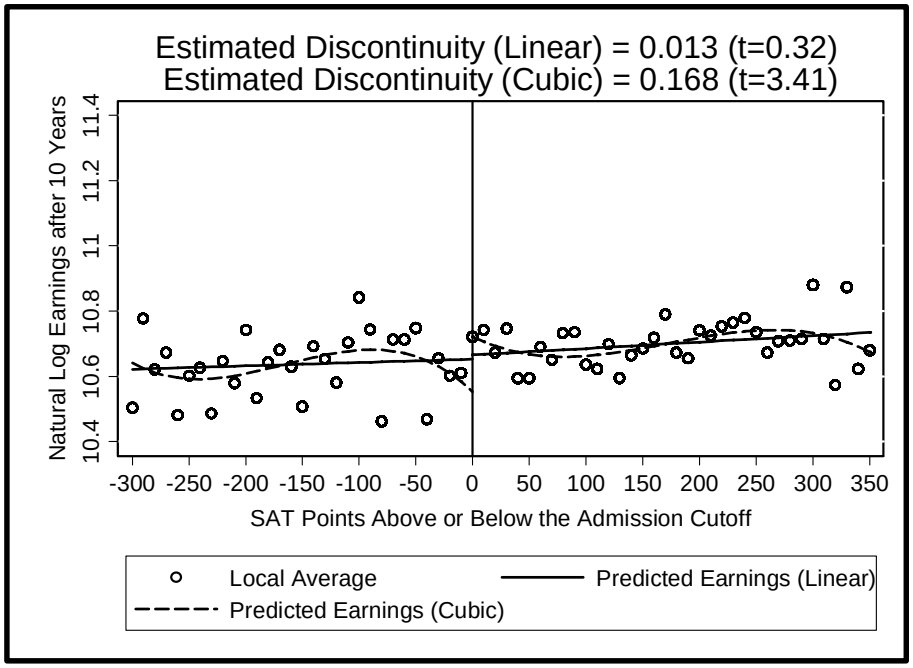

Figure 2-10a: The Natural Log of 4 Consecutive Quarters of Earnings for White Males 10 Years after High School Graduation

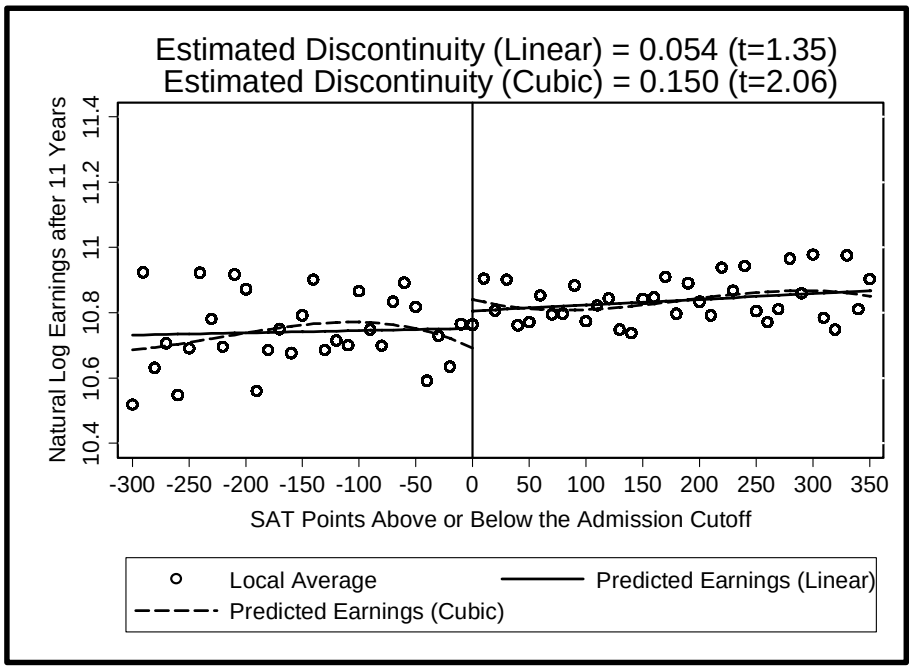

Figure 2-10b: The Natural Log of 4 Consecutive Quarters of Earnings for White Males 11 Years after High School Graduation

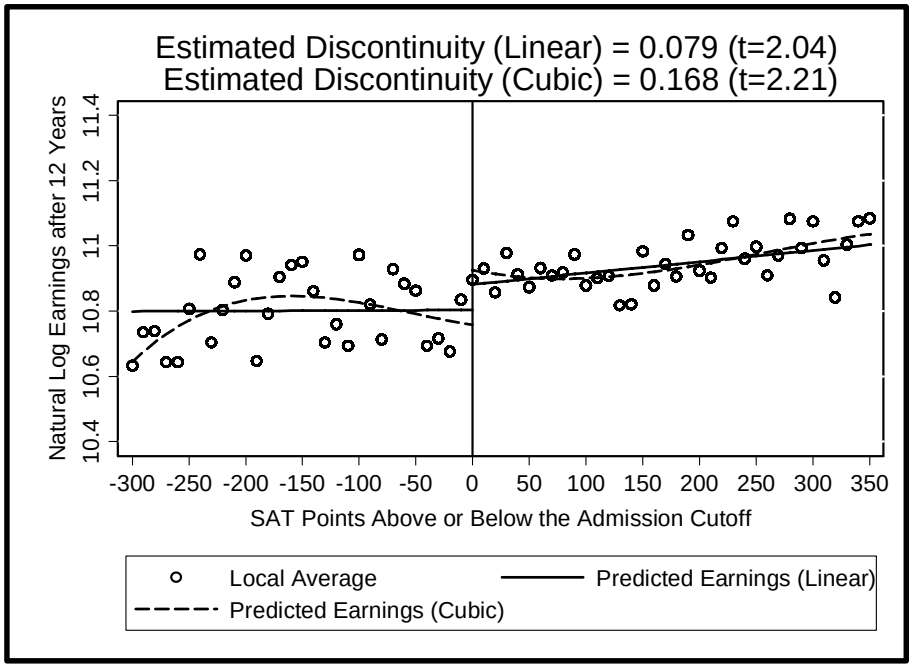

Figure 2-10c: The Natural Log of 4 Consecutive Quarters of Earnings for White Males 12 Years after High School Graduation

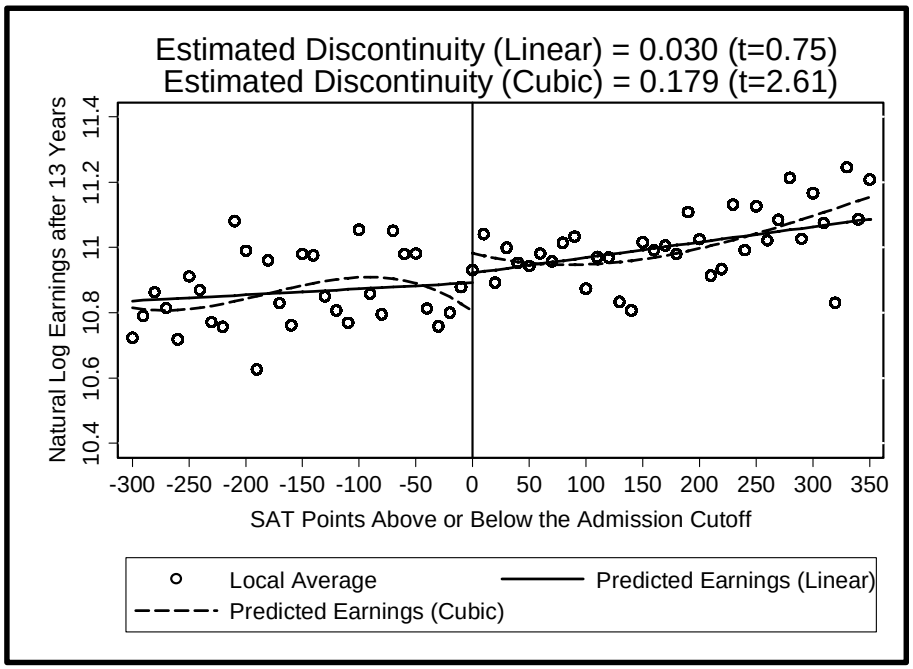

Figure 2-10d: The Natural Log of 4 Consecutive Quarters of Earnings for White Males 13 Years after High School Graduation

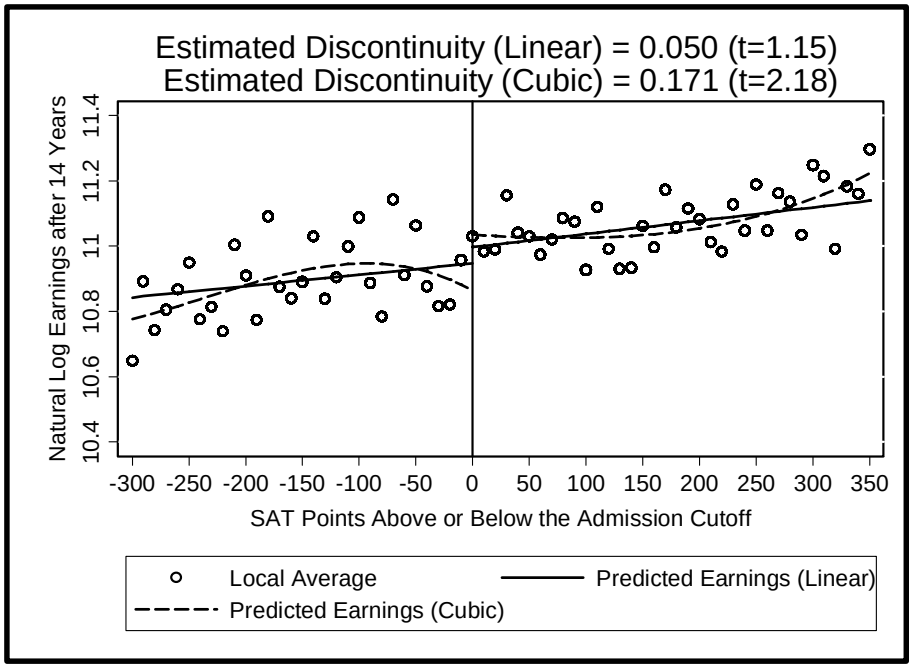

Figure 2-10e: The Natural Log of 4 Consecutive Quarters of Earnings for White Males 14 Years after High School Graduation

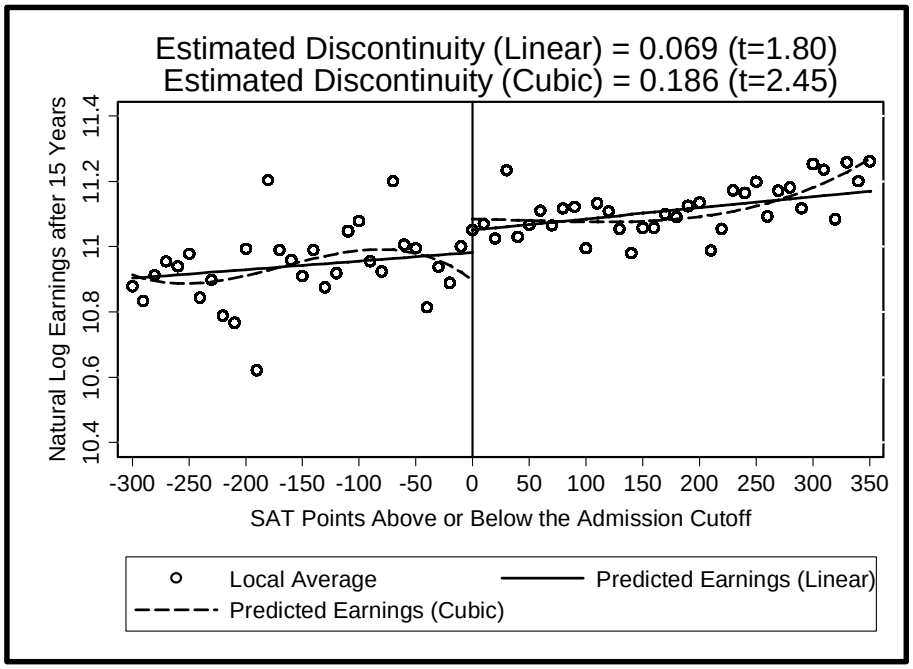

Figure 2-10f: The Natural Log of 4 Consecutive Quarters of Earnings for White Males 15 Years after High School Graduation

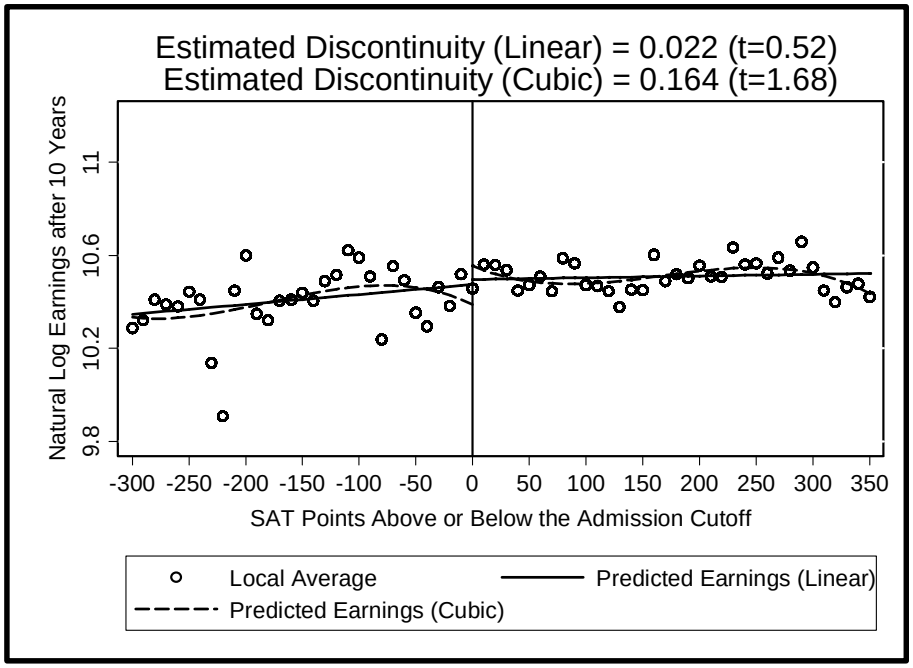

Figure 2-11a: The Natural Log of Annualized Earnings for White Males 10 Years after High School Graduation

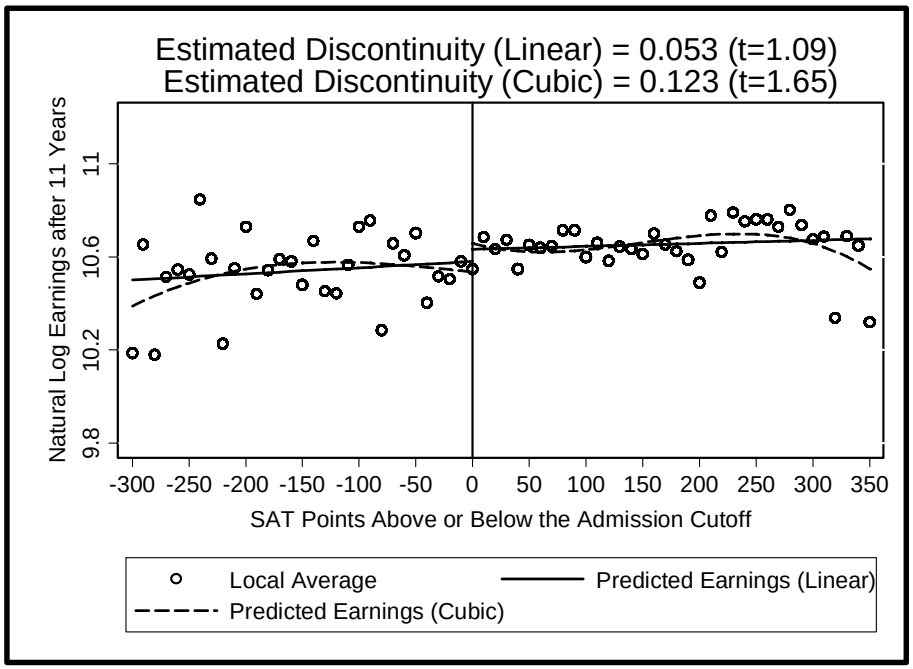

Figure 2-11b: The Natural Log of Annualized Earnings for White Males 11 Years after High School Graduation

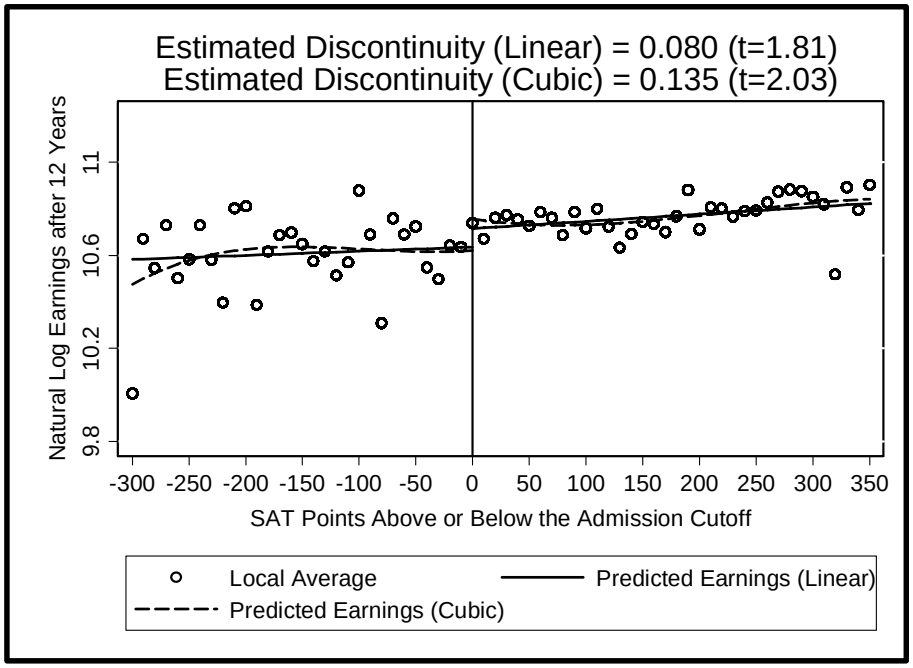

Figure 2-11c: The Natural Log of Annualized Earnings for White Males 12 Years after High School Graduation

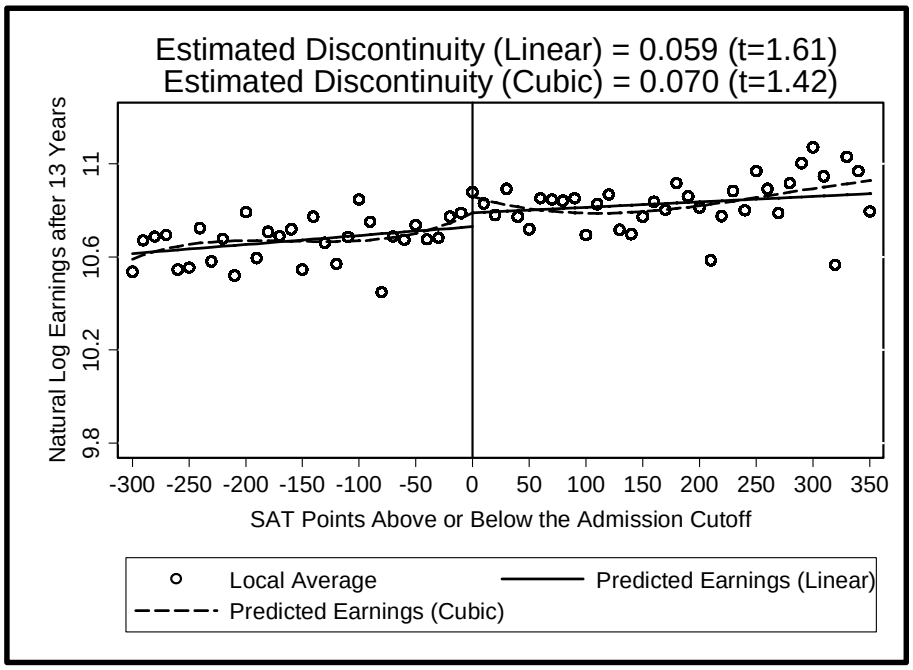

Figure 2-11d: The Natural Log of Annualized Earnings for White Males 13 Years after High School Graduation

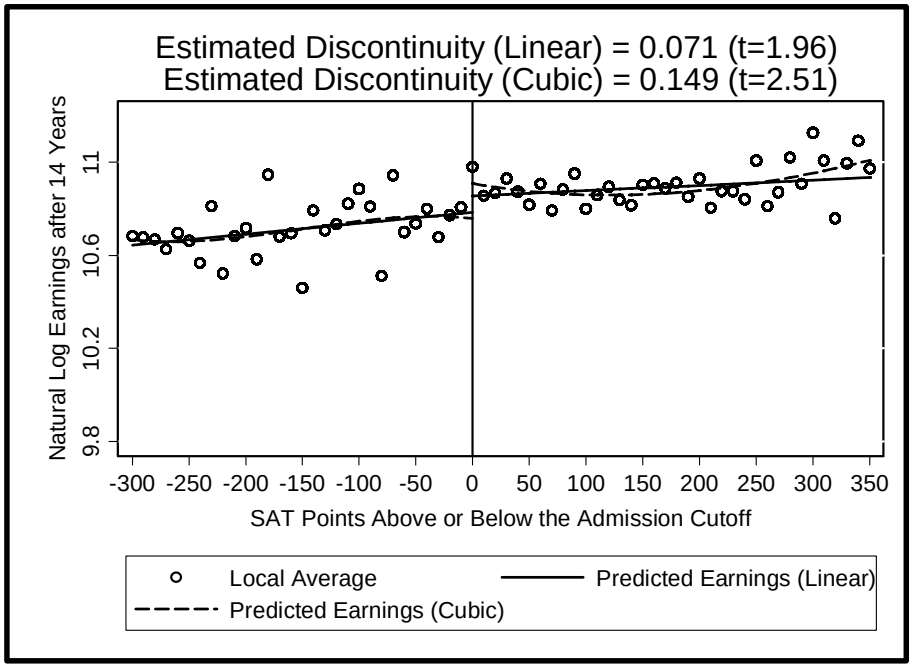

Figure 2-11e: The Natural Log of Annualized Earnings for White Males 14 Years after High School Graduation

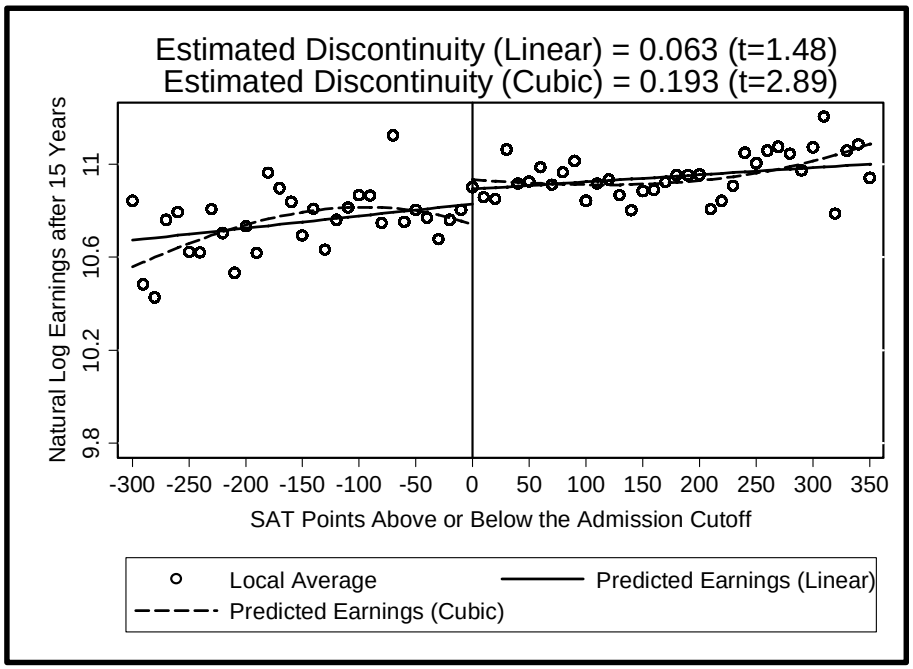

Figure 2-11f: The Natural Log of Annualized Earnings for White Males 15 Years after High School Graduation

Table 2-3: Summary of Regression Discontinuity Estimates for the Earnings of White Men Presented in Figures 10a – 10f and Figures 11a – 11f

Year After High School Graduation	Earnings Measure			
	Four Consecutive Quarters		Annualized	
	Linear	Cubic	Linear	Cubic
10	0.013	**0.168**	0.022	**0.164**
	(0.041)	**(0.049)**	(0.042)	**(0.098)**
	[0.754]	**[0.001]**	[0.604]	**[0.099]**
11	0.054	**0.150**	0.053	0.123
	(0.040)	**(0.073)**	(0.049)	(0.075)
	[0.180]	**[0.043]**	[0.282]	[0.103]
12	**0.079**	**0.168**	**0.080**	**0.135**
	(0.039)	**(0.076)**	**(0.044)**	**(0.067)**
	[0.046]	**[0.031]**	**[0.074]**	**[0.046]**
13	0.030	**0.179**	0.059	0.070
	(0.041)	**(0.069)**	(0.037)	(0.049)
	[0.458]	**[0.011]**	[0.113]	[0.159]
14	0.050	**0.171**	**0.071**	**0.149**
	(0.043)	**(0.079)**	**(0.036)**	**(0.059)**
	[0.255]	**[0.033]**	**[0.054]**	**[0.015]**
15	**0.069**	**0.186**	0.063	**0.193**
	(0.038)	**(0.076)**	(0.043)	**(0.067)**
	[0.076]	**[0.017]**	[0.143]	**[0.005]**

Notes: Robust standard errors clustered at the adjusted SAT score level are in parentheses; p-values are in brackets. Estimates in bold are statistically significant at the 10% level.

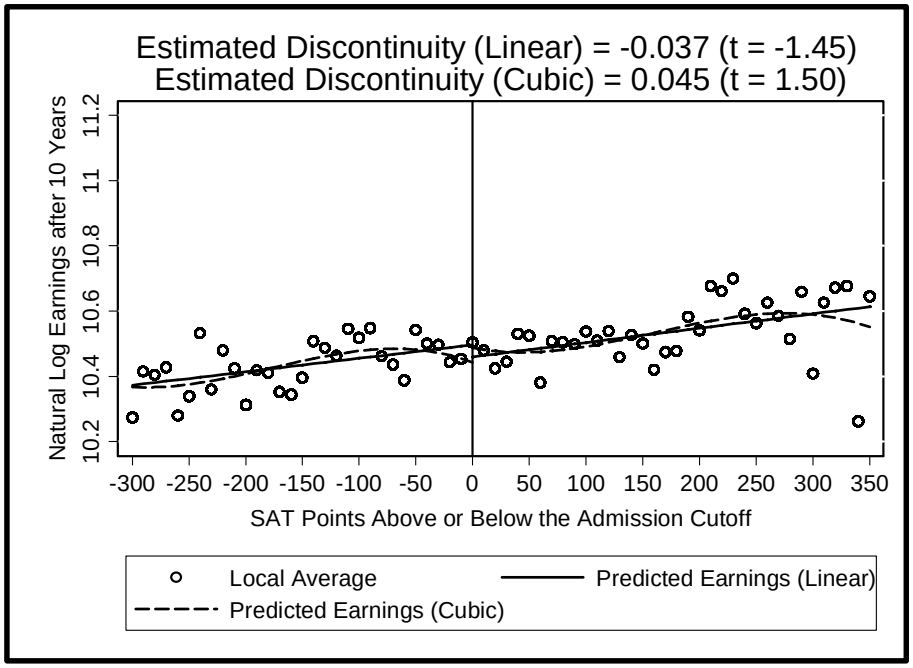

Figure 2-12a: The Natural Log of 4 Consecutive Quarters of Earnings for White Women 10 Years after High School Graduation

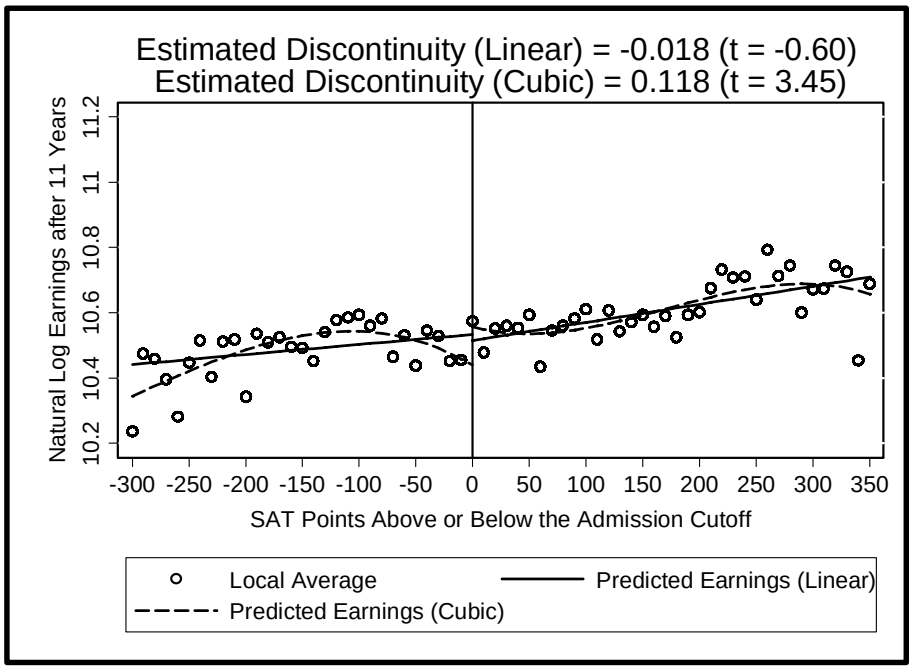

Figure 2-12b: The Natural Log of 4 Consecutive Quarters of Earnings for White Women 11 Years after High School Graduation

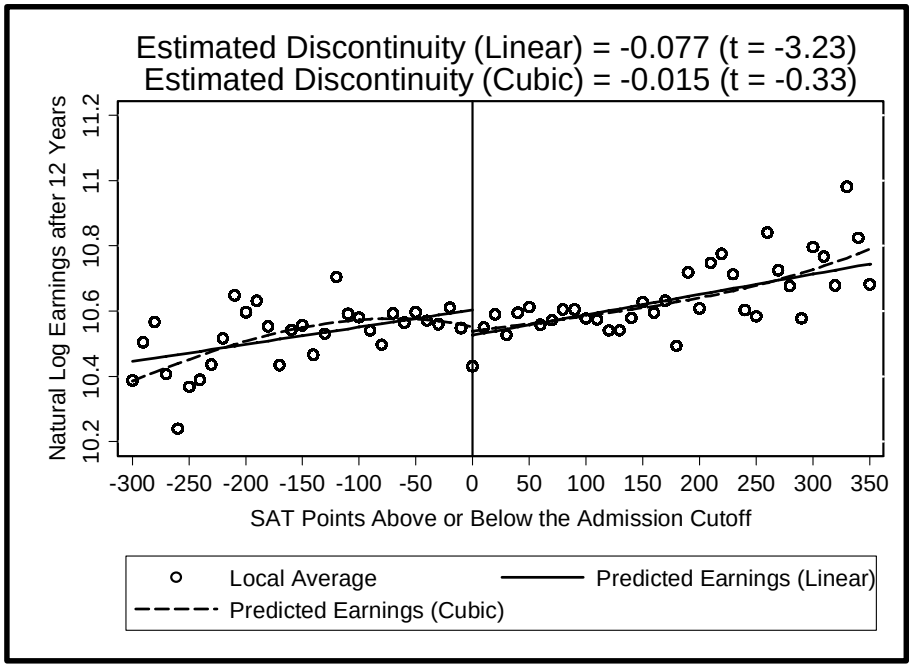

Figure 2-12c: The Natural Log of 4 Consecutive Quarters of Earnings for White Women 12 Years after High School Graduation

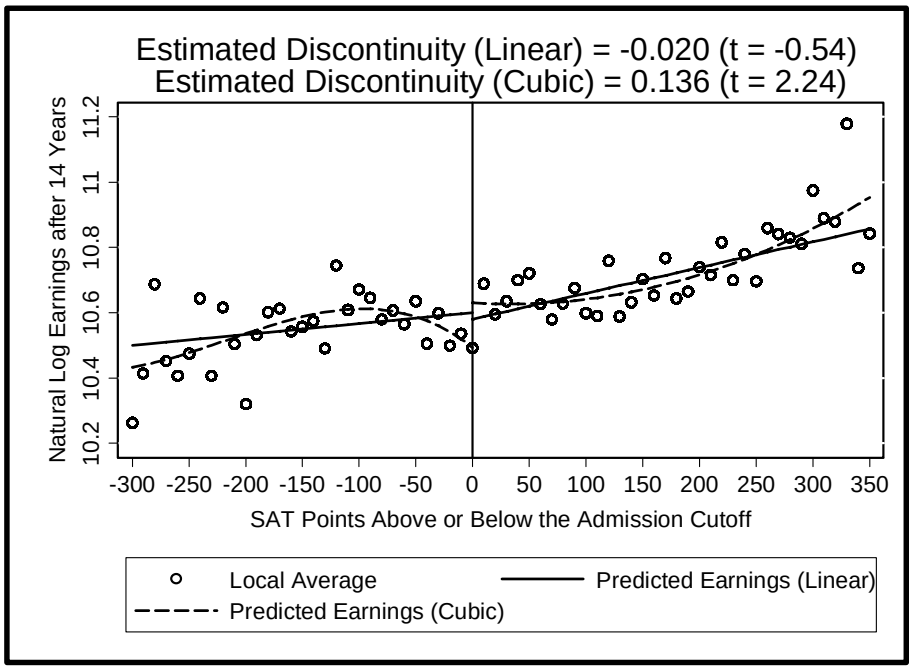

Figure 2-12d: The Natural Log of 4 Consecutive Quarters of Earnings for White Women 13 Years after High School Graduation

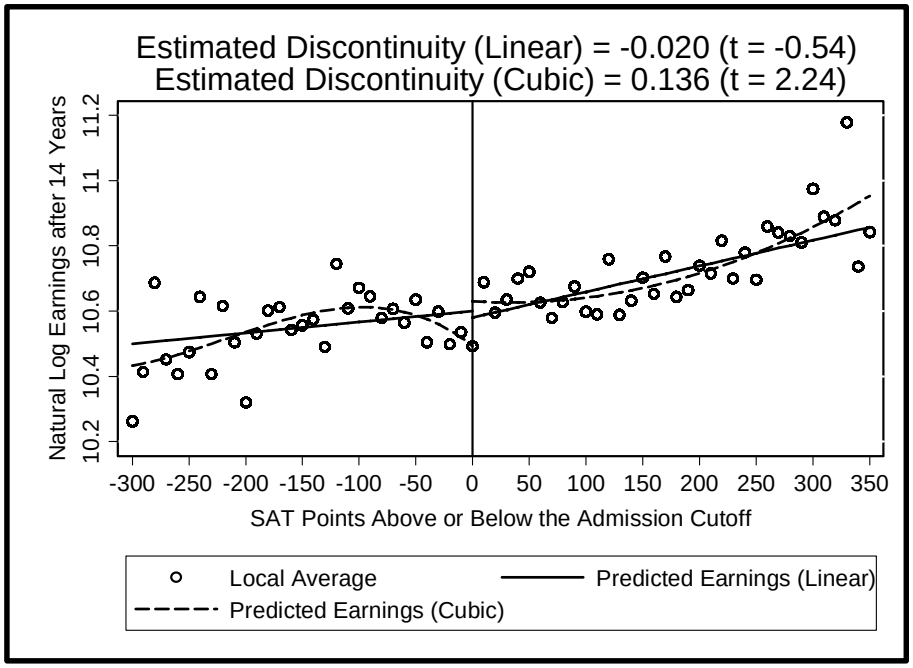

Figure 2-12e: The Natural Log of 4 Consecutive Quarters of Earnings for White Women 14 Years after High School Graduation

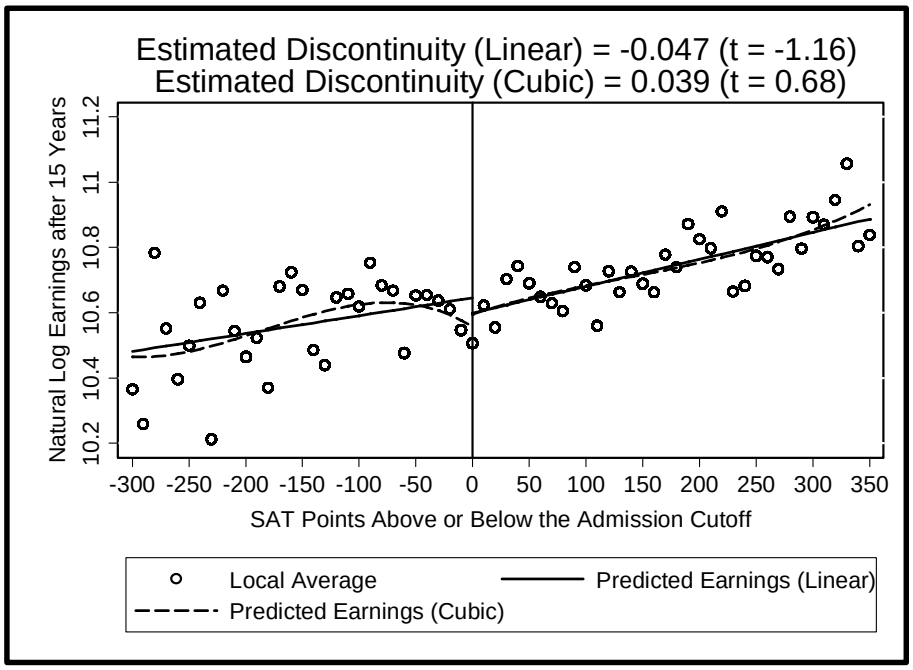

Figure 2-12f: The Natural Log of 4 Consecutive Quarters of Earnings for White Women 15 Years after High School Graduation

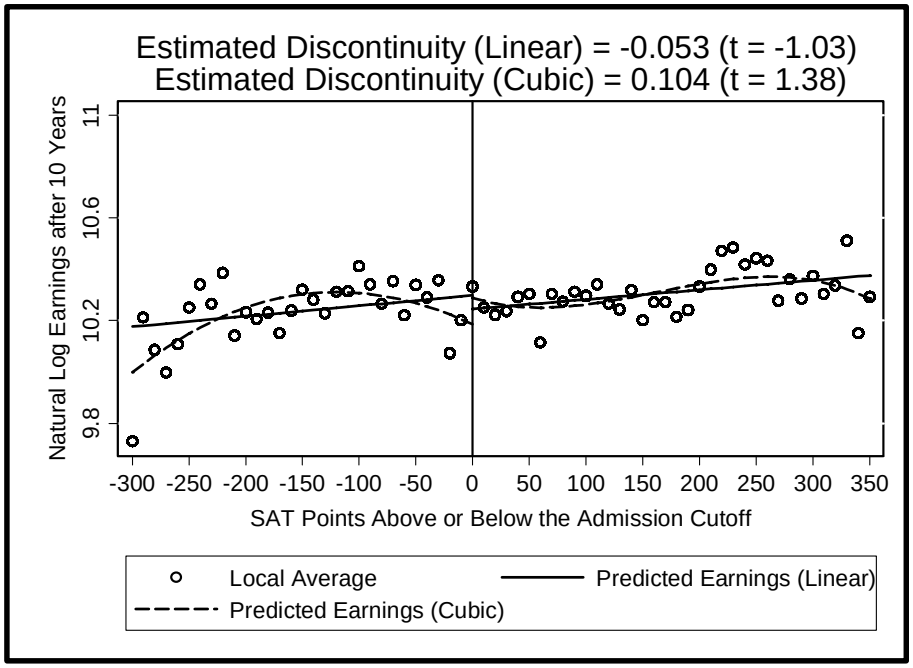

Figure 2-13a: The Natural Log of Annualized Earnings for White Women 10 Years after High School Graduation

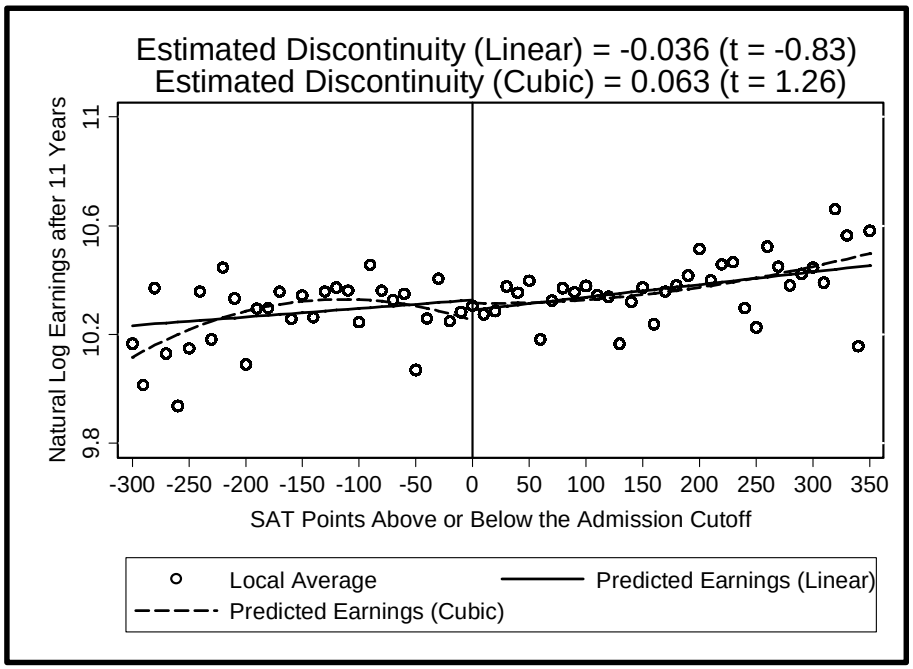

Figure 2-13b: The Natural Log of Annualized Earnings for White Women 11 Years after High School Graduation

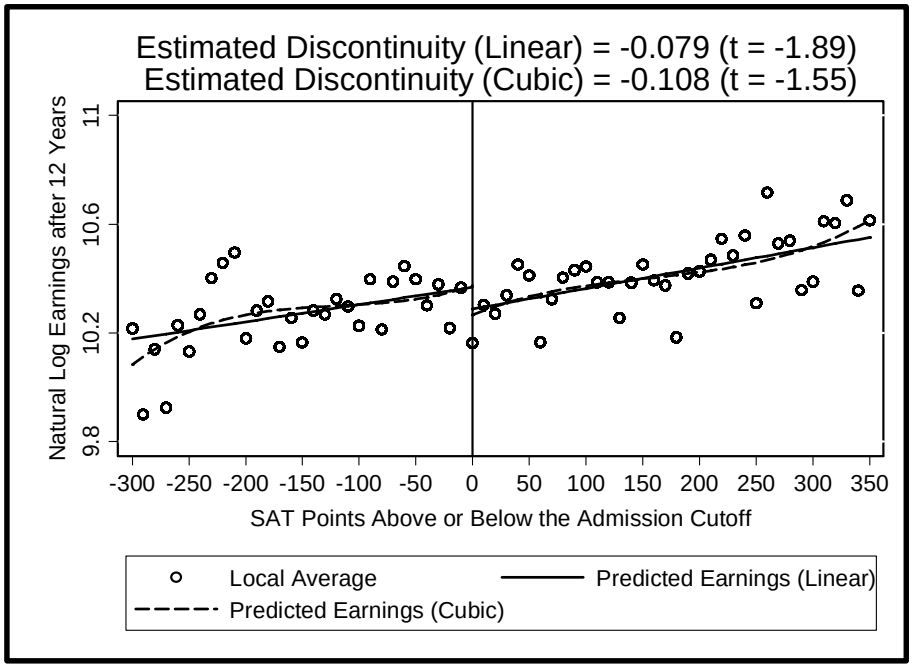

Figure 2-13c: The Natural Log of Annualized Earnings for White Women 12 Years after High School Graduation

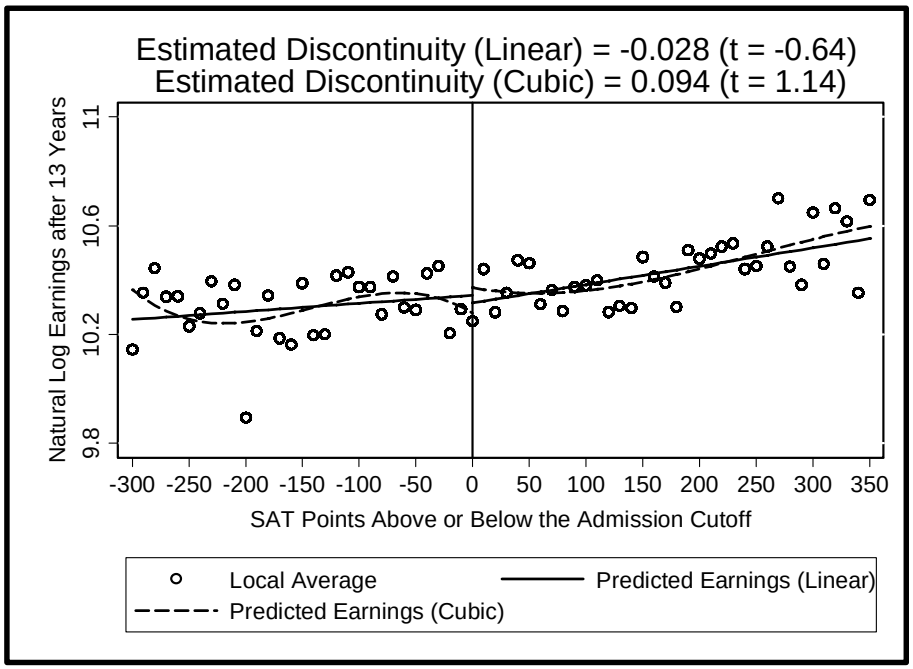

Figure 2-13d: The Natural Log of Annualized Earnings for White Women 13 Years after High School Graduation

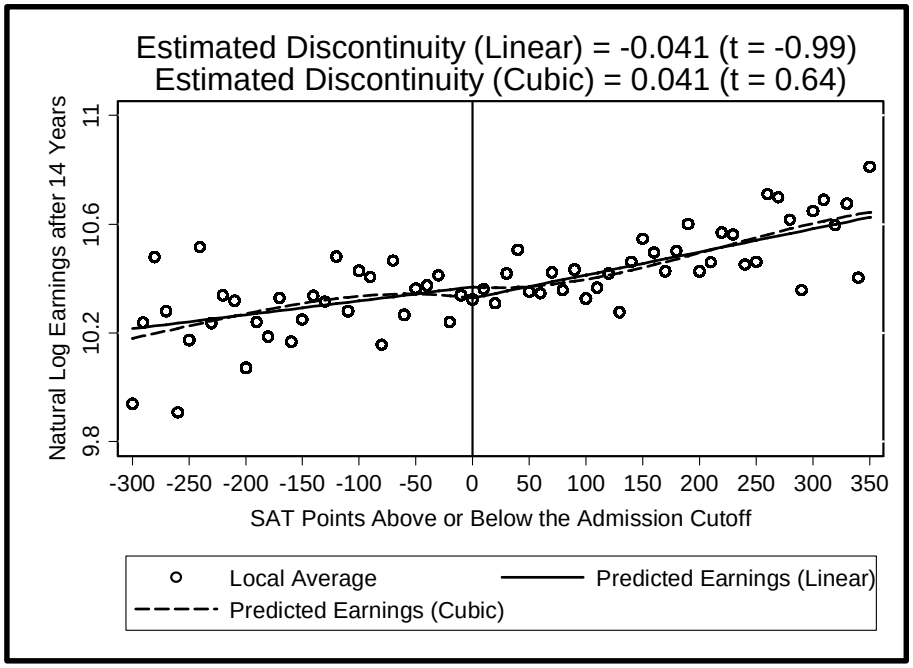

Figure 2-13e: The Natural Log of Annualized Earnings for White Women 14 Years after High School Graduation

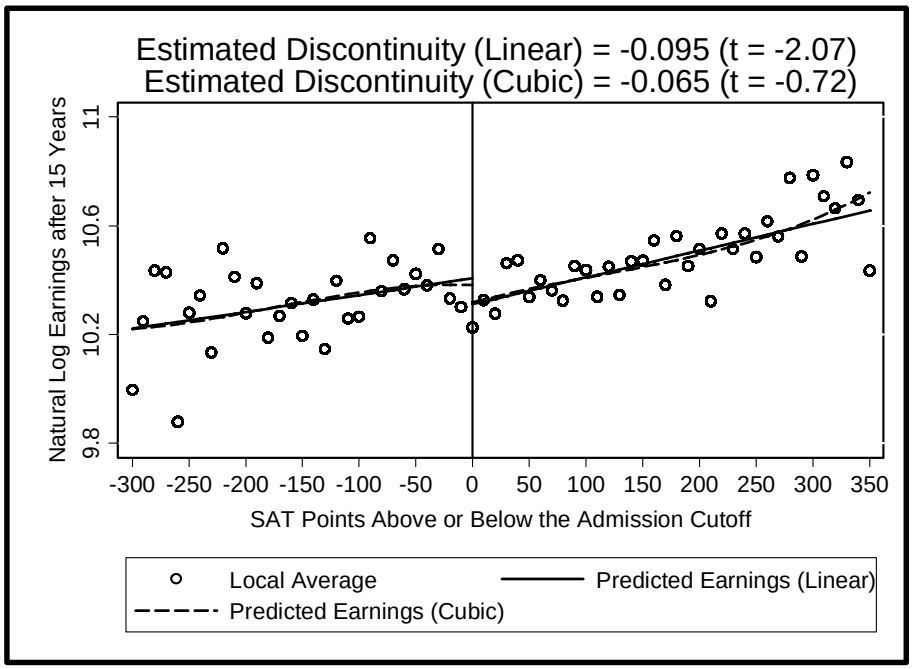

Figure 2-13f: The Natural Log of Annualized Earnings for White Women 15 Years after High School Graduation

Table 2-4: Summary of Regression Discontinuity Estimates for the Earnings of White Women Presented in Figures 12a – 12f and Figures 13a – 13f

Year After High School Graduation	Earnings Measure			
	Four Consecutive Quarters		Annualized	
	Linear	Cubic	Linear	Cubic
10	-0.037	0.045	-0.053	0.104
	(0.026)	(0.030)	(0.051)	(0.075)
	[0.152]	[0.137]	[0.307]	[0.172]
11	-0.018	**0.118**	-0.036	0.063
	(0.031)	**(0.034)**	(0.044)	(0.050)
	[0.553]	**[0.001]**	[0.408]	[0.213]
12	**-0.077**	-0.015	**-0.079**	-0.108
	(0.024)	(0.045)	**(0.042)**	(0.069)
	[0.002]	[0.746]	**[0.064]**	[0.125]
13	-0.045	**0.123**	-0.028	0.094
	(0.038)	**(0.056)**	(0.044)	(0.082)
	[0.243]	**[0.030]**	[0.527]	[0.259]
14	-0.020	**0.136**	-0.041	0.041
	(0.036)	**(0.061)**	(0.041)	(0.064)
	[0.592]	**[0.028]**	[0.324]	[0.525]
15	-0.047	0.039	**-0.095**	-0.065
	(0.040)	(0.057)	**(0.046)**	(0.090)
	[0.249]	[0.498]	**[0.042]**	[0.474]

Notes: Robust standard errors clustered at the adjusted SAT score level are in parentheses; p-values are in brackets. Estimates in bold are statistically significant at the 10% level.

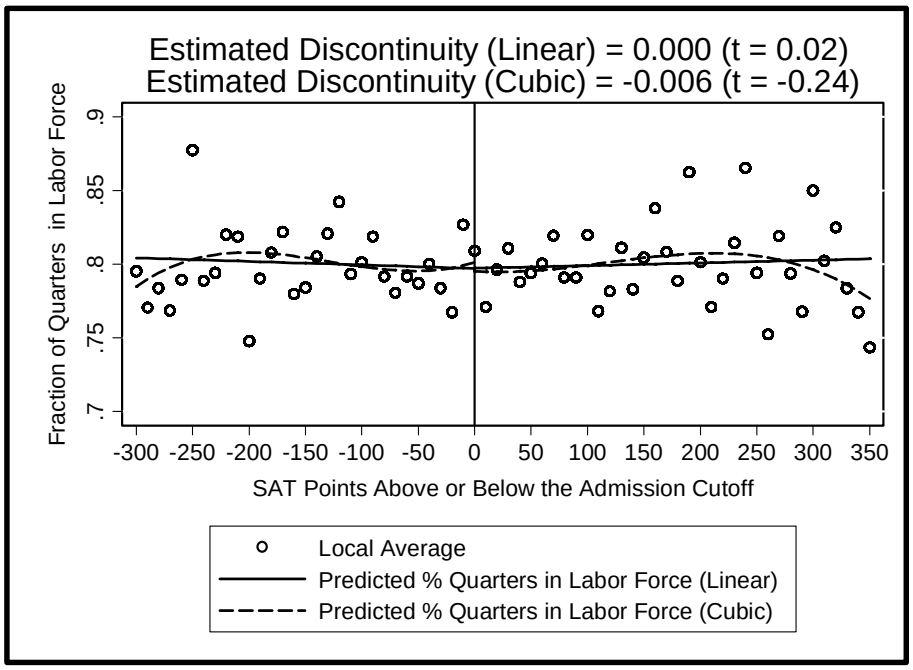

Figure 2-14: The Labor Force Participation of White Women Age 28 – 33 Observed in the Labor Force at Age 33

Table 2-5: Regression Discontinuity Estimates after 12 and 15 Years for Various Specifications and Subsamples

Specification Number	Regression Specification			Estimated Discontinuity after High School Graduation	
	Adjusted SAT Functional Form	**Controls**	**Sample**	**12 Years**	**15 Years**
(1)	linear linear*Admit	No	Full n = 4,943 (12 Years) n = 4,911 (15 Years)	**0.078** **(0.039)** **[0.046]**	**0.069** **(0.038)** **[0.076]**
(2)	linear linear*Admit	Yes	Full n = 4,943 (12 Years) n = 4,911 (15 Years)	**0.078** **(0.039)** **[0.049]**	0.060 (0.040) [0.136]
(3)	quadratic, quadratic*Admit	Yes	Full n = 4,943 (12 Years) n = 4,911 (15 Years)	**0.167** **(0.059)** **[0.006]**	**0.140** **(0.058)** **[0.019]**
(4)	cubic, cubic*Admit	No	Full n = 4,943 (12 Years) n = 4,911 (15 Years)	**0.168** **(0.076)** **[0.031]**	**0.186** **(0.076)** **[0.017]**
(5)	cubic, cubic*Admit	Yes	Full n = 4,943 (12 Years) n = 4,911 (15 Years)	**0.147** **(0.077)** **[0.061]**	**0.171** **(0.080)** **[0.036]**
(6)	linear linear*Admit	Yes	Only applicants always observed 10 - 15 years after high school graduation n = 1,947 (both years)	**0.093** **(0.050)** **[0.069]**	0.069 (0.058) [0.238]
(7)	cubic, cubic*Admit	Yes	Only applicants always observed 10 - 15 years after high school graduation n = 1,947 (both years)	**0.260** **(0.067)** **[0.000]**	**0.254** **(0.094)** **[0.009]**
(8)	linear linear*Admit	Yes	Only applicants who missed or exceeded the admission cutoff by no more than 100 SAT points	**0.167** **(0.070)** **[0.026]** n = 2,196	**0.157** **(0.065)** **[0.026]** n = 2,162
(9) Median Regression	linear linear*Admit	Yes	Full n = 4,943 (12 Years) n = 4,911 (15 Years)	0.034 (0.038) [0.371]	0.056 (0.042) [0.190]
(10) Median Regression	cubic, cubic*Admit	Yes	Full n = 4,943 (12 Years) n = 4,911 (15 Years)	0.098 (0.076) [0.196]	0.097 (0.079) [0.220]

Notes: Each row reports the estimated discontinuities for earnings after 12 and 15 years using the same functional form. Robust standard errors clustered at the adjusted SAT score level are in parentheses; p-values are in brackets. Controls include dummy variables for each year/term of application as well as actual SAT score and high school GPA. Estimates in bold are statistically significant at the 10% level.

Table 2-6: Regression Discontinuity Estimates after 12 and 15 Years for Various Specifications and Subsamples for White Women

Specification Number	Regression Specification			Estimated Discontinuity after High School Graduation	
	Adjusted SAT Functional Form	**Controls**	**Sample**	**12 Years**	**15 Years**
(1)	linear linear*Admit	No	Full n = 4,614 (12 Years) n = 4,096 (15 Years)	**-0.077** **(0.024)** **[0.002]**	-0.047 (0.040) [0.249]
(2)	linear linear*Admit	Yes	Full n = 4,614 (12 Years) n = 4,096 (15 Years)	**-0.082** **(0.024)** **[0.001]**	-0.039 (0.041) [0.343]
(3)	quadratic, quadratic*Admit	Yes	Full n = 4,614 (12 Years) n = 4,096 (15 Years)	-0.026 (0.034) [0.443]	0.010 (0.051) [0.848]
(4)	cubic, cubic*Admit	No	Full n = 4,614 (12 Years) n = 4,096 (15 Years)	-0.015 (0.045) [0.746]	0.039 (0.057) [0.498]
(5)	cubic, cubic*Admit	Yes	Full n = 4,614 (12 Years) n = 4,096 (15 Years)	-0.031 (0.044) [0.484]	0.034 (0.057) [0.554]
(6)	linear linear*Admit	Yes	Only applicants always observed 10 - 15 years after high school graduation n = 1,687 (both years)	**0.077** **(0.042)** **[0.071]**	0.053 (0.048) [0.268]
(7)	cubic, cubic*Admit	Yes	Only applicants always observed 10 - 15 years after high school graduation n = 1,687 (both years)	**0.144** **(0.052)** **[0.007]**	**0.217** **(0.056)** **[0.000]**
(8)	linear linear*Admit	Yes	Only applicants who missed or exceeded the admission cutoff by no more than 100 SAT points	-0.051 (0.035) [0.158] n = 2,215	0.038 (0.047) [0.429] n=1,943
(9) Median Regression	linear linear*Admit	No	Full n = 4,614 (12 Years) n = 4,096 (15 Years)	-0.025 (0.032) [0.434]	-0.019 (0.035) [0.595]
(10) Median Regression	cubic, cubic*Admit	No	Full n = 4,614 (12 Years) n = 4,096 (15 Years)	**0.098** **(0.058)** **[0.094]**	0.037 (0.071) [0.605]

Notes: Each row reports the estimated discontinuities for earnings after 12 and 15 years using the same functional form. Robust standard errors clustered at the adjusted SAT score level are in parentheses; p-values are in brackets. Controls include dummy variables for each year/term of application as well as actual SAT score and high school GPA. Estimates in bold are statistically significant at the 10% level.

LIST OF REFERENCES

Amato, Paul R., and Alan Booth. 1991. "Consequences of Parental Divorce and Marital Unhappiness for Adult Well-Being." *Social Forces* 69 (3): 895-914.

Arcidiacono, Peter. 2005. "Affirmative Action in Higher Education: How Do Admission and Financial Aid Rules Affect Future Earnings?" *Econometrica* 73 (5): 1477-4524.

Ayres, Ian, and Richard Brooks. 2005. "Does Affirmative Action Reduce the Number of Black Lawyers?" *Stanford Law Review* 57 (6): 1807-1854.

Behrman, Jere, Mark Rozenzweig, and Paul Taubman. 1996. "College Choice and Wages: Estimates Using Data on Female Twins." *The Review of Economics and Statistics* 78: 672-685

Black, Dan, and Jeff Smith. 2004. "How Robust Is the Evidence on the Effects of College Quality? Evidence from Matching." *Journal of Econometrics* 121: 99-124.

Borgess, Scott. 1998. "Family Structure, Economic Status, and Educational Attainment." *Journal of Population Economics* 11: 205-222.

Bound, John. 1989. "The Health and Earnings of Rejected Disability Insurance Applicants." *American Economic Review* 79 (3): 482-503.

Brewer, Dominic, Eric Eide, and Ronald Ehrenberg. 1999. "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 (1): 104-123.

Bush, George W. 2002a. "President Announces Welfare Reform Agenda." Press Release from the Office of the Press Secretary at the White House, February 26, 2002. Last accessed June 7, 2006 at http://www.whitehouse.gov/news/releases/2002/02/20020226-11.html

Bush, George W. 2002b. "President Discusses Welfare Reform and Job Training." Press Release from the Office of the Press Secretary at the White House, February 27, 2002. Last accessed June 7, 2006 at http://www.whitehouse.gov/news/releases/2002/02/20020227-5.html

Cherlin, Andrew J., Kathleen E. Kiernan, P. Lindsay Chase-Lansdale. 1995. "Parental Divorce in Childhood and Demographic Outcomes in Young Adulthood." *Demography* 32 (3): 299-318.

Clinton, Hillary Rodham. 1996. *It Takes a Village*. New York: Simon & Schuster.

Corak, Miles. 2001. "Death and Divorce: The Long-Term Consequences of Parental Loss on Adolescents." *Journal of Labor Economics* 19 (3): 682-715.

Dale, Stacy Berg and Alan Krueger. 2002. "Estimating the Payoff to Attending a More Selective College: An Application of Selection on Observables and Unobservables." *Quarterly Journal of Economics* 117 (4): 1491-1527.

Deleire, Thomas, and Ariel Kalil. 2002. "Good Things Come in Threes: Single-Parent Multigenerational Family Structure and Adolescent Adjustment." *Demography*, 39 (2): 393-413.

Ermisch, John F. and Marco Francesconi. 2001a. "Family Structure and Children's Achievements." *Journal of Population Economics* 14: 249-270.

Ermisch, John F. and Marco Francesconi. 2001b. "Family Matters: Impacts of Family Background on Educational Attainments." *Economica* 68: 137-156.

Friedberg, Leora. 1998. "Did Unilateral Divorce Raise Divorce Rates? Evidence from Panel Data." *American Economic Review* 88 (3): 608-627.

Fronstin, Paul, David H. Greenberg, and Philip K. Robins. 2001. "Parental Disruption and the Labour Market Performance of Children When They Reach Adulthood." *Journal of Population Economics* 14: 137-172.

Fryer, Roland, and Steven Levitt. 2004. "The Causes and Consequences of Distinctively Black Names." *Quarterly Journal of Economics* 119: 767-805.

Furstenberg, Frank F. and Kathleen E. Kiernan. 2001. "Delayed Parental Divorce: How Much Do Children Benefit?" *Journal of Marriage and the Family* 63: 446-457.

Gruber, Jonathan. 2004. "Is Making Divorce Easier Bad for Children? The Long Run Implications of Unilateral Divorce." *Journal of Labor Economics* 22 (4): 799-833.

Haveman, Robert, Barbara Wolfe, and James Spaulding. 1991. "Childhood Events and Circumstances Influencing High School Completion." *Demography* 28 (1): 133-157.

Hill, Martha S., Wei-Jun J. Yeung, and Greg J. Duncan. 2001. "Childhood Family Structure and Young Adult Behaviors." *Journal of Population Economics* 14: 271-299.

Keith, Verna M., and Barbara Finlay. 1988. "The Impact of Parental Divorce on Children's Educational Attainment, Marital Status, Timing, and Likelihood of Divorce." *Journal of Marriage and the Family* 50: 797-809.

Kreider, Rose M. and Jason M. Fields. 1996. *Number, Timing, and Duration of Marriages and Divorces: Fall 1996*. Current Population Reports, P70-80. U.S. Census Bureau, Washington, DC.

Lang, Kevin, and Jay L. Zagorsky. 2000. "Does Growing Up With a Parent Absent Really Hurt?" *Journal of Human Resources* 36 (2): 253-273.

Lindahl, Lena and Hakan Regner. 2005. "College Choice and Subsequent Earnings: Results using Swedish Sibling Data." *Scandinavian Journal of Economics* 107 (3):437-457

McLanahan, Sara, and Gary Sandefur. 1994. *Growing Up with a Single Parent: What Helps, What Hurts.* Cambridge, MA: Harvard University Press.

Mincer, Jacob. 1974. *Schooling, Experience, and Earnings*. New York: National Bureau of Economic Research.

Painter, Gary, and David I. Levine. 2000. "Family Structure and Youths' Outcomes: Which Correlations Are Causal?" *Journal of Human Resources* 35 (3): 524-549.

Rose, Heather. 2005. "The Effects of Affirmative Action Programs: Evidence from the University of California at San Diego." *Educational Evaluation and Policy Analysis* 27 (3): 263-289.

Sandefur, Gary D., and Thomas Wells. 1999. "Does Family Structure Really Influence Educational Attainment?" *Social Science Research* 28: 331-357.

Sander, Richard. 2004. "A Systematic Analysis of Affirmative Action in American Law Schools." *Stanford Law Review* 57 (2): 367-483.

U.S. Bureau of the Census. 1999. *Statistical Abstract of the United States*, No. 155 and No. 159. Washington, D.C.

U.S. Bureau of the Census. 1970. *Statistical Abstract of the United States*, No. 75. Washington, D.C.

BIOGRAPHICAL SKETCH

Mark Hoekstra received his bachelor's degree from Hope College in Holland, Michigan. After graduation he will begin employment as an assistant professor at the University of Pittsburgh.