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Economic Decisions in the Financing and Timing of Higher Education

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Economic Decisions in the Financing and Timing of Higher Education

by

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To David and Annabelle

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Economic Decisions in the Financing and Timing of Higher Education

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This dissertation is a collection of three studies in the field of higher education. Chapter 2 evaluates the higher education tax benefits which began in 1998. This study analyzes whether the tax treatment has caused changes in the enrollment behavior among those eligible. It explores the effects on full time and part time enrollment and the effects of the rule changes in 2002 and 2003, as well as examines how marginal changes in the tax benefits affect the probability of enrollment. There is an increase in overall enrollment which can be attributed to the tax benefits, although the expansion of the program had very small effects and there were very few changes in full time student status due to the program.

The second essay examines students who take a break in their schooling but return to school before beginning their careers. This can cause two separate effects; as time passes, they are growing older, maturing and learning about themselves. However, they also risk depreciation of the human capital they have acquired. This study examines these competing effects on outcomes for individuals who took time off between

completing their undergraduate studies and attending law school. Results indicate that those who take time off earn higher grades on average, but that the effect on earnings is dependent on what the individual did during the schooling gap. There does appear to be a small but persistent penalty for those who have a gap in schooling.

In the third essay, a model is where altruistic parents care about the bundle of goods their children consume is presented and analyzed. The model results in some empirically testable predictions, which are tested using the National Education Longitudinal Study (NELS). In particular, students whose parents pay the entire cost of schooling should have a lower return to the amount invested than those who pay some of the cost themselves. However, the data show very little difference in the return to the amount invested between the two groups.

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CHAPTER 1

Introduction

The role of human capital is something of great interest to economists. The concept goes all the way back to Adam Smith's observation that in a pin factory, individuals can specialize and become more skilled in a particular task. The idea of building skills to increase productivity is of primary concern to those who study workers in an economy. Human capital investment can take many forms; it can consist of on-the-job training, an apprenticeship, primary or secondary schooling, higher education, and many other pursuits. This dissertation focuses on analyzing decisions pertaining to higher education from three different perspectives. Each of these three essays makes a unique contribution to the existing literature studying higher education. In chapter two, the decision of the individual to enroll in college or not is studied. This study is focused on how changes in the tax system have changed enrollment behavior. The third chapter studies how the timing of higher education affects future outcomes of individuals by analyzing outcomes of individuals from a particular law school. The fourth chapter studies the relationship between adult children and their parents and looks at how school financing affects returns to schooling.

The second chapter of this dissertation looks at how individuals respond to a change in the price of higher education due to a change in tax policy. It studies the decision of whether or not to enroll in higher education as well as how intensely students choose to enroll. In 1998, the federal government began awarding tax credits to students (or their families) to offset some of the cost of higher education by introducing the Hope Tax Credit and the Lifetime Learning Tax Credit. In 2002 and 2003 the tuition deduction was added and the Lifetime Learning Credit was expanded, and these changes are also examined. A typical economic framework is used, which predicts that when the price of education falls due to the tax changes described we observe two outcomes: individuals enrolling in higher education should increase their enrollment intensity, and the probability of enrolling at all should increase as well. Although the analysis of the program does indicate that enrollment increased by about four to five percentage points as a result of the program, there is little evidence of changes on the intensive margin as defined in the study.

The third chapter of this dissertation studies the effects of having a gap in schooling. More specifically, this chapter considers individuals who finish an undergraduate degree and continue to law school, and looks at the effect of having a gap between these two. This chapter makes a unique contribution to the literature, since those who have looked at schooling gaps before have focused on time off between high school and college or those who leave college and return. The data used in this study are from the University of Michigan law school, so the population considered is nearly homogeneous. Therefore, the effects are identified more easily although generalization should be approached with caution. This is also the first paper to attempt to separate the effects of depreciating human capital during the time away from schooling from the effect of investment during that time. Results presented in this chapter indicate that those who take time off between their undergraduate and law school studies tend to earn slightly better grades than those who did not, but that this result is only robust to selection issues for those who worked in fields related to law during the time off. Results also indicate that there is a small but persistent wage penalty for those who take time off.

The fourth chapter of this dissertation presents a theory of altruistic parents choosing to make an in-kind transfer to their children in order to finance education. It formalizes the idea of paternalistic preferences, and some of the results of the model are testable in an empirical framework. The theory predicts differential returns to education based on who paid the costs of college. This prediction is tested using data from the National Education Longitudinal Survey (NELS), and very little evidence is found that such a difference exists. Further analysis and possible reasons for this are explored.

The three studies in this dissertation all analyze different aspects of higher education. The first studies how changes in price affect whether an individual enrolls in a post-secondary institution at all, and at any intensity level. The third chapter restricts analysis to those that not only attended college but also continued to law school. It studies the schooling outcomes and wage outcomes of these individuals, and compares those who completed all of their education in a single spell to those who did not. The fourth chapter also restricts analysis to those who have chosen to enroll, and examines how the choice of college financing affects returns to the amount invested. This analysis is also restricted to traditional students who are young adults, and studies the interplay between young adults and their parents. While the focus of each chapter is unique, the studies are intertwined and contribute to the literature in the Economics of Higher Education.

CHAPTER 2

Enrollment Effects of Higher Education Tax Benefits

2.1: INTRODUCTION

The complexity surrounding our nation's financial aid system and tax code can be daunting for young men and women. However, the tax benefits for higher education which are available to students are quite simple in comparison. For many, the Hope Credit pays a nonrefundable tax credit of $1200 for the first $1200 spent on tuition and fees and a tax credit equal to 50% on the next $1200 spent for degree-seeking students in their first two years of study. The Lifetime Learning Credit is a tax credit equal to 20% of money spent on tuition and fees up to a maximum credit of $2000. Of course there are caveats about who is eligible and what expenses are eligible, but the basic idea of the credits is fairly straightforward. Because the average tuition and fees per year were $21,588 for private schools and $5,950 for public four-year schools in the 2007-08 school year[1], the Hope Credit would be worth $1800 for those eligible paying the average tuition rates and the Lifetime Learning Credit would be worth $2000 for those attending private schools and approximately $1190 for those attending public schools. (Obviously these numbers vary based on full-time vs. part-time status, individual school cost, other scholarships and grants as well as fees which were not included.)

One can easily see that these tax credits are very valuable in terms of levels. The Lifetime Learning Credit is especially interesting since it is an across the board 20% price decrease for those who pay less than $10,000. However, in previous studies others have not found much evidence of a change in enrollment behavior that can be attributed to the

[1] http://nces.ed.gov/programs/digest/d08/tables/dt08_331.asp

change in the price of education due to tax benefits. This has been attributed to different reasons; since the tax credit is not refundable, one may think that the tax credit does not reach the student on the margin deciding whether or not to attend college. Long (2006) suggests that a large part of the problem is that the marginal student may struggle with liquidity constraints, and that the timing of the tax credit is a problem. (The time delay of the tax credit from a payment can be up to 15 months.) However, since the tax rules are known in advance and since those paying college expenses must have income to qualify, it is likely that they would adjust their withholding and receive the tax benefits in a timelier manner. Others (Smart and Stabile, 2003) have found that individuals are quite responsive to tax incentives in other areas. Long may have difficulty identifying the group of students eligible for the credits in the Current Population Survey (CPS). The income data she uses are not very detailed, and she is not always able to link dependent students to their parents. Other studies that have looked at enrollment effects due to price changes of $1000 (2001 dollars, assuming linear changes) have found enrollment effects of 0.04 (Leslie and Brinkman, 1989), 0.03 (Heller, 1997), 0.05 (Cameron and Heckman, 1999) and 0.085 (Kane, 1995). In light of these other studies, Long's finding that there were no enrollment effects due to the tax credits was quite surprising.

The purpose of this study is to identify the effect on enrollment in higher education of preferential tax treatment on higher education enrollment. This study uses the Panel Study of Income Dynamics (PSID)[2] data which have very detailed information on income to distinguish the eligible from the ineligible groups. This analysis differs from the previous literature because it is able to identify more clearly who is eligible for the tax treatment and estimate how much the tax benefits are likely to be for the eligible

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[2] The Panel Study of Income Dynamics is primarily sponsored by the National Science Foundation, the National Institute of Aging, and the National Institute of Child Health and Human Development and is conducted by the University of Michigan.

individuals. It also separately identifies the effects of the tuition deduction which became available in 2002 and has not been studied in previous literature. It is able to use the information about incomes to identify how much the credit is likely worth to an individual and measure the changes in enrollment probability of marginal changes in the tax benefits.

This study uses two specifications of the empirical model. Presented first is a difference in differences approach to identify the overall effects of tax benefits. Five years of data leading up to the tax change (1993-1997) and four years of data after the tax change (1999-2005, alternating years) are used. Those who are eligible for the tax credit will be the treatment group, while those who are not eligible will be the control group. The control group has both those who earn too much or too little to qualify, and are likely similar to the treatment group on average.[3] Next, average public school tuition levels are used to estimate what the credit is worth to an individual, factoring in the structure of the credit phase-outs for higher income individuals and the rules for the three tax benefit programs. The estimation shows that the average effect of the tax benefits for higher education leads to an increase in the probability of enrollment by 4.1-5.0 percentage points on average, and that the marginal effect of increasing the tax benefit by 1% of tuition leads to an increase in probability of enrollment by 0.02 to 0.13 percentage points under the current rules.

The rest of the chapter is organized as follows: Section 2.2 describes the tax credits in detail and the data which are used. Section 2.3 describes how the budget constraint is altered by each of the tax benefits and the hypotheses associated with these changes. Section 2.4 introduces the model and results with a binary indicator for

[3] If the effect of the policy is nonlinear, this could bias the results. Most likely those who earn less would be more responsive to a price change so the estimates could be biased upwards.

eligibility, Section 2.5 presents the empirical model which uses the amount of the credit, and Section 2.6 concludes.

2.2: STRUCTURE OF THE TAX BENEFITS AND DATA

The education tax credit scheme was passed into law in 1997 as part of the Taxpayer Relief Act and became part of the tax code in 1998. There are two separate tax credits (Hope Credit and Lifetime Learning Credit) as well as a tuition deduction available currently. The tuition deduction was not available until 2002, however. The tax treatments were instituted as a way to make college more affordable for middle class families. During 2006, Maag, Mundel, Rice, and Rueben estimate that the tax expenditure was about $3.5 billion on the Hope Credit, $2.2 billion on the Lifetime Learning Credit and $1.9 billion on the tuition deduction, while the spending on the Pell Grant program was approximately $12.7 billion. While not quite as large an expenditure as the Pell Grant program, preferential tax treatment constitutes a large portion of federal funding of education expenses.

2.2.1: The Hope Tax Credit

The first credit is known as the Hope Tax Credit (HTC). The Hope credit is designed to assist those students in their first two years of higher education. Although the limits have changed over the years, the basic scheme has remained the same. As of 2008, the HTC gives a credit equal to 100% of the first $1200 spent on tuition and required fees and 50% of the next $1200. Therefore, someone who qualifies for the HTC and spends $1000 on tuition would receive a $1000 credit. Someone who qualifies for the HTC and spends $5000 on tuition would receive a credit of $1200 from the first $1200 and $600 for the next $1200 spent, and nothing for the remaining $2600. The maximum credit that an individual could earn with the HTC is therefore $1800.

Those who are eligible for the HTC are in their first two years of study at a qualified institution. Qualified institutions are those postsecondary educational institutions that are qualified to participate in the student financial aid program as determined by the Department of Education. Qualified expenses include tuition and fees required by the institution; they do not include fees for room and board, books, or fees that are not paid to the institution and required as a cost of attendance. In order to qualify for the HTC, the student must be enrolled at least half-time for one academic period in the tax year. (Academic periods can include semesters, trimesters or quarters.) The student must also be enrolled as a degree seeking student. As mentioned before, the student must not have completed his first two years of study as of January 1 of the tax year (this generally means that the student is classified as a freshman or sophomore for at least part of the year), and he cannot have any felony drug charges on his record. The credit cannot be claimed more than twice even though there are usually three calendar years for which the qualifications are met. The HTC is a non-refundable credit; therefore, the tax filer must have some tax liability to qualify for the credit. It is also only a credit on expenses paid by the filer, so any money paid on behalf of the student such as a Pell Grant, other university grant, or scholarship would not be included as expenses. Therefore, these awards could reduce the amount of the credit depending on the amount of tuition and fees and the amounts of the grants.

2.2.2: The Lifetime Learning Tax Credit

The Lifetime Learning Tax Credit (LLTC) has somewhat simpler rules than the HTC. The LLTC allows the filer to claim a tax credit equal to 20% of qualified education expenses up to $10,000. Qualified expenses and institutions are defined in the same way for both the HTC and the LLTC. Therefore, the maximum credit that could be claimed under the LLTC is $2000. For example, if a student had expenses of $5000 in a tax year he would qualify for $1000 in a tax credit. However, if he instead had expenses of $15,000 in that tax year he would get a 20% credit for the first $10,000 and nothing for the next $5000. Therefore, this student would get the maximum credit of $2000. It should also be noted that if there are multiple students on a single return, this maximum is per return while the maximum for the HTC was per student.

The definition of eligible students is somewhat expanded for the LLTC, as implied by the name of the credit. The LLTC is for students at any point in their education. Students don't have to be seeking a degree and can be enrolled part time. As long as the course is being taken for credit or to improve job skills, the tuition and required fees are qualified expenses. This of course includes those who are still completing their first undergraduate degree, but it also includes those who are pursuing graduate study or those who are simply taking a course to update their education or learn new skills. Courses taken for a hobby or a sport however, are not eligible. Like the HTC, the LLTC can only be claimed for expenses paid by the student and therefore can be reduced by scholarships and grants. It is also a nonrefundable credit, so like the HTC the tax filer must have sufficient liability to take advantage of the tax credit.

2.2.3: The Tuition Deduction

The third tax incentive available is the tuition deduction. Unlike the tax credits, the effect of the tuition deduction depends on the marginal tax bracket of the person filing. However, unlike some deductions, the filer does not have to itemize deductions to take advantage of the tuition deduction. (It is an “above the line” deduction.) The filer is able to reduce his taxable income by up to $4000. If a student incurs tuition and fees of $2000 and is in the 25% marginal tax bracket, then the tuition deduction would save that person $500. However, if he incurred qualified expenses of $6000 and he qualified for the maximum deduction, he would only be able to deduct expenses up to $4000 and the deduction would then be worth $1000. Obviously, the value of the tuition deduction is higher if the tax filer is in a higher tax bracket, and is lower if she is in a lower tax bracket. The amount that a person can deduct is dependent on both his income and filing status; Table 2.1 shows the tuition deduction phase-out structure for 2008 (MAGI is modified adjusted gross income).[4]

Qualified expenses are defined in the same way as they are for the tax credits, and books or fees not required for attendance are not included as qualified expenses. As with the tax credits, grants and scholarships reduce the amount of expenses incurred by the student and therefore could decrease the amount of the deduction available.

The tuition deduction is aimed at those who earn too much to qualify for a credit. Those who earn lower incomes may not have sufficient liability to claim a tax credit, or they may qualify for enough need-based financial aid that they do not have enough qualified expenses for either a deduction or a credit. At lower tax brackets, the 20% credit from the LLTC is a better option than the tuition deduction, both because of the higher percentage discount and because the LLTC has a higher maximum value.

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[4] From irs.gov publication 970 for 2008

Table 2.1: Tuition Deduction

IF your filing status is...	AND your MAGI is...	THEN your maximum tuition and fees deduction is...
single, head of household, or qualifying widow(er)	not more than $65,000	$4,000.
	more than $65,000 but not more than $80,000	$2,000.
	more than $80,000	$0.
married filing joint return	not more than $130,000	$4,000.
	more than $130,000 but not more than $160,000	$2,000.
	more than $160,000	$0.

However, as shown in the table below the income cutoffs are higher for the tuition deduction than for the tax credits. Therefore, those who may earn too much to qualify for the tax credits may be able to benefit from the tuition deduction.

Table 2.2 below shows the maximum adjusted gross income that a filer could have earned and qualified for the different tax incentives for all of the years that will be included in the empirical analysis.

Table 2.2: Income Limits for Eligibility of Each Benefit

	Year	Cutoff for Singles	Cutoff for Married Filing Jointly
Hope Credit	1999	$50,000	$100,000
LLC	1999	$50,000	$100,000
Hope Credit	2001	$50,000	$100,000
LLC	2001	$50,000	$100,000
Hope Credit	2003	$51,000	103,000
LLC	2003	$51,000	$103,000
Tuition Deduction	2003	$65,000	$130,000
Hope Credit	2005	$53,000	$107,000
LLC	2005	$53,000	$107,000
Tuition Deduction	2005	$80,000	$160,000

2.2.4: Eligibility for Tax Benefits

All these tax incentives can be claimed either by the student himself if he incurs the expenses, the spouse of the student, or by the person who pays the education expenses if he claims the student as a dependent for the tax year. A dependent child must be a child, foster child, stepchild, sibling, stepsibling or descendant of one of these. The child must be under age 19 by the end of the year, or be under the age of 24 at the end of the year if he is a full time student. Of course, the tax filer must also have provided at least half of the support for the child for the year. There are other tests to qualify as a dependent, but these are the most relevant criteria for the purposes of this study.

As a tax filer's income approaches the limits given above, the amount of the credit is phased out in a linear way. For example, in 2005 the maximum MAGI for those who are single (married filing jointly) is $53,000 ($107,000) to qualify for the credits.

However, if the filer's income is between $43,000 and $53,000 ($87,000 and $107,000) he must take the credit he would be eligible for and multiply it by the fraction (53,000-MAGI)/10,000 (for those married filing jointly, the fraction is (107,000-MAGI)/20,000). The same rules apply for both the HTC and the LLTC, with similar rules for the last $10,000 ($20,000) in the earlier years. The phase out for the tuition deduction is different, and is shown in Table 2.1. In 2003 there was no phase out of the tuition deduction, but the maximum amount of the deduction was only $3000 and the income limits were lower.

If a particular tax return has more than one eligible student, then that could affect which tax benefit(s) are best to use. As stated above, the maximum amount of the credits are $1800 and $2000 for the HTC and LLTC respectively, and the value of the tuition deduction is the marginal tax rate as a percentage of $4000. However, the maximum amount of the HTC is $1800 per student, while the maximum for the LLTC is per return. Therefore, it could be beneficial for a filer with two eligible students to use the LLTC for one and the tuition deduction for the other, or some other combination of the credits and deduction. A filer cannot claim more than one credit for each eligible student, nor can she claim both a tuition deduction and a tax credit for a single student.

2.2.5: Data

The data used are from the Panel Study of Income Dynamics (PSID). The PSID is a longitudinal dataset which began sampling households in 1968. A unique and useful feature of the PSID is that it follows children of the original 1968 households as they grow up and leave the household to form their own families. This is extremely useful when studying young adults who have not completely separated from their parents' household. Individuals are added to the sample by forming new family connections with those who are counted in the sample already. Originally, there were two samples: a cross-sectional national sample and a national sample of low-income families from the Survey of Economic Opportunity (SEO). In 1997 there was a reduction in the number of people from the original SEO sample and an introduction of immigrants who moved to the United States after 1968 to make the sample more representative of the country today. Although the PSID asks a large numbers of questions, for the purposes of this study only a fraction are relevant. The incomes of the head and wife (or cohabiter), the incomes of other individuals in the family unit, mortgage interest, rent, family composition, state of residence, cost of child care and enrollment status are all essential to this analysis. These are the variables that helped estimate an individual's tax liability and marginal tax rate which then helped to determine how much of a tax benefit the individual is eligible for. Demographic characteristics and characteristics of the state of residence are also included in the empirical model.

The sample is restricted to those ages 19-30 in order to focus on those most likely to be affected by the tax change. (Although Seftor and Turner, 2002, found that older students are quite responsive to aid as well.) The average age in the sample is 24.44, although the average age for students is only 20.63. Part time students are a little older on average than full time students. The average family income is larger for students who choose to go to school full time rather than part time. This could be because people who have already separated from their parents' households are more likely to go to school part time than full time, while full time students have not yet separated from their parents' households. This probably explains the differences in eligibility between the two groups. The high percentage of women in the sample is a little puzzling; however, since each year is counted separately, it is possible that women have lower rates of attrition. As expected, most of the students do not have a college degree yet although more of the full time students than part time students have completed a degree already. Table 2.3 shows some summary statistics of the sample.

Table 2.3: Summary Statistics

	Full Sample	Students	Full Time Students	Part Time Students
Observations	12926	1987	1574	343
		.1537	.7921	.1726
Eligible for Tax Benefits	3874	659	597	47
	.2997	.3317	.3793	.1370
Female	7709	1094	849	191
	.5963	.5506	.5394	.5569
Already have a College Degree	1458	94	74	12
	.1128	.0473	.0470	.0350
Average Family Income (if positive)	39101	40206	41411	36225
Std Deviation	34756	36788	36297	39476
Number earning Positive Income	10019	1522	1211	261
	.7751	.7660	.7694	.7609
Average Age	24.44	20.63	20.34	21.43
Std Deviation	3.6	2.2	1.9	2.57

The percentages of full and part time students are given as a fraction of students, not the full sample. The PSID had two questions that determined an individual's enrollment status. The first asked what the individual's employment status was. If the individual was listed as a student, he was counted as enrolled. However, there was a later question that asked whether the individual was a full time student, part time student or neither. If the individual indicated on this portion of the questionnaire that he was either a full time or part time student, I also counted him as enrolled.

2.3: THEORY AND HYPOTHESES

I intend to look at the enrollment effects of the tax benefits. In order to do this, we must consider how the budget constraints for individuals are affected by the different tax treatments. Figures 2.1 and 2.2 show the changes in the budget constraint for an individual purchasing education and consumption goods for the Hope Tax Credit and the Lifetime Learning Tax Credit, respectively. The maximum amounts are denoted with a bar over the variable, E represents education, and C represents consumption.

Figure 2.1: Hope Tax Credit

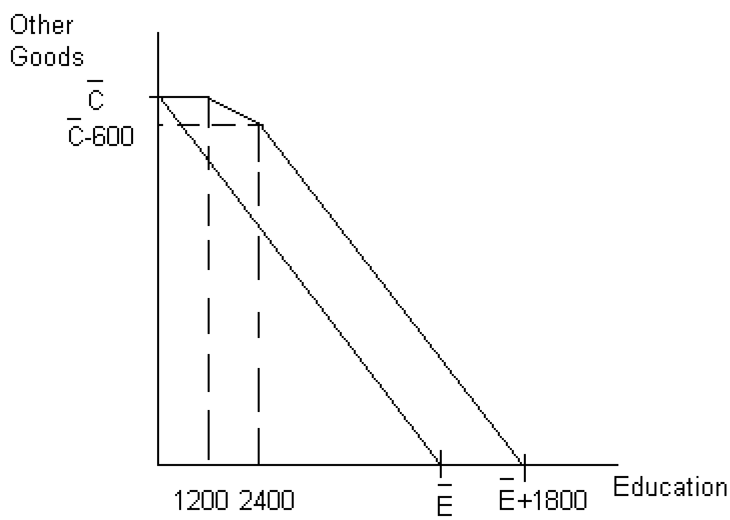
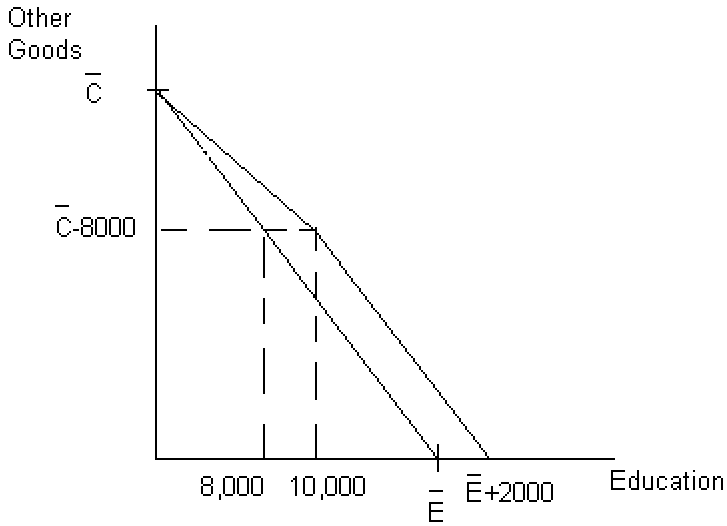


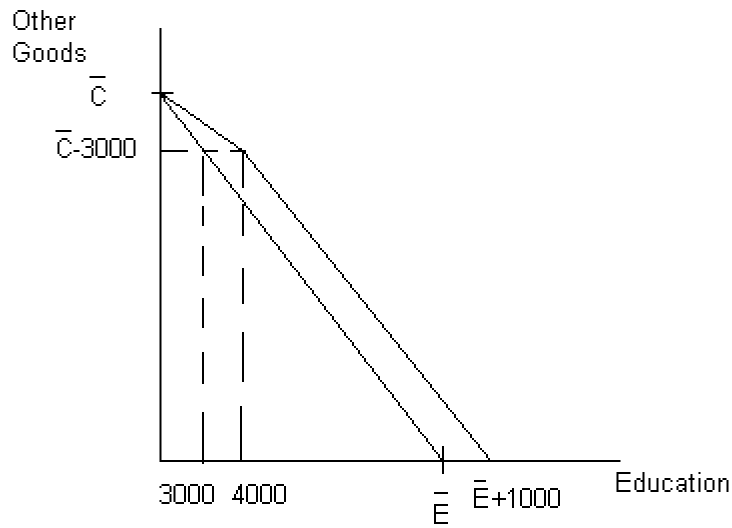
Figure 2.2: Lifetime Learning Tax Credit



Both of these credits essentially change the slope of the budget constraint until the maximum credit is reached, after which there is a parallel shift outward. The change is not so straightforward for someone claiming the deduction, because the slope of the budget constraint will depend on the marginal tax rate. However, if we assume that an individual is in the 25% tax bracket, we will see the budget constraint shift as illustrated in Figure 2.3. All of these figures were drawn with the assumption that the individual we are considering is eligible for the full amount of the credit or deduction.

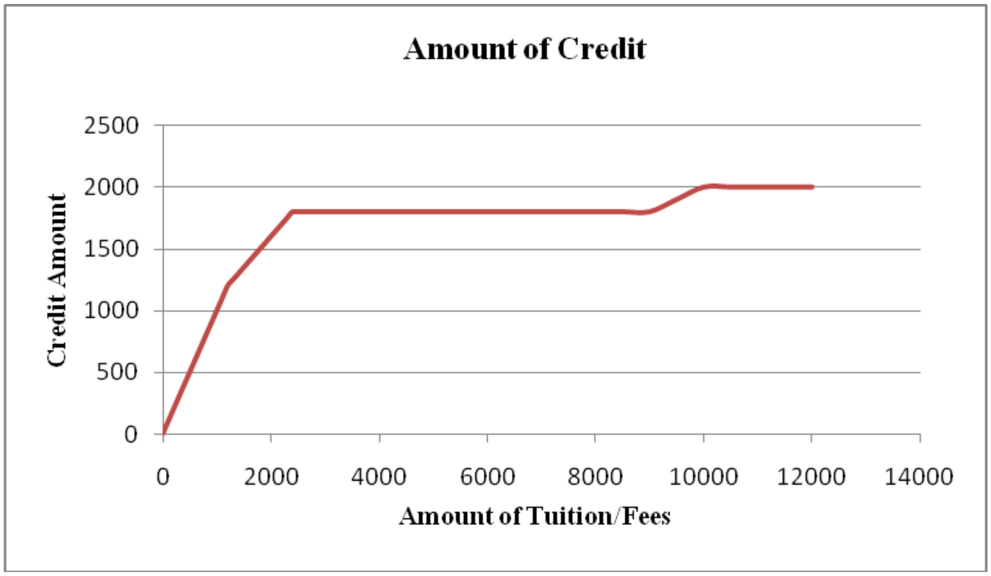
The basic idea of the theory in this model is quite simple; as we reduce the cost of investing in a college education relative to other goods, the probability of enrollment should increase for those making the decision of whether or not to attend college (the extensive margin). For those already deciding to attend college, the tax incentives could reduce the marginal cost of attendance so students may decide to attend a more expensive college, enroll in more courses, or attend longer (intensive margin). Singell (2004) did find an effect of aid on retention, although he looked at a particular university. Another

Figure 2.3: Tuition Deduction



possible consequence of the tax benefits is they could cause more parents to claim a child as a dependent; however, that is only possible among the parents of students who enroll full time. Some have suggested that the students being reached by the tax policy are not those on the margin of deciding whether or not to attend. However, Dynarski (2000) suggests that aid targeted at the middle class has significant effects on enrollment decisions. It is also suggested (Dynarski and Scott-Clayton, 2006) that delivering aid through the tax system may help alleviate the burden of unnecessary paperwork. I will assume that the students (or their parents, if the student is a dependent) will be choosing the optimal tax benefit. The following figure, Figure 2.4, shows the amount of the credit that can be claimed for different amounts of tuition for a student that qualifies for both the HTC and the LLTC but falls in the 15% tax bracket for 2008. The Hope credit is the more beneficial of the two credits for all levels of tuition below \$9000. Although the tuition deduction was not included in the above calculations, if the student qualifies for the Hope Credit that will always be worth more than the tuition deduction. The maximum amount of the deduction is only \$4000, which would mean the tax filer would

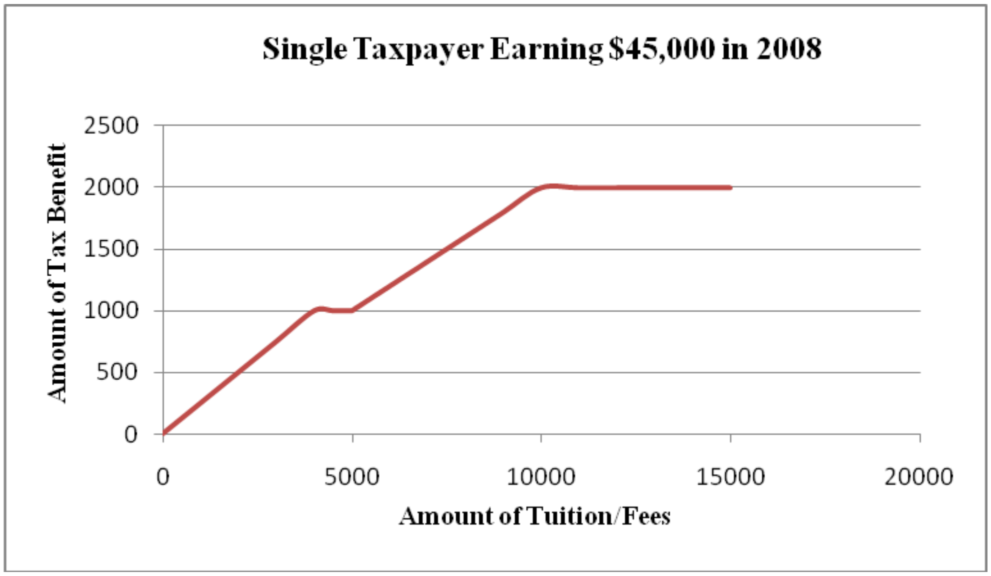
Figure 2.4: Amount of Credit for Tuition/Fees Paid



have to be in a marginal tax bracket above 40% for the deduction to be better than the Hope credit. In 2008 this does not apply at all, but these individuals would be earning too much income to qualify for the credits anyhow.

The criteria for qualifying for the HTC are somewhat stringent, so Figure 2.5 shows the amount of the tax benefit for different tuition levels for a single taxpayer earning $45,000 of Adjusted Gross Income in 2008 who does not qualify for the Hope Credit. This earner would be in the 25% tax bracket and would then prefer the tuition deduction at some levels, and the LLTC at other tuition levels. At tuition levels up to $5000 the student would be better off with the tuition deduction, but at levels above $5000 the LLTC is better because the maximum credit is higher. This is dependent on the marginal tax rate. When comparing the tuition deduction to the Lifetime Learning Credit, the only relevant tax rates in 2008 are the 25% and the 28% brackets since there would be no reason to claim the tuition deduction for a single student if the marginal tax rate is less than 20%.[5]

Figure 2.5: Amount of Tax Benefit for Tuition/Fees Paid



Higher marginal rates are not relevant because individuals in those tax brackets have too much income to qualify for the higher education tax benefits. Exact AGI is also important to determine if the student is able to claim the entire 20% tax credit or if they are in the range of incomes where the credit is phased out.

Regardless of the tax benefit chosen, this shows that the marginal decision of whether or not to attend college, the intensity and how much to spend should be affected by the tax benefits. The PSID does not have information about how much the students actually spent on tuition and other qualified expenses so the only measure of changes on the intensive margin available was whether the student enrolled full or part time. There is variability across states in the list price of tuition which has been exploited by Kane

[5] This may not be true if there are multiple students on the same tax return, since the Lifetime Learning Tax Credit has a maximum of $2000 per return. However, I only consider a single student in this analysis. This is also assuming that the student is eligible for the full amount of the credit; the phase-out at higher incomes may also alter this conclusion.

(1995) among others to try to estimate students' responsiveness to tuition price. This study uses the average public tuition levels in state of residence to calculate the amount of the credit a student would expect. It is likely that when looking at the extensive margin, students deciding whether or not to enroll in higher education would probably be facing the tuition level at the nearest public university. (See Chapman 1981 and Ehrenberg, 2004 among others.) Therefore, when calculating how much the credit is worth to a potential student the average public school tuition in the state of residence is used as the amount paid.

2.4: RESULTS WITH A BINARY INDICATOR FOR ELIGIBILITY

To test how much of the change in enrollment we can attribute to the availability of the tax benefits, a difference in differences model is used. The difference in differences will test if there is an effect on enrollment after the implementation of the tax credits in the eligible group. The control group both before and after the change in tax policy will be those students who are not eligible, either because they (or their families) earn too much to qualify or earn too little that they will probably not have sufficient liability. As mentioned above, the education tax credits are not refundable and those with low incomes are likely to qualify for Pell grants and other need based financial aid which will disqualify them from claiming the credits or the deduction. The difference in differences approach will then be of the following form:

$$P(\text{Enroll}=1|X_i)=\Lambda[\delta(\text{Eligible}_i*\text{After}) +\alpha_1\text{Eligible}_i+\alpha_2*\text{After}+X_i\beta],$$

$$\text{where } \Lambda(X_i\beta)=\frac{e^{x_i\beta}}{1+e^{x_i\beta}}.$$

Enroll is a variable which is equal to one when the observed person enrolls in school, eligible is an indicator variable which equals one if that person is eligible for a tax credit or tuition deduction, and after is an indicator variable equal to one after 1997 when the credits are in effect. As usual, X is a vector of characteristics likely to affect enrollment other than the tax treatment. The control group is the sample of people who are ineligible for the credit while the treatment group is those whose incomes are such that they are eligible. The treatment is then whether the observation is before or after the tax changes took place in 1998. The tax credits were announced in 1997 and implemented in 1998, so it is unlikely that a large number of people anticipated the tax treatment change and shifted their enrollment from before the change to after, especially because the school year does not line up with the tax year. Most people will have made their schooling decisions for the year 1997 before the tax change was announced, but they would have time to make a decision about schooling for 1998 after the tax change was announced. If people are reacting to the tax change in the expected manner, we would expect to see that δ is positive.

Eligibility for the tax credit was determined by the individual's adjusted gross income (AGI) as estimated using TAXSIM. Those with incomes under $30,000 were counted as ineligible because they were likely to receive other grants (See Long, 2003 and Kane, 1999) or have insufficient tax liability. For those individuals who earned over $30,000 Table 2.2 shows the income limits for being able to receive tax benefits for all years of these data.

It is assumed that those who were married were filing jointly. For any (likely) dependent tax-payers[6], the PSID's Family Identification and Mapping System (FIMS)

[6] It has been suggested that using likely dependents instead of actual dependents may create a problem. This is unavoidable for several reasons. First, actual tax dependency status is unobserved. Second, in order to claim an adult child as a dependent for tax purposes they must be enrolled as a full time student, so there are simultaneous decisions being made. However, one potential result of the tax benefits is that they may

mapped the dependent to his or her parents and then used the parents' eligibility status. When data for parents were not available, income of the head and spouse (if applicable) was used to determine eligibility.[7] For most there is little difference between reported income and MAGI. To determine the eligible group in the years before 1998, the (CPI-adjusted) cutoff levels for the tax credits in 1999 were applied. Then the rules for phasing out the credit were applied to determine what percentage of the credit the individual would be eligible for if they were only eligible for part of it. For the indicator variable on eligibility, individuals were perceived as eligible if they were eligible to receive any amount of the credit.

As mentioned earlier, information on whether or not a student is full time or part time is also available. The probability of enrolling full time given that the student enrolls at all is estimated in the same way as the probability of enrolling. Specifically, I look at

$$P(\text{Enroll FT}_i=1|\text{Enroll}=1,X_i)=\Lambda[\delta(\text{Eligible}_i{*}\text{After}) +\alpha_1\text{Eligible}_i+\alpha_2{*}\text{After}+X_i\beta].$$

In this case, a positive value for δ will indicate that those who choose to enroll in college will choose to enroll full time more often as a result of the tax policy. However, if we see that the probability of enrollment is higher after the tax change, we may not see a positive value for δ in this specification even if students are changing their status. This is because if more people are attending college as a result of the tax change, the new students may be enrolling part time more often than the students who would have

encourage parents to support their children longer if they continue in their education. Finally, according to NCES "Part-Time Undergraduates in Postsecondary Education: 2004-2004" 65.3% of full time students are dependents and 72.4% are under the age of 24. Since there are very few circumstances when a student over 24 would be considered a dependent, roughly 90% of those who are under 24 and attend full time are dependents. Once we account for those who are married or with children of their own, almost all of the likely dependents would be dependents given they attended school full time.

[7] This adjustment was made because there was systematic missing data for those most likely to be students which would lead to biased estimates of the most important parameters being estimated.

enrolled anyway. We could then see a change in the overall composition of full and part time students. Therefore, if the overall effect on enrollment is stronger than the effect on the intensive margin we could see δ being estimated as zero or even negative when there is an effect on the intensive margin. However, if δ is positive that would indicate that there is an effect on the intensive margin, though it may be underestimated.

The coefficient vector β is estimated using maximum likelihood estimation. Since this is a nonlinear model, the marginal effects will no longer simply be the value of the coefficient. We would still expect to see that δ is positive; however, a positive value for δ is not sufficient to show that the probability of enrollment is larger as a result of the tax change. A further discussion is presented in Appendix 1A. Columns (1) and (2) of the Table 2.4 show the results from these most basic models. Throughout, the percentage of people in the state with bachelor's degrees, the state unemployment rate and the state income per capita are grouped together as the state control variables are grouped together as State Controls. Race/Gender/Family controls include indicator variables for race, gender and marital status as well as the number of children the individual has. All pooled specifications use standard errors clustered by the individual[8].

As expected, the coefficient on the interaction term is positive and significant in the enrollment regression. This indicates that there is an increase in the probability of enrollment in the eligible group due to the tax change; however the marginal effects will be calculated at the end of this section. Most of the other coefficients have the expected signs. Younger individuals were more likely to enroll. Those who are ineligible because they earn too little are less likely to enroll overall; however, the data show that they are more likely to enroll full time than part time when they do enroll after controlling for income and other variables. This may be because the opportunities available to those

[8] However, when clustering by state we find that the standard errors are very similar.

who come from low income households outside of college may be much less appealing than those who come from higher income households. Therefore, they may choose to go to college with a higher intensity.

Table 2.4: Results with Eligibility Indicator

	(1) Enrolled	(2) Enrolled Full Time	(3)
VARIABLES	Logit	Logit	Ordered Logit
Eligible/After Interaction	0.558**	-1.202	0.462*
	(0.261)	(1.051)	(0.266)
Eligible for any Tax Benefit	0.653**	3.329***	0.820***
	(0.260)	(1.070)	(0.257)
After 1998	-0.368**	0.0421	-0.347**
	(0.162)	(0.299)	(0.160)
Year	0.0207	0.0238	0.0190
	(0.0242)	(0.0469)	(0.0237)
College Degree Indicator	-0.0192	0.971***	0.0218
	(0.172)	(0.330)	(0.173)
Family Income	7.98e-06***	5.89e-06	6.73e-06***
	(1.56e-06)	(3.64e-06)	(1.30e-06)
Ineligible due to Low Income	0.0626	1.517***	
	(0.120)	(0.361)	
Age Indicator Variables	Yes	Yes	Yes
State Controls	Yes	Yes	Yes
Race/Gender/Family Controls	Yes	Yes	Yes
Constant	-43.11	-45.34	
	(48.10)	(93.52)	
Observations	10417	1814	10387

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Having more children and being married increase the cost of attending school, so the signs of both of these coefficients are negative. The coefficient on being female is positive, which is understandable during this time period. Women enrolled in higher education more often than men during these years. A higher unemployment rate and more people with bachelor's degrees in the individual's state decrease the cost and increase the benefits of college respectively, so these have the expected signs although they are not significant. A higher unemployment rate may decrease the probability of attending full time, although these effects are not statistically significantly different from zero. Average income in the state appears to be insignificant, and those with higher family incomes are more likely to enroll in school.

The effect on full time enrollment, however, is not what we may have expected. The tax policy seems to have a negative effect on the probability of full time enrollment. It is not statistically significant, however. It could be negative because of changes in the composition of students, as discussed above, since we do see an increase in overall enrollment. It could also be that there are changes on the intensive margin in ways not measured in these data, or it may be that there is no effect on the intensive margin. As for the other variables, gender and marital status have little to do with the decision to enroll full or part time.

To investigate more fully if there is any reaction to the tax credit on the intensive margin, an ordered logit model is estimated. The ordered logit takes advantage of the intensity structure of enrollment; enrolling part time is more intensive than not enrolling at all, and enrolling full time is more intensive than enrolling part time. Unfortunately, it does not allow the effects of the regressors to vary as we move from one alternative to the next. Making the proportional odds assumption implies that the cumulative odds ratio is cumulative across categories, or that the odds of moving to the next category are the same at each step. Since we have seen that full time enrollment among those who enroll does not appear to be increasing, this could give us more insight. If the composition of students is changing then we would be able to see that there is an increase in the probability of moving up in each step, and we would see a positive effect in the ordered logit model. However, we are imposing the restriction that the increase in full and part time enrollment must change in the same way[9]. Results from the ordered logit model are in column (3) of Table 2.4. It does appear that the tax benefits increased the probability that individuals moved from one level to the next. The size of the effect will be discussed at the end of this section.

In order to account for possible correlation with unobservable characteristics of states and the error term, regressions including state fixed effects were also studied. First consider when the dependent variable is an indicator for enrolling. Although the estimate is not very precise, the point estimate from the fixed effects model is similar to the estimate from the initial logit model[10]. However, when the dependent variable is full time enrollment conditional upon enrolling at all, the coefficient is negative and not significantly different from zero as before. These results are presented in Table 2.5.

From the analysis presented, it does appear that there was an effect on enrollment from the tax credits. However, the overall effect is rather small so it is understandable why others may have not seen a significant effect. Without careful evaluation of the eligibility status of the potential students it is quite possible that many individuals could be misclassified leading to estimates with high variance and the effect essentially lost. Although it appears at first glance that there was little or no discernable effect on the intensive margin of enrolling full time versus part time, we may be seeing the effect overshadowed by the overall effect on enrollment. The positive and significant

[9] An alternative specification, the multinomial logit model, does not impose this restriction but does require the IIA assumption. Since there is likely correlation in the error terms in the choice of full or part time enrollment, I chose to present the ordered logit model instead. The multinomial probit is an alternative which can relax the IIA assumption. However, the estimation technique (maximum simulated likelihood) leads to estimates with very large variances and is therefore not very useful. Results are available upon request.

[10] Using state indicator variables instead of state fixed effects yields very similar results. These two approaches are not equivalent in the nonlinear model.

Table 2.5: Eligibility Indicators with State Fixed Effects

	(1) Enrolled	(2) Enrolled Full Time
VARIABLES	Fixed Effects Logit	Fixed Effects Logit
Eligible/After Interaction	0.458*	-1.344
	(0.254)	(1.041)
Eligible for any Tax Benefit	0.712***	3.382***
	(0.247)	(1.042)
After 1998	-0.446***	0.216
	(0.159)	(0.313)
Year	-0.0362	-0.190**
	(0.0465)	(0.0965)
Age	-0.435***	-0.178***
	(0.0167)	(0.0298)
College Degree Indicator	-0.00272	0.839**
	(0.150)	(0.374)
Family Income	7.18e-06***	4.36e-06
	(1.65e-06)	(2.80e-06)
Ineligible due to Low Income	0.0707	1.451***
	(0.118)	(0.268)
State Controls	Yes	Yes
Race/Gender/Family Controls	Yes	Yes
	10364	1780
	45	37

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

coefficient of the ordered logit model supports this, and as shown below the effect on part time students is much smaller but it is positive. In addition, students could be taking more credit hours, spending more time in school or spending more on postsecondary education as a result of the tax credits instead of (or in addition to) changing their status as part time or full time students.

In order to interpret and compare the models, consider the changes in probability of enrollment before and after the tax change. Since we have made the (implicit) assumption that people's earnings behavior is not changing as a result of the tax credit, the table below presents the change in the probability of enrollment of an eligible individual before and after the tax change. The results presented are the average marginal effects from this sample. (Results from the linear probability models are presented in Appendix 1B.) Table 2.6 shows these marginal effects for most of the results presented in this section.[11]

Table 2.6: Effect of Enacting Tax Policy

	Enrollment	Full Time Enrollment
OLS	0.0934***	-0.0302
	(0.012)	(0.037)
Logit	0.0520**	-0.2030
	(0.028)	(0.153)
Ordered Logit[12]	0.0034**	0.0379**
	(0.001)	(0.019)

The results for the fixed effects models are not present. This is because in order to calculate these probabilities, one must make an assumption about the constant term. If we assume it the constant is zero, the resulting probabilities are not at all in line with what we see in the data; therefore this assumption does not seem to fit the data well. The overall effect of the policy appears to be a change in the probability of enrollment by 4.1 to 9.3 percentage points (see Appendix 1B). However, the logit model fits the data much better, so effects implied by these models of 4.1 to 5.2 percentage points seem much more realistic. The linear probability model does predict negative probabilities for some individuals. Although as discussed above it is difficult to interpret the marginal effect of

[11] Below the estimates are the standard errors in parentheses.
[12] Rather than the overall effect on enrollment, the first column shows the increase in part time enrollment in this specification.

the policy on full time enrollment, it does appear that full time enrollment has experienced no change or a slight decline as a result of the tax policy. Since the effect on enrollment overall is larger, it is quite possible that we are seeing a small change in student composition; in other words, this could indicate that people who would not have enrolled without the tax benefit enroll part time more often than the proportion who enrolled part time before the benefits.

There is a possible problem with income (and therefore eligibility) being endogenous. Rather than eligible individuals changing their enrollment behavior, we could see individuals who enroll manipulate their incomes to become part of the eligible group. Because the credits are phased out as income gets close to the cutoff points, this seems rather unlikely at the top end. Also, although there is no formal phase-out for those who earn less, need-based financial aid reduces the qualifying expenses. Therefore, the credits would be worth less to those who do not earn as much, so those people would be less likely to change behavior. With this in mind, we checked for endogeneity in three ways. Those who were eligible for the credit were sorted into income quartiles. In the first test for endogeneity, all of the regressions above were estimated counting only those in the middle two income quartiles as eligible. If people are not changing incomes to be eligible, we should see that the two inner quartiles are the most affected by the tax change, so the coefficient should be larger than what was previously estimated using the whole eligible group. Indeed, this is true for all of the regressions using an enrollment indicator as well as for the ordered logit model. Using the full time enrollment dependent variable the coefficient being estimated is positive but the p-values are very high.

Second, those who were very close to the cutoff points of eligibility were studied. The groups in the first and fourth quartiles were counted as the eligible group and those who were within $5000 of being eligible were counted as the control group. (Again, this meant that both those who earned too much and too little to qualify for the credit are included.) The same regressions as above are estimated using this subset of the data. In the second set of regressions, we expect to find that the coefficient would be much smaller. In the regressions using enrollment as the dependent variable I actually find that it is indistinguishable from zero. There are too little data to estimate the coefficient using the full time enrollment variable in the second case. Coupling these results with the fact that the credits are (likely) worth less on the upper and lower ends of those who are eligible, it seems reasonable to say that eligibility is not endogenous in this way and that people are not marginally changing incomes to qualify for the credit.

In a third test to see if there may be larger effects on income, we restrict the sample to only those students who were likely dependent on their parents. An example of a larger change would be one member of a two income household leaving the workforce in order to qualify for these tax benefits. I used those who classified themselves as a child (or step-child) of the head of household in the sample above. Next, lagged values of labor income for the head and wife are included as predictors of income in the year where the parents of the child would be able to take advantage of the tax benefits. This is to see if there was a change in the earnings trajectory when a child went to college; by regressing income on lagged income and including an interaction variable of whether the child was a student and whether the tax policy was in effect, changes in income due to the tax credit should be evident. There is no significant effect of the tax policy on labor earnings behavior evident in this estimation.[13]

It does appear that there was a change in enrollment behavior that is due to the tax benefits. Individuals appear to be enrolling in higher education more often than they did

[13] This was done only for children of the head because the income of the students themselves would almost certainly change due to time constraints associated with attending school. Results of all testing for endogeneity are available upon request.

before the tax change. How much the students were spending on tuition is not observed. Therefore, the amounts that the tax treatments are worth to individuals is not known, so the amounts that they are likely to claim are estimated. The next section extends the analysis here to estimate how much increasing the amount of the tax benefits change enrollment decisions.

2.5: Results Using percentage Discount

To examine how the magnitude of the discount affected enrollment probabilities, NBER's TAXSIM program is utilized to estimate the likely amount of the tax credit to an individual. The values of the credits are computed assuming the individual pays the average public school tuition in his state during the year. Tuition data were collected from the NCES. (Using public tuition levels could underestimate the effect because many students choose to attend private schools which usually cost substantially more.) If the amount of the credit exceeded his tax liability, then the tax liability was the maximum amount the credit was worth to that individual since the credits are nonrefundable. For the years before the tax credits were implemented, the CPI-adjusted values of the income cutoff rules from 1998 (the first year of implementation) are used. I then took the amount of the credit as a fraction of the total amount of tuition to get the percentage discount the individual was eligible for.

In 2002 the tuition and fees deduction was added as a tax benefit. Similar to the above situation, the amount that the tuition deduction would be worth is estimated by using the individual's marginal tax rate and the average level of public tuition in their state. The rules for the phasing out of the tuition deduction changed again in 2004; however, since this only affected one year of data the rules from 2003 determine eligibility for the years prior to the change. The limit for the lifetime learning credit was also raised in 2003, from $1000 to $2000. I calculate the maximum tax benefit that an individual would be eligible to take as a fraction of tuition under the rules of both regimes. Similar to the analysis in Section 2.4, the coefficients of the following regression are estimated:

$$ENRPROB_i=\Lambda[\delta_1(PctDiscount1_i*After_{1998}) + \alpha_1 PctDiscount1_i + \alpha_2 * After_{1998} + \delta_2(PctDiscount2_i*After_{2002}) + \alpha_3(PctDiscount2_i - PctDiscount1_i) + \alpha_4 * After_{2002}\ X_i\beta + \varepsilon_i]$$

where PctDiscount1 is the percentage discount that the individual could have received under the rules of the first regime and PctDiscount2 is the percentage discount that the individual was eligible to receive under the rules of the second regime. $After_{1998}$ and $After_{2002}$ are indicators of whether the observation is before or after the implementation of the first and second regime, respectively. It is expected that both δ_1 and δ_2 will be positive. However, since the rules under the second regime were more generous (ie, the percentage discount under the second regime's rule is always greater than or equal to that of the first regime) the added effect of the tuition deduction and expanded LLTC benefits is identifiable. The same model is estimated with indicator variables for each year instead of two separate indicators for after the implementation of each regime. As you can see in Table 2.7 by comparing columns (1) and (3) or columns (2) and (4), this does not change the estimates of the coefficients on the interaction variable or the percentage discount variables by a significant amount.

Using enrollment as the dependent variable, both of the interaction terms are positive and significant, which indicates that people do react to the level of the tax credit. Looking at the effects of the discount on full time enrollment, we see that the point

Table 2.7: Logit Model with Percentage Discount

VARIABLES	(1) Enrolled	(2) Enrolled Full Time	(3) Enrolled	(4) Enrolled Full Time
Percent Discount First Regime/After 1998 Interaction	0.0154**	-0.00551	0.0135**	-0.00816
	(0.00668)	(0.0358)	(0.00681)	(0.0365)
Percentage Discount First Regime	0.0206	0.0817	0.0223	0.0842
	(0.0223)	(0.0629)	(0.0228)	(0.0630)
Percent Discount Second Regime/After 2002 Interaction	0.0136**	0.0173	0.0131**	0.0178
	(0.00593)	(0.0168)	(0.00587)	(0.0169)
Percentage Discount under 2nd regime	-0.0120	-0.0305	-0.0118	-0.0308
	(0.0217)	(0.0526)	(0.0222)	(0.0526)
After 1998	-0.684***	-0.155		
	(0.178)	(0.334)		
After 2002	-0.576***	-0.232		
	(0.166)	(0.345)		
College Degree Indicator	0.0986	1.015***	0.107	1.016***
	(0.167)	(0.304)	(0.167)	(0.304)
Family Income	8.36e-06***	5.89e-06	8.91e-06***	5.99e-06
	(1.58e-06)	(4.41e-06)	(1.61e-06)	(4.50e-06)
Ineligible due to Low Income	-0.0738	1.012***	-0.0953	1.010***
	(0.119)	(0.359)	(0.120)	(0.361)
Year	0.114***	0.0602		
	(0.0337)	(0.0674)		
Year Indicators	No	No	Yes	Yes
Age Indicators	Yes	Yes	Yes	Yes
State Controls	Yes	Yes	Yes	Yes
Race/Gender/Family Controls	Yes	Yes	Yes	Yes
Constant	-229.0***	-117.8	-2.768***	2.275
	(67.15)	(134.7)	(0.573)	(1.514)
Observations	10416	1813	10416	1813

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

estimate is negative, but the effect is not significant. As before, this could indicate that there is little or no change on the intensive margin measured or that as new students are enrolling, they enroll part time more often. The other control variables exhibit similar effects on the probability of enrollment and full time enrollment as they did in the previous section.[14]

To account for unobserved heterogeneity across states, state fixed effects are considered again. As when using the indicator variable for eligibility, this made very little difference in the estimates of the coefficients of interest. Results are presented in columns (1) and (2) of Table 2.8. To take advantage of the intensity structure of enrolling part time over not enrolling at all and enrolling full time over part time, an ordered logit model is estimated. It appears that the probability of increasing intensity of schooling goes up as the percentage discount under the first regime rules increases. However, in the second regime the point estimate of the coefficient on the percentage discount variable is negative and is not statistically significant. It appears as though the expansion had little effect. Again, the marginal effects are easier to interpret, and are presented in Table 2.9. The coefficient estimates are presented in Column (3) of Table 2.8.

[14] What happens when average tuition levels in two year schools are used was also studied. The marginal effects using the enrollment indicator are smaller and the estimates were less precise, but they follow the same patterns presented here. Using full time enrollment I again found no significant effects.

Table 2.8: State Fixed Effects and Ordered Logit

	(1)	(2)	(3)
	Enrolled	Enrolled Full Time	
VARIABLES	Fixed Effects Logit	Fixed Effects Logit	Ordered Logit
Percent Discount First Regime/After 1998 Interaction	0.0156**	-0.00844	0.0141**
	(0.00648)	(0.0227)	(0.00672)
Percentage Discount First Regime	0.0236	0.0959**	0.0324
	(0.0165)	(0.0479)	(0.0219)
After 1998	-0.558***	0.0878	-0.668***
	(0.179)	(0.348)	(0.175)
Percent Discount Second Regime/After 2002 Interaction	0.0130**	0.0183	0.0150**
	(0.00597)	(0.0138)	(0.00590)
Percentage Discount under 2nd regime	-0.0151	-0.0423	-0.0201
	(0.0158)	(0.0436)	(0.0213)
2003 or later	-0.267	-0.178	-0.598***
	(0.188)	(0.382)	(0.163)
Year	-0.0150	-0.203*	0.115***
	(0.0526)	(0.110)	(0.0332)
College Degree Indicator	0.107	0.853**	0.153
	(0.147)	(0.359)	(0.169)
Family Income	7.69e-06***	4.57e-06	8.31e-06***
	(1.68e-06)	(2.95e-06)	(1.30e-06)
Age	-0.439***	-0.185***	
	(0.0166)	(0.0296)	
Ineligible due to Low Income	-0.0393	1.081***	
	(0.117)	(0.259)	
Age Indicators	No	No	Yes
State Controls	Yes	Yes	Yes
Race/Gender/Family Controls	Yes	Yes	Yes
	10363	1779	10386
	45	37	

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

Table 2.9 shows the average marginal effect of a 1 percentage point change in the amount of the tuition discount separately in the first and second regime.

Table 2.9: Marginal Effects of Percentage Discount

	First Regime		Second Regime	
	Enrollment	Full Time Enrollment	Enrollment	Full Time Enrollment
OLS	0.0013***	0.0003	0.0022***	0.0018*
	(0.0003)	(0.0110)	(0.0005)	(0.0011)
Logit	0.0013***	-0.0009	0.0012***	0.0027
	(0.0005)	(0.0044)	(0.0004)	(0.0021)
Logit with Year Indicators	0.0012**	-0.0013	0.0011***	0.0027
	(0.0005)	(0.0045)	(0.0004)	(0.0021)
Ordered Logit[15]	0.0001**	0.0011***	0.0001***	0.0012***
	(0.00005)	(0.00042)	(0.00004)	(0.00037)

Increasing the value of the discount under the rules of the first regime is more effective in increasing enrollment than using the rules of the second regime. The second regime not only increased the amount of the LLTC to those who could already take it, it also added the tuition deduction and thereby increased the pool of eligible individuals. The tuition deduction was aimed at the group of people who earned a little bit too much to qualify for the tax credits. Overall, it appears that increasing the value of the benefit from under the rules of the first regime increases the probability of enrollment by 0.12 to 0.13 percentage points. If we instead increase the benefit in the same way only for those considered eligible by the rules of the second regime, the change in enrollment probability is essentially zero, since we want to look at the difference between the values.

[15] The first and third column show the increase in part time enrollment instead of the overall increase in enrollment in this specification.

The small but positive value of the marginal effects of the tax benefits on part time enrollment and the larger effect on full time enrollment in the ordered logit specifications are encouraging. As discussed in the previous section, it is possible that there is an effect on full time versus part time enrollment that we would not see if the students enrolling due to the tax credit are enrolling part time more often than those who would have enrolled otherwise. We know, however, that if we do see a positive and significant effect on full time enrollment that there is a change along the intensive margin, and that it is likely to be underestimated. What we see here indicates that there may be a measurable increase in full time enrollment due to the tax credits, although the magnitude is difficult to estimate.

2.6: CONCLUSION

Although previous literature has found no significant effect of the education tax benefits on enrollment in higher education, this study finds that there is a behavioral response. It is likely that previous studies did not use detailed enough data to pick up on the relatively small effects. The PSID is very well suited for analysis of a question such as this one because of its detailed information on income as well as its structure of following children as they split off from their parents' households to form their own. Since college students are largely young adults and are likely to be considered dependents for tax purposes, it is very important that we consider this link when we think about the effects of tax policy on their behavior.

Overall the estimates of the increase in enrollment probability appear to be in line with what previous literature has found for price responsiveness to college tuition. The results indicate that overall, a decrease in the price of college by 1% would lead to an increase in the probability of enrollment by 0.12 to 0.13 percentage points and overall the tax credits have increase the enrollment probability by 4.1 to 5.1 percentage points for those eligible to receive them.

This chapter opens the door for future research in studying federally subsidizing higher education. Important questions to study in the future are the effect of tax policies on graduation rates and time to degree completion. If the overall goal of the policy is to increase the average amount of human capital in the population, all of these outcomes are important in overall policy evaluation.

In the results presented, there is very little effect of the tax credit on the intensive margin. Although it is possible that the entire effect is on the extensive margin, it is more likely that there are effects taking place on the intensive margin that are not measured in these data. It is possible that the pool of students changes as the number of individuals enrolling changes. The only measure available for intensity of enrollment was whether the individual enrolled full time or part time. However, the PSID is primarily concerned with the incomes of individuals and families so there is limited information about students. Perhaps there would be a measurable effect if there were more students in the dataset. It is also possible that there are reactions on the intensive margin in other ways. For example, students may be attending more expensive schools or staying in school longer. These effects are not easy to measure with these data, so I leave that to future work.

CHAPTER 3

Taking Time Off: The Effects of Gaps in Schooling

3.1: INTRODUCTION

The path that individuals follow in accumulating human capital and the effects of this path on outcomes is very important to labor economists. Young adults today are taking breaks in their schooling more than in the past (see U.S. Department of Education, Jacobs and Stoner-Eby), and non-traditional students (undergraduates over age twenty-five) are more common than ever. However, there is only a small literature looking at schooling gaps and how they impact outcomes for individuals who choose to take time off. This chapter uses data on law school alumni to evaluate the effects of taking time off between undergraduate and graduate enrollment. This is in the same vein as the prior literature on the topic, although the selection problem is likely very different. In looking at students who end up graduating from law school, it is much less likely that these students are taking breaks because of uncertainty in their ability to graduate. This chapter also expands on the prior literature by analyzing competing effects of the time off on both educational and labor market outcomes, as well as using information on the activity undertaken during the schooling gap.

Light (1995) looks at the effects of taking time off for undergraduates in the National Longitudinal Survey of Youth (NLSY) 1979 cohort, and finds that after graduating the students who took time off do not immediately jump to the age-earnings profile of their peers who did not take time off. However, she does find that they "catch up" rather quickly to others in their age group. Monks (1997) expands on the Light paper by using a different methodology and more inclusive sample, but generally confirms her findings. However, Richardson (1995) found that older students tend to have a better approach to learning, so the issue is worth studying more closely. We also hear anecdotally that older students tend to be more serious. Holmlund, Liu and Skans (2008) studied the "gap year" that is increasing in popularity in Europe between high school and college, and found associated wage penalties. They were unsuccessful in controlling for selection, however. This study finds that controlling for selection is quite important.

The purpose of this study is to evaluate the effects of taking time off during schooling, and to focus on the separate effects of depreciating human capital and increasing maturity during the gap period. A frequent difficulty of older students is that they have trouble remembering things they once knew, even if they were good students before they took time off. However, I postulate that this is not a problem for those who were using their knowledge in a field related to their career field during their time away from school. In the data used for this study, both the length of the schooling gap and the occupation during that time off are known for the individuals. We are therefore able to distinguish the effects of taking a year off from the effects of investing in human capital between schooling spells separately, on both wages and grade point average.

The contribution of this study is twofold. It is the first to try to separate the effects of human capital depreciation and investment during schooling gaps. It also builds on the literature that studies time off in schooling. The data are from the University of Michigan Law School Alumni Survey and are very rich in information about students at the time of law school entry and during their time in law school. It follows these students five, fifteen, twenty five, thirty five and forty five years after graduation. This allows me to look at the effects of time off at different points in their lives, both during law school and during their working life.

The results indicate that those who take time off tend to have higher grades, although when adjusting for endogeneity the effect only remains for those who worked in related fields. Those who have a gap in their schooling have a rather small but persistent wage penalty, although working in a related field may lessen or eliminate this effect. This finding holds up when considering that the amount of time off from school is not exogenous, but the estimated magnitude of the wage penalty is larger when including this consideration. This does not make any judgment about the decision to take time off being optimal or not. When compared to others with the same education levels who are the same age, these students may do worse on average in overall lifetime earnings; they could still be making the optimal decisions given their information at the time of the decisions and their own time horizons.

The rest of this chapter is organized as follows: Section 3.2 discusses the theory associated with time off during schooling. Section 3.3 describes the data in detail. Section 3.4.1 presents the initial estimates, while Section 3.4.2 goes into more detail and presents the results using an instrumental variables approach. Section 3.5 concludes.

3.2: THEORY

The theory in this chapter is based on the standard theory presented by Becker (1967) and Mincer and Polachek (1974). We know that in order to maximize the number of years that one receives a return to his educational investment, it is optimal to complete schooling in a single spell earlier in life rather than later. However, we also know that many people do choose to take time off during school for many reasons such as family commitments, military or other public service, travel, or to work. They may choose to work in order to try out a field and see if they want to continue in a career path, or they may just try to take some time to try to discover their preferences. (See Holmlund, Liu and Skans for a discussion.) Mincer and Polacheck (1974) looked at the returns to human capital (and especially experience) for women, who are much more likely to have gaps in their labor supply. In their model, they begin with the following equation:

$$E_t = E_{t-1} + r_{t-1}C_{t-1} - \delta_t E_{t-1}.$$

E_t is earnings at time t, r_{t-1} is the return to human capital investment in the previous period (C_{t-1}) and δ is depreciation. We can separate schooling and non-schooling investment, so by recursion and by applying the logarithmic approximation that $ln(1+rk^*) \approx rk^*$ we find that the equation simplifies to

$$\ln E_t = lnE_0 + r_s s + \sum_{j=0}^{t-1}(rk_j^* \quad - \delta_j)$$

where k^* is equal to C_t/E_t and r_s is the return to schooling, s. We can think of k as net investment and k^* as gross investment, so $rk_t = rk_r^* - \delta_t$.

As mentioned in the introduction, the data that used for this study are from the University of Michigan Law School Alumni Survey. Therefore, the group of individuals studied will have the same amount of schooling and it will essentially be of the same quality, so heterogeneity in this dimension is overlooked. The data will be described in more detail in the next section. Some assumptions are made at this point about individuals who choose to interrupt their schooling by taking time off between undergraduate and law school studies in terms of whether or not they were investing in human capital during their time off. They are differentiated based on whether the individual is working in a field related to law during this time off. If they are working in a field that is related to what becomes their career, I assume that they are investing in human capital that will yield a return as reflected in future earnings. If they are working in an unrelated field or doing something other than working during that gap period, I assume that they are not investing in their human capital. Therefore, for the variables defined above we will assume that $k^* = 0$ for those not investing in human capital during the gap. A further assumption is that $\delta_t = \delta \; \forall t$ and that $k^* = k \; \forall t$ for those who work in a field related to law. These assumptions allow us to write the following regression

$$Y_i = \beta_0 + \beta_1 t + \beta_2 * related + \beta_3 * (t * related) + X\beta + \varepsilon.$$

Let *Y* be the log of earnings or wages, *t* be the number of years that the individual took off between undergraduate and law school studies and let *related* be an indicator variable equal to one if the individual worked in a field related to law. Then the coefficient β_1 should be the effect of depreciation for each year taken off during the gap, and β_3 should be the per year return on investment during that time off if they worked in a related field, or *rk** in the language used above. We would therefore expect the signs of β_1 and β_3 to be negative and positive, respectively.

There are several key assumptions that we have to make for this result. First, we assume that there is no investment going on during the time off among those who are not working in a related field. While it is likely that these individuals are not building skills that are particularly related to their career, they may be building good work habits or other similar general skills. However, there is no reason to believe that these kinds of skills could not be obtained during their undergraduate education, so this assumption is not likely to pose a large problem. If it were violated, the estimate of depreciation would be understated (ie, it would be biased towards zero). Another key assumption is that after law school all of the individuals invest and enjoy returns the same on average; i.e., after graduating, those who took time off and those who didn't behave the same on average, as well as those who worked in fields related to law and those who didn't. This assumption will be explored further in the next section.

In addition to looking at the effects of time off on wages as in the standard theory presented above, there is a second outcome to consider. It would be interesting to know if taking time off affects how well a student does in his future schooling. Success in coursework can lead to scholarships and other types of financial aid as well as being an important factor that employers seek when hiring these individuals. It is also a measure of "realized academic potential" (DesJardines, Ahlberg and McCall, 2002) as opposed to expected academic potential, which is important to those who make admissions decisions. Schooling outcomes are studied in much the same way as above, assuming Law School grade point average (GPA) is the outcome variable. However, although these individuals may be working in fields related to law during their time off, this may or may not have the same kind of effect on GPA as earnings. Since the individuals are, by definition, not attending law school during the gap, we could think that none of the individuals were investing in schooling human capital during their time off[16]. Alternatively, we could think of working in a related field as building general knowledge about law, and therefore investment would be positive for those working in related fields. In contrast to the discussion above, there is no reason to think that δ is necessarily negative in this context. As individuals grow and mature, their ability to learn might appreciate or depreciate. For this reason, the following two regressions are estimated where the dependent variable is GPA and the other variables are defined in the same way as above:

$$GPA = \beta_0 + \beta_1 t + X\beta + \varepsilon$$

$$GPA = \beta_0 + \beta_1 t + \beta_2 * related + \beta_3 * (t * related) + X\beta + \varepsilon.$$

In the first, we will be able to see the overall effect of taking time off, regardless of the activity during the schooling gap. In the second, we will be able to see if what the individual did during the gap years has significant effects on GPA. If β_1 is positive, we may think that there is an appreciation in the ability to learn as individuals mature. If

[16] We could consider graduate work in other fields as related or not because it is schooling but by definition it is not in the field of law. For this study, it is considered unrelated.

occupation during the gap years matters, this would support that there is accumulation of knowledge related to law which can help improve GPA.

3.3: DATA

The data are from the University of Michigan Law School Alumni Survey (UMLSAS). The earliest data compiled are from the class of 1952, and the most recent cohort is the class of 2001. The University annually mailed out surveys for many years surveying students who had graduated five, fifteen, twenty five, thirty five and forty five years prior. The project did not start out with quite as large a scope as it ended with, however, so not every survey is available for every year. In particular, those who graduated between 1968 and 2001 were followed up after 5 years, those who graduated between 1952 and 1991 were followed up after 15 years, those who graduated between 1972 and 1981 were surveyed after 25 years, those who graduated between 1962 and 1971 were surveyed after 35 years, and finally those who graduated between 1952 and 1961 were surveyed after 45 years. The survey was discontinued after 2006.

As mentioned briefly, an advantage of these data is the homogeneity of the population. Although that means that generalization of these estimates should be made with caution, it also means that there are fewer problems with selection. It has been suggested that previous literature suffers from large selection issues because those who take time off from schooling may do so because they are uncertain about the probability of success, which implies that they are weaker students to begin with. This argument seems much less likely in the context of a selective law school. Although it is possible that some students take time off because they were unable to gain admission to a selective law school in a given year, it seems more likely that their decisions were based on other factors since they were all admitted to UM at some point. Not only are the students who took time off more likely to be similar to those who did not because they all attended the same quality law school, but they also chose the same field and are doing very similar work. This degree of homogeneity makes it easier to exploit differences such as the timing of education.

Although we might expect there to be problems with responses for a survey such as this one, the response rates are actually quite high and range from about 55% to 70%. Response rates are lower for the most recent surveys. The administrative data are included even for those who do not return the survey, so there is some information on all individuals in the population. Questions ranged from family situation to which courses were useful in preparing for their careers. Students who did better in law school were more likely to respond; however, if there is a worry that this sample selection would bias the results, we would have to assume that those who had a gap in schooling were more likely to do poorly and less likely to respond. This could bias the results upwards, since we would be more likely to observe those who did better than average among those who had a schooling gap. However, in analyzing the response rates in each year there is no evidence that this occurs; the response rates are very similar in the two groups. We also find that those who took time off tend to perform better, not worse, in their coursework in law school.

Due to changes in the questions that were asked and the way the collection was handled, the sample is restricted to those individuals who graduated in 1980 or later. The most important reason for this was problems with the variable that calculates time between graduating college and law school matriculation. For several individuals finishing law school before 1980, this variable was negative. Since the law school has required a Bachelor's degree for many years, and because some of the earlier data seemed to have large amounts of measurement error, I chose to restrict my attention to the later waves. As a result, only the five and fifteen year follow up surveys are available.

The following table shows a little bit about what these students were like prior to and during law school.

Table 3.1: Before and During Law School[17]

	Full Sample	Took Time Off	Didn't Take Time Off
Number of Observations[18] (5 yr sample)	8238	3377	3605
Survey Response Rate*	.6437	.6411	.6621
Number of Observations (15 yr sample)	4511	2115	2372
Survey Response Rate	.5847	.5754	.5902
Number of Children Pre-LS***	.0646	.1059	.0210
Number of Children Upon Leaving LS***	.1044	.1720	.0374
Married Pre-LS***	.1221	.1915	.0553
Married Upon Leaving LS***	.2316	.2996	.1717
Went to UM for UG*	.2007	.2004	.2277
Other Public Sch in MI for UG*	.0891	.0967	.0840
Other Public Sch for UG	.2263	.2196	.2291
Ivy League/7 Sisters**	.1281	.1387	.1146
Other Private for UG	.3458	.3373	.3445
Average Gap in Schooling (Years)	1.409	2.958	N/A
Took Time Off	.4686	N/A	N/A
Change in Career Plans during LS**	.4275	.4102	.4476
Law Review / Journal***	.13.92	.1238	.1676
Avg Standardized LSAT Score	-.00022	.00537	-.01554

[17] The appropriate variables are labeled with a *, ** or *** in Table 3.1 and 3.3 to indicate that the means are different between the two groups at the 10%, 5% and 1% significance levels.
[18] This is the number of observations from the administrative data on all students, because there is at least partial information available for all students. There is more information for those who returned follow up surveys, of course.

The average standardized LSAT score deserves some explanation. In order to protect the privacy of the respondents, the scores were ranked on a percentile scale where zero was high for the year they took the exam. They were then standardized in the typical way. For interpretive purposes, more negative values indicate that an individual did better on the exam relative to their cohort. The distance from zero is the number of standard deviations the individual is away from the mean percentile ranking, not the mean exam score. So although both groups were very similar to the average percentile score (ie, the means are close to zero) those who took time off did slightly worse on average than those who did not take time off. Since the LSAT is one of the variables that we use as a control in the regression, it is interesting to note that those who took time off did worse on the LSAT on average and are earning slightly more five years after graduation (see Table 3.3). This is somewhat suggestive that the skills earned during their time off may be able to make up for differences in ability.[19]

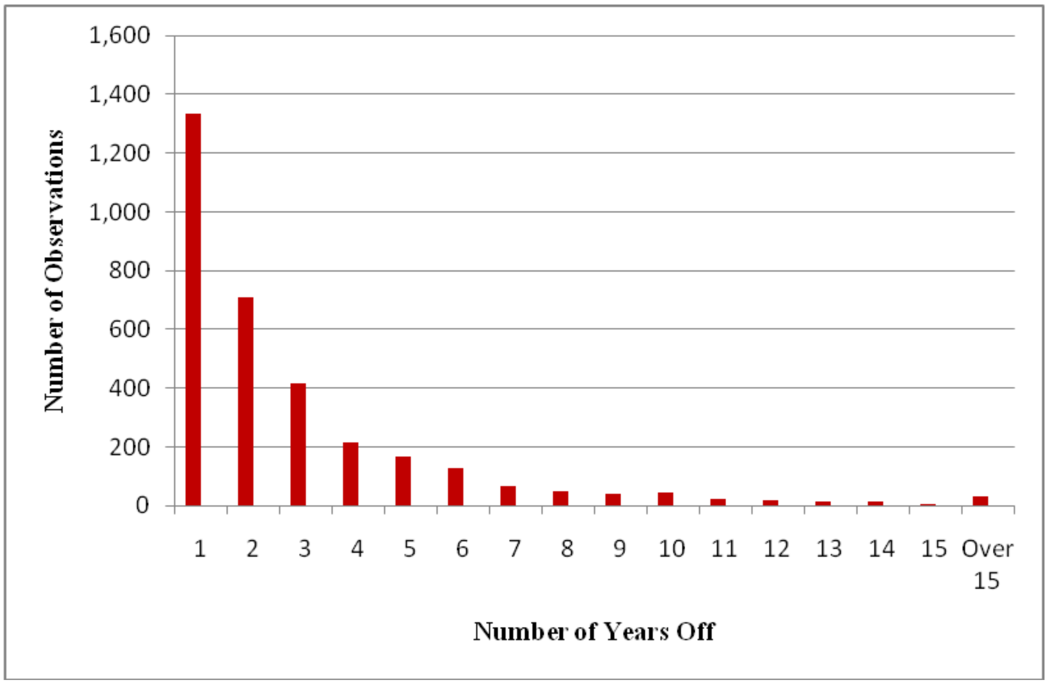
Each class had roughly 350-400 students, and the response rate was about eight percentage points higher for the five year survey than the fifteen year survey[20]. The information that is key to this analysis (the number of years between college and law school and the occupation during that time) is taken from student admissions records and is therefore not subject to recall bias. However, since there cannot be variation in years off between undergraduate studies and law school for an individual after law school graduation, doing individual fixed effects estimation will not allow me to estimate the effect of the time off. Therefore, the five and fifteen year follow up surveys are treated separately as cross sectional data, although they are linked to the information about these

[19] Because the LSAT could be taken when the student is nearing the end of his undergraduate studies or much later if he takes time off, the same regressions were estimated leaving out the LSAT variable. Results were very similar and are available upon request from the author.

[20] In general, response rates were better for later follow ups, but there is a trend of decreasing response rates over time. These particular rates are a result of using this subsample.

individuals while they were students. Figure 3.1 shows the distribution of the number of years between undergraduate studies and law school for all those who had a gap in schooling.

Figure 3.1: Years Between BA and Law School



The occupation data for the students during their time off between completion of their undergraduate degrees and entering law school are categorical. However, the same individuals have been responsible for the survey since 1980 so the definitions of the categories have remained quite stable and are available for all graduating classes through 1991. The categories are: not employed, legal assistant, manager/professional, white collar, blue/pink collar, homemaker, public service and other. Those who were not employed, blue or pink collar professions, homemakers or public servants are in areas considered not related to law for this study. Legal assistants were certainly related. However, the manager/professional and other white collar occupations were somewhat trickier. Managers included management at firms or within government. Professionals were more narrowly defined as requiring some sort of certification; examples include CPAs, nurses and actuaries. Although managers and professionals are likely building some skills that will be useful in a law career, the category is rather broad so it is difficult to know just how much these skills will translate to a future career. For example, they may be learning familiarity with an industry in which they plan to work after law school, applying their knowledge of the industry to a particular firm. They are likely not working directly in the field of law, however. The same is probably true for other white collar jobs, although this definition is even broader. For this reason whether an individual worked in a field related to law is defined in three ways, expanding the definition to encompass these broader categories. For the strictest definition only those who worked as legal assistants during their time off are considered to be working in a related field. The second definition includes both those who worked as legal assistants and those who worked as managers and professionals. The third definition encompasses both of these categories as well as all other white collar jobs. The following table describes the fraction of individuals in each type of job between their undergraduate studies and law school.

Table 3.2: During Time Off

Not Employed[21]	.2068
Legal Assistant	.1018
Managerial/Professional	.1371
White Collar	.1575
Blue/Pink Collar	.0701
Homemaker	.0050
Public Service	.344
Other	.2873

As mentioned in Section 3.2, it is important to see if there are other differences between the groups who take time off and those who did not, especially in the time after graduation from law school. Table 3.3 below presents some summary statistics about the sample that we are studying in their time after law school and we see that for many of the variables there is little or no difference between those who took time off and those who did not[22].

Note first that the sample sizes for the 5 and 15 year follow-ups are not very different because of attrition, but because the survey was truncated in 2006 and not enough time had passed for the later cohorts to be followed up after fifteen years. Women seem slightly more inclined to take time off during their studies, and African-Americans seem slightly less likely to do so. Marital status is very similar between the two groups within five years of graduation. Of course, those who took time off were more likely to be married upon entering law school (and upon leaving), which is sensible because they are older. The average number of children is still slightly higher for those who took time off after five years, but the numbers are very similar after fifteen so the disparity is due to age.

Since leaving law school, both groups work similar amounts of time on average, although those with a gap in schooling work just slightly less than those without. There is little difference in incomes between those who took time off and those who did not, which is interesting because of the differences in LSAT and experience since law school. Income is asked to be pretax and from the alumnus's principal occupation. Both of these measures are quite variable, and the variation is much more pronounced after fifteen years for those who did not take time off than for those who did.

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[21] Some students used their time between undergraduate and law school studies to attend graduate school in another field. In the next section there is an indicator variable controlling for whether or not the individual has a completed master's degree, whether it was before or after their time in law school.

[22] For all variables determined before entering law school the 5-year follow-up sample was used.

Table 3.3: After Law School

	Full Sample	Took Time Off	Didn't Take Time Off
Observations (5 year)[23]	8139	3280	3605
Observations (15 year)	4449	2054	2371
Percentage Female***	.3664	.3851	.3370
Percentage White*	.8361	.8506	.8361
Percentage Black**	.0735	.0677	.0807
Married Currently (5 yr)	.6046	.6198	.5959
Avg Number of Kids (5 yr)***	.472	.585	.366
Avg Number of Kids (15 yr)	1.705	1.714	1.697
Parent is an Attorney	.1367	.1301	.1418
Mean Age (5 yr)***	31.75	33.11	30.31
Spouse/Partner Attorney	.2811	.2706	.2911
Average Yearly Income (5 yr, 2007$)	112422.8	108539.4	108383.2
Std Deviation	67273.06	61920.24	56544.03
Average Yearly Income (15 yr, 2007$)***	234551.7	210587.7	254193.7
Std Deviation	293519.2	222979.1	341330.4
In Private Practice (5 yr)**	.6826	.6625	.6990
Avg %Years Worked Since LS (5 yr)*	.9808	.9792	.9840
Avg %Years Worked Since LS (15 yr)	.9688	.9671	.9704
Avg Hours Worked Per Year (5 yr)***	2504.72	2483.14	2531.42
Avg Hours Worked Per Year (15 yr)	2263.93	2243.17	2279.39

3.4: Empirical Results

In order to identify the effect of depreciating human capital, I use data on what individuals were doing during their time off of school. The data are organized categorically, as discussed in Section 3.3. The most closely related profession is legal assistant; however, the sample size is rather small, so it is difficult to get precise

[23] As you can see, approximately 15% of the sample has missing data for the time between undergraduate and law school variable. On average, these individuals earn more than those whom are not missing this data after five years.

estimates using this as my criteria for whether the individual worked in a related field. Therefore, the two expanded definitions of relatedness discussed above are applied as well. Below, “definition 2” refers to defining a related career as either a legal assistant or a manager or professional, and “definition 3” includes all of these as well as the white collar category.[24]

3.4.1: Ordinary Least Squares Empirical Results

The following tables show the estimation results for the equations defined in Section 3.2[25]. Table 3.4 uses standardized GPA as the dependent variable. In the first column is the regression of GPA on only the years off variable and controls; this regression is included in both the specification using GPA as the dependent variable and in the earnings regressions discussed in Section 3.2 for comparison. Columns (2), (3) and (4) use expanding definitions of how related to law the occupation during their schooling gap is. As mentioned, GPA has been standardized to protect privacy, so the magnitudes of the coefficients should be interpreted as standard deviations from the mean.

Interestingly, the results here indicate that taking time off increases GPA in law school, although the magnitude is small. Also, the coefficient for working in a related job is positive while the sign on the coefficient of the interaction variable is negative. This indicates that both taking time off and working in a related field during time off increase GPA, but there is no extra benefit for working in a related field for additional years. The

[24] It is possible that the effect of a schooling gap may be different across genders. However, when estimating the regressions below separately by gender I find very similar patterns as presented in the rest of this section. The major differences were a larger premium to women from working in private practice, and the race differential was larger for men. While interesting, these do not relate directly to this paper. See Corcoran and Duncan for a further discussion of race and gender differences in earnings.

[25] Although there is a dummy variable for whether an individual received a master’s degree in all specifications, anyone who reports having earned a Ph.D. is dropped from the sample in order to preserve homogeneity. If those with masters’ degrees are dropped as well, the patterns shown are generally preserved although the estimates are less precise.

Table 3.4: Effects of Time Off on GPA

VARIABLES	(1) GPA	(2) GPA	(3) GPA	(4) GPA
Years BA to Law School	0.0414***	0.0403***	0.0505***	0.0480***
	(0.00614)	(0.00692)	(0.00779)	(0.00879)
Legal Assistant During Time Off		0.130		
		(0.0937)		
Legal Assistant/Years Off Interaction		-0.00362		
		(0.0338)		
Related Work During Time Off Definition 2			0.237***	
			(0.0598)	
Related Defn 2/Years Off Interaction			-0.0498***	
			(0.0129)	
Related Work During Time Off Definition 3				0.201***
				(0.0477)
Related Defn 3/Years Off Interaction				-0.0348***
				(0.0118)
White Indicator	0.431***	0.396***	0.393***	0.389***
	(0.0502)	(0.0604)	(0.0603)	(0.0603)
Female Indicator	-0.138***	-0.136***	-0.138***	-0.140***
	(0.0255)	(0.0295)	(0.0294)	(0.0294)
LSAT %tile Standardized	-0.281***	-0.300***	-0.301***	-0.303***
	(0.0174)	(0.0203)	(0.0203)	(0.0203)
UG U of Michigan	-0.0527	-0.0404	-0.0392	-0.0409
	(0.0330)	(0.0379)	(0.0378)	(0.0378)
UG Other School in Michigan	-0.314***	-0.271***	-0.269***	-0.272***
	(0.0454)	(0.0514)	(0.0512)	(0.0512)
UG Ivy League or Seven Sisters	0.306***	0.289***	0.286***	0.284***
	(0.0413)	(0.0474)	(0.0473)	(0.0473)
UG Other Public School	-0.0264	-0.00814	-0.00411	-0.00649
	(0.0332)	(0.0380)	(0.0379)	(0.0379)
UG GPA Standardized	0.251***	0.262***	0.263***	0.265***
	(0.0148)	(0.0170)	(0.0170)	(0.0170)
No. Kids when Grad LS	0.0134	0.00294	0.0109	0.0111
	(0.0308)	(0.0348)	(0.0348)	(0.0348)
Has Masters Degree	0.0125	0.0482	0.0340	0.0437
	(0.0518)	(0.0580)	(0.0579)	(0.0580)
Cohort Indicators	Yes	Yes	Yes	Yes
Observations	4406	3410	3410	3410
R-squared	0.280	0.286	0.289	0.289

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

magnitude of the coefficient on working in a related field is relatively large, and is almost as important to explaining GPA as LSAT percentile or undergraduate institution. The other control variables generally have the expected signs; the coefficient estimate on LSAT is negative as expected, and the coefficient on undergraduate GPA is positive. It also shows that those who attended other schools in Michigan (besides the University of Michigan) do worse than the excluded group; this makes sense because the University of Michigan is the highest ranked school in Michigan. The excluded group is those who attended private schools or military academies outside of Michigan. Those who attended Ivy League or Seven Sisters colleges do better on average than those who attended any of the other undergraduate institutions.

The next table, Table 3.5 shows the coefficient estimates from the regressions discussed in Section 3.2 using the natural logarithm of earnings as the dependent variable. These results use earnings five years after law school graduation. Recall that the coefficient on the number of years taken off should be equal to the wage penalty for human capital depreciation. Mincer and Ofek (1982) estimated the wage penalty as 0.6-1.1%, and Corcoran and Duncan (1979) found that labor force attachment explained about 3% of the gap between white men's and women's wages and work history explained about 28%, which is significant but it is not directly comparable in terms of magnitudes to the analysis here. Kunze (2002) using similar methodology to this study distinguishes between types of employment gaps and finds gross depreciation between 13 and 18% for young German workers if we assume that there is no investment during parental leave.

Table 3.5: Effects of Time Off on Salary 5 Years After LS

VARIABLES	(1) Logrsalary	(2) Logrsalary	(3) Logrsalary	(4) Logrsalary
Years BA to Law School	0.00148	0.00485	0.00668	0.00905
	(0.00474)	(0.00555)	(0.00610)	(0.00714)
Legal Assistant During Time Off		0.138*		
		(0.0816)		
Legal Assistant/Years Off Interaction		-0.0994***		
		(0.0348)		
Related Work During Time Off Definition 2			0.0852*	
			(0.0480)	
Related Defn 2/Years Off Interaction			-0.0232*	
			(0.0119)	
Related Work During Time Off Definition 3				0.0613
				(0.0380)
Related Defn 3/Years Off Interaction				-0.0185*
				(0.0101)
LSAT %tile Standardized	-0.0406***	-0.0442***	-0.0436***	-0.0443***
	(0.0133)	(0.0166)	(0.0166)	(0.0166)
UG U of Michigan	0.00631	0.0120	0.0131	0.0114
	(0.0253)	(0.0305)	(0.0306)	(0.0305)
UG Other School in Michigan	-0.136***	-0.145***	-0.148***	-0.147***
	(0.0368)	(0.0439)	(0.0439)	(0.0439)
UG Ivy League or Seven Sisters	0.0364	0.0718*	0.0660	0.0648
	(0.0333)	(0.0409)	(0.0409)	(0.0409)
UG Other Public School	0.0200	0.00267	0.00817	0.00748
	(0.0249)	(0.0296)	(0.0297)	(0.0297)
%Years Employed since LS	0.0216***	0.0216***	0.0216***	0.0215***
	(0.00191)	(0.00216)	(0.00216)	(0.00216)
Has Masters Degree	-0.0159	-0.0110	-0.0110	-0.0106
	(0.0427)	(0.0517)	(0.0518)	(0.0519)
Hours Worked per Year	0.000287***	0.000274***	0.000276***	0.000276***
	(1.70e-05)	(2.04e-05)	(2.04e-05)	(2.04e-05)
Cohort Indicators	Yes	Yes	Yes	Yes
Demographic Controls[26]	Yes	Yes	Yes	Yes
Observations	2583	1809	1809	1809
R-squared	0.238	0.247	0.245	0.245

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

[26] Demographic controls include dummy variables for being female or white, and the number of children upon law school graduation.

The results here are a little bit surprising. Although not significant, the coefficient estimate on the number of years between undergraduate degree and commencing law school is positive when it was expected to be negative. Even more surprising is that the coefficient on the interaction variable is negative and significant when it was expected to be positive. The stronger positive effect of working in a related job may provide some explanation. It could be that working in a related job does increase future earnings, but that the additional years of experience do not. Recall that the average amount of time between undergraduate studies and law school was about three years for those who had a gap, so the specification in column (3) estimates modestly higher earnings for those who took the average amount of time off working in a related field (1.56%) than those who did not take time off, but lower earnings for those who had a longer gap. Those who took time off but did not work in a related field do not have very different earnings from those who did. This could be due to heterogeneity in the activity during the time off, since we only know the activities of those who worked in common fields for these individuals.

One variable that is not present in the earnings regressions that could be important is whether the individual worked in a private law firm. Sector of employment is one of the most important determinants of salary in the field. (see Wood, Corcoran and Courant). Those who work in larger private practices tend to earn more than those who work in smaller firms or other areas of law. However, the choice of employment sector could be correlated with the decision to take time off and would therefore be endogenous in earnings regressions. Appendix 2A contains a table looking at how time between undergraduate study and law school affects the decision to work in private practice five years after graduation using a simple linear probability model. It does appear that those who have a gap in their schooling are somewhat less likely to work in private practice, although the effect is only marginally significant.

Table 3.6 is constructed in the same way as the Table 3.5 except it uses the logarithm of salary fifteen years after graduation as the dependent variable. In this specification we see that the coefficient on the number of years off is negative and significant, though small in magnitude. While the coefficient on the interaction variable is positive as expected, it is not significant in any specification. The coefficient on the indicator for working in a related field is not significant after fifteen years either. This may mean that though experience before law school could make a difference in wages early on, by fifteen years into a career that experience no longer makes a difference in earnings. There is a small but persistent negative effect on earnings for taking time off, although that could be due to the choice of sector as discussed above. The control variables again have the expected sign, although the effects seem to be muted as the years go on. The coefficient on the LSAT percentile is negative but no longer significant, so it may be that after three years of law school and fifteen years in the labor force, the LSAT is too noisy of a signal to convey information. There does appear to be a persistent wage premium to attending Ivy League and Seven Sisters colleges, although undergraduate institution does not vary much beyond those institutions. The amount of time working (or not taken off) seems to be a very important determinant of salary, and since it is possibly correlated with years taken off between college and law school, it is included it in this specification. As would be expected, it is positive and there is a penalty for taking time away from work. In the same vein, the effect of yearly hours is also positive and significant.[27]

[27] All of the regressions in this section were also estimated with an indicator for whether the individual took off a single year and the appropriate interactions. Results were largely unchanged from those presented here. However, nonlinearity of the effects estimated is a possible direction for future research.

Table 3.6: Effects of Time Off on Salary 15 Years After LS

VARIABLES	(1) Logrsalary	(2) Logrsalary	(3) Logrsalary	(4) Logrsalary
Years BA to Law School	-0.0163**	-0.0189**	-0.0204**	-0.0199**
	(0.00662)	(0.00743)	(0.00884)	(0.0100)
Legal Assistant During Time Off		-0.130		
		(0.136)		
Legal Assistant/Years Off Interaction		0.0145		
		(0.0600)		
Related Work During Time Off Definition 2			-0.0944	
			(0.0793)	
Related Defn 2/Years Off Interaction			0.0122	
			(0.0162)	
Related Work During Time Off Definition 3				-0.0783
				(0.0594)
Related Defn 3/Years Off Interaction				0.0100
				(0.0149)
LSAT %tile Standardized	-0.0122	-0.0282	-0.0280	-0.0279
	(0.0230)	(0.0253)	(0.0253)	(0.0253)
UG U of Michigan	-0.00247	-0.00523	-0.00370	-0.00110
	(0.0429)	(0.0462)	(0.0462)	(0.0461)
UG Other School in Michigan	-0.0822	-0.0661	-0.0637	-0.0610
	(0.0594)	(0.0642)	(0.0641)	(0.0641)
UG Ivy League or Seven Sisters	0.0981*	0.113**	0.112**	0.114**
	(0.0513)	(0.0549)	(0.0549)	(0.0549)
UG Other Public School	0.0699	0.0273	0.0282	0.0300
	(0.0438)	(0.0473)	(0.0473)	(0.0472)
%Years Employed since LS	0.0284***	0.0286***	0.0286***	0.0286***
	(0.00351)	(0.00394)	(0.00394)	(0.00394)
Has Masters Degree	0.0254	0.0462	0.0495	0.0461
	(0.0707)	(0.0781)	(0.0780)	(0.0781)
Hours Worked per Year	0.000595***	0.000604***	0.000603***	0.000604***
	(2.93e-05)	(3.15e-05)	(3.15e-05)	(3.15e-05)
Cohort Indicators	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes
Observations	2085	1792	1792	1792
R-squared	0.311	0.317	0.317	0.317

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

3.4.2: Results Using Instrumental Variables

Although the empirical results in the previous section are somewhat mixed, there is a small but persistent penalty to a gap in schooling fifteen years after law school graduation, regardless of occupation. Early in the individual's career some experience in a related field may have a benefit, although there is a penalty for more years out of school. Taking time off from schooling to work is not a random occurrence, however. It is possible that the decision to take time off is correlated with unobserved attributes that cause these individuals to earn more or less or perform better or worse in law school in the future. For this reason, an instrumental variables approach is adopted to estimate the previous regressions, assuming that the years off and the interaction variables are endogenous.

The instruments employed are the unemployment rate in the year of college graduation, an indicator variable equal to one when the individual was a business or economics major, and the interactions of those variables with the measures of relatedness of their occupation[28]. These are likely to explain time off, because if labor market conditions are bad when an individual graduates, he is more likely to go to graduate school than to enter the labor force (ie, there should be a negative correlation between the unemployment rate and the number of years taken off of schooling). Business and economics majors are somewhat more likely than others to attend law school, and they may also be more sensitive to labor market conditions when they graduate than other majors. Overall they are less likely to take time off between college and law school. However, while undergraduate major does not affect earnings directly, it does have a

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[28] For those who question the validity of college major as an instrument, results are available upon request using only the unemployment rate and interactions as an instrument. The point estimates are almost always within a standard deviation of those presented below, although the estimated standard errors are sometimes larger.

direct effect on GPA in law school. From looking at the data, we find a higher unemployment rate means fewer years off before law school and business and economics majors also take fewer years off.

It has been suggested that labor market conditions in the year that an individual enters the labor market may have lasting effects on earnings (see Baker, Gibbs and Holmstrom, 1994). However, this is not likely to be a problem for the estimation presented. There are two reasons for this; first, the instrument that is used is the unemployment rate in the year that the individuals graduated with their bachelor's degrees. Although there may be correlation of earnings with labor market conditions the year of graduation from law school, it is unlikely that there would be a persistent effect of earnings from a job held before schooling was completed. This is especially true since we know that one cannot practice law without passing the bar exam, so there is no concern that these individuals held the job of an attorney during their time away from schooling. Further, although there is a high correlation of unemployment rates from one year to the next, there is very little persistence beyond one year (Barro, 1988). The year of college graduation is at least three years prior to becoming an attorney. The use of indicator variables for the law school class cohorts should further mitigate any concerns about the validity of this instrument.

Ideally, instruments should be correlated with the endogenous variable but not with the error term in the second stage regression. Several instruments were considered in crafting this section. Light (1995) used indicators of geographic region, union status, occupation category, whether the individual is from an urban area, number of siblings, and parent's educational attainment as instruments for taking time off in schooling. Some of these are obviously not suited for these data; however, other instruments considered were: whether the individuals' parents were attorneys, whether they were from the south, or if they were from an urban area. These are very weakly correlated with schooling gaps in these data. The same was true of other college majors besides business and economics as well as the unemployment rate in Michigan, so the set of instruments was restricted as stated above.

Table 3.7 below is the same as Table 3.4 in Section 3.4.1 above, but uses two-stage least squares to estimate the number of years off and the interaction variables, and then uses those estimated values as regressors in the second stage. The excluded instrument in this specification is only the national unemployment rate in the college graduation year. The dependent variable is the student's final law school GPA. Recall that magnitudes should be interpreted as standard deviations from the mean GPA.[29]

It is possible that the conclusion reached in Table 3.4, that those who take time off do better in classes than others on average is still true, but the effect is smaller than previously estimated. It is no longer significant at any reasonable confidence level and the point estimates are negative (although the variance is quite large). Working in a related occupation has a much stronger effect than was estimated in the previous section. The point estimates on the interaction variables are not significant, but are negative in the first two definitions of relatedness. This supports the findings in Section 3.4.1 and indicates that while taking time off to gain some work experience does increase GPA, taking off additional years does not add to the effect and may actually work against it.

Similar to this, Table 3.8 should be compared with Table 3.5 in Section 3.4.1. This specification (and all of those following) uses the national unemployment rate, an indicator for a business or economics undergraduate major and their interactions with related work as instruments for the length of the schooling gap and interactions, and then

[29] The first stage regressions from each table in this section can be found in Appendix 2. Note that while cohort indicators and a constant are included, they are not reported.

Table 3.7: Effects of Time Off on GPA: Unemployment as an Instrument

VARIABLES	(1) GPA	(2) GPA	(3) GPA	(4) GPA
Years BA to Law School	-0.116	-0.0462	-0.0194	-0.0313
	(0.0969)	(0.0537)	(0.0790)	(0.0642)
Legal Assistant/Years Off Interaction		-0.0275		
		(0.124)		
Legal Assistant During Time Off		0.234		
		(0.250)		
Related Defn 2/Years Off Interaction			-0.0481	
			(0.0624)	
Related Work During Time Off Definition 2			0.347***	
			(0.127)	
Related Defn 3/Years Off Interaction				0.00175
				(0.0527)
Related Work During Time Off Definition 3				0.233**
				(0.107)
White Indicator	0.556***	0.463***	0.438***	0.432***
	(0.0987)	(0.0809)	(0.0812)	(0.0761)
Female Indicator	-0.0696	-0.0903**	-0.106**	-0.112***
	(0.0486)	(0.0399)	(0.0442)	(0.0365)
LSAT %tile Standardized	-0.294***	-0.308***	-0.308***	-0.308***
	(0.0222)	(0.0245)	(0.0245)	(0.0243)
UG U of Michigan	-0.0785**	-0.0500	-0.0443	-0.0470
	(0.0382)	(0.0396)	(0.0388)	(0.0384)
UG Other School in Michigan	-0.287***	-0.246***	-0.249***	-0.258***
	(0.0567)	(0.0595)	(0.0595)	(0.0578)
UG Ivy League or Seven Sisters	0.297***	0.279***	0.276***	0.278***
	(0.0416)	(0.0459)	(0.0451)	(0.0454)
UG Other Public School	-0.0294	-0.00723	-0.00273	-0.00641
	(0.0356)	(0.0381)	(0.0382)	(0.0378)
UG GPA Standardized	0.176***	0.219***	0.233***	0.236***
	(0.0487)	(0.0316)	(0.0340)	(0.0284)
No. Kids when Grad LS	0.345*	0.196	0.159	0.143
	(0.206)	(0.124)	(0.141)	(0.109)
Has Masters Degree	0.542	0.345*	0.263	0.258
	(0.331)	(0.190)	(0.239)	(0.175)
Cohort Indicators	Yes	Yes	Yes	Yes
Observations	4406	3410	3410	3410
R-squared	0.171	0.252	0.269	0.271

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.8: Effects of Time Off on Salary 5 Years After LS: Unemployment and Major as Instruments

VARIABLES	(1) Logrsalary	(2) Logrsalary	(3) Logrsalary	(4) Logrsalary
Years BA to Law School	-0.0564	-0.123*	-0.122	-0.101
	(0.0664)	(0.0709)	(0.0868)	(0.0888)
Legal Assistant/Years Off Interaction		0.0956		
		(0.339)		
Legal Assistant During Time Off		-0.163		
		(0.630)		
Related Defn 2/Years Off Interaction			0.0298	
			(0.0630)	
Related Work During Time Off Definition 2			0.116	
			(0.135)	
Related Defn 3/Years Off Interaction				0.0217
				(0.0624)
Related Work During Time Off Definition 3				0.150*
				(0.0846)
LSAT %tile Standardized	-0.0405***	-0.0426**	-0.0449**	-0.0445**
	(0.0141)	(0.0189)	(0.0184)	(0.0177)
UG U of Michigan	-0.00690	-0.0212	-0.00231	0.00708
	(0.0283)	(0.0345)	(0.0320)	(0.0294)
UG Other School in Michigan	-0.135***	-0.125**	-0.106*	-0.116**
	(0.0378)	(0.0615)	(0.0617)	(0.0558)
UG Ivy League or Seven Sisters	0.0410	0.0613	0.0708	0.0667
	(0.0416)	(0.0561)	(0.0593)	(0.0592)
UG Other Public School	0.00783	-0.0245	-0.0151	-0.00355
	(0.0286)	(0.0352)	(0.0354)	(0.0316)
%Years Employed since LS	0.0216***	0.0216***	0.0222***	0.0220***
	(0.00257)	(0.00314)	(0.00312)	(0.00308)
Has Masters Degree	0.213	0.481	0.433	0.313
	(0.264)	(0.305)	(0.298)	(0.227)
Hours Worked per Year	0.000278***	0.000259***	0.000264***	0.000267***
	(2.76e-05)	(3.28e-05)	(3.26e-05)	(3.13e-05)
Cohort Indicators	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes
Hansen's J-Statistic	7.764	0.111	0.070	0.119
P-Value	0.0053	0.9462	0.9657	0.9421
Observations	2578	1807	1807	1807
R-squared	0.195	0.018	0.054	0.133

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

uses the estimated values from the first stage to estimate the same regressions as presented in Tables 3.5 and 3.6 in Section 3.4.1.

The estimate for depreciation is now negative and much larger in magnitude for each specification. The interpretation of these coefficients indicates that taking a year off leads to human capital depreciation that will result in 5.6-12.3 percent lower salaries five years after graduation per year taken off. The estimated variances for these coefficients are quite large though, and the coefficient estimate is only significant at the 10% level in the second column. The coefficient estimates on the interaction variables are not significant either, although they are positive as expected. The coefficient estimates on the indicator for working in a related field, while also imprecise, are positive in the third and fourth specifications. This indicates that those who work in related fields during their time off earn higher salaries than those who do not. The point estimates suggest that working in a related field recoups some of the penalty for taking time off, and that those who only take off a short amount of time could even enjoy a slight premium. The null hypothesis that the instruments are valid (uncorrelated with the dependent variable) is not rejected at standard confidence levels for any of the three specifications including the activity during the time off. It is rejected in the initial specification, however, so the specification excluding occupation during time off should be looked at with caution.

Fifteen years after completing law school, the negative effect of taking time off seems to persist somewhat. These results are presented in Table 3.9. It is rather variable though, because the coefficient estimate is only marginally significant when we control for what the individual did during time away from school. The magnitude of the effect (3.87-6.10 percent) is certainly smaller than was estimated after five years, presented above in Table 3.8. The coefficient estimate on the interaction variable is positive using the last two definitions of relatedness, but none of the coefficient estimates is significant.

The same is true for the coefficients on working in a related job. There is a negative effect on salary earnings from taking time off, although the magnitude weakens over time.

We observe that there is a wage penalty for a schooling gap, both five and fifteen years after law school graduation. The level of depreciation estimated using the IV approach is larger in magnitude than what was measured using OLS, and the differences in the two sets of estimates indicate that accounting for the endogenous nature of taking time off is important. Those who will not experience as much of a wage penalty are more likely to have a gap, and selection biases the OLS results in the way we would expect. Although the magnitudes seem quite large, they are not so different from those measured by Kunze (2002). The coefficient estimates are also quite imprecise, so it is quite possible that the actual amount of depreciation is smaller. This study does provide evidence that there is depreciation during a schooling gap. Although the OLS results indicated that working in a related occupation during the time off might have increased wages, the IV results do not support that there are differences.

Table 3.9: Effects of Time Off on Salary 15 Years After LS: Unemployment and Major as Instruments

VARIABLES	(1) Logrsalary	(2) Logrsalary	(3) Logrsalary	(4) Logrsalary
Years BA to Law School	-0.0556**	-0.0387	-0.0427	-0.0610*
	(0.0264)	(0.0244)	(0.0346)	(0.0328)
Legal Assistant/Years Off Interaction		-0.287		
		(0.264)		
Legal Assistant During Time Off		0.415		
		(0.453)		
Related Defn 2/Years Off Interaction			0.00461	
			(0.0385)	
Related Work During Time Off Definition 2			-0.0135	
			(0.108)	
Related Defn 3/Years Off Interaction				0.0463
				(0.0397)
Related Work During Time Off Definition 3				-0.100
				(0.101)
LSAT %tile Standardized	-0.0106	-0.0308	-0.0279	-0.0286
	(0.0216)	(0.0219)	(0.0219)	(0.0219)
UG U of Michigan	-0.00503	-0.00473	-0.00124	0.00223
	(0.0432)	(0.0472)	(0.0471)	(0.0468)
UG Other School in Michigan	-0.0758	-0.0494	-0.0602	-0.0640
	(0.0563)	(0.0637)	(0.0619)	(0.0623)
UG Ivy League or Seven Sisters	0.101*	0.115**	0.112**	0.119**
	(0.0522)	(0.0546)	(0.0541)	(0.0544)
UG Other Public School	0.0699	0.0238	0.0295	0.0271
	(0.0440)	(0.0473)	(0.0473)	(0.0473)
%Years Employed since LS	0.0280***	0.0279***	0.0281***	0.0281***
	(0.00520)	(0.00597)	(0.00597)	(0.00601)
Has Masters Degree	0.188	0.152	0.142	0.149
	(0.124)	(0.119)	(0.123)	(0.108)
Hours Worked per Year	0.000601***	0.000610***	0.000609***	0.000608***
	(4.01e-05)	(4.31e-05)	(4.33e-05)	(4.33e-05)
Hansen's J-Statistic	3.875	3.242	4.410	4.094
P-Value	0.0490	0.1977	0.1103	0.1291
Observations	2085	1792	1792	1792
R-squared	0.299	0.304	0.313	0.311

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

3.5: CONCLUSION

The results seem to suggest that while those who have a gap between undergraduate and law school do earn better grades on average, but much of this can be attributed to selection. Those who take time off are those who are least likely to experience a penalty from doing so. This is likely because those who have taken time off are more mature, have better time management or for other reasons are more able to learn the material and communicate their understanding effectively. Those who work in fields during their gaps related to law are able to draw from their experience and perform much better than their peers in the classroom, and this finding is robust to both the ordinary least squares and the instrumental variables models.

While the experience and maturity gained during a gap in schooling does have positive effects on grades, the same is not true for earnings after law school graduation. This is surprising because class rank is important for placement at top law schools. We see that selection does produce the bias we would expect in the ordinary least squares regressions; those who are least likely to face a wage penalty (or who are likely to face a smaller penalty) are more likely to take time off in schooling. When we take the endogeneity of the decision into account we find that there is a substantial rate of depreciation during schooling gaps. Contrary to what was found by Light (1995), this study finds that the gap in wages is persistent for this group of individuals. There is some evidence that working in a related field keeps the human capital stock from depreciating either by keeping the individual current in their field or by learning skills that will help them later in the work force. This does not mean, however, that it is ex post optimal in terms of lifetime income maximization to take a gap in schooling.

The optimal decision may not be the same ex ante and ex post for an individual who is choosing whether or not to take a gap in their schooling. The optimal decision depends on the wages earned during the schooling gap, the individual's discount factor, and the reason for taking the time off. If the person is taking time to collect information about his own abilities and preferences, it may be optimal to take the gap ex ante. Ex post they may wish that they had not so they could earn higher wages for a longer period. It is also quite possible that the activity undertaken during the time off has nonmonetary benefits and the individual is maximizing more than just lifetime income. This could include traveling while young and free of responsibilities or raising children.

While the findings presented here are a first step in exploring how the timing of higher education can influence earnings, there is surely more work to be done on the topic. These data are very rich in information about the schooling behavior of these individuals; this includes the courses they took in law school and the sectors of law in which they practice. This information could lead to better definitions of how related to their field of law the jobs they had after college were, as well as allow us to look at the quality of the job match after graduation. Although there was little done in this study to explore differences by gender, it is possible that there are some issues surrounding the timing of fertility that could impact both the decision to take a gap in schooling as well as GPA and future earnings. Fertility timing would almost certainly affect men and women differently, and how it affects these outcomes would certainly be of interest. This could be explored simultaneously with the sector of law in which the individuals chooses to practice, since there is some indication that the benefits of working in private practice may differ across gender.

It is very important to consider the context of these estimates when comparing them to other studies of gaps in schooling (such as Light, 1995 and Monks, 1997). Many other papers look at the age earnings profile, and comparing individuals of the same age with different education patterns. In this study we have individuals who are at the same stage in their careers, but are different ages because of the different schooling patterns. If we compared two individuals of the same age, but one started their law career two years later because of a schooling gap, we would likely find that the individual with the schooling gap earned less than the individual who had been working for two more years as an attorney. What we have found in this study is that the individual with the schooling gap would also earn less than another attorney who graduated in the same year but did not have a schooling gap. The gap in their earnings would probably be exaggerated if we compared individuals of the same age because the individual with the schooling gap would suffer from both the penalty associated with the gap as well as fewer years experience.

CHAPTER 4

Altruistic Parents and Paying for College

4.1: INTRODUCTION

The relationship between a parent and child is one that everyone experiences, on one side or both. There are interesting economic questions that can arise from these interactions, especially if we think that parents are altruistic towards their children. The vast majority of parents care a great deal about their children's welfare, and invest a lot of time and hard work to ensure their children's happiness. With these sentiments in mind, this chapter studies the question of why parents give transfers to their children "in kind;" more specifically, it looks at the transfers that parents give to their children in the form of college tuition and living expenses while in college.

The most basic economic theory of tied transfers says that the tying of the transfer adds a constraint to the bundle of goods that the recipient can consume. Therefore, it cannot be welfare improving. If the giver cares only about the welfare of the recipient, he should give a transfer as cash so that the recipient may maximize their utility with the fewest number of constraints. However, many have shown that this basic model may not hold when we consider the idea that the recipient may be able to bargain with the giver and possibly manipulate the magnitudes of transfers in different time periods. Bruce and Waldman (1991) build on Buchanan's (1975) model to discuss why the Samaritan's dilemma may lead a recipient of a transfer to behave in a way that increases the size of this transfer. More specifically, Bruce and Waldman discuss why children may under-invest in something like a college education if they believe that their parents will take care of them in the future when they have low incomes. Within a purely altruistic framework, they predict that parents may want to tie some of the transfer which they give to a child to investment (for example, investing in human capital) in an earlier period in order to make a smaller transfer to the child in a later period. This model is tested and empirical evidence was found by Brown, Mazzocco, Scholz and Seshadri (2006).

The purpose of this chapter is to explore whether giving transfers to children for college is treated differently by parents, or if it is equivalent to a cash transfer for both the parent and child. This chapter also explores how children's choices of college attainment change based on the giving by their parents. More specifically, it develops a theoretical model where altruistic parents will care both about their child's consumption and education as well as their own consumption, and will see how this "spillover" effect of the child's education will change standard predictions. The model is then tested to see if these implications hold up in data and if this model explains behavior in a way that the standard model does not.

The contribution of this paper is a new way to consider tied transfers given to children. The idea laid out in Pollak (1988) is formalized and used to build on the standard altruism model, and empirically testable predictions are derived. We should see different returns to the amount invested in education across groups based on who paid for college if the model is correct. In the data there is actually very little evidence that this occurs. Possible reasons and alternative theories are discussed in Section 4.6.

The theory of this model predicts that there should be differential returns to educational investment based on whether the child pays for some of her own education or if the parent pays the entire amount. In particular, the theory predicts that the students who pay their own way should get a higher return to the amount invested. This is tested in both an ordinary least squares (OLS) framework and also using instrumental variables (IV) to account for unobserved characteristics of those who pay for their own schooling.

However, both models generally show the same result; there is no significant difference in the return to the amount invested in education between the two groups.

The section below presents the theoretical model. Section 4.3 presents the empirical predictions of the model and sets up the model that will test these predictions. Section 4.4 describes the National Education Longitudinal Survey, which is used to test the empirical model. Section 4.5 presents the empirical results, and Section 4.6 concludes.

4.2: The Model

The model presented below is a slightly modified version of Becker's (1974) standard altruism model. The standard model assumes that children maximize $u^k(c^k)$ subject to the constraint that $pc^k \leq y^k + g$ where $u(\cdot)$ is a standard utility function with $u' > 0$ and $u'' < 0$, c is consumption, y is income, p is the price of consumption goods, g is the money transfer (gift) from parent to child, and the superscript k denotes the child's utility and consumption. Thus, the altruistic parent is maximizing $u^p(c^p) + \eta u^k(c^k)$ subject to the constraint $pc^p \leq y^p - g$. The "η" is called the altruism parameter, and it measures how much weight the parent gives to the child's utility. Throughout this paper, the superscript k will denote the child and the superscript p will denote the parent.

If we allow the child's income $y^k = y(e)$ to be a function of education and allow for both the parents and the child to contribute to this education, then $e = e^k + e^p$ where the superscripts denote which party makes the contribution. Notice that we're not thinking of e as years of schooling in this model, but amount invested in education which would include both time (opportunity) costs as well as direct costs. Still working with the standard model, we would find that the child's first order condition that characterizes the optimal amount of education is $\frac{\partial u^k}{\partial c^k}\frac{1}{p}(y'(e)-1)=0$, which implies that the child would want to invest in education until the marginal return is equal to the price which has been normalized to 1. The parent, anticipating the child's response would solve his problem optimally and his solution would be characterized by the following equations:

Equation 4.1
$$\eta\frac{\partial u^k}{\partial c^k}=\frac{\partial u^p}{\partial c^p}$$

Equation 4.2
$$\eta\frac{\partial u^k}{\partial c^k}y'(e)=\frac{\partial u^p}{\partial c^p}.$$

From this we can infer that $y'(e)=1$. The parent has no reason to interfere with the child's decision of the amount of education to obtain, and both he and the child want to invest in education until the marginal return in terms of consumption is equal to the price. This is a case where any tied transfer would not change the consumption bundle or investment decision of the child. This is the result that Brown et. al (2006) arrived at as well.

We will modify this model only slightly. It will still be assumed that the child's income y^k is not exogenous, but is a function of the education that the child receives which will be denoted *e*. Pollak (1988) introduced the idea of paternalistic preferences, where the parent not only cares about the welfare of his child but also about the consumption bundle that the child consumes. In this model the child will only benefit from education as an increase in lifetime income as before. However, the parent will also get utility from the child being educated. Perhaps this is because the parent gets pride from the child being educated, or perhaps it makes the child more enjoyable somehow. Kotlikoff and Spivak (1981) suggest that parents may tie transfers because of the expectation that the child will support the parent in his old age; however in this model only "downstream" transfers will take place and the child will receive but not give transfers. Pollak indicates, however that "upstream" transfers are rather small and uncertain. Cox (1987) theorizes that rather than future upstream transfers, parents exchange transfers for services such as visits from their children. He does find some empirical support for this hypothesis when comparing exchange and altruism. This may motivate a transfer, but would not cause the tying of transfers that this study investigates. The parent may feel more secure because his child is educated; this could be a means of insuring himself against a bad event, or perhaps he is more risk averse about his child's ability to consume in the future than the child is about her own future consumption. In any case, the parent's utility function will be represented by $u^p(c^p, e)$ which will be twice differentiable and increasing and concave in both its arguments. To solve the model, we will again begin with the actions of the child and work backwards to the parent's solution. The problem of the child becomes:

Equation 4.3 $$\max_{c^k, e^k} u^k(c^k)$$

$$s.t.\, y(e^k + e^p) + g \geq e^k + c^k \; and \; e^k \geq 0.$$

As seen above, the parent and the child may both contribute to the child's education. The child will treat the parent's contribution as given; that is, the child cannot choose e^p, but she can choose e^k and therefore the sum of e^p and e^k which is the total amount of education. Of course, *e* will be constrained to be at least equal to the level of e^p. Assume that the parent will act first in the manner of a Stackelberg leader. Therefore, the parent will have to treat the reaction of the child as a constraint. If we let λ_1 and λ_2 be the multipliers on the two constraints in equation 3.3 respectively, we arrive at the following first order conditions which characterize the solution to the child's problem along with the complementary slackness conditions:

Equation 4.4 $$u^{k'}(c^k) - p\lambda_1 = 0$$

Equation 4.5 $$\lambda_1 y'(e^k + e^p) - \lambda_1 + \lambda_2 = 0.$$

If we assume that the budget constraint is binding (i.e, $\lambda_1 > 0$) but allow the non-negativity constraint to be nonbinding, we arrive at two possibilities. Either $y'(e) = 1$ or $y'(e) < 1$. If $y'(e) < 1$, the child is "over consuming" education in a sense because the marginal benefit is less than the marginal cost. The child would prefer to sell some of her education and consume more. However, she is constrained to consume an education level of at least e^p, so the child simply chooses to set $e^k = 0$ and spends her entire income on consumption. If instead $y'(e^p) \geq 1$, the child will invest a positive amount in her own education until the point where $y'(e) = 1$, where the marginal return to education is equal to the marginal cost which has been normalized to 1.

Now the parent, who acts as a Stackelberg leader, knows that he will be in one of two cases. Either his child will choose to invest $e^k = 0$ in her education or the child will invest a positive amount such that $y'(e^k + e^p) = 1$. If the parent is in the first situation, he knows that $e = e^p$ so the problem he is solving becomes the following:

Case 1:

Equation 4.6
$$\max_{c^p, e^p, g} u^p(c^p, e^p) + \eta u^k(c^k)$$
$$s.t. \, y^p = pc^p + e^p + g.$$

This yields the following equalities which characterize the solution:

Equation 4.7
$$\frac{\partial u^p}{\partial e^p} + \frac{\eta}{p}\frac{\partial u^k}{\partial y(e^p)} y'(e^p) = \frac{1}{p}\frac{\partial u^p}{\partial c^p} = \frac{\eta}{p}\frac{\partial u^k}{\partial c^k}.$$

If we label the three terms A, B and C respectively, we see intuitively why this holds. A is the sum of the parent's private benefit from the amount of education that the child receives and the increased ability of the child to consume weighted by the altruism parameter η. B is the parent's marginal cost of providing education for her child, which is their own forgone consumption. We see that both of these will also be equal to forgone consumption of the child, since the child would have preferred to spend the extra education money on consumption goods; however, this is again weighted by η since the parent does not value his child's consumption in the same way as his own. We could also think of this as the forgone gift that the child gives up. So the parent is equating the marginal benefit from the education with his own marginal cost and his child's marginal cost.

Suppose now that we are in the second case, where $e = e^k + e^p$, and $e^k > 0$. In this case the parent knows that the child will invest enough in education such that $y'(e^k + e^P) = 1$. We can look at the child's response function as a constraint so that the parent's maximization problem becomes

Case 2:

Equation 4.8 $$\max_{c^p, e^p, g} u^p(c^p, e^p) + \eta u^k(c^k)$$

$$s.t. \, y^p \geq pc^p + e^p + g \; and \; y'(e^k + e^p) = 1.$$

If we assume that the second constraint is binding ($y'(e) = 1$) and use a Lagrangian multiplier of λ on the first constraint, we can use the fact that the child's budget constraint is binding so $c^k = \frac{1}{p}(y(e) + g - e^k)$. Then we have the following first order conditions:

Equation 4.9 $$\frac{\partial u^p}{\partial c^p} - \lambda p = 0$$

Equation 4.10 $$\frac{\partial u^p}{\partial e} - \lambda + \eta \frac{\partial u^k}{\partial c^k}\left(\frac{1}{p} y'(e^p + e^k)\right) = 0$$

Equation 4.11 $$-\lambda + \frac{\eta}{p}\frac{\partial u^k}{\partial c^k} = 0.$$

We can simplify these to the following equation which characterizes the solution:

Equation 4.12 $$\frac{1}{p}\frac{\partial u^p}{\partial c^p} = \frac{\partial u^p}{\partial e} + \frac{\eta}{p}\frac{\partial u^k}{\partial c^k}.$$

While this equation is the same as one of the two equalities that characterized the solution in the first case, the cases aren't identical. In the first case, the child faces more constraints than the parent. She has to invest in more education than she would have chosen on her own. However, in the second case the child is free to optimize as she wishes. Therefore, there is no cost being imposed on the child by imposing a minimum level of education since the minimum is not binding. The marginal cost to the child in terms of forgone consumption is already embodied in the parent's additional constraint. If we substitute in the constraint that $y'(e) = 1$ in part A from case 1, we see that the solution that comes out of the second case is identical to the A=B portion of the equalities in the solution of case 1. An important distinction between the two cases is to note that $y'(e) < 1$ when the parent is investing more in the child's education than the child would like; in other words, when the child would prefer to spend the parent's resources on consumption instead of education. This equality is more likely to hold when $\frac{\partial u^p}{\partial c^p}$ is smaller, or when the marginal utility to the parent of additional consumption is lower. This seems like it would be the case when the parent has more income to spend, and so his own level of consumption is higher. However, when his own income is low and he cannot consume as much, the parent's marginal value of consumption is higher and the child will "subsidize" her education which gives the parent a direct benefit. This will be discussed in more detail in later sections.

4.3: EMPIRICAL PREDICTIONS

The theory predicts that the returns to education should be different among groups of people who were given all their education as an in-kind transfer and those who had to supplement some of the investment through their own forgone consumption. In particular, this translates to adult children who worked and borrowed to fund their schooling having a higher marginal return to education than those who were given the money for their education. There are, of course, other considerations that will make the estimation difficult. For instance, although a parent can fund his child's education, the parent cannot force the child to go to college, nor can he force the child to put forth the effort necessary to complete a college degree. The child may have a lower return to education as a consequence. Also, those who choose to fund their own education may have other unobserved characteristics which would result in a higher return to education than average.

Unlike traditional models with returns to education, education was not just defined as years of schooling in this model. This model included all inputs to education in e. This would include the traditional opportunity costs, but direct costs as well. These are the costs most easily transferred by parents, since a parent can pay his child's tuition, for example. Since parents can pay the direct costs themselves, there is no worry that the student could use a transfer intended for educational purposes for anything else. Also, one might expect to see parental transfers given to mitigate the opportunity costs of college to appear as cash transfers in data if they appear at all. These transfers may not be identified as tied to education. Sauer (2004) presents a similar empirical question looking at how parental transfers affect the probability that a law student works or borrows, as well as looking at the effect on lifetime earnings. He does not find an effect of parental transfers on lifetime earnings, but transfers do influence consumption.

To test the empirical prediction we consider the following two econometric models:

Equation 4.13 $$logwage = \alpha^k schoolcost + X\beta$$

Equation 4.14 $$logwage = \alpha^p schoolcost + X\beta.$$

In these, α^p is the increase in wages due to amount invested if the parent pays the entire direct cost of schooling, and α^k is the increase in wages due to the amount invested when the child pays some of the cost. In order to support the model, the coefficient α^k

should be greater than α^p. Since α^p and α^k are mutually exclusive, we can introduce an indicator variable δ^p equal to one when the parent pays the entire cost of schooling. The above models can then be condensed down to the following:

Equation 4.15 $$logwage = \alpha(\delta^p * schoolcost) + \gamma_1 \delta^p + \gamma_2 schoolcost + X\beta.$$

If the assumptions mentioned above are satisfied, we should estimate a negative value for α since the return should be lower when the parent pays the entire amount of schooling than when the child pays some of the cost. This coefficient represents the difference in the returns between the two groups, not the return itself. The coefficient γ_2 is expected to be positive since the return to amount invested in schooling is expected to be positive. The indicator variable δ^p is included in case there is a direct effect on wages of parents paying for schooling, although the expected sign (after controlling for amount invested) is not clear.

A final consideration when taking the step from theory to data, which is discussed in further detail in Section 4.4, is that of selection. The model presented in Section 4.2 discusses investment in a child's education, but the data specifically consider those who chose to continue their education to the post-secondary level. We will not observe those that would have attended college under different circumstances, but were unwilling to fund the entire cost themselves. These are likely the students that would have earned lower returns, so the estimates presented here could be biased in the positive direction. Related to this problem, there is also selection on the part of the parents when deciding whether or not to fund a child's schooling. Parents could choose to only finance the education of a child who is likely to earn a high return from the investment, which would decrease the magnitude of any effect we might observe. This is the basis for using the instrumental variables approach presented in Section 4.5.2.

4.4: DATA

To test the implications of the model described above, data from the National Educational Longitudinal Study of 1988 (NELS) are used. The individuals studied were a nationally representative sample of eighth grade students in 1988; the study follows them and asks questions of the students, their parents and their teachers. The follow-up surveys were conducted in 1990, 1992, 1994 and 2000 when the typical students were in tenth grade, twelfth grade, college sophomores, and in the labor force, respectively. The questionnaires covered a broad range of topics: everything from educational resources, after-school jobs and neighborhood characteristics, to drug and alcohol use. Questionnaires were given to the students as well as their parents and the students' teacher(s). Information about the post-secondary institutions the students attended and student loan data are also available, and are linked to the students' individual data.

The sample is restricted to individuals who had attended a four-year post-secondary institution by the 2000 follow-up. However, because of the need to observe that individual's wages after the completion of their education, we do not include those who were still in school at the time of the 2000 follow-up. While not excluded entirely, portions of the following analysis will ignore those who attended a four-year college but only obtained a 2-year degree or did not complete a degree. These portions will restrict the analysis to those who received a bachelor's degree and nothing more. The individuals in this sample will also be restricted to those working before the 2000 follow-up interview. Those who attended for-profit institutions or programs lasting less than two years are also dropped. This leaves a sample of approximately 6730[30] individuals overall and a subsample of 3200 individuals.

[3030] All sample sizes in this chapter have been rounded to the nearest ten in order to protect privacy of respondents.

By limiting the sample in this way, we limit the population of study to those who attend typical schools and do so in a typical time frame. We ignore non-traditional students. Also, as a consequence of the timing of the surveys we are likely missing many of those who pursue post-graduate study. Even without a schooling gap, many students pursuing Ph.D.s or professional degrees will be dropped from the sample because they were still students in the last follow-up. Finally, this ignores the problem that those who do not participate in the labor force during the time considered do not have an observable wage. This could bias the results if not participating in the labor force were correlated with education financing. For example, if those who did not pay for their education were less likely to work and report earnings, this would bias the indicator variable upward. (See Heckman, 1979.) Approximately 90% of those surveyed in the 2000 NELS worked at some time during 1997-1999, and an even higher fraction of those who attended college worked during that time period. Therefore, there are not very many individuals for whom we observe no wage, so this bias should be mitigated simply because of the high rate of labor force participation in the age group studied.

Unfortunately, there is no variable that simply states whether a parent paid for school or whether the student contributed. Therefore, different indicators which are likely to convey this information (with some noise) are examined and the estimates are compared. As shown in the results below, all indicators generally support the same findings. These indicators include whether the parent saved for his child's college, the child saved for her own college, the child borrowed for college, the child borrowed more than the cost of tuition, the child had grants and whether the child took advantage of a work-study program. If the parent saved, I assume that $\delta^p = 1$, and if any of the other conditions hold I assume that $\delta^p = 0$.

Table 4.1 shows some information about the sample described. College attendees and graduates are more likely to be female than male. Graduates are more likely to have a parent who was also a college graduate than those in sample who did not graduate, and are more likely to report all types of funding studied here. They are also more likely to have declared a college major. Many parents prepared in some way to send their children to college, and therefore intended to pay for at least part of the cost of attendance. However, as evidenced by the high numbers of students taking out loans and saving themselves, most of the students were also paying part of the cost. Because the variable "grants" is not very specific, it should be approached with caution. Grants include federal grants, need-based aid from the institution, need-based scholarships and merit-based scholarships. Since there is both a financing component and an honor component to these awards, this variable could be correlated with ability and therefore earnings.

Table 4.1: Summary Statistics

	Whole Sample	Sub-Sample
Observations	6730	3200
Female	.5367	.5501
African American	.0803	.0644
Hispanic	.1101	.0730
Avg PSAT Math Score	47.35	49.16
Avg PSAT Verbal Score	42.48	43.91
Mother Graduated College	.3422	.4336
Father Graduated College	.4197	.5345
Ag Major	.0136	.0163
Social Science Major	.1367	.1476
Science/Math Major	.0729	.0968
Humanities Major	.0921	.0984
Business Major	.1614	.1619
Education Major	.1082	.1115
Engineering Major	.0683	.0786
Fine Arts Major	.0395	.0438
Health Major	.1246	.0936
Parent Reported Saving for College (1992)[31]	.7245	.7722
Teen Reported Saving for College (1992)	.5333	.5675
Took Out Loans	.4804	.5626
Took out Loans more than Tuition	.4109	.4282
Earned a Grant	.4725	.5177
Participated in Work-Study	.1142	.1517
Attended a Two Year School	460	450
Attended a Four Year School[32]	2810	2540
Avg Yearly Tuition: Public 2 year (averaged over students in sample)[33]	$3059	$3142
Avg Yearly Tuition: 4 Year Private and Public (average over students in sample)	$6662	$7288
Married (2000 follow-up)	.3634	.3201
Average Income 1997-1999 (99$)	$23401.57	$25119.70

[31] The parents were asked in what ways they prepared financially for their child's education after high school. This included savings accounts, savings bonds, real estate investment and other forms of saving.

[32] For some students this distinction was not recorded. Other students attended schools that were less than two year programs or schools that were for profit; these were not counted.

[33] For students that reported attending more than one school, the results presented here and in Section 4.5 use the tuition at the first listed school attended. However, results were very similar if the last school attended is used instead.

4.5: RESULTS

4.5.1: Ordinary Least Squares Results

Recall that in order to have empirical support for the model presented in Section 4.2 we would expect that the estimated coefficient α in Equation 3.14 would be negative, or that the return to the amount invested in education is lower for those students who did not pay for it themselves. Although I mentioned in passing that other factors could contribute to the return to education in the $X\beta$ vector, none of these were mentioned specifically. The factors that should be included are characteristics that may influence whether a parent pays for a child's education and that could also be correlated with the return to education. Though a parent's financing of higher education could be contingent on many different factors, I include several that are likely important. The sex of the individual could influence whether a parent agrees to pay for college, and very likely influences the return to education. Race and Hispanic ethnicity indicators are included because of the possibility of systematic differences in college financing and returns to education. High School GPA is included to proxy for ability; a parent may be more willing to pay for college for a more able child, or may be more willing to support a less able child[34]. Also included in the estimation using the full sample is whether the child graduated from college. Indicators for broad groups of majors are included as well as an indicator for whether each parent is a college graduate. College major could be correlated with parental financial support in several ways; the child could choose her major based on whether or not the parent is financing her education, or the parent could exert influence over the major choice if he is financing the child's education. Whether each parent is a college graduate could again work in different ways; a college-educated

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[34] Other measures of ability were considered, including PSAT, SAT, and ACT scores. High School GPA was included because of completeness.

parent could be more likely to support his child both financially and in other ways to ensure a higher return to education, or those who were unable to attend college themselves may be more (or less) willing to support their children financially when the child goes to college. Finally, the family income from the base year of the survey and the year the student was a senior in high school are included as control variables. As mentioned in Section 4.2, the amount that the parent is able or willing to spend on education is going to be very closely tied to his own ability to consume. Since a child's earnings are correlated with those of her parents', we need to control for the possibility that this is also correlated with her return to education.

As mentioned in Section 4.4, there were six variables that were studied to try to determine whether the parent paid for college or if the child paid part of the cost. The first two look at saving. If the parent reported saving for his child's college education, this would support the idea that $\delta^p = 1$ or that the parent paid the cost of education. Conversely, if the child reported saving for her own college that would support the idea that $\delta^p = 0$ or that the child anticipated having to pay some of the cost of college. As shown in Table 4.2 below, using either indicator leads to the same results. There does not appear to be a measurable difference between the returns to the amount invested based on who financed the student's education.

After controlling for a number of variables, we find that most of the estimated coefficients have the expected signs. Females earn less than males, and married individuals earn more than singles. High School GPA and the amount of tuition seem to have almost no effect. Family background variables include parents' income and indicators for whether each parent is a college graduate. These seem to have little influence, as the coefficients were all statistically insignificant. Surprisingly, in the set of

Table 4.2: Effects of Savings Behavior on Returns

VARIABLES	(1) δ^p=Parent Saved Full Sample Log Avg Salary	(2) δ^p=Teen Saved Full Sample Log Avg Salary	(3) δ^p=Parent Saved Subsample Log Avg Salary	(4) δ^p=Teen Saved Subsample Log Avg Salary
δ^p * Tuition	-.00004	-.00001	-.0001	-.00007
	(.0002)	(.0002)	(.0002)	(.0002)
δ^p	-.0079	-.0075	.0436	-.0004
	(.0469)	(.0406)	(.0688)	(.0557)
Tuition[35]	.0005**	.0004***	.0006***	.0005***
	(.0002)	(.0001)	(.0002)	(.0001)
Female Indicator	-.2486***	-.2479***	-.0887**	-.0893**
	(.0322)	(.0323)	(.0394)	(.0397)
African American	-.0190	-.0214	-.1191	-.1249
	(.0634)	(.0639)	(.0826)	(.0839)
Hispanic	-.0005	.0044	-.0104	-.0065
	(.0551)	(.0552)	(.0852)	(.0853)
Married	.07302**	.07628**	.0946**	.0967**
	(.0320)	(.0321)	(.0399)	(.0401)
HS GPA	-.0004	-.0003	.0004	.0005
	(.0005)	(.0005)	(.0006)	(.0006)
College Graduate	.1217***	.1190***		
	(.0345)	(.0347)		
Family Background Controls	Yes	Yes	Yes	Yes
Major Indicators	Yes	Yes	Yes	Yes
Observations	1830	1820	930	950
R-squared	0.0991	0.0979	0.1377	0.1376

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

college majors only two had significantly different earnings from health majors (the excluded group): business and engineering majors earned more.

[35] In the estimation, tuition is measured in in hundreds of dollars.

The next indicator that the child had to pay for part of her own schooling was based on taking out loans during college. These results are presented in Table 4.3. Because of easy access to low interest loans during college, many students take advantage of federal student loan programs. Of course, who repays these loans is up to the student and her parents. If a student is paying for college on her own, however, we would expect that she would need to borrow more. If the parent is paying the cost of college, we expect that he may borrow for tuition costs since that is an added expense. He could maintain the same expenditure on the child that he had paid while the child was in high school, though. (For example, if the parent is planning on paying the whole cost of higher education he may have to borrow for tuition but ask the student to live at home during college.) Although this is not a perfect indicator, we can determine whether a student takes out loans for more or less than the amount of tuition based on tuition prices for the years he attended and use that as an indicator for whether or not she contributed to her own education. In Table 4.3, in the first and third columns we assume that $\delta^p = 1$ if the child borrows less than just the amount of tuition and $\delta^p = 0$ if they borrow more. We assume for the second and fourth column that $\delta^p = 1$ if the child doesn't borrow at all, or $\delta^p = 0$ if the child does borrow.

Overall, the patterns that we observed above are generally followed. The one important difference is in the coefficient estimate of the interaction variable when we consider the amount borrowed. The estimate is positive rather than negative and significant in the subsample, indicating that those who did not borrow more than the amount of tuition for college have a slightly higher return to the amount invested than those who did borrow more.[36] The dummy variable indicating whether a student took out loans or not was likely the most promising in separating those whose parents' paid the

[36] While this seems counterintuitive, the result is not very robust. Further review should be considered in the future, but is outside the scope of this study.

Table 4.3: Effects of Borrowing Behavior on Returns

VARIABLES	(1) δ^p=Loans More than Tuition Full Sample Log Avg Salary	(2) δ^p=Loans Indicator Full Sample Log Avg Salary	(3) δ^p= Loans More than Tuition Subsample Log Avg Salary	(4) δ^p= Loans Indicator Subsample Log Avg Salary
δ^p * Tuition	.0004	-.0002	.0013**	-.00002
	(.0005)	(.0002)	(.0006)	(.0002)
δ^p	.0680	.1229***	-.0836	.0860
	(.0710)	(.0419)	(.0956)	(.0566)
Tuition	.0001	.0006	-.0007	.0006***
	(.0005)	(.0001)	(.0006)	(.0001)
Female Indicator	-.2356***	-.2533***	-.1306**	-.0931**
	(.0453)	(.0317)	(.0528)	(.0392)
African American	-.0559	-.0205	-.1292	-.1132
	(.0819)	(.0625)	(.0968)	(.0816)
Hispanic	-.1239	-.0175	-.0188	-.0059
	(.0835)	(.0544)	(.1050)	(.0849)
Married	.1257***	.0784**	.1022**	.0988**
	(.0450)	(.0316)	(.0516)	(.0397)
HS GPA	.0005	-.0003	.0003	.0004
	(.0007)	(.0005)	(.0008)	(.0006)
College Graduate	.1690***	.1366		
	(.04794)	(.0347)		
Family Background Controls	Yes	Yes	Yes	Yes
Major Indicators	Yes	Yes	Yes	Yes
Observations	840	1860	510	960
R-squared	0.1341	0.0949	0.1803	0.1418

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

entire cost of schooling from those who paid some of the cost. Although students can still take out loans when the cost is being paid by their parents, students who did not take out loans likely were not paying much (or any) of the cost themselves. (Of course, there could be a small fraction of students with full scholarships as well.) In this sample we only have students who went to college at traditional ages. Because of the difficulty of working full time and going to school, borrowing is the only option for many students whose parents do not help them pay for school. Remember all the students in this sample were roughly the same age and must have completed their schooling and be working by 1999 to be included in the sample, when they were roughly 25 years old.

Finally, there are two alternative sources of funding that were studied to determine if the student paid her way for schooling or if it was paid for by the parents. These results are presented in Table 4.4. These are indicators for whether the student received grants or if she engaged in a work-study program during her time in college. In both cases, δ^p=0 when the student brings in an outside source of funding and δ^p=1 when there are no grants or work-study aid. Again we see very similar patterns. There is very little difference in the return to education based on earning a grant or participating in a work study program. The return to the cost of schooling is positive, though somewhat imprecisely measured. Other control variables are generally similar in sign, magnitude and significance to the previous results.

One interesting item to note is that there is no clear dominant sign for the coefficient on δ^p itself. It is only significant in the specification with the indicator variable for those who took out loans, indicating that those who did borrow earn less on average than those who did not. (Recall δ^p=1 when the individual did not borrow.) This is somewhat counter-intuitive to the theory that people borrow in anticipation of higher future earnings, since it is unclear why the need for access to liquidity would be correlated with earnings capacity.

Table 4.4: Effects of Grants/Work-Study on Returns

VARIABLES	(1) δ^p=Grants Full Sample Log Avg Salary	(2) δ^p=Work Study Full Sample Log Avg Salary	(3) δ^p=Grants Subsample Log Avg Salary	(4) δ^p=Work Study Subsample Log Avg Salary
δ^p * Tuition	-.0001	.00028	.00006	.0005
	(.0002)	(.0002)	(.00016)	(.0002)
δ^p	.0554	-.0744	-.0527	-.1719
	(.0432)	(.0777)	(.0575)	(.0873)
Tuition	.0005	.0002	.0005	.0001
	(.0001)	(.0002)	(.0001)	(.0002)
Female Indicator	-.2557***	-.2574***	-.0971**	-.0973**
	(.0320)	(.0319)	(.0396)	(.0395)
African American	-.0160	-.0257	-.1242	-.1180
	(.0630)	(.0627)	(.0822)	(.0817)
Hispanic	-.0053	-.0113	-.0111	-.0124
	(.0547)	(.0545)	(.0855)	(.0850)
Married	.0805**	.0841***	.1007**	.1094***
	(.0318)	(.0318)	(.0401)	(.0401)
HS GPA	-.0003	-.0003	.0005	.0005
	(.0005)	(.0005)	(.0006)	(.0006)
College Graduate	.1242***	-.0303		
	(.0347)	(.0393)		
Family Background Controls	Yes	Yes	Yes	Yes
Major Indicators	Yes	Yes	Yes	Yes
Observations	1850	1850	950	950
R-squared	0.1028	0.1029	0.1403	0.1442

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

4.5.2: Instrumental Variables Results

Being born to parents who are able to pay for college might be a random occurrence; the decision to incur the cost oneself is surely not random. It is possible that there are unobserved differences among students who pay for college and those who are given the funds for education which could explain an earnings gap. For this reason it is necessary to consider other factors that might indicate whether or not the child is likely to pay for college but that do not have a direct effect on wages. If there are unobserved differences between the two types of students, such as differences in ability or motivation, there would be a downward bias in the interaction coefficient. That is to say, we would observe those who were given the money for education earning a lower return than those who paid for some of their own education due to the second group having higher levels of motivation or drive on average, aside from their parents' treatment of education. Therefore, instruments should be included which influence whether a student is likely to fund some of her own education but are not likely to directly influence earnings several years later.

These instruments will be referred to as "motivation" variables. Included are self-reported variables which asked the student how much she agrees or disagrees with the statements "I feel good about myself," "I don't have enough control over my life," and "on the whole, I am satisfied with myself." These were from the survey administered in 1988. There are also variables indicating how often the student is absent, which is self reported in the first follow up (10^{th} grade) and reported by teachers in the initial study and the first two follow ups (8^{th}, 10^{th} and 12^{th} grades). Also available are variables indicating how often a student is tardy, reported by teachers at each of these points in time. Finally, there is a question from the first follow-up of the survey when the respondents were in tenth grade asking how far in school the respondent expected to go. All of these variables could influence whether the parent or the child pays for college but none is likely to directly affect earnings. Therefore, they will be used as instruments to predict δ^p, and the predicted values will be used in second-stage regressions. Absences and tardies seemed to be especially promising, but as seen in the appendix tables, all of the instruments are rather weakly correlated with paying for college.

Because of the weak nature of these instruments, the estimates below should be approached with some caution. However, as mentioned above, the OLS estimators should be downward biased if there are unobserved differences in these groups. The IV estimates would be biased in the same direction if the instruments are weak (see Stock, Wright and Yogo, 2002). However, the results generally show that the coefficient of interest is insignificant. If there is a bias we would have expected to see negative estimates, and they would be larger (in absolute value) than otherwise estimated. Because we do not see significant negative effects in either set of results (see the tables below), it is likely that these biases are small if they exist.

The table below should be compared to Table 4.2 above. These are the same regressions estimated using an instrumental variables approach instead of ordinary least squares. The coefficient on the interaction variable is still being estimated as negative and insignificant. Although it is larger in magnitude, the standard errors are much larger as well, so this is probably due to the lack of precision in the instrumented variables.

While they were not included as instruments, some of the control variables are good predictors of savings behavior. If the student's mother is a college graduate, the parents are more likely to report saving for college. Parents with higher incomes save more, although students who come from lower-income households save more. It is interesting to note that those who major in the humanities are more likely to have had parents who saved and were less likely to have saved themselves when they were teenagers. More absences in high school are correlated with less saving by both the teenager and her parents, and those who reported being more satisfied overall with life

Table 4.5: Effects of Savings Behavior on Returns: IV Regressions

VARIABLES	(1) δ^p=Parent Saved Full Sample Log Avg Salary	(2) δ^p=Teen Saved Full Sample Log Avg Salary	(3) δ^p=Parent Saved Subsample Log Avg Salary	(4) δ^p=Teen Saved Subsample Log Avg Salary
δ^p * Tuition	-.0009	.0011	-.0013	.0004
	(.0010)	(.0016)	(.0012)	(.0011)
δ^p	-.5031	-.6622	-.0360	-.5076
	(.3937)	(.7891)	(.3928)	(.5251)
Tuition	.0010	-.0002	.0016	.0003
	(.0008)	(.0007)	(.0009)	(.0005)
Female Indicator	-.1417***	-.1372	-.0699	-.0354
	(.0444)	(.0466)	(.0606)	(.0692)
African-American	-.0016	-.0510	.0260	.0806
	(.0951)	(.1000)	(.1014)	(.1273)
Hispanic	-.0703	-.0461	.0633	.0313
	(.0812)	(.0773)	(.0981)	(.1036)
Married	.1372***	.1226	.0990*	.0859
	(.0420)	(.0485)	(.0560)	(.0594)
HS GPA	.0005	.0009	.00002	.0004
	(.0007)	(.0009)	(.0010)	(.0010)
College Graduate	.1386	.1153		
	(.0434)	(.0485)		
Family Background Controls	Yes	Yes	Yes	Yes
Major Indicators	Yes	Yes	Yes	Yes
Sargan Test Statistic	21.016	29.113	24.157	25.849
P-Value	0.3962	0.0856	0.2356	0.1709
Observations	1440	1440	800	800

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

were more likely to plan for the tuition at a more expensive college. Full results of the first stage regressions are found in Appendices 3A and 3B.

Table 4.6 is constructed in the same way as Table 4.3 except that it uses the predicted values of the δ^{p} indicator variable and the interaction with tuition in place of the realized values in the second stage regression. The results using the IV approach are similar to what was found in the rest of this chapter. How schooling was financed makes little difference in the return to the amount invested. The coefficient estimate on the interaction variable is not significant in any of these specifications, and neither is the estimate on the tuition variable. This also predicts higher earnings for those who took out loans, which is more in line with predictions about expected earnings, although the estimate is not precise.

There is nonrandom selection into taking out student loans. Those whose parents were college graduates were less likely to take out student loans, as were humanities majors. Those who perceived themselves as having less control took out more student loans, as did those who went further in school. Those who had more tardies and absences were less likely to have loans. See Appendices 3C and 3D for full first-stage results.

Last, presented in Table 4.7 are the results of the instrumental variables approach using the alternative sources of funding as the indicator for who paid for college. The same patterns observed in the rest of the results are evident, that these measures of who paid for schooling seem to be insignificant in determining return to investment.

Overall the IV estimates generally reinforce the idea from Section 4.5.1 that the indicators presented about who pays for college have little impact on the return to educational investment. The factors that do matter which are robust to both specifications are whether the individual is female (which is less important and less well estimated when we consider only those who graduate) and whether the individual is married at the 2000 follow up.

Table 4.6: Effects of Borrowing Behavior on Returns: IV Regressions

VARIABLES	(1) δ^p=Loans More than Tuition Full Sample Log Avg Salary	(2) δ^p=Loans Indicator Full Sample Log Avg Salary	(3) δ^p=Loans more than Tuition Subsample Log Avg Salary	(4) δ^p=Loans Indicator Subsample Log Avg Salary
δ^p * Tuition	.0012	.0013	.0026	-.0013
	(.0014)	(.0010)	(.0022)	(.0012)
δ^p	.4032	.7060**	.2915	.2277
	(.3325)	(.2796)	(.4011)	(.3494)
Tuition	-.0014	.0011**	-.0026	.0011**
	(.0013)	(.0004)	(.0022)	(.0005)
Female Indicator	-.1939***	-.1450***	-.1998**	-.0744
	(.0566)	(.0425)	(.0799)	(.0603)
African American	.0517	.0059	.0967	.0138
	(.1049)	(.0930)	(.1236)	(.1004)
Hispanic	-.0894	-.0884	.0541	.0237
	(.1105)	(.0791)	(.1296)	(.1045)
Married	.1935***	.1139***	.2263	.1053
	(.0536)	(.0405)	(.0742)	(.0547)
HS GPA	.0007	.0003	-.0008	.0003
	(.0010)	(.0007)	(.0015)	(.0010)
College Graduate	.2263***	.2005***		
	(.0639)	(.0483)		
Family Background Controls	Yes	Yes	Yes	Yes
Major Indicators	Yes	Yes	Yes	Yes
Sargan Test Statistic	27.957	23.560	17.097	25.184
P-Value	0.1104	0.2621	0.6467	0.1945
Observations	780	1460	450	810

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

Table 4.7: Effects of Grants/Work-Study on Returns: IV Regressions

VARIABLES	(1) δ^p=Grants Full Sample Log Avg Salary	(2) δ^p=Work Study Full Sample Log Avg Salary	(3) δ^p=Grants Subsample Log Avg Salary	(4) δ^p=Work Study Subsample Log Avg Salary
δ^p * Tuition	-.0009	1.0002	-.0001	-.0005
	(.0010)	(.0018)	(.0011)	(.0018)
δ^p	.6221	.3830	-.0905	.2686
	(.3959)	(.7710)	(.6019)	(.9040)
Tuition	.0010*	.0006	.0005	.0010
	(.0006)	(.0016)	(.0006)	(.0016)
Female Indicator	-.1246***	-.1457***	-.0856	-.0715
	(.0464)	(.0434)	(.0673)	(.0643)
African American	.0651	-.0461	.0068	.0248
	(.1148)	(.0879)	(.1354)	(.0982)
Hispanic	-.0118	-.0624	.0393	.0407
	(.0789)	(.0771)	(.0982)	(.0956)
Married	.1685***	.1453***	.1044**	.1023
	(.0432)	(.0381)	(.0585)	(.0632)
HS GPA	.0004	.0005	.0003	.0002
	(.0007)	(.0007)	(.0010)	(.0009)
College Graduate	.1813***	.1564***		
	(.0480)	(.0464)		
Family Background Controls	Yes	Yes	Yes	Yes
Major Indicators	Yes	Yes	Yes	Yes
Sargan Test Statistic	26.518	30.010	28.573	28.655
P-Value	0.1494	0.0697	0.0965	0.0948
Observations	1450	1450	800	800
R-squared	0.0126	0.0613	0.0935	0.0943

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

4.6: CONCLUSION

Overall, there is very little empirical support for the paternalistic preferences presented in Section 4.2 of this chapter. The theory is based on the idea that parents may feel differently about giving for college, and they also realize a private benefit from a child attending college. Although the evidence that we produced in this study did not support the theory, this does not necessarily mean that parents do not derive a private benefit from their child attending college. Recall that there were two cases that a child could find herself in when making her investment decision; her parents could be paying more or less than she would have otherwise spent. If the parents receive a private benefit but they are not willing to pay so much that the child would prefer to sell her education for consumption, we would not observe parents paying the entire cost of an education very often. Given the number of students funding their education in some way (see Table 4.1), this could be one reason that we see so little empirical evidence. It could be that we have very few students who find themselves characterized by Case 1 presented in Section 4.2.

An alternative theory, which was presented by Bruce and Waldman and tested by Brown et al, suggests that parents' motives tying transfers are based in bargaining for the magnitudes of future transfers. Although the evidence does support that those who graduate from college need fewer transfers in the future, the bargaining model is not completely at odds with the model presented in this chapter. Parents may be trying to insure that they can make smaller transfers in the future but may also feel more risk-averse about their child's ability to consume in the future. They may then want to tie transfers to investment for both of these reasons. These decisions are complex and there may not be a single correct model.

One possible problem with the empirical specification in this model comes from the necessity of capturing who pays the cost of college. As evidenced by the six different ways that this is measured, this is not an easy variable to capture. If this variable is not being measured correctly, the results will be biased toward finding no effect, whether one exists or not. More complexity is added when we consider the interplay with the financial aid system. It is expected that traditional college-age students are receiving help from their parents. It is very difficult for a traditional college student to be considered independent of her parents unless she is married, has children of her own or has some extenuating circumstances such as being divorced from her parents (see fafsa.ed.gov). Therefore, given these expectations, it might be difficult to disentangle preferences about attending college from this complex decision.

Finally, although a student cannot sell her education in order to finance consumption, the student does make the decision about which college to attend. If a parent will finance part (or all) of the opportunity cost of college attendance, a child could increase her consumption by choosing a school with lower tuition costs. Sauer uses data from a single law school to look at the differences in consumption among students who receive a large part of their financial support in law school from family and those who work during law school. He finds there are large differences in consumption, but not in earnings after graduation. In our more heterogeneous sample, the student may be able to get around the constraint faced in the first case, that they are not able to sell their education to finance consumption. Therefore, because of heterogeneity in the price of education it is possible that students may not face as strict a constraint as assumed in the model; they can essentially pick the price as well as the quantity when making decisions about their educational investment. Although the price may be correlated with school quality, the correlation is not perfect.

Although the empirical evidence is not promising, it is possible that in the future better data will become available and make a question like this easier to answer. Although the bargaining model does have some support, it seems unlikely that this is the only reason we would see parents tying transfers given to their children. Anecdotally, there are parents who will only give a transfer if it is tied to education without implicit agreements for future transfers of any magnitude. Socially, it has been assumed that parents will help their children pay for college, and that is so ingrained in the process that federal financial aid is computed using the "expected family contribution," not just the student's contribution. For these reasons the support of children who are transitioning into adulthood and their own households is worth studying in further detail.

CHAPTER 5

Concluding Remarks

This dissertation consists of three essays analyzing different aspects of higher education. The role of human capital accumulation in economics has been an interesting topic of study, and these chapters further this research area in several ways. Chapter 2 considers the effects of financing on education decisions by examining the effects on enrollment in post-secondary education as a result of changes in the tax system. Chapter 3 focuses on the timing of higher education by studying the effect of not going straight to law school after completing an undergraduate degree. Chapter 4 studies traditional college students and the effects of different kinds of financing.

In contrast to some of the previous literature, the results in Chapter 2 show that the recent tax benefits have had an effect on enrollment behavior in higher education. This is likely due in part to the detailed data used. This study not only evaluates the effect of the benefits overall, but also takes this a step further to analyze the effects of the magnitude of the tax benefit. Results indicate that a decrease in the price of college by 1% would lead to an increase in the probability of enrollment by 0.12 to 0.13 percentage points, and overall the tax credits have increase the enrollment probability by 4.1 to 5.1 percentage points for those eligible to receive them. There is little evidence that students change from part-time to full-time enrollment as a result of the tax policy, but this could be due to considerations outside the direct costs of schooling. This could also be because the enrollment effect overshadows changes on the intensive margin.

Other outcomes of subsidizing education through the tax system should be studied as topics of future research. First, the work presented here only considered enrollment behavior. It did not consider the rate of degree completion or time to completion. Both of these outcomes are important when considering how a policy such as this one affects human capital accumulation in the population. Although it was not available in the data used for this study, the cost and quality of the school attended should also considered. If students are changing behavior by attending higher-quality (or more expensive) schools, the effect of the policy on human capital accumulation would be greater than otherwise estimated. However, if students are spending more time completing degrees or the students enrolling are not completing their degrees at the same rate as before, the positive effects of the policy may be overestimated.

The third chapter examines the effects of taking time off between completing an undergraduate degree and attending law school. The study uses what the individual did during the schooling gap to identify separately the effects of depreciation of human capital and the effect of investment during the time off. Those who took time off tend to perform better in coursework than those who did not, and this is strengthened for those who worked in fields related to law. However, after controlling for selection we find that the effect only remains for those who worked in related fields. There also appears to be a persistent wage penalty for taking time off, although the average penalty is smaller for those who choose to take time off than what we would estimate for the entire population. Therefore, selection is a crucial consideration in this outcome as well.

This chapter deals with the question of selection in ways that previous literature has been unable to do. As evidenced by the results, selection is an important consideration in studying this topic. The homogeneity of the individuals studied is very useful in identifying these effects, although it does mean that generalizations should be made with caution. In the future, this paper could lead to many new research topics. There was some evidence that men and women received different returns to working in private practice, and this is a topic that could be of interest to those who study gender earnings gaps. With the rich data available about the individuals while they were in school, details such as specific coursework, fertility timing, and other behavior could lead to more better understanding of gender wage gaps.

The fourth chapter presents a model where parents care about their child's welfare and about the bundle of goods they consume; specifically, the parent derives direct utility from a child's education. This leads to the empirically testable prediction that the return to education should be lower if the parent pays the cost of education than if some of the cost is borne by the child. However, the empirical results do not support this notion. This could be due to the small number of students who do not bear any of the cost of education. The manner in which education is funded is also very complex, and with many different factors at work it could be that the effect is too small to be measured. The financial support of children who are transitioning into adulthood is worth studying further, since the results did indicate that there were different returns and major choices related to the amount an individual borrows.

The study of human capital accumulation, and more specifically higher education, is certainly a significant topic. The three essays in this dissertation each further this research area in a different way. These essays address questions about when individuals will decide to enroll in college and how much of a return they can expect for doing so. As this acquired human capital can increase levels of productivity in a society, understanding the channels through which returns are attained can help us to understand the role of education more broadly.

Appendices

APPENDIX 1A

The marginal effect of any variable in the logit equation is dependent on the point at which the function is being evaluated. The marginal effect for an interaction variable is not only dependent on the values of the other variables but on the marginal changes in the variables which make up the interaction. (See Ai and Norton, 2003 for a discussion.) The full marginal effect of the interaction variable can be represented by the following:

$$[\beta_{12}x_2 + \beta_1][\beta_{12}x_1 + \beta_2]\Lambda''(x\beta) + \beta_{12}\Lambda'(x\beta)$$

where x_1 represents the indicator variable for whether the individual is eligible for the tax credit, x_2 represents the indicator variable for whether the observation is from before or after 1997, β_{12} is the coefficient on the interaction variable, β_1 is the coefficient on x_1, and β_2 is the coefficient on x_2. I also let x denote the entire vector of control variables and β with no subscript denote the entire vector of coefficients.

APPENDIX 1B

Table 2.10: Ordinary Least Squares Estimates

VARIABLES	(1) Enrollment	(2) Full Time Enrollment	(3) Enrollment	(4) Full Time Enrollment
Eligible/After Interaction	0.0934***	-0.0302		
	(0.0121)	(0.0372)		
Eligible for any Tax Benefit	0.0628***	0.300***		
	(0.0114)	(0.0500)		
Percent Discount First Regime/After 1998 Interaction			0.00135***	0.000282
			(0.000313)	(0.00120)
Percentage Discount First Regime			0.00198	0.0110
			(0.00133)	(0.0120)
After 1998	-0.0456***	-0.00323	-0.0601***	-0.0234
	(0.0155)	(0.0386)	(0.0176)	(0.0456)
Percent Discount Second Regime/After 2002 Interaction			0.00219***	0.00182*
			(0.000476)	(0.00109)
Percentage Discount under Second Regime			-0.00151	-0.00740
			(0.00136)	(0.0120)
After 2002			-0.0598***	-0.0390
			(0.0166)	(0.0460)
Year	0.00254	0.00289	0.0104***	0.00868
	(0.00228)	(0.00596)	(0.00322)	(0.00897)
College Degree Indicator	-0.00643	0.143***	0.00478	0.162***
	(0.0100)	(0.0390)	(0.0103)	(0.0393)
Family Income	7.13e-07***	6.65e-07	7.45e-07***	5.71e-07
	(1.30e-07)	(4.07e-07)	(1.36e-07)	(4.48e-07)
Ineligible due to Low Income	0.0109	0.210***	-0.0231**	0.109***
	(0.0118)	(0.0485)	(0.0113)	(0.0410)
Age Indicators	Yes	Yes	Yes	Yes
State Controls	Yes	Yes	Yes	Yes
Race/Gender/Family Controls	Yes	Yes	Yes	Yes
Constant	-4.925	-4.861	-20.64***	-16.61
	(4.523)	(11.86)	(6.408)	(17.90)
Observations	10417	1814	10416	1813
R-squared	0.362	0.128	0.355	0.105

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

APPENDIX 2A

Table 3.10: Likelihood of Working in Private Practice after 5 Years

VARIABLES	(1) Private Practice	(2) Private Practice	(3) Private Practice	(4) Private Practice
Years BA to Law School	-0.00691	-0.00905*	-0.00906	-0.00332
	(0.00448)	(0.00521)	(0.00577)	(0.00674)
Legal Assistant During Time Off		0.0123		
		(0.0763)		
Legal Assistant/Years Off Interaction		0.0181		
		(0.0313)		
Related Work During Time Off Definition 2			0.0737	
			(0.0454)	
Related Defn 2/Years Off Interaction			-0.00746	
			(0.0110)	
Related Work During Time Off Definition 3				0.0425
				(0.0361)
Related Defn 3/Years Off Interaction				-0.0145
				(0.00947)
LSAT %tile Standardized	-0.0248*	-0.0168	-0.0177	-0.0178
	(0.0127)	(0.0156)	(0.0156)	(0.0156)
UG U of Michigan	0.00233	-0.0225	-0.0192	-0.0231
	(0.0244)	(0.0291)	(0.0291)	(0.0291)
UG Other School in Michigan	0.00956	0.00877	0.0124	0.00867
	(0.0351)	(0.0415)	(0.0415)	(0.0415)
UG Ivy League or Seven Sisters	0.0385	0.0707*	0.0723*	0.0693*
	(0.0321)	(0.0392)	(0.0392)	(0.0392)
UG Other Public School	0.0221	0.00739	0.00935	0.00754
	(0.0240)	(0.0283)	(0.0283)	(0.0283)
%Years Employed since LS	0.00235	0.00335	0.00348*	0.00339
	(0.00184)	(0.00211)	(0.00211)	(0.00211)
Has Masters Degree	0.0246	0.0109	0.0159	0.0169
	(0.0407)	(0.0486)	(0.0486)	(0.0486)
Hours Worked per Year	8.12e-05***	7.64e-05***	7.69e-05***	7.67e-05***
	(1.62e-05)	(1.93e-05)	(1.93e-05)	(1.93e-05)
Cohort Indicators	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes
Observations	2662	1871	1871	1871
R-squared	0.044	0.050	0.051	0.050

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

APPENDIX 2B

Table 3.11: First Stage Regressions from Table 3.7

	(1) First Specification	(2) Second Specification: Related Job Defn. 1	(3)	(4) Third Specification: Related Job Defn. 2	(5)	(6) Fourth Specification: Related Job Defn. 3	(7)
VARIABLES	Gap Length	Gap Length	Interaction with Legal Assistant	Gap Length	Interaction w/ Defn. 2	Gap Length	Interaction w/ Defn. 3
Demographic and Undergrad Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
	(0.206)	(0.240)	(0.0400)	(0.227)	(0.200)	(0.227)	(0.217)
Unemployment Rate	-0.157***	-0.291***	-0.00370	-0.229***	-0.0380*	-0.279***	-0.00879
	(0.0552)	(0.0643)	(0.00434)	(0.0614)	(0.0226)	(0.0659)	(0.0306)
Legal Assistant During Time Off		0.892	4.974***				
		(1.372)	(1.584)				
Unemployment Rate/Legal Assistant Interaction		-0.0318	-0.412**				
		(0.180)	(0.208)				
Related Work During Time Off Definition 2				4.029***	9.051***		
				(1.102)	(1.263)		
Unemployment Rate/Related Defn 2 Interaction				-0.335**	-0.810***		
				(0.146)	(0.167)		
Related Work During Time Off Definition 3						3.291***	7.959***
						(0.825)	(0.837)
Unemployment Rate/Related Defn 3 Interaction						-0.194*	-0.668***
						(0.108)	(0.107)
F-Statistic (Excluded Instruments)	8.12	10.89	2.17	13.12	12.52	17.11	19.51
P-Value	0.0044	0.0000	0.1138	0.0000	0.0000	0.0000	0.0000
Observations	4406	3410	3410	3410	3410	3410	3410
R-squared	0.376	0.407	0.576	0.445	0.546	0.481	0.547

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

APPENDIX 2C

Table 3.12
First Stage Regressions from Table 3.8
Effects of Time Off on Salary after Five Years

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	First Specification	Second Specification: Related Job Defn. 1		Third Specification: Related Job Defn. 2		Fourth Specification: Related Job Defn. 3	
VARIABLES	Gap Length	Gap Length	Interaction with Legal Assistant	Gap Length	Interaction with Defn. 2	Gap Length	Interaction with Defn. 3
Demographic and Undergrad Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
%Years Employed since LS	0.000776	0.00299	0.00159	0.00715	0.00468	0.00647	0.00519
	(0.00691)	(0.00687)	(0.00104)	(0.00666)	(0.00399)	(0.00640)	(0.00493)
Hours Worked per Year	-0.000170**	-0.000128	-5.60e-06	-9.75e-05	1.34e-06	-8.13e-05	2.75e-05
	(6.98e-05)	(8.81e-05)	(1.11e-05)	(8.57e-05)	(4.37e-05)	(8.49e-05)	(5.32e-05)
Unemployment Year finished BA	0.0613	-0.100	-0.00299	-0.0778	-0.0678**	-0.0975	-0.0587
	(0.0758)	(0.0970)	(0.00511)	(0.0974)	(0.0295)	(0.107)	(0.0405)
Business/Econ Major	-0.312***	-0.383***	0.00527	-0.313***	0.0188	-0.296***	0.0366
	(0.0797)	(0.0941)	(0.00586)	(0.0905)	(0.0195)	(0.0811)	(0.0299)
Legal Assistant During Time Off		-0.274	2.665***				
		(0.972)	(0.609)				
Unemployment Rate/Legal Assistant Interaction		0.0983	-0.102				
		(0.125)	(0.0772)				
Business or Econ Major/Legal Assistant Interaction		0.792**	-0.173				
		(0.313)	(0.275)				
Related Work During Time Off Definition 2				2.564**	5.944***		
				(1.294)	(1.364)		
Unemployment Rate/Related Defn 2 Interaction				-0.139	-0.383**		
				(0.172)	(0.180)		

Table 3.12 Continued

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	First Specification	Second Specification: Related Job Defn. 1		Third Specification: Related Job Defn. 2		Fourth Specification: Related Job Defn. 3	
VARIABLES	Gap Length	Gap Length	Interaction with Legal Assistant	Gap Length	Interaction with Defn. 2	Gap Length	Interaction with Defn. 3
Business or Econ Major/Related Defn 2 Interaction				-0.300	-0.762***		
				(0.291)	(0.252)		
Related Work During Time Off Definition 3						3.746***	6.824***
						(1.056)	(0.936)
Unemployment Rate/Related Defn 3 Interaction						-0.236*	-0.485***
						(0.136)	(0.116)
Business or Econ Major/Related Defn 3 Interaction						-0.310	-0.805***
						(0.243)	(0.202)
F-Statistic (Excluded Instruments)	8.34	4.68	0.87	4.65	4.31	6.53	7.18
P-Value	0.0002	0.0009	0.4813	0.0010	0.0018	0.0000	0.0000
Observations	2578	1807	1807	1807	1807	1807	1807
R-squared	0.344	0.363	0.669	0.399	0.579	0.457	0.597

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

APPENDIX 2D

Table 3.13
First Stage Regressions from Table 3.9
Effects of Time Off on Salary after Fifteen Years

	(1) First Specification	(2) Second Specification: Related Job Defn. 1	(3)	(4) Third Specification: Related Job Defn. 2	(5)	(6) Fourth Specification: Related Job Defn. 3	(7)
VARIABLES	Gap Length	Gap Length	Interaction with Legal Assistant	Gap Length	Interaction with Definition 2	Gap Length	Interaction with Definition 3
Demographic and Undergrad Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
%Years Employed since LS	-0.0112	-0.0177	-0.00110	-0.0197	-0.00650	-0.0189	-0.00716
	(0.0128)	(0.0156)	(0.00140)	(0.0140)	(0.00854)	(0.0135)	(0.00959)
Hours Worked per Year	0.000131	0.000141	6.65e-06	0.000192**	9.40e-05*	0.000170*	7.15e-05
	(9.37e-05)	(9.88e-05)	(1.16e-05)	(9.53e-05)	(5.38e-05)	(8.96e-05)	(6.21e-05)
Unemployment Year finished BA	-0.545***	-0.652***	-0.000295	-0.556***	-0.0769***	-0.632***	-0.0781**
	(0.0903)	(0.100)	(0.00296)	(0.0970)	(0.0213)	(0.105)	(0.0305)
Legal Assistant During Time Off		-0.0487	2.870***				
		(1.028)	(0.901)				
Unemployment Rate/Legal Assistant Interaction		0.121	-0.130				
		(0.130)	(0.106)				
Business/Econ Major	-0.390***	-0.313***	0.0110*	-0.288***	0.0372**	-0.251***	0.0711***
	(0.0996)	(0.0992)	(0.00636)	(0.0900)	(0.0154)	(0.0842)	(0.0222)
Business or Econ Major/Legal Assistant Interaction		-0.424	-0.904***				
		(0.311)	(0.234)				
Related Work During Time Off Definition 2				6.598***	12.34***		
				(1.773)	(1.798)		

Table 3.13 Continued

VARIABLES	(1) First Specification Gap Length	(2) Second Specification: Related Job Defn. 1 Gap Length	(3) Second Specification: Related Job Defn. 1 Interaction with Legal Assistant	(4) Third Specification: Related Job Defn. 2 Gap Length	(5) Third Specification: Related Job Defn. 2 Interaction with Definition 2	(6) Fourth Specification: Related Job Defn. 3 Gap Length	(7) Fourth Specification: Related Job Defn. 3 Interaction with Definition 3
Unemployment Rate/Related Defn 2 Interaction				-0.561***	-1.154***		
				(0.215)	(0.217)		
Business or Econ Major/Related Defn 2 Interaction				-0.690	-1.520***		
				(0.454)	(0.487)		
Related Work During Time Off Definition 3						4.229***	9.775***
						(1.320)	(1.225)
Unemployment Rate/Related Defn 3 Interaction						-0.231	-0.854***
						(0.162)	(0.145)
Business or Econ Major/Related Defn 3 Interaction						-0.464	-0.993***
F-Statistic (Excluded Instruments)	24.03	14.52	3.75	15.53	9.04	17.31	9.93
P-Value	0.0000	0.0000	0.0048	0.0000	0.0000	0.0000	0.0000
Observations	2085	1792	1792	1792	1792	1792	1792
R-squared	0.244	0.275	0.640	0.350	0.584	0.403	0.561

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

APPENDIX 3A

Table 4.8: First Stage Regressions for Parent Saved for Child's College

VARIABLES	(1) Full Sample Parent Saved * Tuition Interaction	(2) Parent Saved	(3) Subsample Parent Saved * Tuition Interaction	(4) Parent Saved
Family Background, Demographic,Major Controls	Yes	Yes	Yes	Yes
Tuition	-1.1014***	-.0017	-1.4767**	-.0017
	(.4085)	(.0016)	(.6063)	(.0020)
Feels Good about Self	-5.9388	-.0698**	2.6427	-.0491
	(7.9717)	(.0311)	(13.616)	(.0457)
Not Enough Control	-6.8303	.0225	-10.1274	.0071
	(5.0008)	(.0195)	(8.5772)	(.0288)
Satisfied Overall	-4.6978	.0251	-16.8251	-.0326
	(7.1380)	(.0279)	(12.7980)	(.0430)
How Far in School	-3.7990	.0011	-4.4141	-.0209
	(2.8304)	(.0110)	(5.9264)	(.0199)
Absences (8th grade teacher)	-12.3413	-.0676	-18.974	.0512
	(19.9405)	(.0778)	(47.6272)	(.1599)
Tardies (8th grade teacher)	4.7236	.1442	26.4436**	.3108
	(28.6991)	(.1120)	(57.8143)	(.1941)
Tardies (8th grade teacher)	-55.7942*	.1025	-126.74	.0228
	(31.3562)	(.1224)	(56.1503)	(.1885)
Absences (10th grade self rept)	.98974	-.0056**	-.3291	-.0218
	(3.7036)	(.0145)	(6.0950)	(.0205)
Absences (10th grade teacher)	-6.2296	-.0623	-13.4077	-.0785**
	(7.0336)	(.0275)	(11.6154)	(.0390)
Absences (12th grade teacher)	-1.2882	-.0148	-11.5751	-.0614
	(6.5796)	(.0257)	(11.5093)	(.0387)
Tardies (12th grade teacher)	-2.1604	.0143	.7274	.0165
	(5.4232)	(.0212)	(9.5045)	(.0319)
Feels Good about Self Interaction	.00012	.0001	-.0274	-2.91e-06
	(.0001)	(.0001)	(.0419)	(.0001)

Table 4.8 Continued

VARIABLES	(1) Full Sample Parent Saved * Tuition Interaction	(2) Full Sample Parent Saved	(3) Subsample Parent Saved * Tuition Interaction	(4) Subsample Parent Saved
Not Enough Control Interaction	.00005***	.00006	.0720***	.00005
	(.00007)	(.00007)	(.0249)	(.00008)
Satisfied Overall Interaction	.0001***	.0001	.1292***	.0002*
	(.0001)	(.0001)	(.0408)	(.0001)
How Far in School Interaction	-3.90e-06*	-3.90e-06	-.00107	1.71e-06
	(.00005)	(.00005)	(.0189)	(.00006)
Absences (8th grade teacher) Interaction	.0002	.0002	.1505	.0001
	(.0005)	(.0005)	(.1812)	(.0006)
Tardies (8th grade teacher) Interaction	-.0003	-.0003	-.1496	-.0007
	(.0004)	(.0004)	(.1998)	(.0007)
Tardies (8th grade teacher) Interaction	.00058***	.0006	.9117***	.0009
	(.0005)	(.0005)	(.1798)	(.0006)
Absences (10th grade self rept) Interaction	2.98e-06	2.98e-06	.0212	.00008
	(.00005)	(.00005)	(.0184)	(.00006)
Absences (10th grade teacher) Interaction	.00006	.00006	-.0022	.00008
	(.0001)	(.0001)	(.0349)	(.0001)
Absences (12th grade teacher) Interaction	.00002	.00002	.04835	.0002
	(.00009)	(.0001)	(.0352)	(.0001)
Tardies (12th grade teacher) Interaction	-.00006	-.00007	-.0223	-.00008
	(.00008)	(.00008)	(.0283)	(.0001)
F-Statistic (excluded instruments)	4.84	1.74	3.37	1.73
P-Value	0.0000	0.0185	0.0000	0.0204
Observations	1440	1440	800	800
R-squared	0.7684	0.1158	0.7672	0.1337

Standard errors in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

APPENDIX 3B

Table 4.9: First Stage Regressions for Teen Saved for College

VARIABLES	(1) Full Sample Teem Saved * Tuition	(2) Teen Saved	(3) Subsample Teen Saved * Tuition	(4) Teen Saved
Family Background, Demographic, and Major Controls	Yes	Yes	Yes	Yes
Tuition	-.0152	-.0007	.3731	-.0007
	(.5446)	(.0020)	(.8422)	(.0026)
Feels Good about Self	-16.59	-.0320	-24.5045	-.0480
	(10.6301)	(.0382)	(18.8557)	(0580)
Not Enough Control	-5.4451	-.0009	-12.0865	-.0237
	(6.6691)	(.0240)	(11.8581)	(.0365)
Satisfied Overall	-.5757	.0066	7.2340	.0413
	(9.5388)	(.0343)	(17.6938)	(.0544)
How Far in School	-3.3730	-.0141	-7.4376	-.0059
	(3.7581)	(.0135)	(8.2111)	(.0253)
Absences (8th grade teacher)	6.3486	-.0032	-26.8190	-.2910
	(26.5628)	(.0956)	(65.8589)	(.2025)
Tardies (8th grade teacher)	-12.5015	-.1001	33.3265	.0989
	(38.2361)	(.1376)	(80.1560)	(.2465)
Tardies (8th grade teacher)	8.8816	.0253	24.8901	-.0454
	(42.7644)	(.1539)	(82.9731)	(.2552)
Absences (10th grade self rept)	1.903	.0168	2.7631	.0380
	(4.9363)	(.0178)	(8.4778)	(.0261)
Absences (10th grade teacher)	5.5103	.0020	5.1601	-.0563
	(9.3976)	(.0338)	(16.0882)	(.0495)
Absences (12th grade teacher)	2.4467	.0388	4.6117	.0191
	(8.8017)	(.0317)	(16.0143)	(.0493)
Tardies (12th grade teacher)	-7.1611	-.0125	-7.9770	.0190
	(7.2731)	(.0262)	(13.2092)	(.0406)
Feels Good about Self Interaction	.1062***	.0001	.1475**	.0002
	(.0383)	(.0001)	(.0579)	(.0002)
Not Enough Control Interaction	.0148	-.00003	.0407	.00002
	(.0237)	(.00009)	(.0344)	(.0001)
Satisfied Overall Interaction	-.0320	-.00009	-.0745	-.0002
	(.0352)	(.0001)	(.0564)	(.0002)

Table 4.9 Continued

VARIABLES	(1) Full Sample Teem Saved * Tuition	(2) Full Sample Teen Saved	(3) Subsample Teen Saved * Tuition	(4) Subsample Teen Saved
How Far in School Interaction	.0386**	.0001*	.0712***	.0001
	(.0166)	(.00006)	(.0261)	(.00008)
Absences (8th grade teacher) Interaction	-.1996	-.0003	-.0987	.0002
	(.1635)	(.0006)	(.2505)	(.0008)
Tardies (8th grade teacher) Interaction	.1037	.0003	-.2808	-.0005
	(.1275)	(.0005)	(.2763)	(.0008)
Tardies (8th grade teacher) Interaction	.0825	.0002	.0531	.0003
	(.1781)	(.0006)	(.2537)	(.00078)
Absences (10th grade self rept) Interaction	-.0091	-.00004	-.0177	-.0001
	(.0172)	(.00006)	(.0256)	(.00008)
Absences (10th grade teacher) Interaction	-.0962***	-.00019	-.0899*	-.00005
	(.0341)	(.0001)	(.0483)	(.00015)
Absences (12th grade teacher) Interaction	-.0120	-.0001	-.0589	-.0002
	(.0329)	(.0001)	(.0488)	(.0002)
Tardies (12th grade teacher) Interaction	.0954***	.0002*	.1243***	.0001
	(.0267)	(.0001)	(.0393)	(.0001)
F-Statistic (excluded instruments)	2.11	0.74	1.97	1.00
P-Value	0.0020	0.7971	0.0053	0.4605
Observations	1440	1440	800	800
R-squared	0.5227	0.0395	0.4711	0.0643

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

APPENDIX 3C

Table 4.10: First Stage Regressions for Took out Student Loans Indicator

	(1)	(2)	(3)	(4)
	Full Sample		Subsample	
VARIABLES	Loans * Tuition	Loans	Loans * Tuition	Loans
Family Background, Demographic and Major Controls	Yes	Yes	Yes	Yes
Tuition	.4649	-.0018	-.7257	-.0008
	(.5270)	(.0019)	(.8140)	(.0024)
Feels Good about Self	-4.1999	-.0266	-.1699	-.0639
	(10.2342)	(.0361)	(18.1404)	(.0545)
Not Enough Control	9.4948	.0106	12.7528	.0438
	(6.4499)	(.0228)	(11.5060)	(.0345)
Satisfied Overall	1.4052	-.0312	-14.6854	-.0139
	(9.16443)	(.0323)	(17.0876)	(.0513)
How Far in School	-10.9697***	-.0468***	-18.8367**	-.0388
	(3.5664)	(.0126)	(7.9288)	(.0238)
Absences (8^{th} grade teacher)	11.6842	.3437***	1.0491	.4190**
	(25.7989)	(.0910)	(63.9340)	(.1919)
Tardies (8^{th} grade teacher)	-7.0802	-.0912	-60.3681	-.3281
	(37.1393)	(.1311)	(77.6153)	(.2330)
Tardies (8^{th} grade teacher)	-46.1811	-.3027**	-49.2369	-.1279
	(39.2116)	(.1384)	(75.3782)	(.2263)
Absences (10^{th} grade self rept)	-5.7278	-.0161	-8.4267	-.0368
	(4.7156)	(.0166)	(8.0384)	(.0241)
Absences (10^{th} grade teacher)	3.2966	.0613*	-2.2201	.0211
	(9.0318)	(.0319)	(15.5197)	(.0466)
Absences (12^{th} grade teacher)	15.8304*	.0215	23.2968	.0310
	(8.4685)	(.0299)	(15.4217)	(.0463)
Tardies (12^{th} grade teacher)	-9.8509	.0140	-4.0522	.0400
	(6.9572)	(.0245)	(12.7116)	(.0382)
Feels Good about Self Interaction	.0253	.00009	-.0027	.0001
	(.0371)	(.0001)	(.0562)	(.0002)
Not Enough Control Interaction	-.0831***	-.0001	-.0845**	-.0002
	(.0230)	(.00008)	(.0335)	(.0001)
Satisfied Overall Interaction	-.0547	-.00006	.0292	-.00006
	(.0340)	(.0001)	(.0547)	(.0002)
How Far in School Interaction	.0597***	.0002***	.0691***	.00007
	(.0160)	(.00006)	(.0253)	(.00008)

Table 4.10 Continued

VARIABLES	(1) Full Sample Loans * Tuition	(2) Loans	(3) Subsample Loans * Tuition	(4) Loans
Absences (8th grade teacher) Interaction	.0265	-.0007	.0128	-.0009
	(.1590)	(.0006)	(.2433)	(.0007)
Tardies (8th grade teacher) Interaction	-.1691	-.00005	.0291	.0004
	(.1240)	(.0004)	(.2683)	(.0008)
Tardies (8th grade teacher) Interaction	.5251***	.0013**	.4412	.0008
	(.1715)	(.0006)	(.2415)	(.0007)
Absences (10th grade self rept) Interaction	.0381**	.00006	.0141	.00004
	(.0166)	(.00006)	(.0246)	(.00007)
Absences (10th grade teacher) Interaction	-.0291	-.0002*	.0027	-.00006
	(.0329)	(.0001)	(.0468)	(.0001)
Absences (12th grade teacher) Interaction	-.1407***	-.0002	-.1417***	-.0001
	(.0319)	(.0001)	(.0472)	(.0001)
Tardies (12th grade teacher) Interaction	.0987***	.00008	.0480	-.00004
	(.0257)	(.00009)	(.0380)	(.0001)
F-Statistic (excluded instruments)	3.64	2.42	1.77	1.20
P-Value	0.0000	0.0003	0.0159	0.2437
Observations	1460	1460	810	810
R-squared	0.5145	0.1467	0.4746	0.1851

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

APPENDIX 3D

Table 4.11: First Stage Regressions for Loans>Tuition

	(1)	(2)	(3)	(4)
	Full Sample		Subsample	
VARIABLES	Loans>Tuition * Tuition	Loans>Tuition	Loans>Tuition * Tuition	Loans>Tuition
Family Background, Demographic and Major Controls	Yes	Yes	Yes	Yes
Tuition	-.3013	.0005	.4157	.0013
	(.3975)	(.0024)	(.4853)	(.0029)
Feels Good about Self	-1.4134	-.0308	5.4347	-.0773
	(7.4316)	(.0442)	(10.3408)	(.0615)
Not Enough Control	-1.9313	-.0120	-10.1624	-.0463
	(5.2064)	(.0309)	(7.4499)	(.0443)
Satisfied Overall	-.6934	-.0088	-9.4527	.0289
	(6.9779)	(.0415)	(10.6901)	(.0636)
How Far in School	-5.1066	-.0251	-5.6858	.0149
	(2.7792)	(.0165)	(4.8640)	(.0289)
Absences (8th grade teacher)	-21.5927	.2007**	-20.9611	.2598
	(16.6725)	(.0991)	(30.1293)	(.1793)
Tardies (8th grade teacher)	-10.8849	.2883	-4.9267	-.0666
	(30.2186)	(.1796)	(58.7925)	(.3498)
Tardies (8th grade teacher)	-10.0121	-.0682	1.2870	-.0237
	(42.3584)	(.2518)	(51.2538)	(.3050)
Absences (10th grade self rept)	-4.8892	-.0531**	-2.2745	-.0354
	(3.5768)	(.0213)	(4.9772)	(.0296)
Absences (10th grade teacher)	7.4077	.1062**	-.8512	.0321
	(7.0988)	(.0422)	(9.9727)	(.0593)
Absences (12th grade teacher)	4.3489	-.0118	-.8381	-.0014
	(6.7052)	(.0399)	(9.7342)	(.0579)
Tardies (12th grade teacher)	1.7668	-.0287	-.2370	-.0642
	(5.7851)	(.0344)	(8.1883)	(.0487)

Table 4.11 Continued

	(1)	(2)	(3)	(4)
	Full Sample		Subsample	
VARIABLES	Loans>Tuition * Tuition	Loans>Tuition	Loans>Tuition * Tuition	Loans>Tuition
Feels Good about Self Interaction	.0043	-.00004	.0193	.0001
	(.0267)	(.0002)	(.0339)	(.0002)
Not Enough Control Interaction	-.0087	.00001	.0197	.0001
	(.0197)	(.0001)	(.0242)	(.0001)
Satisfied Overall Interaction	.0285	.0002	.0290	.00006
	(.0248)	(.0001)	(.0354)	(.0002)
How Far in School Interaction	.0118	-.00002	.0075	-.0001
	(.0113)	(.00007)	(.0154)	(.00009)
Absences (8th grade teacher) Interaction	.2906***	-.0002	.3504***	.0002
	(.1027)	(.0006)	(.1255)	(.0007)
Tardies (8th grade teacher) Interaction	.3872***	.0002	-.0246	.00002
	(.1080)	(.0006)	(.1625)	(.0010)
Tardies (8th grade teacher) Interaction	-.0049	.0002	-.0471	-.0001
	(.1206)	(.0007)	(.1344)	(.0008)
Absences (10th grade self rept) Interaction	.0161	.0001	.0006	.00005
	(.0121)	(.00007)	(.0154)	(.00009)
Absences (10th grade teacher) Interaction	-.0492**	-.0002	-.0330	-.00005
	(.0239)	(.0001)	(.0292)	(.0002)
Absences (12th grade teacher) Interaction	-.0272	-4.92e-06	-.0211	-.0001
	(.0246)	(.0001)	(.0310)	(.0002)
Tardies (12th grade teacher) Interaction	.0097	.0002*	.0094	.0002
	(.0199)	(.0001)	(.0238)	(.0001)
F-Statistic (excluded instruments)	2.17	1.63	1.32	0.85
P-Value	0.0015	0.0336	0.1502	0.6685
Observations	780	780	450	450
R-squared	0.1111	0.3254	0.1477	0.3837

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

APPENDIX 3E

Table 4.12: First Stage Regressions for Student Earned Grants

	(1)	(2)	(3)	(4)
	Full Sample		Subsample	
VARIABLES	Grants * Tuition	Grants	Grants * Tuition	Grants
Family Background, Demographic and Major Controls	Yes	Yes	Yes	Yes
Tuition	-.8506*	-.0024	-1.6046**	-.0027
	(.5135)	(.0018)	(.7781)	(.0024)
Feels Good about Self	-3.6402	.0260	-4.1657	-.0030
	(9.9887)	(.0346)	(17.3827)	(.0539)
Not Enough Control	14.0049**	.0400*	19.3382*	.0325
	(6.2847)	(.0217)	(10.9984)	(.0341)
Satisfied Overall	3.2135	.0056	6.2358	.0546
	(8.9351)	(.0309)	(16.3585)	(.0507)
How Far in School	-13.3664***	-.0516***	-24.0422***	-.0506**
	(3.4790)	(.0120)	(7.5849)	(.0235)
Absences (8th grade teacher)	-.9115	-.0191	-30.3615	-.2080
	(25.1354)	(.0870)	(61.0965)	(.1895)
Tardies (8th grade teacher)	2.6674	.0562	-55.0963	-.0729
	(36.1784)	(.1252)	(74.1839)	(.2301)
Tardies (8th grade teacher)	-50.0527	-.1572	-77.0007	.0116
	(38.2004)	(.1322)	(72.0350)	(.2234)
Absences (10th grade self rept)	-1.8275	-.0001	-9.5476	.0109
	(4.5963)	(.0159)	(7.6852)	(.0238)
Absences (10th grade teacher)	-7.1179	.0425	-12.7662	-.0165
	(8.8165)	(.0305)	(14.8480)	(.0461)
Absences (12th grade teacher)	11.3289	.0265	17.4321	.0496
	(8.2776)	(.0286)	(14.7752)	(.0458)
Tardies (12th grade teacher)	-7.9839	-.0043	-5.3742	.0040
	(6.7807)	(.0235)	(12.1484)	(.0377)
Feels Good about Self Interaction	.0024	-.00008	-.0150	-.00008
	(.0365)	(.0001)	(.0543)	(.0002)
Not Enough Control Interaction	-.1026***	-.0002**	-.1281***	-.0001
	(.0224)	(.00008)	(.0320)	(.0001)
Satisfied Overall Interaction	-.0320	-.00005	-.0454	-.0002
	(.0335)	(.0001)	(.0531)	(.0002)
How Far in School Interaction	.0939***	.0002***	.1088***	.0002**
	(.0157)	(.00005)	(.0243)	(.00008)

Table 4.12 Continued

VARIABLES	(1) Full Sample Grants * Tuition	(2) Full Sample Grants	(3) Subsample Grants * Tuition	(4) Subsample Grants
Absences (8th grade teacher) Interaction	.1482	.0004	.1827	.0008
	(.1549)	(.0005)	(.2326)	(.0007)
Tardies (8th grade teacher) Interaction	-.2079	-.0003	.0212	-.0001
	(.1208)	(.0004)	(.2564)	(.0008)
Tardies (8th grade teacher) Interaction	.3894**	.0005	.4950**	.0002
	(.1671)	(.0006)	(.2309)	(.0007)
Absences (10th grade self rept) Interaction	.0336**	.00007	.0510**	.00003
	(.0162)	(.00006)	(.0236)	(.00007)
Absences (10th grade teacher) Interaction	.0818**	-.00006	.0826*	.00009
	(.0321)	(.0001)	(.0448)	(.0001)
Absences (12th grade teacher) Interaction	-.0770**	-.00008	-.0898**	-.0001
	(.0311)	(.0001)	(.0451)	(.0001)
Tardies (12th grade teacher) Interaction	.0598**	.00004	.0581	.00003
	(.0251)	(.00009)	(.0364)	(.0001)
F-Statistic (excluded instruments)	4.47	1.74	3.02	0.77
P-Value	0.0000	0.0182	0.0000	0.7622
Observations	1450	1450	800	800
R-squared	0.5462	0.2225	0.5435	0.2107

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

APPENDIX 3F

Table 4.13: First Stage Regressions for Student Participated in Work-Study

VARIABLES	(1) Full Sample Work-study * Tuition	(2) Full Sample Work-study	(3) Subsample Work-study * Tuition	(4) Subsample Work-study
Family Background, Demographic and Major Controls	Yes	Yes	Yes	Yes
Tuition	-1.1417**	-.0038***	-2.2113***	-.0060***
	(.4825)	(.0013)	(.7256)	(.7256)
Feels Good about Self	-6.6046	-.0026	-1.1612	-.0024
	(9.3859)	(.0244)	(16.2103)	(.0392)
Not Enough Control	5.7246	.0245	11.1990	.0338
	(5.9054)	(.0153)	(10.2566)	(.0248)
Satisfied Overall	2.2532	.0091	4.0392	.0134
	(8.3958)	(.0218)	(15.2551)	(.0369)
How Far in School	-5.6532*	-.0219**	-13.4368*	-.0249
	(3.2690)	(.0085)	(7.0733)	(.0171)
Absences (8th grade teacher)	-18.6092	-.0673	-52.1470	-.1952
	(23.6184)	(.0614)	(56.9756)	(.1379)
Tardies (8th grade teacher)	-5.4728	-.0736	-66.4297	-.1743
	(33.9949)	(.0883)	(69.1803)	(.1674)
Tardies (8th grade teacher)	-24.2337	-.0430	-64.1709	-.1636
	(35.8949)	(.0933)	(67.1763)	(.1626)
Absences (10th grade self rept)	-1.8254	.0057	-8.3136	-.00009
	(4.3189)	(.0112)	(7.1668)	(.0173)
Absences (10th grade teacher)	-4.9845	.0062	-9.2242	-.0023
	(8.2844)	(.0215)	(13.8465)	(.0335)
Absences (12th grade teacher)	8.9599	.0270	11.2279	.0094
	(7.7780)	(.0202)	(13.7786)	(.0333)
Tardies (12th grade teacher)	-6.5215	-.0107	-2.6061	.0109
	(6.3715)	(.0166)	(11.3290)	(.0274)
Feels Good about Self Interaction	.0760**	.0001*	.0316	.00008
	(.0343)	(.00009)	(.0506)	(.0001)
Not Enough Control Interaction	-.0397*	-.00009*	-.0625**	-.0001
	(.0211)	(.00005)	(.0298)	(.00007)
Satisfied Overall Interaction	-.0308	-.0001	-.0219	-.00008
	(.0315)	(.00008)	(.0495)	(.0001)
How Far in School Interaction	.0458***	.0001***	.0666***	.0001*
	(.0147)	(.00004)	(.0227)	(.00005)

Table 4.13 Continued

VARIABLES	(1) Full Sample Work-study * Tuition	(2) Full Sample Work-study	(3) Subsample Work-study * Tuition	(4) Subsample Work-study
Absences (8th grade teacher) Interaction	.2504	.0005	.3774*	.0009*
	(.1456)	(.0004)	(.2169)	(.0005)
Tardies (8th grade teacher) Interaction	.1209	.0004	.3862	.0008
	(.1135)	(.0003)	(.2391)	(.0006)
Tardies (8th grade teacher) Interaction	.2665	.0003	.3766*	.0007
	(.1570)	(.0004)	(.2153)	(.0005)
Absences (10th grade self rept) Interaction	.0211	.00002	.0568**	.00007
	(.0152)	(.00004)	(.0220)	(.00005)
Absences (10th grade teacher) Interaction	.0787	.00007	.0807*	.00007
	(.0302)	(.00008)	(.0418)	(.0001)
Absences (12th grade teacher) Interaction	-.0441	-.00006	-.0467	-1.81e-06
	(.0292)	(.00008)	(.0421)	(.0001)
Tardies (12th grade teacher) Interaction	.0581**	.00008	.0299	2.78e-06
	(.0236)	(.00006)	(.0339)	(.00008)
F-Statistic (excluded instruments)	2.59	1.69	2.12	1.10
P-Value	0.0001	0.0235	0.0020	0.3359
Observations	1450	1450	800	800
R-squared	0.3109	0.1783	0.3203	0.2220

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

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