DO TAX CREDITS FOR PARENTS AFFECT CHILD COLLEGE ENROLLMENT?

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Abstract

This paper estimates the effect of tax credits on college outcomes. I first study effects of college tax credits such as the American Opportunity Tax Credit (AOTC) on child outcomes using changes in benefits at adjusted gross income (AGI) phase-outs combined with the sudden expansion of credits in 2009 under the American Recovery and Reinvestment Act. I find that college tax credits have at most small effects on college enrollment for children from middle- and high-income families relative to the large effects found in the financial aid literature. I also study effects of income tax credits received by parents of adolescents in two ways. I first examine effects of anticipated income variation using nonlinearities in Earned Income Tax Credit (EITC) eligibility with respect to AGI. I find no discernable impact of anticipated income on college enrollment, but cannot reject impacts predicted by prior research. I then examine effects of large, unanticipated income changes at other points in the AGI distribution by exploiting variation in the timing of parental layoffs around childrens' ages of college entry. Unanticipated income losses generate significant, small negative effects on child college enrollment, quality, and other outcomes. Results suggest that income transfers to parents of adolescent children, and the insurance provided by progressive income taxation during these child ages, have at most small impacts on child college outcomes, especially for low-income children.

I Introduction

How do tax credits received by parents affect long-term outcomes of children? Family income transfers such as Child Tax Credits and the Dependent Exemption are a simple way for governments to expand family access to child inputs such as education and health care, and these transfers receive wide, bipartisan support in the US. However, little is known about their long-term effectiveness relative to interventions that only increase family access to specific child inputs such as tax credits for education or health care. The two key reasons this question remains poorly understood are the scarcity of large, quasi-experimental variation in family income, and the scarcity of data sets linking child family characteristics to long-term outcomes.

I provide new evidence on these questions using selected tables drawn from the population of United States tax records spanning 1996-2010. I focus on how tax credits affect college enrollment and college characteristics–long-term outcomes that are more directly tied to adult economic status than earlier childhood outcomes such as test scores or grade point averages. I create a panel data set linking parents to adolescent and post-adolescent children during the years 1996-2009[1]. The analysis data set contains information on parental tax credits, parental layoffs, child college enrollment and college characteristics, family income and marital status, individual earnings of parents and children, housing and neighorhood quality, and geographic mobility.

Part I of this paper estimates the effects of two types of tax credits on child college enrollment directly, using variation in the tax credits. I first construct a new research design to estimate the effects of college tax credits including the American Opportunity Tax Credit (AOTC), Lifetime Learning Credit (LLC), and Tuition Deduction on child college outcomes. College tax credits attempt to improve child outcomes by tying transfers to college expenditures, thereby lowering the effective price of college. I show that the 2009 American Recovery and Reinvestment Act (ARRA) dramatically expanded college tax credits across the income distribution due to the replacement of Hope Scholarships with the much more generous AOTC. In particular, the AOTC increased credits sharply for families just above the old phase-out range but below the new phase-out range. By comparing the time trend of college enrollment for children at precise points in the AGI distribution around the passage of ARRA in 2009, I can assess the impacts of credits on relatively high-income families.

While I find clear evidence of a very sharp break in credit receipts for families just below the new phaseout region starting in 2009, I find that these credits had at best a much smaller impact on enrollment than one would expect based on the financial aid literature. College tax credits are estimated to be at most 1/3 as effective as traditional financial aid and zero effects cannot be rejected. I argue that the reasons for this are most likely that college tax credits differ from traditional financial aid in two key ways. First, they are received after college decisions have already been made and after college bills have already been paid. This impairs their ability to relax liquidity constraints. Second, these credits may be relatively non-transparent and non-salient relative to other components of college cost, program characteristics that have been shown to reduce effectiveness (Bettinger et al 2009).

Part I of the paper also examines effects of cash transfers to parents that are not tied to particular expenditures on children. These programs include Earned Income Tax Credits (EITC),

[1] The length of the panel combined with the fact that children do not enter college until age 18 prevents me from exploring effects of parental layoffs before a child's teenage years.

Child Tax Credits, and Dependent Exemptions. I estimate effects of cash transfers to parents for low-income families, here defined as families with incomes under $40,000, using differences in EITC benefits across the pre-tax income distribution, as in Chetty, Friedman and Rockoff (2011, henceforth CFR). The idea is to exploit nonlinear differences in EITC benefits while controlling for smooth effects of pre-tax income on college enrollment.

As in CFR and much prior work, I find that realized EITC benefits are large, about 20% of income for some low-income families. However, whereas CFR find visual evidence that these income transfers improve early childhood test scores, I do not find any evidence that such transfers received just before college ages improve college enrollment. Part 2 of the paper, discussed below, suggests two explanations why no effects are found: families only devote a small fraction of additional income to child college investments, and low-income children do not rely heavily on parental income to finance college. Instead, low-income children finance college with financial aid, their own earnings, and student loans. This stands in contrast to child inputs at earlier ages such as school quality, health care, and nutrition, which are financed primarily out of parental income across the entire income distribution.

Part II of this paper turns to a different source of variation in family income to assess likely effects of income tax credits on long-term child outcomes: parental layoffs. Parental layoffs provide large, more unanticipated income variation than the EITC during this period. Moreover, parental layoffs provide income variation across the entire income distribution, not just for the low-income families eligible for the EITC. This is critical because middle- and high-income parents receive a large share of all tax credits for parents.

The estimation strategy in Part II is to exploit sharp variation in the timing of fathers' layoffs with respect to child age. I focus on layoffs of fathers for consistency with prior literature on effects of layoffs, and because the average family depends on father's earnings significantly more than mother's earnings, thereby increasing statistical power. I employ a difference-in-difference (DD) research design, implemented non-parametrically in an event-study framework.

I show that paternal layoffs induce large, sudden reductions in transitory and permanent income for millions of families in the US. Some fathers experience layoff before children reach a particular college decision, such as whether to enroll at age 19. Other fathers experience layoff after this decision has already been made. These two types of fathers share many unobserved characteristics of workers picked by managers for layoff, and therefore provide plausible treatment and control groups. Several studies have examined effects of paternal layoffs on long-term child outcomes

(Oreopoulos et al 2008, Rege et al 2011, Bratberg et al 2008, Coelli 2011, Kertesi and Kezdi 2007, Shea 2000), but none of these studies have been able to exploit the full potential of the timing approach to study long-term outcomes. Most have relied exclusively on cross-sectional variation across fathers who do and do not suffer layoff, raising concerns about selection into layoff.

I document that layoffs have large effects on family earnings, income, and consumption, consistent with prior research (e.g., Jacobson et al 1993, Wachter et al 2009). I document that progressive taxation provides significant 20% insurance of family income against father's earnings losses. Because permanent income and housing consumption both fall by similar amounts after layoff, the results are consistent with an interpretation of layoffs as unanticipated, permanent income shocks in a basic lifecycle model.

I then measure the effects of these income losses on child college outcomes. Event studies adapted to child outcomes show that fathers' layoffs reduce child college enrollment by 0.4 (standard error 0.2) percentage points per year during ages 18-22, or about 1% of base mean annual enrollment. Children are also 1.9% less likely to attend college out of their home state, 1.4% less likely to attend a four-year college, 1.1% less likely to attend non-public colleges, and attend colleges with 0.3% lower alumni earnings. In addition, children are more likely to work after paternal layoff, both during their adolescent and college-age years. These effects suggest that the benefits of cash transfers to parents, and of insurance provided to parents by progressive taxation, can be calculated by focusing on their direct benefits to parents: the externality on child college outcomes is not a large part of this benefit.

All of these effects on childrens' college and earnings, with the exception of adolescent earnings, are larger and more significant for females. This result mirrors findings in prior research on effects of financial incentives on educational outcomes, including the effect of financial aid on college enrollment (Dynarski 2008). The effects are also larger for higher-income children. I present evidence in support of two explanations for this pattern: higher-income children suffer larger income losses from layoff, and higher-income children rely more heavily on parental income to finance college.

How large are these effects on children? The effects are 10% of the cross-sectional correlation between college enrollment and permanent family income (shown in Figure 1.a), implying that most of this correlation is due to factors other than post-adolescent income, such as income at earlier ages or family preferences and abilities. My effects are also about 10% of the point estimates that I obtain when using firm closure to instrument for parental layoff, the most common strategy for overcoming endogeneity of parental layoff in prior research. I replicate these results in my data and show they are due to selection into firm closure. Though my effects are therefore "small," they are consistent with the large price effects found in the financial aid literature (Deming and Dynarski 2012) if families reduce college contributions by 3% of their permanent income loss from layoff, and children respond to family college contributions in the same way they respond to financial aid. In the cross-section, families do in fact appear to spend 2-5% of marginal permanent income on college contributions.

Finally, I perform a variety of tests to confirm that fathers' layoffs reduce childrens' college enrollment primarily due to income losses, rather than due to other effects of layoffs such as declines in parental health or mental well-being (Wachter 2009, Carroll 2007, Kassenboehmer and Haisken-DeNew 2009). The tests all strongly support the income interpretation. The results in this paper therefore may have implications for US tax policy toward children. In 2008 the US federal government oversaw at least $60 billion or 2.6% of federal revenues in transfers to parents of children ages 12-18, largely through tax credits. These transfers include programs such as the Child Tax Credit, the EITC, and the Dependent Exemption. These transfers are not pure cash transfers because they are tied to parental earnings and income. But unlike college tax credits or other forms of financial aid, these credits are not tied to expenditures on children. The results here suggest that such cash transfers to parents of older children are not an effective way to increase child college enrollment. This is just one possible benefit of these programs, and not necessarily their primary goal. But the finding should be kept in mind when shaping policies that explicitly seek to increase college enrollment. This is particularly true given the large estimated effects of financial aid found in other studies. The fact that I find small, significant effects of family income on child college outcomes in this paper is consistent with these findings of large effects in the financial aid literature, because it is likely that parents allocate a very small fraction of marginal income to spending on college.

The paper proceeds as follows. Section II describes the data. Section III estimates effects of college tax credits on child college enrollment. Section IV estimates effects of anticipated income tax credits on college enrollment. Section V develops parental layoffs as a source of large, unanticipated variation in family permanent income across the entire income distribution, and documents the important role played by progressive taxation in providing insurance against such shocks. Section VI estimates the effects of income losses on child college outcomes. Section VII verifies that that layoffs affect children primarily through family income, rather than through other parental resources that are not related to income. Section VIII shows that methods used by prior researchers to estimate effects of family income on long-term child outcomes yield incorrect results due to selection problems. Section IX concludes.

II Data and Summary Statistics

II.A Description of Key Variables

The data contain selected variables from form forms filed by individuals over the years 1996-2009. Variables taken from the 1040 include AGI, EITC benefits, college tuition deduction claims, marital status, residential location, and other taxes paid and rebates received. Form 8863 contains information on the Lifetime Learning Credit (LLC), the Hope Credit, and the American Opportunity Tax Credit (AOTC) starting in 2004, including the benefits received and the number of household members eligible for such benefits. I only observe these variables when families file taxes.

The selected tables also contain variables from third-party-reported sources, or "information returns," for 1999-2009, that are filed by institutions rather than individuals. Formal-sector earnings and deferred compensation are obtained from the W2 form, state unemployment insurance from Form 1099G, disability insurance and social security payments from Form 1099-SSA, college enrollment from Form 1098T, mortgage interest payments from Form 1098, and interest payments from banks from Form 1099-INT.

A student is defined as enrolled in college in a tax year if an institution has filed a 1098T form on that child's behalf in that tax year. All post-secondary educational institutions in the US that are eligible for federal financial aid, i.e., all Title IV institutions, are required to file a 1098T form for every student paying any tuition, and many schools file 1098T's for students paying zero tuition. Title IV institutions cover virtually all colleges and universities as well as vocational schools and other post-secondary institutions (Internal Revenue Service 2013).

My analysis data set raises three issues worth a brief discussion, though none of them represent serious problems for the research designs. The first issue is that I use unemployment insurance (UI) benefit collection to identify layoffs when studying effects of unanticipated income losses. UI take-up rates are about 72-83% in the US (Currie 2006), and I show that layoffs affect parental earnings and consumption in similar ways to layoffs in prior research (Lalonde, Sullivan 1993, Wachter, Song, Manchester 2009, Chetty and Szeidl 2007).

Second, this paper focuses on children who can be linked to fathers. In matching fathers to children, I discard 35% of children who I cannot match to fathers through the 1040 form: 10% that are only claimed by mothers, and 25% that are claimed by no adults or by too many adults to identify the father with confidence. Most low-income parents are eligible for substantial EITC benefits and therefore file taxes (Athreya *et al* 2010). The children who I fail to match have unstable families or families that claim different children in different years. Unsurprisingly, the children I fail to match to parents have lower college enrollment rates than the matched children. I show below that exclusion of some lower-SES children from my sample most likely imparts an *upward* bias to my estimated effects of anticipated and unanticipated income. The bias is not relevant to effects of college tax credits in this paper, because the research design for college tax credits only examines higher-income families.

The final issue is that the 1098T form reflects enrollment for the vast majority of students in the US, but not all students. In Appendix 5 I document the extent of this problem, discuss why it is unlikely to bias my results on effects of unanticipated income substantially, and perform two robustness checks using alternative measures of college enrollment. One alternative measure relies on parental claiming of children over age 18, and one measure restricts to colleges that file 1098T forms for an extra subset of students. Both measures generate treatment effects consistent with those reported in the body of the paper.

II.B Four Samples: Layoffs, Average, Survivor, Closure

The section of the paper that exploits variation in unanticipated family income relies on four samples of fathers drawn from the US population. The samples vary in the years in which events can occur, but all of them cover child outcomes from 1999-2009, and contain parental variables covering 1996-2009.

- Layoffs. The layoff sample contains 100% of fathers who experience an event defined as a "layoff." I define a "layoff" as occurring in year T if a father receives positive unemployment insurance (UI) in year T, and receives zero UI in the prior year $T-1$[2]. The no-UI-in-prior-year restriction serves two purposes: it assures that the layoff spell begins in the current year, and it eliminates many repeated short-term layoffs followed by recall, since such layoffs generate UI in consecutive years after the first layoff. I focus on fathers for consistency with prior research, and because layoffs are closer to pure income shocks for men than women due to fathers' lower elasticity of labor supply.

- Survivors. Survivor fathers experience "survival" in year T if they work at a firm that lays off at least one father at T. They also must receive zero UI at $T-1$, to match this restriction on

[2] UI take-up rates among eligible workers in the US are about 72-83% (Currie 2006).

Layoff fathers. By this definition, there are no survivors for workers who lose their job in firm closure, and there are no survivors at firms with no workers in the Layoff sample. Workers are often survivors at some firm in every year they are observed. For example, every worker at a very large firm will be a "survivor" in every year, because very large firms will likely have at least one Layoff father every year. For computational reasons, I therefore take a 30% random sample of survivors. The Survivor sample is propensity-score reweighted to match the Layoff sample on pre-event characteristics[3]. I choose reweighting rather than regression as a method of controlling for these pre-event differences for reasons of convenience[4].

- Closures. Fathers experience "closure" in year T if they work at a firm in year T that never issues another W2 to any worker after year T. I impose the same restrictions described in Oreopoulos *et al* (2008) in order to compare my results with results presented in that paper for fathers in Canada, using Canadian administrative tax records. This sample is described in more detail in Appendix 2.

- Average. "Average" fathers are a 30% random sample of the US population of fathers. Average fathers experience a placebo event at T generated by a random variable, imposing the restriction of positive earnings and no UI-takeup at $T-1$. I use this sample mainly to replicate prior research on firm closures.

Note that if a father has N children and suffers K events (layoffs, placebos, survivals, or closures), he enters the data separately NK times. This means that "parent-based" samples are "adult-based" samples weighted by NK[5].

[3] The propensity score is estimated on the fraction of the displacing firm that takes up UI, fixed effects for two-digit NAICS industry of displacing firm, fixed effects for three-digit zipcode of displacing firm, gender, whether the worker has any self-employment income 1996-1999, whether the worker had any deferred compensation in 1999, whether the worker had any mortgage interest payments in 1999, fixed effects for average number of children claimed by worker 1996-1999, fixed effects for age of wife at child's birth (no mother found all coded with same age), fixed effects for age of father in year of layoff, marital status of father 1996-199 interacted with quartic in total family income 1996-1999, year of layoff interacted with firm size in year prior to layoff interacted with quartic in earnings of father in year prior to layoff.

[4] The main results are unchanged when controlling for observable variables using regression rather than propensity-score reweighting.

[5] I ignore the implied clustering issues. Clustering at the father level only affects the estimated sampling error in the grouped means I use in most of my analysis, and I ignore this sampling error because it is reduced to almost nothing by my very large sample size.

II.C Summary Statistics and Cross-Sectional Correlations

Table 1 displays summary statistics for children at age 19 (the age of highest college enrollment in the data) in the Layoff and Survivor groups. Each sample is described by three columns: one column for children who reach age 19 pre-event, one for children who reach age 19 post-event, and one column containing the percent change from pre-event to post-event. The Layoff sample shows large differences in family resources but small differences in child outcomes at age 19. The Layoff sample is fairly affluent due to selection of households headed by middle-aged men with children and with enough labor-force attachment to experience layoff. The reweighted Survivor sample matches the Layoff sample well in the pre-event period, but displays much higher paternal earnings after the event, as expected. Finally, the Closure sample exhibits significantly higher pre-event earnings than other samples, most likely due to the stronger sample restrictions applied there. Closures appear comparable to "layoffs" as defined here, in terms of effects on earnings. The Average sample shows very small differences by timing of event, except for child earnings, which have a strong secular downward trend that has been documented elsewhere (Aaronson *et al* 2007).

Figure 1 plots the dramatic cross-sectional effects of family income on long-term child outcomes. Figure1.a plots college enrollment at Age 19 by mean parental income from 1996-1999 in the Survivor sample. The relation is fairly linear, with a slope of about 0.5 percentage points of enrollment per $1,000 of family income. Figure 1.b shows the analogous slope for total years enrolled in college over ages 18-22 is about .025 years of college per $1,000 of family income. The slopes of these graphs serve as benchmarks for interpreting the magnitude of the effects found below. They also help to validate the sample, because they are consistent with the cross-sectional effects implied in prior work using the National Longitudinal Survey of Youth (author's calculations combining reported statistics in Bailey and Dynarski 2011 and Belley and Lochner 2007). Appendix 1 provides an additional validation of my sample by showing that its income distribution lines up exactly as would be expected with comparable samples in the 2001 American Community Survey.

Figure 1.c plots child earnings at age 27 by family income, and Figure 1.d plots the extensive margin of earnings at age 27 by family income. Several years after college investments have typically concluded, children of higher-income parents are much more likely to work in the formal sector, and have much higher earnings. Some of these differences in earnings likely stem from the much larger college investments achieved by higher-income children. Indeed, standard returns to schooling, combined with the higher quality of schools attended by higher-income children, can explain a

substantial fraction of young adult earnings inequality. If public policies could narrow these gaps in human capital and labor market outcomes by improving outcomes for low-income children, they would significantly increase both national wealth and equality in future generations.

Part I

Estimated Impacts of Tax Credits on College Enrollment: Variation from Tax Credit Eligibility

III Price Effects: College Tax Credits

There are three main tax credits for college in the federal tax system. The Hope Credit and LLC began with the Taxpayer Relief Act of 1997 (Internal Revenue Service 2012)[6]. The tuition deduction began in 2002. Starting in 2009, Hope Credits were replaced by AOTCs. Data were available for tax years 2004 to 2008. Hope credits provided up to $1,800 of credits for the first two years in college by covering the first $1,200 of costs and 50% of the next $1,200 of costs. Hope Credits only covered tuition and fees, not room, board, books, or other expenditures. LLCs provided up to $2,000 of credits after the first two years in college by covering 20% of the first $10,000 of costs. The tuition deduction reduces taxable income by up to $4,000, which over this period could be worth up to $1,120 in the highest tax bracket eligible for the deduction. The deduction is "above the line" in that it can be claimed by taxpayers who do not itemize their deductions. All of these subsidies were non-refundable. They therefore provided no subsidies to taxpayers with low incomes and zero or negative tax bills. All of these subsidies also phased out at AGI cutoffs that varied across married and single families.

In 2009, ARRA dramatically expanded college tax credits across several dimensions by expanding the Hope Credits (renamed AOTCs). AOTCs have rendered all the other college subsidy programs obsolete for virtually all taxpayers[7]. AOTCs cover 100% of the first $2,000 of college costs, and 25% of the next $2,000. Costs can now include books in addition to tuition and fees. Up to $1,000 of benefits are now refundable. AOTCs can be used for the first four years enrolled at least half-time in a degree-granting higher educational program, not just the first two. Finally, phase-

[6] Further details of these credits can be found in Chenevert (2009), upon which this section draws extensively.

[7] The one exception is for taxpayers in Midwestern Disaster Areas, who receive expanded Hope credits that are more generous for some taxpayers than the new AOTCs.

outs shifted up from \$50,000/\$100,000 for single/married families under Hope to \$80,000/\$160,000 under AOTC.

Table 2 displays the total dollar value and number of recipients for these three programs among families with children in college between ages 18-22, by tax year. Hope Credits dominate all other credits, especially starting in 2009 under ARRA. Total federal spending on college through tax credits for children in this age group more than doubled with the passage of ARRA, from \$6.5 billion to \$13.5 billion.

Figure 2 shows the distribution of benefits by modified AGI (MAGI), before and after ARRA in 2008 and 2009, respectively, for all three credits, for children in college ages 18-22. Figure 2.a display credit receipt for children with parents who file jointly, and Figure 2.b for children of parents who file as single. Starting in 2009, LLC and tuition deduction credits become much less important, as virtually all children in this age range claim the expanded AOTC instead.

The key empirical question I address here is whether college tax credits result in higher college enrollment rates. It is important to understand, however, that existing college tax credits are very different from traditional financial aid. While college tax credits and financial aid both reduce the price of college, college tax credits are received after families have to pay bills for college. This means that college tax credits cannot address liquidity constraints. It also means that college tax credits may not enter perceptions of college price in a salient way at the time of college decisions. Both of these considerations lead to the expectation that effects of college tax credits are likely smaller than effects of traditional financial aid.

III..1 Identification Approach: Time-Series Identification from Credit Expansion under ARRA in 2009

The key obstacle to measuring the effects of college tax credits is that they depend explicitly on parents' AGI and marital status, both of which may affect child college outcomes independently, and in ways that change over time.

The passage of ARRA in 2009 provides a way around these obstacles. Children of joint-filer parents with MAGI below \$160,000 experienced a dramatic increase in access to college tax credits starting in 2009, while filers with MAGI above \$180,000 did not. Similarly, children of single-filer parents with MAGI between \$50,000 and \$80,000 gained a large increase in college credit access, while filers with MAGI above \$90,000 did not. If college tax credits affect enrollment, then I might expect to see enrollment of children in these difference regions of the MAGI distribution going to college at different rates starting in 2009.

One potential problem with this approach is that children make college decisions about half-way through the tax year, and I do not observe family expectations of being above or below the key MAGI cutoffs at this point in time. I therefore must develop proxies for these expectations, and proxies involve measurement error. I develop two approaches to this problem.

First note that predictions of MAGI in year T based on polynomials of MAGI lags, past parental earnings lags, and other prior information in IRS data have very high R-squared, in the range of 0.8. This prediction fails to incorporate a lot of idiosyncratic information available to families but not in IRS data, as well as all information in the first half of year T. It therefore seems likely that families can predict their end-of-year MAGI with very high precision at the time of college decisions.

I therefore proxy for family MAGI predictions with actual MAGI, and focus on families with MAGI not "too close" to the cutoff, so that the vast majority of families on either side of the cutoff predict their tax credit eligibility with high precision. This approach uses actual MAGI as a proxy for the MAGI that is expected when making college decisions. This seems reasonable in light of the high R-squared on income predictions that can be achieved with a small subset of information available to families.

Figure 3 plots total college tax credits by tax year for children ages 18-22, for families above and below the upper MAGI phase-out ranges, separately for children of single and joint filer parents. This is the "first stage" in the quasi-experiment. It shows that these two groups receive vastly different amounts of college tax credits starting in 2009. The sudden jump in tax credits for children in college is about $1,400 for joint filers and $1,000 for single filers. The financial aid literature broadly suggests that $1,000 of financial aid tends to increase enrollment by about 2.6 percentage points (Deming and Dynarski 2012).

Figure 4 replicates Figure 3 but replaces college credits for children in college with college enrollment rates for all children. This is the "second stage" in the quasi-experiment. The graphs suggest small enrollment effects for children in these MAGI ranges. The estimates imply[8] an enrollment increase from ARRA of 1.0 [0.3] percentage points for children of joint filers, and of 0.70 [0.40] percentage points for children of single filers. This is about 25-35% of the effect I would expect from the financial aid literature. This difference could stem from the low relative salience of future

[8]The estimates here come from regressions of the difference between enrollment of the treatment and control groups on a linear time trend and a dummy for years after 2008.

college tax credits when college decisions are made, or from liquidity constraints preventing some families from spending credits before they have been received. It could also stem from measurement error in realized MAGI as a proxy for expected MAGI.

This conclusion is qualitatively robust to other choices of the MAGI windows above and below the phase-out ranges. It is also largely robust to the use of predicted MAGI to group families rather than realized MAGI, with the exception that an enrollment response consistent with the financial aid literature cannot be rejected for single filers, due to the much smaller size of the treatment. The overall pattern suggests a treatment effect on college that is not zero, but is substantially less than the effect that would be predicted based on studies of financial aid, most likely due to the low salience of the credits, the small change in value of the credits relative to the incomes of affluent families near the phase-out region, and to measurement error in my proxy for families' expected credits early in the tax year.

IV Anticipated Income Effects: The Earned Income Tax Credit[9]

Following CFR I attempt to identify the effects of family income tax credits on child college outcomes using non-linearities in the Earned Income Tax Credit (EITC). This approach allows families to differ with respect to pre-tax income in a smooth fashion. For families receiving higher EITC benefits, pre-tax income yields higher post-tax income. This can be seen in the EITC benefit schedule depicted in Figure 5. For families with one child and taxable income below $10,000, an additional $1 of taxable income generates much more than $1 of post-tax income. For families with one child and taxable income between $10,000 and $16,000, an additional $1 of taxable income generates $1 of post-tax income. And for families with one child and taxable income exceeding $16,000, $1 of taxable income generates less than $1 of post-tax income. While the resource gains from marginal taxable income vary sharply across this income distribution, it is plausible that the selection effects from sorting into $1-higher taxable income do not vary sharply across this income distribution. Therefore, controlling for pre-tax income should capture this selection effect, while leaving variation in post-tax income that is exogenous to family characteristics that affect college enrollment.

We therefore test whether children in families that benefit from the EITC schedule go to college more than would be predicted from their pre-tax income alone. While the EITC provides useful variation in this respect, it also has two major problems. The first problem is that EITC benefits

[9] This section draws heavily on the SOI white paper of Chetty, Friedman, and Rockoff (2011).

are anticipated and may therefore be smoothed into consumption levels prior to receipt. The second problem is that EITC variation only affects very low-income families, and these families may rely more or less heavily on parental income to finance child college investments.

The key difference between the empirical exercise here and that in CFR is that while CFR measure contemporaneous effects of family income on test scores early in childhood, I measure effects of adolescent and contemporaneous family income on college enrollment. Based on findings in CFR, Dahl and Lochner (2011), and Stabile and Milligan (2011), the expected direct effect of family income tax credits on child college enrollment should be about 0.04 percentage points per $1,000 of tax credits[10]. It is important to realize that I am therefore most likely looking for a very small effect relative to the large trends in college-enrollment with respect to pre-tax family income. Another difference between this paper and CFR is that I expand the analysis to all US states, rather than focusing on the single state for which CFR matched tax records to student test scores.

The EITC is one of the largest anti-poverty transfer programs in the US, and it distributes virtually all of its benefits to parents. Benefits are allocated as a subsidy for family earned income. Benefits are phased in at high negative tax rates, then remain constant after a cutoff, and then are phased out gradually. Benefits as a function of AGI therefore exhibit a pyramidal shape. In 2009 families with one dependent could claim a maximum of $3,050 of credits, while families with two dependents can claim a maximum of $5,036 of credits. Credits fall to zero around $40,000 of income.

Children are eligible to be claimed under the EITC if they remain either under age 19 or full-time students under 24 for the entire tax year. One question is whether most of these credits go to families of younger or older children. My matched sample allows me to partially address this question. Figure 6 plots average EITC benefits per child by age, after restricting the sample to families with incomes under $80,000. The figure shows that younger children receive higher EITC benefits than older children. This may be an advantage of the EITC if contemporaneous income matters more at younger ages, as some research suggests (e.g., Carneiro and Heckman 2003). However, it is also possible that transfers to adolescent children increase readiness for college, and that transfers to college-age children increase parental ability to finance college. I will address this question in more detail below.

The EITC also plays an important insurance role for many families in the US. This is somewhat

[10]These authors estimate $1,000 of family income to raise child test scores by about .08 SD, and CFR estimate 1 SD of test scores to increase child college enrollment by about 0.5 percentage points, implying $1,000 of family income should increase college enrollment by about 0.04 percentage points.

counterintuitive, in that the EITC is an earnings subsidy and therefore might be expected to fall when earnings fall, thereby amplifying rather than insuring earnings volatility. However, the EITC is a proportional earnings tax over the phase-out range, and therefore provides insurance over this range, like all proportional and progressive taxation, in the sense that it buffers income against earnings changes from year to year. Overall, the EITC provides insurance because many more families have earnings that place them on or just above the phase-out range than families that have earnings placing them on the phase-in range. Figure 7 shows this dramatically. For earnings up to the blue line, the EITC amplifies earnings shocks. In contrast, for earnings between the red line and the green line, where vastly more families are located, the EITC provides insurance against earnings shocks. As is clear, the EITC amplifies earnings shocks for only a few small subset of families. This additional insurance benefit of the EITC is an example of the more general insurance provided by the progressivity of the entire tax system. The value of this insurance for children is an important question addressed below where I turn to variation from parental layoffs.

IV..2 Identification Approach: Cross-Sectional Identification from Policy Nonlinearities

The key assumption in the analysis is that determinants of child college enrollment depend on pre-tax family income in a relatively smooth way. This assumption means that if children receiving credits enroll in college at higher rates than I expect based on their pre-tax income, I can attribute their extra achievement to the tax credits.

Figure 8 shows EITC benefits at in the prior year for children ages 18-22 by AGI in the prior year, and college enrollment ages 18-22 also by AGI in the prior year. Whereas CFR observed a jump in child test scores where EITC benefits reached their peak, no such jump is visible for college enrollment. However, it is also clear that college enrollment in this sample is not perfectly stable with respect to AGI. Regressions of college enrollment on EITC benefits, controlling for a flexible function of AGI, cannot reject a hypothesis that EITC benefits increase enrollment by the small amount prior research would suggest. In contrast to the case of test scores studied by CFR, the estimates for college enrollment are highly dependent on the flexibility permitted to the AGI function. This suggests that the amount of nonlinear variation in EITC benefits is too small to identify college enrollment effects with any robustness using this strategy.

Part II

Estimated Impacts of Tax Credits on Enrollment: Unanticipated Income Variation

The EITC provides small, anticipated variation in family income for families toward the bottom of the income distribution. As discussed above, there are reasons to think that while family income may be more important during early childhood for low-income families, it may be less important as a direct determinant of college enrollment. Moreover, most tax credits are not refundable and therefore primarily target middle- and upper-income families. Therefore, it is useful to examine variation in family income across the entire income distribution.

It is also useful to examine income variation that is unanticipated. This is true for two reasons. First, unanticipated, permanent income variation shifts contemporaneous consumption in a fashion similar to a change in permanent income over the child's entire upbringing, which is what tax credits such as the EITC effectively do. Second, unanticipated income variation is useful because it allows measurement of an important potential benefit of the insurance provided by progressive income taxation. Though progressive taxation is often viewed as transferring resources from higher-income individuals to lower-income individuals, it also transfers resources from luckier states of the world to less lucky states of the world within individual lives, and this insurance function therefore benefits all taxpayers. If this insurance function also increases long-term child outcomes, these benefits will affect the optimal degree of progressivity in the tax system.

V Effects of Unanticipated Income on Family Income and Consumption

I first document that layoffs have large effects on family income and consumption, and that progressive taxation plays a key role in reducing the size of these income losses relative to the decline in father's pre-tax earnings. I follow Jacobson *et al* (1993) in estimating the dynamic effects of layoffs on parental outcomes using an event-study design. I use reweighted Survivor fathers to remove year-of-outcome effects, and Layoff fathers to estimate dynamic effects of layoffs[11].

Let t_O index the year in which an outcome is observed, and t_E index the year in which an event is experienced. Define $k \equiv t_O - t_E$ as "period" or "years after event." Let $g \in \{T, C\}$ index whether

[11] Throughout the analysis, the use of Survivor families to remove trends in the Layoff sample is important. In appendix 4, I show that results are similar, though for child outcomes much less precisely-estimated, without the use of Survivors as controls.

a family experiences a "treatment event" T or a control event C where the treatment event is layoff and the control event is survival. Note that variation in k comes from variation in both t_O and t_E.

I estimate an event study model that allows effects of treatment and control to vary by period. The coefficients on the period*treatment interaction terms can be combined linearly to form a variety of difference-in-difference estimators for the effects of layoff over time. The event study model is:

$$y_{g,t_O,t_E} = \alpha + \sum_{j=k_{\min}}^{k_{\max}} \beta_j^T \cdot I\{g = T, k = j\} + \sum_{j=k_{\min}}^{k_{\max}} \beta_j^C \cdot I\{g = C, k = j\} + \Gamma X_{g,t_O,t_E} + u_{g,t_O,t_E}, \quad (1)$$

$$| \quad k_{\min} < 0 < k_{\max}$$

where α is a constant, β terms are coefficients on the period dummies, X_{t,t_W} is a vector of observable covariates, Γ is a vector of coefficients, and u_{t,t_W} is an independently-distributed error term.

I run this regression on data collapsed into (t_O, t_E) cells, which is the level of treatment variation for parents, and run regressions on this collapsed data. Robust standard errors on data grouped at the (t_O, t_E) level are almost identical to those obtained by using micro data and clustering at the (t_O, t_E) level, as long as the group sizes are the same orders of magnitude, which is true in this application.

Figure 9.a plots β_k^T and β_k^C separately for father's earnings around year of layoff with no additional controls (empty X_{t,t_W}). Mean earnings are close in levels and trends prior to layoff. Starting in the year of layoff, earnings of Layoff fathers fall by a large amount relative to earnings of Survivor fathers. Five years after layoff, recovery is only partial and appears to be slowing down, suggesting permanently lower earnings for Layoff fathers. Figure 9.b plots the difference between the two lines in Figure 9.a, and similar differences for UI and post-tax income. Define this single difference in each period as $\Omega_k \equiv \beta_k^T - \beta_k^C$. For all other results on parents, I plot the estimated Ω_k by period k. This is simply the difference in means across Layoffs and Survivors by period. Changes in Ω_k around $k = 0$ answer the intuitive question: "How much higher is the outcome for survivors than it is for their laid-off colleagues, before and after the layoff?" For a pair of k values k_1 and k_2 such that $k_1 < 0 < k_2$, the term $\Omega_{k_2} - \Omega_{k_1}$ is a traditional difference-in-difference estimator for the effect of layoff.

Figure 9.b shows that layoffs increase UI claims by $\$5,000$ in the year of layoff, but UI drops off to almost nothing within two years. On average, therefore, layoffs induce only temporary entry into the unemployment insurance system. Average paternal earnings fall by $\$15,000$, after starting at a very comparable level with the reweighted survivor sample, converging to a long-run decline of $10,000. Post-tax family income falls by $10,000 at first, converging to a long-run decline of $6,000. In addition, DI rises by $300 and wife's earnings rise by $600 (not shown).

Progressive taxation is a very important source of insurance. Two years after layoff, taxes have fallen by almost $2,800, and five years after layoff taxes are still $1,900 lower than they were before layoff. This implies that progressive taxation is providing about 20% earnings insurance, both in the short-run and the long-run. Note that layoffs are just one source of income uncertainty. Overall income variation is large, and while some of this is most likely anticipated, some of it is likely unanticipated and not well-insured. With constant relative risk-aversion of 2, for example, 20% insurance of a 20-year earnings stream that is $50,000 in 50% of cases and $35,000 in 50% of cases raises the value of that earnings stream by nearly 2% of mean total consumption. Progressive taxation therefore may increase the value of total output substantially just by providing insurance. The value of this insurance will be even higher if layoffs and other income shocks adversely affect human capital investments of children, a question addressed below.

Figure 10.a estimates equation (1) for mobility, showing that layoffs increase both "local" moves defined as moves across zipcodes but within states, and "distant" moves defined as moves across states (the figure normalizes both series to start at zero for ease of comparison) [12]. Figure 10.b estimates the event-study for two measures of housing consumption: mortgage interest payments, and neighborhood quality as reflected in the average home value in a family's zipcode as of 2000. Following layoff, families reduce mortgage interest payments and move to zipcodes with less expensive homes. The long-term decline in mortgage interest payments is about 7%. However, only 70% of families have mortgages prior to layoff, so this suggests a decline in expenditures on housing that is very close to the percent decline in permanent income. All of these treatment effects are highly correlated with actual and predicted earnings losses from layoff, in the expected directions (not shown).

Table 3 summarizes the estimated DD impacts of layoffs on parental outcomes at one-year and five-year time horizons. The estimates are consistent with the figures discussed above. The table

[12] Note that housing variables all pick up the year before layoff because layoffs occuring after January can induce moves before April, when tax returns are filed, and can therefore show up as moves one tax year prior to layoff.

shows that layoffs can be thought of as unanticipated negative income supplements that reduce both short-term and long-term income and that families respond by reducing their spending on housing, and moving to worse neighborhoods. It also shows that progressive income taxation and mothers' baseline earnings provide the bulk of insurance against fathers' earnings shocks, while DI and changes in mothers' earnings provide smaller but still detectable insurance, especially as families adjust to the new lower level of fathers' earnings.

The results in Table 3 and the preceding figures paint a clear picture. Total, permanent post-tax family income falls by 10% following a paternal layoff, and would fall by an additional 2% were it not for the insurance provided by progressive federal taxation. Average housing consumption falls proportionally one-for-one with permanent post-tax family income. This picture is consistent with a view that layoffs are permanent, unanticipated, uninsured earnings shocks in a lifecycle framework in which households have not accumulated large savings at the time of the shock, and in which households accomodate short-term income losses by reducing more flexible expenditures. If liquidity constraints are not operative, then such a shock will induce spending levels that would result from shifting a family's entire (past and future) income profile down by about 10%. If liquidity constraints operate, than the short-run effect on spending will be even larger, reflecting the larger 15% transitory income decline rather than the 10% permanent income decline. It is therefore reasonable to worry that layoffs may cause parents to reduce contributions for childrens' college, and that taxation may provide insurance that is even more valuable than previously thought.

VI Effects of Unanticipated Income on Child College Outcomes

VI.A Identification

The identification strategy for child outcomes is very similar to that used for parent outcomes. The key difference is the introduction of an age dimension into the data. Whereas parental outcomes are not critically different across ages, child's college outcomes vary dramatically and systematically by age. I also restrict ages to 18-22 for college outcomes. This means that much of the variation in period k will come from variation in event-year t_E rather than outcome-year t_O. Let a index age, and write the estimating equation for children as

$$y_{a,g,t_O,t_E} = \alpha + \sum_{j=k_{\min}}^{k_{\max}} \beta_j^T \cdot I\{g = T, k = j\} + \sum_{j=k_{\min}}^{k_{\max}} \beta_j^C \cdot I\{g = C, k = j\} + \Gamma X_{a,g,t_O,t_E} + u_{a,g,t_O,t_E} \quad (2)$$

$$| \quad k_{\min} < 0 < k_{\max}.$$

The only variables I include in X_{a,g,t_O,t_E} are dummies for event-year and cohort where cohort equals $t_O - a$[13]. As above, DD estimators are then constructed with linear combinations of the β terms. The key identifying assumptions for traditional DD are parallel trends in outcomes prior to events and orthogonality of the period effects with the error term. The orthogonality condition embeds the "no-manipulation" assumption that parents do not carefully control the timing of their layoffs, the "no-anticipation" assumption that parents do not reduce spending on child inputs before layoffs occur in anticipation of future income losses, and the "no-contemporaneous shocks" assumption that layoffs are not caused by, for example, large parental health shocks. All of these assumptions are plausible and I provide evidence to support them as well as various robustness checks below.

The key advantage of the event-study approach taken here is that it permits arbitrarily large pre-event differences in child college outcomes across Layoff and Survivor families. For example, if fathers who experience layoff have more trouble gaining job seniority or performing well on the job for various reasons, and these characteristics affect whether children of such fathers attend college, those differences will be removed by the event study approach. The DD approach is not critical when measuring effects of layoffs on parental outcomes because I can mechanically control for pre-event differences in these outcomes. However, I do not observe college enrollment or other

[13] I assume that Layoff and Survivor families have equal cohort and event-year effects, but different event-age effects to account for the treatment effects of Layoff. Identification of the key parameter of interest (the $\{\Gamma\}$ terms defined below) is still possible if we allow separate cohort effects across F and S groups, or separate event-year effects across F and S groups, but not both. Including this richer set of controls does not substantially change the results.

When assumed equal across groups, event-year and cohort effects do not change the point estimates of the Ω or Γ terms. This is a consequence of the weighting scheme, the key identity, and the exact overlap across Layoff and Survivor groups in cohort and event-year. Adding controls for event-year and cohort does not have any effect on the linear combinations of moments (averages of DD's) used to solve for the parameter of interest, $\Omega_{k_1} - \Omega_{k_2}$. Any variable that is differenced out of these linear combinations can be added to the regression for a "free" efficiency gain. This is similar to a case in which we regress y on x, and we have a variable z for which $x \perp z$ and $\tilde{}\,(y \perp z)$. In this case, adding z to the regression increases efficiency for the estimated coefficient on x without changing it's value. This case often arises in experiments where x is randomly assigned. In my case, x is independent of z not through random assignment, but mechanically as a consequence of the data structure.

The efficiency gained from including cohort and event-year controls in the model with outcome levels can be almost replicated by estimating a model of outcome *differences* across Layoff and Survivors within event-year by cohort cells, without any additional covariates in the regression. (The Ω_k point estimates are identical, with slight differences in standard errors, which I think are due to degree-of-freedom adjustments.)

long-term child outcomes before children turn 18, and therefore if layoffs occur before 18 I cannot control for pre-event differences in these outcomes directly. The DD approach solves this problem.

VI.B Results

Figure 11a plots estimated child college enrollment effects by period for children of Layoff and Survivor fathers. Figure 11b plots the difference across Layoffs and Survivors by period in child college enrollment. These figures are analogous to the figures shown for fathers' earnings in Figure 9, but with the outcome variable changed from fathers' earnings to children's college enrollment. Figure 11 is appealing for several reasons. First, the difference betwen Layoff and Survivor children appears roughly constant prior to these events, suggesting stable differences between Layoff and Survival children prior to the event. Second, the treatment from -1 to 0 is about half the size of the treatment from -1 to 1. This is reassuring because only about half of layoffs in period 0 occur before tuition and fee payments are due for period-0 Fall semester enrollment. Third, the difference between Layoff and Survivor children rises toward its pre-event level over the many years following layoff. This suggests that layoffs impose short-term constraints that dissipate over time. However, several factors affect the slope of the line following layoff, including some that do not have useful economic interpretations. Therefore this pattern must be interpreted with caution.

Table 4, Column 1 displays estimates of short-run treatment effects of layoff defined as $\Gamma_{1,-1}$ separately at ages 18-22 as well as pooling all these ages into a single regression. These estimates rely on the Assumption $A2$. Effects are negative at all ages, as expected, and about 1% of enrollment when pooling all ages, as discussed above for Figure 13. Column 2 shows this same treatment effect for the child's family income at age a, rather than her college enrollment. Children whose fathers were laid off prior to college have over $\$8,500$ or 15% lower post-tax family income in the year they attend college than children whose fathers are laid off after they attend college. Column 3 shows the reduction in college enrollment that would be predicted based on this layoff-induced fall in family income, using the cross-sectional correlation between family income and college enrollment. This "prediction" is an interesting benchmark, but I expect it to be a large over-prediction because the cross-sectional correlation captures effects of income at every age up to the present, as well as effects of many variables correlated with both family income and child achievement. Column (4) converts the estimated "causal" effects into percent of these "cross-sectional effects," divided Column (1) by Column (3).

The effect of a father's layoff on child college enrollment is very small. The effects are about

10% of the cross-sectional effect. This captures the simple fact that layoffs reduce family income enormously, but only reduce college enrollment by a small amount. This implies that most of the cross-sectional effect is driven by family income at earlier child ages, or from other factors that correlate with family income such as family endowments and preferences. The effect is about 1% of base college enrollment. Under strong assumptions, I predict layoffs to reduce a child's lifetime earnings by $1,000-3,000 dollars. This is a significant externality in families with multiple children, but even in a family with three children it represents at most 15% of the direct earnings loss experienced by the father.

There is a simple explanation for these small impacts of family income reductions on children's college enrollment: the level of parental cash assistance for children enrolled in college is surprisingly insensitive to parental income levels (author's calculations in Sallie Mae and NPSAS survey data). These data suggest that cash transfers of $1,000 to parents can be predicted to increase parental cash contributions by only $40-120. As layoffs reduce family income by about $6,000-8,000, we can expect children to lose only a few hundred dollars of cash transfers from parents per year of potential college attendance. This insensitivity of spending to income can be interpreted as a locally flat Engel curve in parental college contributions. A key implication of the flat Engel curve explanation is that the small impacts of parental income estimated in this paper are fully consistent with large impacts of financial aid and other college price subsidies on children's enrollment, as prior researchers have indeed found when these subsidies are salient and easily-obtained by children.

VI.B.1 Other Outcomes

Table 5 calculates DD treatment effects pooling ages 18-22 on a variety of outcomes, along with t-statistics and base levels. Children experiencing paternal layoff are less likely to attend college out of state and less likely to attend a four-year university. These results suggest that children attend lower-priced colleges, in addition to reducing enrollment. This translates into lower-quality colleges based on the alumni-earnings measure of college quality. To see effects across the distribution of college quality I calculate treatment effects on dummies for enrollment in colleges above various quality cutoffs, e.g., $20,000, $30,000, and so forth up to $60,000. Effects are signficant above the lowest cutoff, suggesting children do adjust on the quality margin along with the extensive enrollment margin. This is consistent with results on enrollment at out-of-state and four-year colleges. It seems more likely that children who would have enrolled in out-of-state and four-year colleges will enroll in cheaper colleges than decline to enroll in college at all.

Children also adjust earnings, though by smaller amounts than college outcomes relative to base levels. Once again, to distinguish extensive and intensive margin responses I calculate treatment effects on dummies for earnings above three cutoffs: \$0, \$2,000, and \$10,000. Results suggest that layoffs push more children to get "real jobs" that yield substantial earnings. These effects are smaller than the college effects, however; treatment effects are all under half of one percent of base levels.

The overall pattern is one of children making small adjustments across a variety of margins. In addition, there are many important adjustment margins that I do not observe. Many college students work as waiters, bussers, bartenders, babysitters, and tutors. Payments in these occupations take place between workers and customers directly and in some cases may facilitate underreporting of income. Second, the average college student consumes about \$10,000 of goods including housing, food, transportation, entertainment, clothing, and vacation (Paulin 2001). It is likely that children respond to lower parental transfers by reducing consumption. Moreover, I do not observe changes in student loans and financial aid. Children therefore have many margins across which they can adjust to spread out any potential reduction in parental assistance after a layoff, and as discussed above this reduction may be small to begin with.

VI.B.2 Robustness

Table 6 presents the results of various robustness checks.

I employ Survivors throughout my analysis in order to increase precision. However it is possible to estimate treatment effects using only Layoffs. In Appendix 3 I derive an estimator that uses only the Layoff sample. Column (1) of Table 6 displays treatment effects using this approach. The estimator yields results that are consistent with those reported above, but are much less precise. The loss of precision occurs because Survivors non-parametrically control for cohort by event-year shocks in the Layoff sample. Cohort shocks occur because of nonlinear secular trends in college outcomes. Event-year shocks occur because selection into layoff on children's college outcomes (and most likely other measures of family achievement) is counter-cyclical: firms only lay off their least productive workers during booms, but lay off higher-productivity workers during recessions.[14] Interactions between cohort and event-year trends are harder to interpret, but turn out to be important relative to the size of treatment effects.

The β_{k_1,k_2} estimators assume parallel trends in outcomes prior to events. When estimating

[14] Mueller (2012) finds a similar pattern for father's pre-layoff earnings.

$\beta_{k_1,-1}$ for some k_1, as I do above, I require parallel trends in outcomes with respect to k for $k < 0$. There is no evidence to reject this assumption for college enrollment on the full sample in Figure 11.b. However, to check this on outcomes other than college enrollment in Table 3, I also estimate treatment effects that allow for linear differential trends in outcomes with respect to k for $k < 0$. This would arise if the line for $k < 0$ in Figure 11.b were not flat, but rather had some non-zero, linear slope. This is a weaker version of the parallel-trends assumption, and can be viewed as a triple-difference estimator. In Appendix 4 I derive formulas for the point estimates and standard errors of such an estimator. Results are presented in Column (2) of Table 6. These estimates are nearly identical, and remain precise.

The β_{k_1,k_2} estimator assumes that families do not reduce spending on college in anticipation of layoff $-k_2$ years ahead of time. I have used $k_2 = -1$, assuming that families do not pre-emptively reduce spending one year ahead of time. It is possible that families anticipate and respond to layoffs one year before they occur. I therefore estimate treatment effects $\beta_{1,-3}$, requiring that families not reduce spending pre-emptively three years before layoffs take place. The results are presented in Column (3) of Table 6. The results on college are nearly identical, while the effect on fraction of children working is no longer significant. There are also substantive reasons to believe that families do not smooth college contributions in anticipation of layoff. First, families adjust spending on housing only after layoffs occur, and the size of the adjustment is similar to the decline in permanent income. This is consistent with effects of layoffs on food expenditures in the PSID (Stephens 2001). Moreover, evidence in Stephens (2004) suggests that families do not incorporate their (limited) idiosyncratic knowledge of future layoff propensities into their spending plans.

The β_{k_1,k_2} estimator assumes that children choose outcomes independently at each age 18-22. This would be violated if, for example, starting college involved a fixed cost, so that marginal costs of continuing after one's first year are relatively low. The opposite extreme assumption is that children make college enrollment decisions for all ages 18-22 at a single point in time, say age 17 or 18. To address this I average outcomes over ages 18-22 and compare this value for children experiencing events before age 18 with children experiencing events after age 22, continuing to use the DD approach above. All of the variation in k now comes from event-time t_E. Column (4) of Table 6 presents the results. The results are noisier but similar. This also suggests that intertemporal substitution of outcomes within the age 18-22 age window, such as delaying college for a year until one's parents recover from layoff, does not account for the treatment effects.

I assume that parents do not carefully postpone being laid-off until their children have enrolled in college, and that layoffs are not driven by time-varying shocks to other parental resources such as illness or divorce. I test this by examining results of mass layoffs. I define mass layoffs for firms that employ over 30 workers in the period $k = -1$ just before layoff, and in which at least 20% of workers claim UI in period $k = 0$. This is not a perfect test, but mass layoffs are driven somewhat less by idiosyncratic factors than the average layoff. Column (5) of Table 6 shows that effects of mass layoffs on children are similar to layoffs in the full sample, though once again considerably noisier. The same result will be shown to hold below for firm closures.

I match Survivors to Layoffs using a propensity-score reweighting procedure. I can also estimate the period effects in Equation (2) using regression to control for observables, rather than propensity-score reweighting. I do this only for college, because each outcome requires a separate regression. This approach requires me to estimate coefficients on cohort by event-year dummy variables, then use these coefficients to estimate Equation (2). The estimated treatment effect on annual enrollment during ages 18-22 using this approach is almost identical to the results above: 0.43 percentage points (SE .110).

VI.B.3 Heterogeneity

While the results suggest that insurance against shocks from progressive taxation does not have large effects on children on average, it is still possible that this insurance is important for particular subgroups of children.

Table 7 estimates effects separately by gender, parental income group, and parental bank savings. Table 7.a shows that females drive virtually all the treatment effects. The only significant effect on males is to push them into colleges in their home state. For females, all of the college variables show large and significant effects, including the college quality margin up to the 80th percentile of the college quality distribution for females in college (quality over $40,000). Females also increase earnings, on both the extensive and intensive margins. These much larger and more significant results for females echo findings in many other studies of monetary incentives for academic achievement and college enrollment, summarized in Angrist and Lavy (2008, p. 25-27). The reasons for this disparity are not well-understood.

I now estimate effects separately for higher- and lower-income families. Table 7.b displays estimated treatment effects $\hat{\beta}_{1,-1}$ on other outcomes for families with incomes above and below $40,000. There are no significant effects on low-income families, whereas all the college and labor supply outcomes show significant effects in the expected directions for higher-income families.[15]

Figure 12 examines this pattern in more detail. Figure 12.a displays treatment effects $\beta_{1,-1}$ of fathers' layoffs on family income, grouped by income level prior to events. Layoffs reduce income levels by more in higher-income families.[16] Figure 12.b displays corresponding treatment effects $\beta_{1,-1}$ on child college enrollment, for the same income groups. Treatment effects are close to zero at the lowest incomes, rise steadily, and may begin to decrease at the highest incomes.

Figure 12.c displays the college effects as percentage points of enrollment per $1,000 of income lost, and reveals a striking U-shaped pattern of treatment effects. This U-pattern is unlikely to arise if layoffs mainly affect children through non-income factors such as father's becoming depressed after layoffs. In contrast, an income-loss channel provides a clear explanation. At low incomes, children do not rely as heavily on parental cash contributions to finance college. In the Sallie Mae data, children with family incomes below $35,000 finance only 19% of college expenses out of parental resources (loans, income, and savings), compared to 41% for families with incomes between $35,000 and $100,000, and 61% for families with incomes above $100,000 (Sallie Mae 2011). Low-income children who do find a way to attend college make up for the lower parental contributions with greater financial aid, student loans, and student earnings. They also attend lower-cost colleges. Lower-income children may also compensate for lower parental cash contributions in other ways that we cannot observe in this data set, such as consuming fewer goods and services while enrolled in college.

Income losses can also explain the decline in treatment effects as incomes continue to rise past $60,000. There are two leading explanations. The first is that families view college as an investment and face liquidity constraints, as in Becker (1994). The market for private student loans was active in the U.S. over the sample period, suggesting that subsidized federal Stafford loans did not fully meet demand. The interest rates on private loans for college–for the subset of students who qualified–were often much higher than interest rates on collateralized debt such as home mortgages (Delisle 2012). Under liquidity constraints, low-income parents allocate transfers

[15] There is a concern that this pattern could arise spuriously from weaker father-child links at lower incomes, even within the constraints imposed by my parent-child matching algorithm. To examine this I restricted the sample to children only ever claimed by one adult, that adult being the father, rather than my normal restriction of only being claimed by at most one male and one female adult. This did not change the pattern displayed in the figure. I also checked the probability of being claimed by this one adult at ages before 18 across income levels, and found that this probability is 85% at the very lowest income levels, but quickly rises above 90% for incomes over $10,000. Therefore even low-income children only claimed by one adult–the father receiving the layoff–most of whom are claimed almost every possible year by that one adult, do not exhibit an enrollment response to father's layoff.

[16] Note that DD treatment effects at base incomes far from the mean exclude mean-reversion due to the use of Survivors.

to children in the form of human capital investments. As incomes, transfers, and human capital investments rise, the return on human capital declines to the interest rate on financial assets, at which point parents allocate marginal transfers in forms other than human capital. This means that as incomes rise, the marginal change in transfers from parents to children is less likely to reduce spending on college.

The second reason why effects on college might decline at higher incomes is that families view college partly as a consumption good, and spending on this good becomes a smaller fraction of total spending at higher incomes, e.g., the Engel curve in logs declines with income (Mulligan 1997). For example, a middle-income family that spends 20% of its budget on college will reduce college spending by $20 out of a $100 income loss, while a family that spends 10% of its budget on college wil reduce college spending by only $10 out of a $100 income loss. For college, a natural explanation for declining Engel curves is that each child only needs to enroll in one college, and tuition is bounded by institutions.[17] Therefore the fact that college becomes less sensitive to income shocks at higher incomes cannot distinguish between investment and consumption mechanisms.

The ideal test to distinguish consumption and investment mechanisms would be to vary current and permanent income losses from layoff. Unfortunately, these two variables are too highly correlated to identify their separate effects. A more feasible test is to examine treatment effects separately for families with high and low financial wealth, as in Zeldes (1989). For this exercise, I restrict the sample to families with pre-layoff incomes above $40,000. Table 7.c shows effects of parental layoffs on various child outcomes for families with pre-layoff interest income above and below $500,[18] corresponding to an asset cutoff of about $10,000-$25,000 if interest rates on savings are 2-5%. The effects on child college outcomes in these two groups are not statistically different on an absolute or per-dollar basis.[19] These results provide no evidence of liquidity constraints among middle-to-high-income families, and suggest that parents at these income levels view marginal college expenditures as consumption.

VI.B.4 Adolescent Outcomes

I can also use this approach to study effects of parental layoffs on adolescent outcomes. I implement this by restricting the sample to ages 14 to 17, rather than 18 to 22. I drop ages below 14 because most states restrict child labor supply prior to age 14. I examine impacts on various measures of

[17] There is some evidence that Engel curves decline with family income in the NPSAS data.

[18] This is about the 80th percentile of interest income for families with incomes over $40,000 in my sample.

[19] Different cutoffs for interest income from $0 up to $3,000 do not change the pattern described here, although confidence intervals get wide as the cutoff gets higher.

adolescent earnings. Table 8 presents the results for the full sample. There is a significant response on the extensive earnings margin: parental layoffs increase the likelihood that children will earn positive formal sector earnings by 1.4%, and raise the probability that children earn at least $2,000 by 1.7%. There are no significant effects on the intensive margin in levels or logs. I also break out adolescent outcomes by gender and family income. Results (not shown) are somewhat more significant on the labor supply variables for males, and for children from higher-income families, but all groups have similar point estimates and are either significant or marginally-significant on these variables.

The finding that parental layoffs raise formal labor market participation by adolescent children suggests that families do not fully insure spending on children against layoffs. The results also suggest an income channel, rather than a psychic stress channel, because while adolescents could escape a stressful family environment in many ways such as spending more time with friends, the fact that they work suggests that they are responding to lower family income. While there is substantial evidence that higher family income reduces child labor supply for families in low-income countries (Edmonds 2007), to my knowledge this finding is the first quasi-experimental evidence for this relationship in a high-income country.

VII Are Layoffs Valid Instruments for Unanticipated Family Income in DD Estimation?

In order to use the results here to make statements about the value of progressive taxation and transfers to parents, it is important to confirm that layoffs are affecting parents and children mainly through an income channel, rather than a psychological channel that is not related to income. The evidence presented above already strongly pushes in the direction of an income interpretation. First, children attend lower-cost colleges after parents are laid off, which suggests an attempt to reduce expenditures. Second, adolescents increase labor supply following parental layoff. These two considerations are hard to explain if layoffs mainly affect children through increased family turmoil that is not related to family income. I now provide additional evidence in favor of the income interpretation.

VII.A Predicted Earnings Losses

I now explore an additional source of evidence on whether income losses explain the main effects. I first explore one measure of economic vulnerability to layoff: father's earnings share. Earnings losses of fathers reduce family income by more, proportionally, when fathers earn a larger share of family income prior to layoff. This observation suggests that if income losses are driving the effects on children, then effects on children should increase in father's earnings share. Father's earnings shares, however, are not randomly-assigned. The two most important components of family income are father's earnings and mother's earnings. As father's earnings increase, family socioeconomic status (SES) rises, and fraction of income lost from the father's layoff should also rise. As mother's earnings inrease, family SES rises, but now fraction of income lost from the father's layoff should *fall*. By examining these two sources of variation separately, I estimate effects of proportional income losses from layoff that should be biased in opposite directions by confounding variation in family SES.

Define father's pre-event earnings share in period $k = -1$ as $\omega \equiv \frac{W_{-1}^{dad}}{W_{-1}^{dad}+W_{-1}^{mom}}$. I first divide the sample into ω bins.[20] I then reweight these bins by mother's earnings W_{-1}^{mom} in order to isolate variation in ω from father's earnings W_{-1}^{dad}, or vice versa in order to isolate variation in ω from mother's earnings W_{-1}^{mom}. For this exercise I restrict the share groups to a range with enough observations to reweight them on W_{-1}^{dad} or W_{-1}^{mom}, and to relatively high incomes due to the finding above that children's college decisions appear unresponsive to layoff in low-income families.

Figure 13 implements this exercise using variation in mother's earnings. Panel (a) shows this variation. As father's earnings share rises from 70% to 100% on the x-axis, mother's earnings fall by $30,000, while average father's earnings remain constant. Panel (b) plots the effect of layoffs on family income by earnings share. The 30 points of earnings share variation yields an additional 6 percentage points of income loss. Panel (c) plots the effect of layoffs on child college enrollment by earnings share. Enrollment declines more in higher-share groups that experience larger proportional income losses from layoff.

Figure 14 repeats this exercise using variation in father's earnings. Now as the shares increase from 40% to 60%, father's earnings rise from $30,000 to $65,000, while average mother's earnings remain constant. Whereas family SES declined in father's earnings share in Figure 13, it now rises in father's earnings share. Despite the different source of earnings share variation that moves family unobservable characteristics in the opposite direction, once again college enrollment declines more in higher-share groups that experience larger proportional income losses from layoff.

These figures can be interpreted as instrumenting for proportional income losses with father's

[20] I do not scatter the points in this graph because it is not possible to obtain equally-sized bins, largely because there is a large mass of mothers at zero earnings.

earnings share. The implied Wald estimator that instruments for proportional income losses with mother's earnings variation in Figure 13 is .12, while the analogous Wald estimator for father's earnings variation in Figure 14 is .14. The Wald estimator obtained on the full sample of layoffs by instrumenting for proportional income losses with a dummy for layoff is 0.1. These estimates therefore provide no evidence to reject the hypothesis that income losses account for the full treatment effects of layoffs on children's college enrollment.

I have focused on father's earnings shares as one factor that predicts income losses from layoff, in order to show how separate variation in mother's and father's earnings can address concerns about endogeneity of predicted income losses. However, this approach only exploits a small fraction of the information available to predict income losses from layoff, and mother's earnings only provide variation in proportional income losses, not absolute income losses. It is possible to obtain much more precise predictions of father's earnings losses, and variation in absolute income losses, by exploiting all pre-event information about fathers. Define a father's proportional earnings loss around an event as $L_{i,g} = \frac{W_{i,g,k_1} - W_{i,g,k_2}}{W_{i,g,k_2}}$ where $k_1 > 0 > k_2$ as before, and define a vector of pre-event variables X_{i,k_2}. These pre-event variables include information about the father's industry, firm, location, wife, and other demographics.[21] I generate comparable earnings loss predictions for all fathers as follows. I first regress $L_{i,T}$ on X_i restricting to the Layoff sample, then separately regress $L_{i,C}$ on X_i restricting to the Survivor sample. This yields estimated coefficient vectors $\hat{\pi}_T$ and $\hat{\pi}_C$, respectively. I then calculate predicted earnings losses under realized and counterfactual events, yielding $\hat{\pi}_T X_{i,T}$ and $\hat{\pi}_C X_{i,T}$ for Layoff fathers and $\hat{\pi}_T X_{i,C}$ and $\hat{\pi}_C X_{i,C}$ for Survivor fathers. I then group fathers by the difference between these two predictions $\hat{D}_i$, where $\hat{D}_i = (\hat{\pi}_C - \hat{\pi}_T) X_i$ for all fathers. These differences capture fathers' vulnerability to earnings losses from layoff, excluding mean-reversion and other movements in earnings that would happen within X_i groups, even if layoff were not experienced.

Figure 15 presents the results from this exercise, where $W_{i,g,k_2} \equiv W_{i,g,-1}$ and $W_{i,g,k_1} \equiv \frac{1}{5}\sum_{j=1}^{5} W_{i,g,j}$, or average earnings one year to five years after events.[22] Figure 15.a graphs total post-tax family income losses $\hat{\beta}_{1,-1}$ by this measure of father's predicted earnings loss $\hat{D}_i$. An additional percentage point of father's earnings loss increases the loss in family income by \$618. Figure 15.b graphs the college enrollment decline $\hat{\beta}_{1,-1}$ against the father's predicted earnings loss $\hat{D}_i$. An additional percentage point of father's earnings loss increases the decline in college enrollment by .04 percentage

[21] Note that $L_{i,g}$ is large across the entire income distribution, and therefore relies on different variation from that explored in the income cuts displayed in Figure 7.

[22] Results are similar for other definitions of post-event earnings W_{i,g,k_1}.

points. Using predicted earnings losses to instrument for income losses yields a Wald estimator for the effect of family income on college enrollment of 0.07 (standard error .012) percentage points of enrollment per $1,000 of income. This is exactly the slope required for income losses to "explain" the entire treatment effect of layoffs on children, and is consistent with the results obtained using father's earnings share variation.

VIII Are Firm Closures Valid Instruments for Unanticipated Family Income in Cross-Sectional Estimation?

Prior work on the importance of family income in college finance has relied on firm closures to address the endogeneity of layoffs (Oreopolous *et al* 2008, Rege and Votruba 2011, Bratberg *et al* 2008, Shea 2001). This approach assumes that displacement via firm closure is exogenous to child long-term outcomes after conditioning on various observable family pre-layoff characteristics. This assumption will be violated if there is selection of firms into closure on firm characteristics and selection of workers across types of firms on unobserved worker quality. There are many reasons why, for example, lower-quality workers may wind up at less-profitable firms, and there exists some evidence that firms with worse workers are more likely to close (Abowd *et al* 1999). One benefit of the approach used in this paper is that I can test this assumption directly[23].

Figure 16 suggests that this strategy is not valid using the event study approach from above. Figure 16.a plots college enrollment of children before and after their fathers experience an event, where the events are "closure" and "non-closure." The non-closure sample is here propensity-score reweighted to match the closure sample on a variety of pre-event observable variables. The figure shows that children of Closure fathers are less likely to enroll in college even several years prior to the closure, and that any increase in this difference after closures take place is too small to see on the scale of this selection effect. Figure 16.b plots the earnings of these children's fathers for comparison, and clearly shows that closure reduces the earnings of fathers substantially, with partial recovery five years later. Table 9 estimates the cross-sectional and DD impacts explicitly in a regression framework and conveys the same message as Figure 16.

Oreopoulos *et al* (2008) also finds differentially larger effects of parental layoff on children in low-income families (Oreopolous *et al* 2008). I can here check if this result is driven by differential

[23] Rege *et al* (2011) perform a similar exercise in Table 5, for the outcome of child test scores in tenth grade. However, they do not use true firm closures but rather firms laying off at least 90% of their workforce, and they also pool this semi-closure group with all firms laying off any workers in their test, presumably due to power limitations. Grogger (1995) also performs a similar exercise to show that cross-sectional estimates of the effect of arrest on future earnings are largely due to selection.

selection into firm closure. Figure 17 plots impacts of fathers' firm closures on children's college enrollment based on the prior approach that only exploits past firm-closures and assumes random-assignment conditional on observable variables, separately by pre-closure family income quartile. These estimates are denoted β. Figure 17 also plots impacts based on the DD methodology used here, removing selection effects as observed by impacts of future firm closures, also separately by pre-closure family income quartile. These estimates are denoted β_{DD}. While the β estimates suggest that closures have larger adverse impacts on low-income children, the β_{DD} estimates suggest that impacts across the income distribution are small and hard to distinguish from each other on this small sample of layoffs (closures account for less than 1% of all layoffs). This pattern clearly suggests that the heterogeneneous effects on children found in Oreopoulos *et al* (2008) may be driven by selection.

IX Conclusion

This paper has measured the impacts of tax credits received by parents of older children on these children's college outcomes. I first exploit variation in college tax credits to suggest that transfers that are tied to spending on college may have positive effects on enrollment among high-income families, although these effects appear much smaller than measured effects of other financial aid programs that are more salient and easily-obtained by children.

I then move on to measuring impacts of credits that are not tied to spending on children, i.e. parental income tax credits. I exploit nonlinearities in the EITC benefit schedule to establish that small, anticipated cash transfers to parents of older children have no detectable effects on college enrollment. I then exploit layoffs of fathers to isolate large, permanent income variation across the entire income distribution. This larger variation allows me to identify the causal effect of marginal unanticipated family income transfers on child college outcomes, which is closely related to impacts of many income tax credits. I find that this effect of these credits on childre is likely positive but small.

I also confirm that the progressivity of the income tax provides substantial insurance against shocks to parental earnings, and thereby helps to reduce the size of the impacts of these shocks on children. While the gains to college enrollment from this insurance in late childhood are small, the insurance may play a more important role in protecting children from income shocks at earlier ages.

Current US tax policies attempt to improve child outcomes using a variety of tools. Some of these tools involve income support and insurance; other tools involve credits for particular child inputs and thereby change the prices that families face for these inputs. In addition, tax credits can target children at different ages, for example pre- and post-adolescence. The results in this paper are fully consistent with prior research that has suggested large potential impacts of parental income tax credits on pre-adolescent children. The results are also surprisingly consistent with prior research showing large impacts of salient and easily-obtained college price subsidies on children's college enrollment. The results therefore imply that these alternative approaches to improving long-term outcomes for children, especially disadvantaged children, are likely far more effective than income tax credits targeting parents of post-adolescent children.

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Appendix 1: Matching Algorithm and Effects on Sample

I first discuss the general logic of the match of fathers and mothers to children and then document the exact routine employed. Linking parents and children in IRS data for my event study and event-age study designs requires care for several reasons. Marital status and children are only reported by filers, and filing is reduced by layoff. Therefore it is important to use information prior to layoffs to match parents and children, a rule I follow with one exception, discussed below. All matches of fathers with children rely on claims from 1996-1998, giving a buffer of two years before the first layoff can occur, in 2000. I also restrict to claims in years before a child turns 18, because after that age claims depend endogenously on child college outcomes for eligibility reasons. Over 90% of matches occur in the first available year, 1996, while virtually all the rest are made in 1997[24]. About 10% of children are only claimed by mothers and therefore excluded from my sample. An additional 25% of children are either never claimed, or claimed by too many different people for my matching algorithm to assign them a single father in all years 1996-2009 with confidence, and therefore removed from the sample. Multiple claimers are a much bigger problem than no claimers, because most low-income parents file taxes and claim children in order to collect large EITC benefits (Athreya *et al* 2010).

Removing children claimed by multiple fathers before age 18, even when a second father claims the child after a first father is laid-off, violates the rule that only pre-layoff information be used in matching children to parents. My match algorithm errs on the side of strong linkages to assure that children are linked with their primary, contempary source of parental support. Measurement error in family linkages can be a minor problem when using parents as background controls in the context of some external treatment, because even a non-contemporaneous parent likely contains a lot of information about fixed characteristics of a child's family background. However, erroneous linkages are a major problem when measuring the effects of *changes* in parental circumstances over time on child outcomes. Even if all of a child's claiming fathers have highly-correlated fixed characteristics, changes in their time-varying characteristics are likely far less correlated. Therefore I err on the side of excluding children claimed by more than one father to assure I have strong parent-child linkages. This restriction eliminates nearly 20% of children ever claimed in IRS records. Unsurprisingly, children claimed by multiple fathers or no father have much lower college enrollment than children claimed by one father (note that college enrollment is observed for all children, both matched and unmatched). To the extent we think income matters more for children in lower-SES families, my estimates may be smaller than estimates for the full population of children. My estimates may also be smaller if layoffs affect children through mechanisms that correlate with divorce and remarriage (the most likely path to multiple claimers) before a child turns 18.

Figure A1.1 shows two simple validations of my algorithm for matching parents and children. It plots the pre-tax income distribution for a random sample of children in my IRS data who are age 14-16 in 2001, compared with two samples of age 14-16 children drawn from the 2001 American Community Survey (ACS). My sample selection criteria cannot be validated exactly in ACS data because it relies on the time dimension of my panel data. I therefore use two ACS samples with income distributions that I expect to "bracket" that of the IRS sample. The main issue is that children in my data are in households that were headed by men at some point during 1996-1998, several years before the year of observation, 2001. These households are higher-SES than average Census households due to their male-headed status, but lower-SES than Census households headed by men in 2001. Figure A1.1.a confirms this intuition when these three income distributions are normalized into PDF's. The income distribution resulting from my linked sample looks very close to what would be expected from ACS data. Figure A1.1.b makes another point: the Average sample of children is smaller (using appropriate sampling weights) than the Census male-headed sample. This is because I exclude children who are claimed by more than one father before age 18, and children who are never claimed on tax returns.

The algorithm is as follows.

1. Make a list all unique pairs of children and claimers in every year in the sample, 1996-2009. Each individual is indexed by a unique identifier. About 95% of individuals currently in the US who were children during this sample period are linked to at least one claimer. Some of the remaining 5% may have arrived as immigrants after age 18.

[24]The claims data are mostly missing in 1998-2000. It is therefore reassuring that many more kids are claimed for the first time in 1996 than 1997, because this suggests that the missing data only cause a tiny fraction of missed linkages.

2. Restrict this list to rows in which the child is under 18, because claims beginning in 18 are typically only valid conditional on college enrollment.

3. Get the sex of all claimers and the unique identifier of each claimer's spouse, if any, in every year. In each year call the claimers "PE's" for "primary earnings," and their spouses "SE's" for "secondary earners."

4. Case 1: Child has only one PE claimer (63.7% of children)

   -Restrict to SE's who claim child largest number of times.

   -If multiple SE's, break tie by selecting SE who claims child first

5. Case 2: Child has exactly one male PE claimer and one female PE claimer (11.4% of children)

   -Assign child this man and this woman as mother and father

6. Discard remaining children (20% of children)

Appendix 2: Firm Closure Sample: Details

I here briefly discuss the sample restrictions of the closure sample I construct in IRS data, which are based on those in OPS.

Firms can only enter the sample if they employ at least 30 workers at time of closure.

One restriction requires that fewer than 35% of workers experiencing closure at T at a particular firm be working at the same firm in a future year, and is intended to remove re-organizations mistakenly identified as closures. This restriction eliminates 45% of candidate firm closures.

Other restrictions require at least two years of zero UI and two years of tenure at the closing firm. The tenure restriction eliminates 35% of workers and the zero-UI restriction eliminates 15% of the remaining workers. To identify closure at T under these restrictions, we need to confirm zero employment at that firm in T+1, no excess bunching of displaced workers at the same firms in T+2, and the tenure and no-UI restrictions in T-1 and T-2. It is also important to note that many spurious closures arise if less than two years are allowed for late updates of the W2 earnings data in IRS records. Imposing all these restrictions require me to limit my sample to closures to 2001-2007. I also impose a restriction that fathers earn less than $150,000 (2009 dollars) in both of the two years prior to layoff, because there is not enough overlap in this region to adequately reweight Non-Closures to Closures.

The resulting closures sample is a 100% sample of workers displaced by closure who take up UI, combined with a 30% random sample of workers displaced by closure who do not take up UI, with appropriate sampling weights.

Table A2.1 shows number of firms that close and their average size, by year of closure, in my sample. While some fairly large firms do close every year, the vast majority of closing firms are small. This leads to the small average size of closing firms.

Table A2.2 displays summary statistics for the Closure and Non-Closure samples, and is analogous to Table 1. The Closure and Non-Closure samples display similar overall patterns, though smaller declines in child earnings because many fewer cohorts are included in this sample due to the computational demands of identifying closures according to the above restrictions.

Appendix 3: Identification Using Only Layoffs

In this appendix I develop an estimator that relies entirely on the Layoff sample.

The challenge is to estimate period effects when event-year and cohort both have effects on outcomes that are large relative to period effects, given that these three variables are linearly dependent[25]. This linear dependence makes it infeasible to estimate period effects while controlling for cohort and event-year fixed effects. I first discuss the approach I take intuitively, and then formalize it using the notation developed above.

Estimating treatment effects requires estimation of potential outcomes under non-layoff for the Layoff children after layoff takes place. Above, I use Survivors for this. Here, I use Layoff children prior to realization of layoffs. This is another form of DD estimator. The first difference is the same: the difference between two moments in the Layoff sample on either side of period $k = 0$. Above, the second difference is between the two corresponding moments in the Survivor sample. Here, the second difference is between two moments in the layoff sample, both of which involve $k < 0$. There are typically a number of candidate pre-layoff differences that can be used to estimate the desired potential outcomes. The approach I develop here pinpoints a particular weighted combination of these differences that addresses the problem of confounding event-year and cohort shocks.

Figure A3.1.a displays average enrollment for the Layoff group at age 19 for three cohorts, each plotted by event-age $a_E \equiv a - (t_O - t_E)$. One option would be to pool all of these cohorts into event-age means, but this throws away a lot of useful information. The key information to exploit is that event-age is collinear with event-year for a fixed cohort. Consider the difference $A - B$ for the 1984 cohort. This is a particular "treatment difference," which can be defined as the outcome at an event-year that takes place after the age of the outcome, minus the outcome at an event-year that takes place before the age of the outcome. For cohort 1984, the points $A - B$ reflect outcomes for children with event-year 2002 minus outcomes for children with event-year 2004. The difference $A - B$ therefore contains both the desired difference in outcomes across children with different event-ages, and a confounding difference across children with event-years 2002 and 2004. We would like to estimate the confounding difference across children with these event-years.

There are many ways to estimate this confounding difference. Any cohort for which both of these event-years occur too late to affect the outcome at age 19 provides an estimate of this event-year effect difference. Define a "control difference" as a difference across outcomes for event-years that occur too late to affect these outcomes. Figure A3.1.a presents two such control differences. The control difference $C - D$ uses the 1986 cohort to estimate the difference across event-years 2002 and 2004. The control difference $E - F$ uses the 1988 cohort to estimate the difference across event-years 2002 and 2004. There are many such control differences. Each control difference yields a different double-difference estimator of the treatment effect. One estimator is $A - B - (C - D)$. Another estimator is $A - B - (E - F)$. The unweighted mean of these double-differences provides one estimate of the difference in event-age effects across event-ages 18 and 20, which is the short-run treatment effect of interest.

This estimate relies on the treatment difference in cohort 1984. There are many other cohorts, each of which offers one treatment difference across event-ages 18 and 20. Each cohort uses a different pair of event-years in its treatment difference, and therefore requires a different set of control differences to remove the confounding event-year variation. Each cohort then yields a different "treatment effect," defined as a treatment difference minus the mean of all available control differences that share the event-years used in the treatment difference. I then take the mean of all these treatment effects.

A similar argument holds for treatment differences that occur within event-years, rather than

[25] The problem occurs when linearly dependent covariates enter a conditional expectation function, and the researcher is primarily interested in effects of a subset of these linearly dependent variabless. Leading examples of this are age, year, and cohort effects in labor economics (Hall, Mairesse and Turner 2005) and age, year, and vintage effects in studies of capital goods (Hall 1971, Berndt, Griliches and Rappaport 1995).

within cohorts. The analogous graph is presented in Figure A3.1.b. I omit the discussion of these estimators to save space; it is conceptually analogous to that just presented. The surprising fact, however, is that the treatment effects that emerge from these two approaches contain independent information. The amount of independent information decreases in the smaller dimension of the outcome-year by event-year matrix. I therefore calculate the complete set of treatment effects and pool them into a single estimate. This approach does count some information multiple times, and therefore overestimates the precision of the final estimate. I ignore this problem.

Write birth cohort as $t_B \equiv t_O - a$. I now rewrite the model in terms of cohort instead of outcome-year. Write the potential child outcome function in terms of a treatment effect, and age interacted with cohort effects and event-year effects, dropping the g subscript because now all observations set $g = T$:

$$y_{a,t_B,t_E} = \alpha + \sum_{j=k_{\min}}^{k_{\max}} \beta_j \cdot I\{k = j\} + \sum_{j=t_B^{\min}}^{t_B^{\max}} \theta_{a,j} \cdot I\{t_B = j\} + \sum_{j=t_E^{\min}}^{t_E^{\max}} \psi_{a,j} \cdot I\{t_E = j\} + u_{a,t_B,t_E},$$

where β_j is the effect of layoff on the outcome, $\theta_{a,j}$ is the effect of cohort j on the outcome at age a, $\psi_{a,j}$ is the effect of selection into layoff in year j on outcome at age a, and u_{a,t_B,t_E} is an error term. A key restriction here is that cohort effects are constant across event-years, and event-year effects are constant across cohorts, within age groups. Without Survivors we have no way to distinguish these interaction terms from event-age effects. This is why the estimates using only Layoffs are much noisier: event-ages are capturing both treatment effects and random cohort by event-year by age interaction shocks.

It is not possible to identify all of the parameters in this model without further assumptions, due to the collinearity $k = t_O - t_E = a + t_B - t_E$. I therefore make an additional selection that period effects not driven by treatment effects are linear:

$$A1 : \beta_j = \phi_{a,0} + \phi_{a,1} k + \sum_{j=0}^{k_{\max}} \lambda_j \cdot I\{k = j\},$$

where $\phi_{a,0}$ and $\phi_{a,1}$ capture the linear trend in period, and λ_k captures treatment effects, assumed to equal zero for outcomes prior to events. This is not a strong additional assumption; it is weaker than the parallel trends assumption $\phi_{a,1} = 0$ used for the main results.

Under these assumptions we identify many different treatment effects using DDs, as previously described. I here characterize the set of these treatment effects. All such DD's consist of one treatment difference that crosses the cutoff where $k = 0$ (e.g.,$a_W = a$, depicted as difference $A - B$), and one control difference that is contained entirely in the untreated region where $k < 0$ (e.g., $a_W > a$, depicted as differences $C - D$ and $E - F$).

Writing event-age in terms of event-year and cohort and fixing age a for simplicity, this set of DD's identifying treatment effects $k = a + t_B^{i_2} - t_E^{i_2}$ years after layoff can be characterized as:

$$\begin{aligned} \Gamma_a\left(t_E^{i_1}, t_E^{i_2}, t_E^{i_3}, t_E^{i_4}, t_B^{j_1}, t_B^{j_2}, t_B^{j_3}, t_B^{j_4}\right) \equiv\ & E\left[y_a\left(t_E^{i_2}, t_B^{i_2}\right) - y_a\left(t_E^{i_1}, t_B^{i_1}\right)\right] \\ & - E\left[y_a\left(t_E^{i_4}, t_B^{i_4}\right) - y_a\left(t_E^{i_3}, t_B^{i_3}\right)\right] \ \text{(DD's in event-age)} \end{aligned} \tag{3}$$

such that

$$1.\ t_E^{i_1} - t_B^{i_1} + 1 \quad < \quad a + 1 \leq t_E^{i_2} - t_B^{i_2} \leq t_E^{i_3} - t_B^{i_3} < t_E^{i_4} - t_B^{i_4} \tag{4}$$

(one treatment difference minus one control difference)

and *either* of the following hold:

$$\text{2A. } t_B^{i_1} \quad = \quad t_B^{i_2},\ t_B^{i_3} = t_B^{i_4},\ t_E^{i_1} = t_E^{i_3},\ \text{and } t_E^{i_2} = t_E^{i_4} \tag{5}$$

(treatment differences removes cohort, control difference removes event-year)

$$\text{2B. } t_E^{i_1} \quad = \quad t_E^{i_2},\ t_E^{i_3} = t_E^{i_4},\ t_B^{i_1} = t_B^{i_3},\ \text{and } t_B^{i_2} = t_B^{i_4} \tag{6}$$

(treatment differences removes event-year, control difference removes cohort).

To avoid clutter we can re-write $\Gamma_a \left(t_E^{i_1}, t_E^{i_2}, t_E^{i_3}, t_E^{i_4}, t_B^{j_1}, t_B^{j_2}, t_B^{j_3}, t_B^{j_4} \right)$ as $\Gamma_a \left(a_E^{i_1}, a_E^{i_2}, a_E^{i_3}, a_E^{i_4} \right)$, where the assumptions embodied in $\Gamma_a (.)$ are implicit. As stated above for the example, under assumption $A1$ selection terms cancel out in DD's of this nature, and we have:

$$\Gamma_a \left(a_E^{i_1}, a_E^{i_2}, a_E^{i_3}, a_E^{i_4} \right) = \lambda_k. \tag{7}$$

Table A3.1 calculates treatment effects as the unweighted average all these DD's, and is analogous to Table 3. Column 1 shows the mean treatment effect for $\pi_a (a-1, a+1)$ by age a for ages $18-22$, as well as a total effect that combines all ages in this range. The estimated effects are very similar to those estimated with Survivors as a control group, but much noisier due to the lack of any way to eliminate event-year by cohort interaction terms.

Appendix 4: Linear Differential Trends

In this appendix I derive formulas for point estimates and standard errors on a treatment effect estimator that allows for linear differential selection in outcomes with respect to period k for $k < 0$. This is a weaker version of the parallel-trends assumption.

The key parameters are the β_k^T and β_k^C terms; their difference captures the difference in child outcomes around period of father's layoff. These terms are estimated using OLS on Equation (2). I here employ a small amount of new notation for convenience. Write a conditional expectation function for a scalar child outcome Y as

$$E[Y|X] = \beta X,$$

where Y is the child's outcome, β is a K by 1 vector of parameters, and X contains the covariates, including the period terms interacted with type of event (layoff or survival) and controls for event-year and cohort. Let $V_\beta = Var\left(\hat{\beta}\right)$.

First define a 7 by K matrix L_T such that $L_T\beta = \left(\beta_{-7}^T, ..., \beta_{-1}^T\right)$, and similarly define L_C such that $L_C\beta = \left(\beta_{-7}^C, ..., \beta_{-1}^C\right)$, where I have here imposed a cutoff of seven years prior to layoff. Now let $L = L_1 - L_0$, such that $L\beta = \left(\beta_{-7}^T - \beta_{-7}^C, ..., \beta_{-1}^T - \beta_{-1}^C\right)$. This vector contains the points in the pre-treatment region of the graph. We want to estimate a line through these points, i.e., we want to regress these points on a constant and on a linear period trend, where period goes from -7 to -1. Therefore define a covariate matrix

$$Z = \begin{pmatrix} 1 & -7 \\ 1 & -6 \\ ... & ... \\ 1 & -1 \end{pmatrix},$$

and define the parameter vector γ as the least-squares approximation

$$\gamma \equiv \arg\min_a (L\beta - Za)' (L\beta - Za) = \left(Z'Z\right)^{-1} Z'L\beta.$$

We can write the estimator of γ as $(Z'Z)^{-1} Z'L\hat{\beta} \equiv \Omega\hat{\beta}$, and the covariance matrix for $\hat{\gamma}$ as $\Omega V_\beta \Omega'$.

The target parameter is the estimated difference between the imputed counterfactual outcome under survival and the realized outcome under layoff in period $k > 0$. Define this scalar parameter as $\theta \equiv (\gamma_0 + k \cdot \gamma_1) - \left(\beta_k^T - \beta_k^C\right)$, where we here focus on the case of $k = 1$, the year after layoff. This can be rewritten by defining two matrices $H_0 = (1, k)$ and H_1 such that $\theta = H_0\gamma - H_1\beta$, or

$$\theta = (H_0\Omega - H_1)\,\beta.$$

This neatly writes the target parameter as a linear combination of the original regression of outcomes on period dummies for each group and other controls. We can therefore write the variance of $\hat{\theta}$ as

$$V_\theta = (H_0\Omega - H_1)\, V_\beta\, (H_0\Omega - H_1)',$$

yielding a standard error on $\hat{\theta}$ as $V_\theta^{1/2}$.

Appendix 5: Institutional Non-Filing of the 1098T Form

My results rely on data contained in 1098T forms filed by all Title IV post-secondary institutions. Title IV institutions contain most four-year, two-year, and professional schools in the U.S.. However, recent work by Cellini and Goldin (2012) suggests that 27% of college students are not enrolled in Title IV institutions, and will therefore not receive 1098Ts. In addition, schools are only required to file 1098T forms for individual students who pay any positive dollar amount for tuition, room, board, or other fees, net of financial aid received from the school or other sources.

My analysis data set defines college enrollment as a non-missing 1098T form, and non-enrollment as a missing 1098T form. I have interpreted reductions in 1098T filing for children with recent paternal layoffs as reductions in college enrollment. However, a decline in non-missing 1098T forms could be generated by increased enrollment in non-Title-IV institutions, or by an increase in financial aid for students that pushes their net payments to zero at a school that does not report 1098Ts when not legally required to do so. Therefore, it is possible in theory that my key enrollment decline results represent a switch from Title IV to non-Title-IV institutions, or an increase in 1098T non-filing. The switch into non-Title-IV institutions seems unlikely because Cellini and Goldin (2012) estimate that these schools are approximately equal in price to Title IV schools, after accounting for subsidies (mainly Pell Grants) at Title IV schools. Therefore the main worry is that layoffs increase 1098T non-filing rather than decrease real college enrollment. While the evidence I present in this appendix suggests 1098T-nonfiling is not driving my results, such a problem would anyway work in favor of my conclusion that layoffs and their associated income declines have at most very small effects on child college outcomes.

To address this concern I first construct an alternative measure of college enrollment in IRS data. I then provide two tests suggesting that the problem is unlikely to drive my main results.

The alternative measure of college enrollment is based on the claiming of children age 19-24. Parents are allowed to claim children ages 19-24 if and only if the child is "permanently and totally disabled" or enrolled full-time at a school[26]. The key features of this rule, for our purposes, are that children can qualify as students if the family pays zero net tuition, and if the school is not a Title IV school. Therefore, conditional on parents filing a tax return, the fraction of parents that claim a child age 19-24 represents a potential alternative measure of college enrollment that can validate findings with 1098T-based enrollment.

Figure A5.1 plots the two measures of college enrollment for children at age 19 in 2002 by mean three-year family income. I restrict to children in 2002 to facilitate comaparison with statistics in the NLSY 97 for cohorts 1979-1982 in Bailey and Dynarski (2011, Figure 2). I restrict to age 19 because after age 19 children gradually start to claim themselves as dependents. While parent claims continue to track 1098T-based enrollment for higher-income families, the mechanical decrease in levels makes them less useful as a gauge of total enrollment.

The first pattern is that, for richer children, college enrollment based on claims roughly tracks college enrollment based on 1098Ts. About 20% more high-income children are enrolled in college using the claims measure. This is slightly less than the fraction of students estimated to be enrolled in non-Title-IV colleges in Cellini and Goldin (2012). The smaller figure here could be due to the small fraction of 19-year-olds who claim themselves and hence cannot be claimed by their parents.

The second pattern is that, for poorer children, college enrollment based on claims is much too high to represent actual college enrollment. While 1098T-based enrollment matches enrollment in the NLSY for children in the bottom quartile of family income, the claims-based measure of college enrollment is about twice this level. The most likely explanation for these implausibly-high claiming rates is that low-income families claim children after age 18 in order to claim the EITC[27]. This seems especially likely since the only reason most low-income families file a 1040 at all is to claim EITC benefits[28]. Therefore the claims-based measure of college enrollment cannot help us validate the 1098T-based measure of college enrollment for low-income children: false claims overwhelm genuine non-1098T college enrollment with zero tuition payments or at non-Title IV schools.

I would like to see if the main results in the paper hold up when using this alternative enrollment measure. For this to make sense, I restrict to richer families for two reasons. The first reason, as just discussed, is that this measure appears to approximate enrollment only for richer families. The second reason is that layoffs reduce filing rates, because filing correlates positively with income. This introduces a potential spurious effect by reducing claims by reducing filings. However, note that families who do not file are unlikely to have children in college, because such families can claim EITC benefits. Nonetheless, restricting to higher-income families alleviates most of the filing problem. I use the same definition of "high-income" used in the main results in the text, three-year mean incomes 1996-1999 above $40,000.

Figure A5.2.a plots treatment effects from Equation (2) where the outcome is a dummy for whether the child is claimed by her parents, instead of whether the child receives a 1098T as

[26] See instructions for the 1040 online at <http://www.irs.gov/instructions/i1040a/ar01.html>.

[27] Note that by using three-year mean family income on the x-axis, families with incomes in the EITC benefit range for at least one of these three years are likely to have much higher three-year mean income, due to mean-reversion, explaining why the excess claims extend "too high" relative to the EITC benefit range, which reaches zero around $30,000.

[28] Only 60% of the poorest parents in this graph file, where as almost 100% of parents file starting at around $50,000

in Figure 13. The claim-based enrollment measure shows almost exactly the same pattern of treatment effects as the 1098T-based measure for high-income families: about one percentage point lower enrollment for children experiencing a father's job loss one year before college decisions versus one year after college decisions. I conclude that this test supports the main results of the paper: layoffs reduce college enrollment by a small amount.

I also develop a second robustness check on the main 1098T enrollment results. I create a dummy variable that equals one when a child receives a 1098T from a college that often files 1098Ts for students with zero net-tuition payments[29]. I then estimate Equation (2) again for this outcome variable, now for all children, not just high-income children. The resulting event-age effects are much noisier due to the much lower rate of enrollment at this restricted set of institutions, but the overall pattern is reassuring: children experiencing paternal layoffs one year before making enrollment decisions enroll in these colleges about 1% less than children experiencing paternal layoffs one year after making enrollment decisions. The only way this pattern could be generated spuriously is if these schools (1) raise financial aid for students who experience paternal layoff and then (2) selectively decide not to file 1098Ts for students whose greater aid package reduced their net payments to zero, despite filing 1098Ts for many other students with zero net payments. As this seems much less likely than the alternative explanation that paternal layoffs cause a small fraction of students to forego college, I conclude that this test also supports the main results of the paper.

[29] I define "often" by ranking colleges by the fraction of their 1098Ts recording zero net tuition payments, and restricting to those in the 75th percentile of this distribution.

Table 1: Summary Statistics 1999-2009 for Children at Age 19

Sample:	Layoff			Survivor		
	Pre-Shock	Post-Shock	% Diff	Pre-Shock	Post-Shock	% Diff
Parent Outcomes						
Father's Earnings	51,523	37,699	-26.8%	50,782	47,193	-7.1%
Father Married	0.779	0.754	-3.2%	0.778	0.770	-1.0%
Mother's Earnings	21,440	23,212	8.3%	20,053	21,397	6.7%
Father's UI Benefits	493	1,607	226.0%	230	554	140.2%
Post-Tax Family Income	60,725	53,883	-11.3%	59,480	59,193	-0.5%
Child Outcomes						
Enrollment	0.464	0.498	7.4%	0.469	0.511	9.1%
Years Enrolled 18-22	2.033	2.096	3.1%	2.067	2.138	3.5%
Teen Mother	0.082	0.078	-5.6%	0.0784	0.0721	-8.0%
Earnings	7,359	6,134	-16.6%	7,333	6,124	-16.5%
Freq	3,976,779	3,046,107		17,880,678	15,305,421	

Notes: Survivor sample is propensity-score reweighted to match Layoff sample on observables. "Pre-Shock" includes cells in years before events occur;"Post-Shock" includes cells in years after events occur. "% Diff" is the percent difference between Post-Shock and Pre-Shock columns. Averages pool all cohorts turning 19 during the sample period. All dollar variables deflated to 2009 using CPI-U.

Table 2: College Tax Credits Claimed for Children in College Age 18-22

	Hope/AOTC		LLC		Tuition Deduction	
Tax Year	Credits ($1,000's)	Claimers	Credits ($1,000's)	Claimers	Credits ($1,000's)	Claimers
2005	1,832,422	1,197,402	807,635	458,021	1,166,969	1,188,653
2006	2,371,953	1,425,660	1,467,215	879,789	1,238,161	1,328,686
2007	2,335,695	1,429,154	2,101,029	1,263,800	1,596,340	1,749,439
2008	2,691,387	1,465,345	2,186,915	1,305,203	1,571,967	1,787,642
2009	12,778,241	4,495,697	658,546	454,371	458,665	544,363
2010	15,168,152	5,034,234	308,710	232,785	274,142	342,940

Notes: Table displays average per-child dollar value of credits claimed (in thousands) and the number of individual children receiving Hope/American Opportunity Tax Credits (AOTC), Lifetime Learning Credits (LLC), and the Tuition Deduction, restricting to claims for children enrolled in college and between ages 18-22.

Table 3: Short-Run and Long-Run Effects of Layoff on Parent Outcomes

	Short-Run (One Year)			Long-Run (Five Years)		
Outcome	Effect	SE	Effect/Base	Effect	SE	Effect/Base
Father's Earnings	-$14,865*	$629	-29.18%	-$9,025*	$538	-17.71%
Post-Tax Family Income	-$8,221*	$490	-13.74%	-$5,641*	$466	-9.43%
Father's UI	$2,427*	$421	674.75%	$555*	$172	154.32%
Mortgage Interest Payments	-$383*	$48	-5.76%	-$460*	$46	-6.91%
Father's DI	$83	$44	45.02%	$297*	$93	161.05%
Mother's Earnings	$271*	$92	1.31%	$506*	$82	2.46%
Median Home Value in Zipcode	-$910*	$374	-0.74%	-$1,487*	$482	-1.20%
Fraction Inter-State Moves (PP)	0.76*	0.08	33.55%	0.01	0.05	0.55%
Fraction Local Moves (PP)	0.52*	0.14	6.78%	-0.05	0.11	-0.61%

Notes: (*) indicates statistical significance at 5% level. Presents DD estimates and standard errors using differences across outcomes for Layoff and Survivor parents, and across periods 1 and -1 (short-run) or periods 5 and -1 (long-run). Effect/Base uses base mean outcomes in years before events occur.

Table 4: Layoffs and Survivors: Causal Effects of Paternal Layoff

Age	College(a-1,a+1)	Income(a-1,a+1)	College(a-1,a+1) Cross-Section	Fraction Causal
	(1)	(2)	(3)	(4)
18	-0.0039	-7,957	-0.0354	10.9%
	(0.0018)	(387)	(0.0017)	(5.1)
19	-0.0084	-8,529	-0.0518	16.2%
	(0.0021)	(434)	(0.0026)	(4.1)
20	-0.0044	-8,184	-0.0464	9.4%
	(0.0021)	(449)	(0.0025)	(4.4)
21	0.0000	-7,820	-0.0403	0.0%
	(0.0021)	(430)	(0.0022)	(5.1)
22	-0.0030	-8,071	-0.0367	8.1%
	(0.0021)	(429)	(0.0019)	(5.8)
18-22	-0.0041	-8,138	-0.0421	9.6%
	(0.001)	(223)	(0.0012)	(2.3)

Notes: Column (1) displays the DD estimate of the decrease in college enrollment from experiencing paternal layoff one year before age *a* compared to one year after age a, based on Assumption A2. Column (2) presents the same estimate for the child's post-tax family income at age *a*. Column (3) multiplies Column (2) by the age-*a* cross-sectional correlation between mean family income 1996-1999 and college enrollment at age *a*. Column (4) displays Column (1) as a percentage of Column (3).

Table 5: Effects of Paternal Layoff on College-Age Children: Ages 18-22

Outcome	Effect	SE	T-Statistic	Base	Effect/Base
Percentage Points					
College Enrollment	-0.432*	0.094	-4.580	40.66%	-1.06%
College Enrollment: Out of State	-0.529*	0.115	-4.610	25.90%	-2.04%
College Enrollment: Four-Year	-0.27*	0.114	-2.362	19.61%	-1.38%
College Enrollment: Non-Public	-0.101*	0.047	-2.174	6.79%	-1.49%
College Quality: > $20,000	-0.407*	0.088	-4.620	38.39%	-1.06%
College Quality: > $30,000	-0.286*	0.077	-3.700	28.73%	-1.00%
College Quality: > $40,000	-0.092*	0.043	-2.137	9.15%	-1.01%
College Quality: > $50,000	-0.024	0.020	-1.215	1.84%	-1.30%
College Quality: > $60,000	-0.008	0.008	-1.034	0.48%	-1.75%
Earnings > 0	0.181*	0.064	2.805	84.97%	0.21%
Earnings > $2,000	0.209*	0.074	2.814	71.89%	0.29%
Earnings > $10,000	0.142	0.074	1.910	32.47%	0.44%
Dollars					
Earnings	$0.77	$17.77	0.043	$8,488	0.01%
College Quality: Alumni Earnings	-$83.61*	$19.86	-4.210	$23,930	-0.35%
Family Income	-$8,138.09*	$219.53	-37.071	$59,832	-13.60%

Notes: (*) indicates significance at 5% level. Presents DD estimates and standard errors using differences across outcomes for children of Layoff and Survivor parents, and across periods 1 and -1. Effect/Base uses base mean outcomes before events occur.

Table 6: Treatment Effects Under Alternative Assumptions

Outcome (Percentage Points)	1	2	3	4	5
College Enrollment	-0.442	-0.418*	-0.426*	-0.341*	-0.313*
	(0.479)	(0.106)	(0.093)	(0.088)	(0.155)
College Enrollment: Out of State	-0.623	-0.482*	-0.604*	-0.391*	-0.267
	(0.701)	(0.114)	(0.103)	(0.085)	(0.178)
College Enrollment: Four-Year	-0.253	-0.274*	-0.387*	-0.381*	-0.104
	(0.724)	(0.113)	(0.1)	(0.078)	(0.184)
College Enrollment: Non-Public	-0.087	-0.078	-0.171*	-0.135*	-0.125
	(0.329)	(0.048)	(0.045)	(0.039)	(0.082)
Earnings > 0	0.014	0.163*	-0.058	0.084	0.097
	(0.295)	(0.072)	(0.066)	(0.057)	(0.119)

Notes: (*) indicates significance at 5% level. Standard errors in parentheses. Column (1) displays treatment effects using only Layoff sample, as described in Appendix 3. Column (2) displays treatment effects relaxing the parallel trends assumption to a linear differential trends assumption, as described in Appendix 4. Column (3) displays treatment effects that difference across periods -3 and +1. Column (4) presents treatment effects that difference across event-ages before 18 and event-ages after 22, allowing for intertemporal substitution of outcomes within ages 18-22 and allowing for decisions to be made only one time rather than independently year-by-year. Column (5) displays treatment effects using mass layoffs rather than all layoffs.

Table 7: Effects of Paternal Layoff on College-Age Children: Ages 18-22

(a) Gender

	Male			Female		
Outcome	Effect	SE	Effect/Base (%)	Effect	SE	Effect/Base (%)
Percentage Points						
College Enrollment	-0.331*	0.100	-0.93%	-0.55*	0.118	-1.19%
College Enrollment: Out of State	-0.463*	0.114	-2.08%	-0.611*	0.137	-2.04%
College Enrollment: Four-Year	-0.209	0.111	-1.25%	-0.342*	0.138	-1.50%
College Enrollment: Non-Public	-0.04	0.052	-0.70%	-0.17*	0.061	-2.15%
College Quality: > $20,000	-0.316*	0.095	-0.94%	-0.512*	0.109	-1.18%
College Quality: > $30,000	-0.25*	0.085	-0.98%	-0.33*	0.100	-1.02%
College Quality: > $40,000	-0.031	0.053	-0.37%	-0.159*	0.059	-1.59%
Earnings > 0	0.113	0.082	0.13%	0.255*	0.076	0.30%
Earnings > $10,000	0.054	0.101	0.15%	0.245*	0.089	0.84%
Dollars						
Earnings	-$39.36	$26.63	-0.42%	$47.19*	$16.76	0.62%
College Quality: Alumni Earnings	-$59.01*	$21.65	-0.26%	-$112.05*	$24.95	-0.45%
Family Income	-$8,052.56*	$224.12	-13.54%	-$8,229.81*	$222.21	-13.67%

(b) Income

	Low Income (<= $40,000)			High Income (> $40,000)		
Outcome	Effect	SE	Effect/Base (%)	Effect	SE	Effect/Base (%)
Percentage Points						
College Enrollment	-0.058	0.100	-0.28%	-0.714*	0.119	-1.47%
College Enrollment: Out of State	-0.08	0.114	-0.65%	-0.881*	0.127	-2.79%
College Enrollment: Four-Year	0.008	0.111	0.10%	-0.529*	0.134	-2.19%
College Enrollment: Non-Public	0.019	0.052	0.62%	-0.199*	0.057	-2.41%
College Quality: > $20,000	-0.013	0.095	-0.07%	-0.688*	0.117	-1.49%
College Quality: > $30,000	0	0.085	0.00%	-0.521*	0.109	-1.47%
College Quality: > $40,000	0.017	0.053	0.58%	-0.188*	0.065	-1.62%
Earnings > 0	0.023	0.082	0.03%	0.202*	0.054	0.23%
Earnings > $10,000	-0.103	0.101	-0.36%	0.241*	0.081	0.71%
Dollars						
Earnings	-$41.44	$26.63	-0.56%	$13.79	$19.54	0.15%
College Quality: Alumni Earnings	-$4.34	$21.65	-0.02%	-$148.50*	$28.42	-0.58%
Family Income	-$4,450.52*	$224.12	-14.89%	-$9,790.19*	$217.71	-13.65%

(c) Financial Wealth (All with Income > $40,000)

	Low Wealth (Interest <= $500)			High Wealth (Interest > $500)		
Outcome	Effect	SE	Effect/Base (%)	Effect	SE	Effect/Base (%)
Percentage Points						
College Enrollment	-0.713*	0.121	-1.57%	-0.624*	0.205	-0.93%
College Enrollment: Out of State	-0.88*	0.130	-2.97%	-0.738*	0.222	-1.69%
College Enrollment: Four-Year	-0.494*	0.119	-2.28%	-0.519*	0.238	-1.36%
College Enrollment: Non-Public	-0.163*	0.054	-2.22%	-0.321*	0.143	-2.37%
College Quality: > $20,000	-0.68*	0.119	-1.59%	-0.638*	0.207	-0.99%
College Quality: > $30,000	-0.48*	0.104	-1.50%	-0.638*	0.205	-1.17%
College Quality: > $40,000	-0.139*	0.058	-1.46%	-0.375*	0.176	-1.61%
Earnings > 0	0.13*	0.055	0.15%	0.636*	0.133	0.74%
Earnings > $10,000	0.163*	0.079	0.46%	0.688*	0.164	2.55%
Dollars						
Earnings	-$8.75	$19.84	-0.10%	$139.17*	$34.17	1.80%
College Quality: Alumni Earnings	-$135.04*	$26.56	-0.55%	-$186.02*	$55.90	-0.60%
Family Income	-$9,319.11*	$223.98	-13.61%	-$12,837.68*	$279.13	-14.21%

Notes: (*) indicates significance at 5% level. Presents DD estimates and standard errors using differences across outcomes for children of Layoff and Survivor parents, and across periods 1 and -1. Effect/Base uses base mean outcomes before events occur.

Table 8: Effects of Paternal Layoff on Adolescents, Ages 14-17

Outcome	Effect	SE	T-Statistic	Base	Effect/Base
Earnings	7.483	4.854	1.542	748.151	1.00%
Log(Earnings)	0.001	0.009	0.149	7.052	0.00%
Earnings > 0	0.003*	0.001	3.091	0.249	1.40%
Earnings > $2,000	0.002*	0.001	2.914	0.115	1.70%
Earnings > $10,000	0.000	0.000	0.480	0.009	0.80%

Notes: Effects significant at 5% denoted with "*". Effects estimated using Assumption A2'.

Table 9: Effects of Firm Closures on College Enrollment

Dependent Variable:	College Enrollment at 19, cohort=1987			
	(1)	(2)	(3)	(4)
Layoff at age 15-17	-0.109	-0.044	-0.014	-0.006
	(0.001)	(0.001)	(0.001)	(0.001)
Family background controls**		x		x
Compare to average fathers	x	x		
Compare to post-19 layoff fathers			x	x
R-squared	0.01	0.20	0.00	0.19
Observations	860,534	860,534	792,783	792,783

**Controls: family income quartic interacted with 1998 marital status of father, 1998 zipcode fixed effects, any deferred compensation in 1999, any Schedule C income in 1999, two-digit NAICS industry code of father's primary job in 1999, mother under age 22 at child's birth, female. Standard errors not clustered.

Table A2.1: Firm Closures by Year and Size

Year of Closure	Number of Firms	Mean Firm Size
2000	12,894	156
2001	13,164	153
2002	10,679	164
2003	9,652	121
2004	8,966	118
2005	8,979	140
2006	9,688	121
2007	10,910	99
Total	84,932	136

Notes: Table displays annual number of firms and mean firm size in the Closure sample.

Table A2.2: Summary Statistics 1999-2009 for Children at Age 18

Sample:	Closure			Non-Closure		
	Pre-Shock	Post-Shock	% Diff	Pre-Shock	Post-Shock	% Diff
<u>Parent Outcomes</u>						
Father's Earnings	49,373	40,421	-18.1%	50,947	52,080	2.2%
Father Married	0.792	0.798	0.8%	0.811	0.809	-0.2%
Mother's Earnings	21,885	23,041	5.3%	23,370	23,805	1.9%
<u>Child Outcomes</u>						
Enrollment	0.346	0.368	6.5%	0.393	0.409	3.9%
Earnings	4,128	3,917	-5.1%	3,973	3,862	-2.8%
Freq	35,207	54,031		1,796,769	1,839,753	

Notes: Non-Closure sample is propensity-score reweighted to match Closure sample on observables. "Pre-Shock" includes cells in years before events occur, "Post-Shock" includes cells in years after events occur. "% Diff" is the percent difference between Post-Shock and Pre-Shock columns. Averages pool all available cohorts.

Table A3.1 Layoffs Only: Causal Effects of Paternal Layoff

Age	College(a-1,a+1)	Income(a-1,a+1)	College(a-1,a+1) Cross-Sectional Prediction	Fraction Causal
	(1)	(2)	(3)	(4)
18	0.0016	8,420	0.0375	4.4%
	(0.0041)	(819)	(0.0036)	(11)
19	0.0069	8,650	0.0525	13.1%
	(0.0047)	(866)	(0.0053)	(9)
20	0.0061	8,771	0.0498	12.2%
	(0.004)	(737)	(0.0042)	(8)
21	0.0034	8,819	0.0455	7.5%
	(0.0043)	(759)	(0.0039)	(9.5)
22	0.0044	8,868	0.0403	11.0%
	(0.0043)	(858)	(0.0039)	(10.7)
18-22	0.0045	8,664	0.0449	10.1%
	(0.0047)	(816)	(0.0042)	(10.6)

Notes: Column (1) displays the DD estimate of the increase in college enrollment at age *a* from experiencing paternal layoff one year after age *a* compared to one year before age a. Column (2) presents the same estimate for the child's post-tax family income at age *a*. Column (3) multiplies Column (2) by the age-*a* cross-sectional correlation between mean family income 1996-1999 and college enrollment at age *a*. Column (4) displays Column (1) as a percentage of Column (3).

FIGURE 1

Child Outcomes and Parental Income in the Cross-Section: Survivor Sample

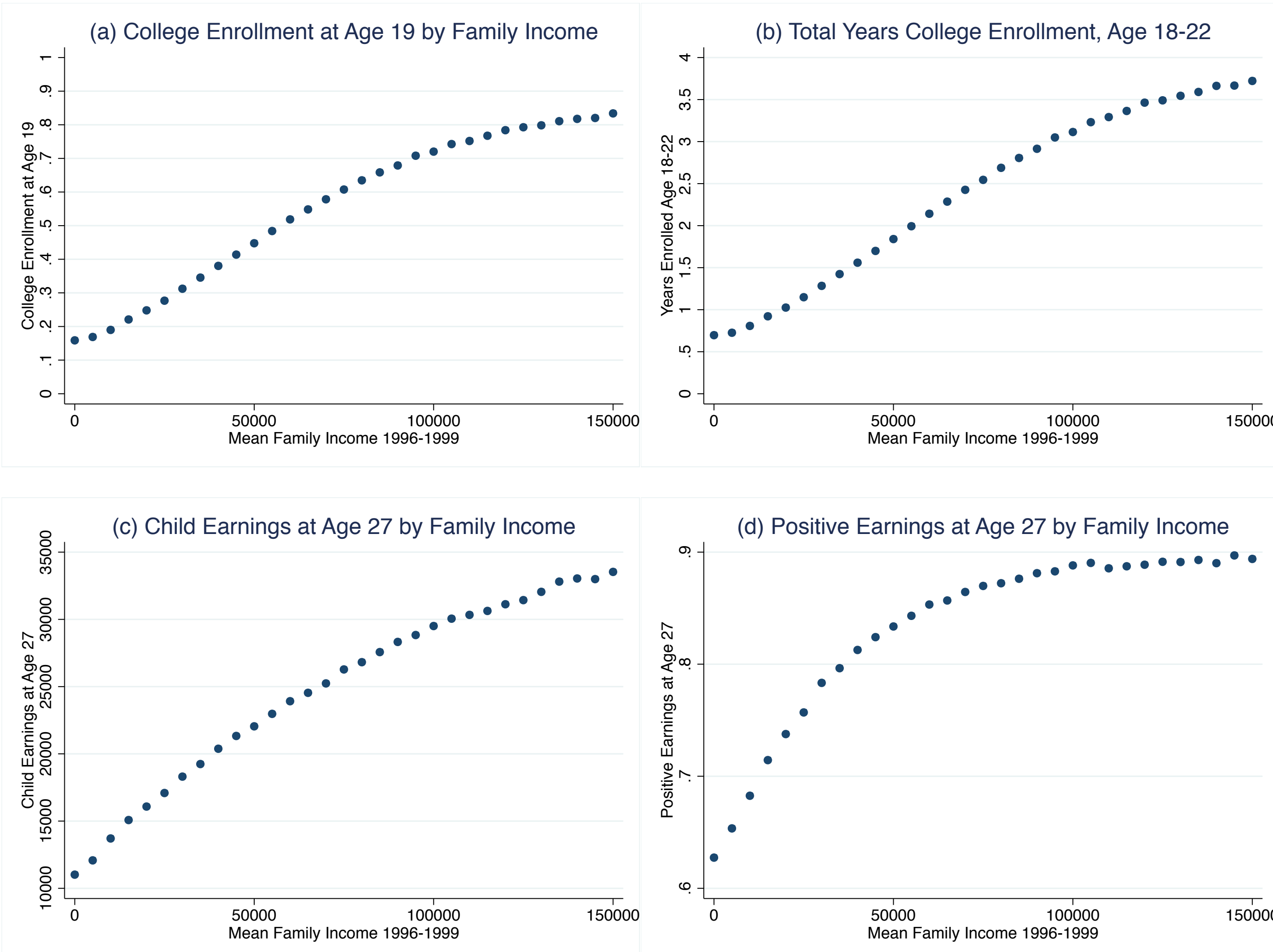


FIGURE 2

Total College Tax Credits by Modified AGI Before and After Passage of ARRA

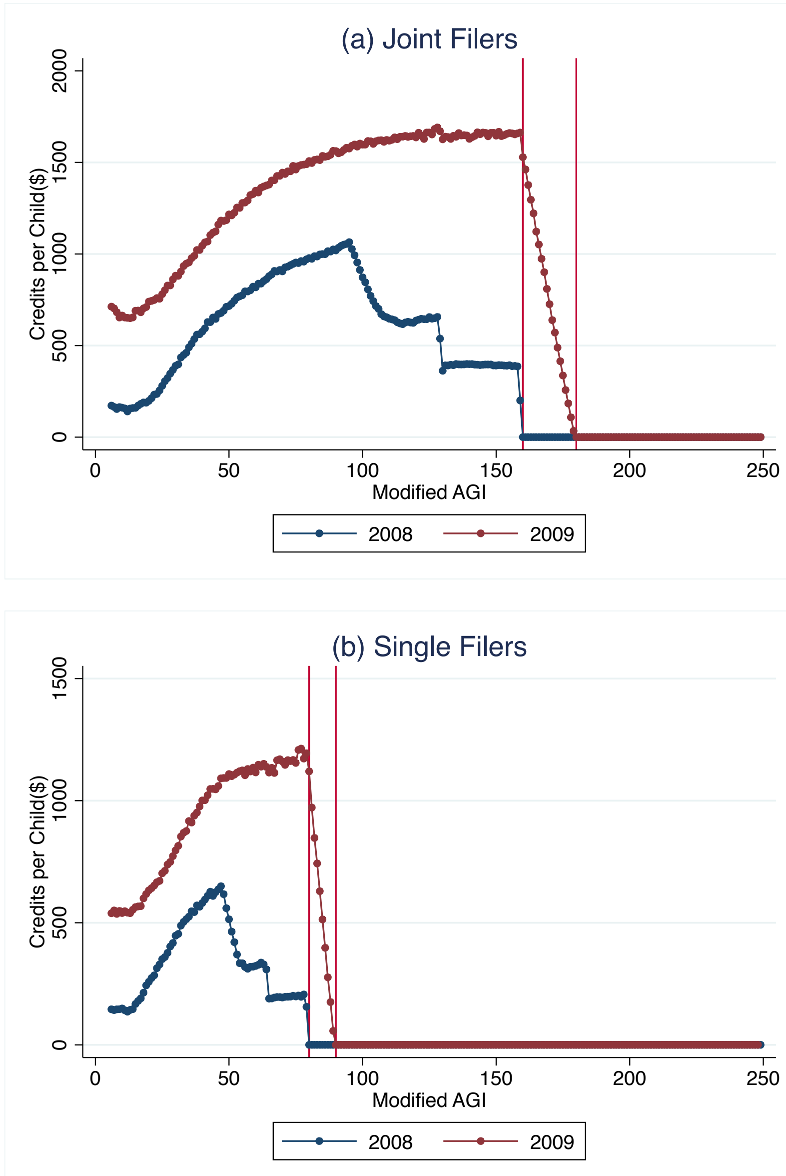


FIGURE 3

College Credits by Tax Year, Above and Below Phase-Out Region of Modified AGI

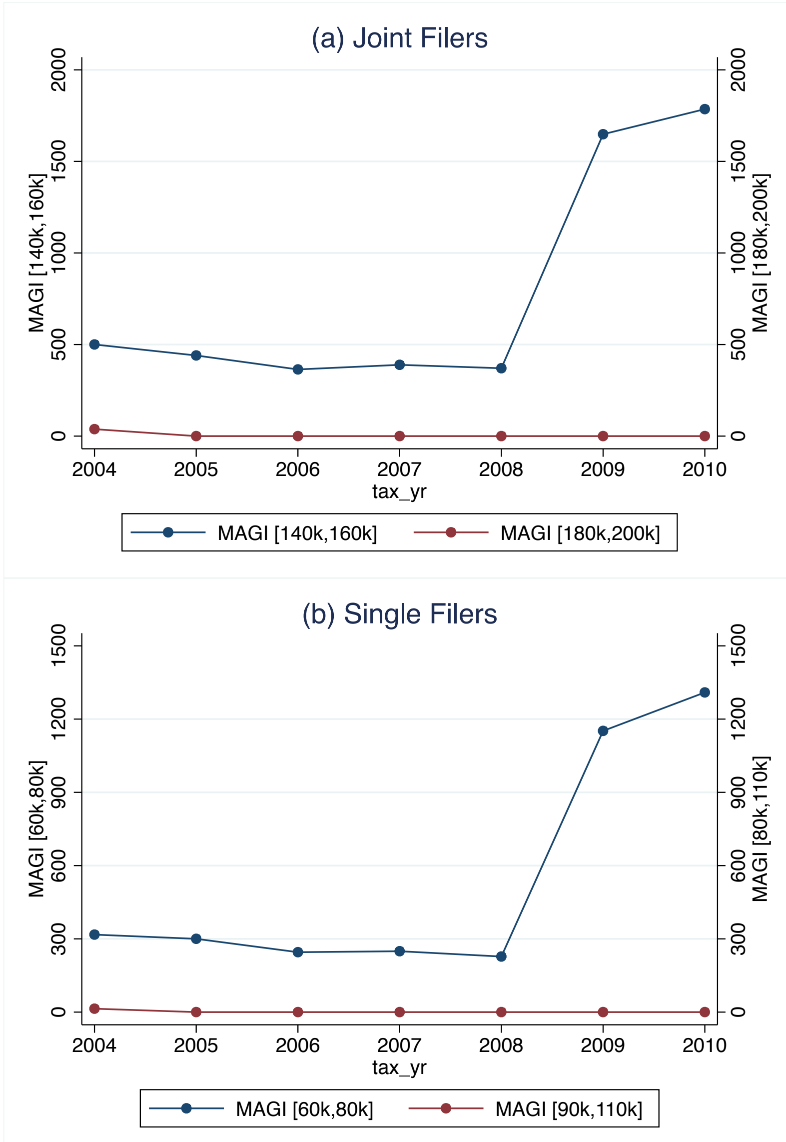


FIGURE 4

College Enrollment by Tax Year, Above and Below Phase-Out Region of Modified AGI

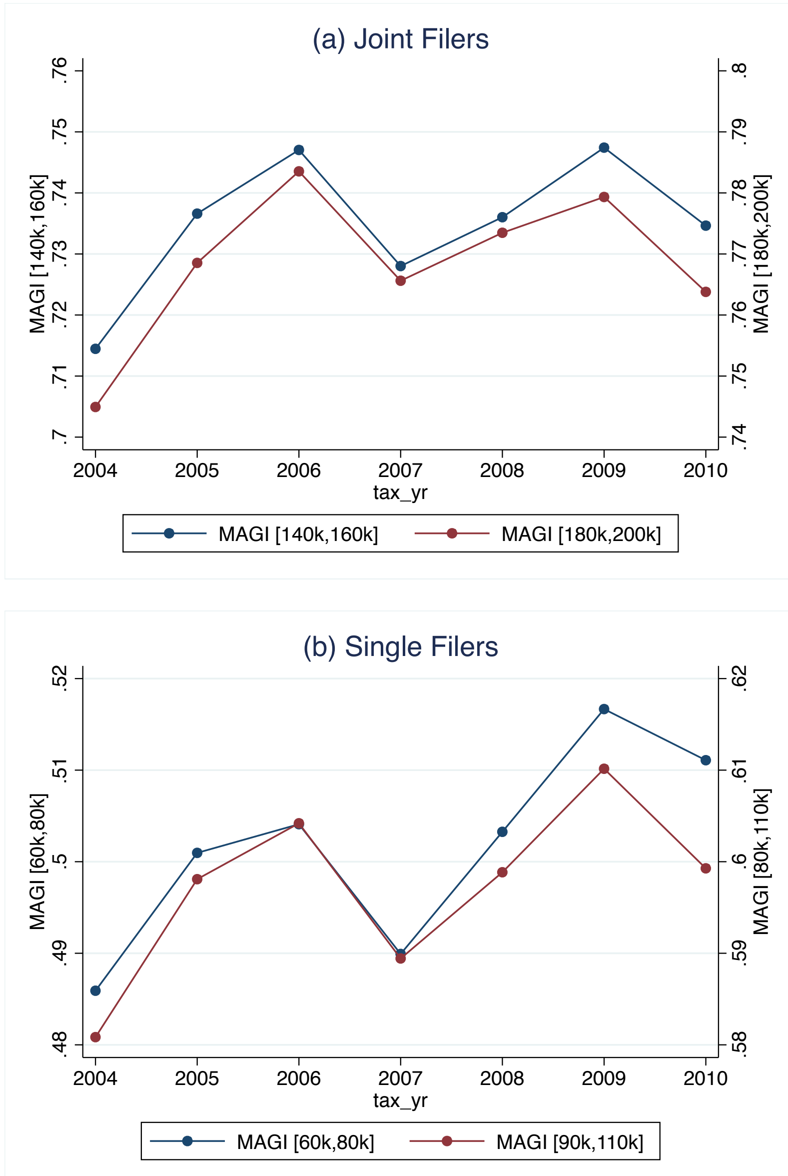


FIGURE 5

Federal EITC Schedule for a Single Filer with Children 1996-2008

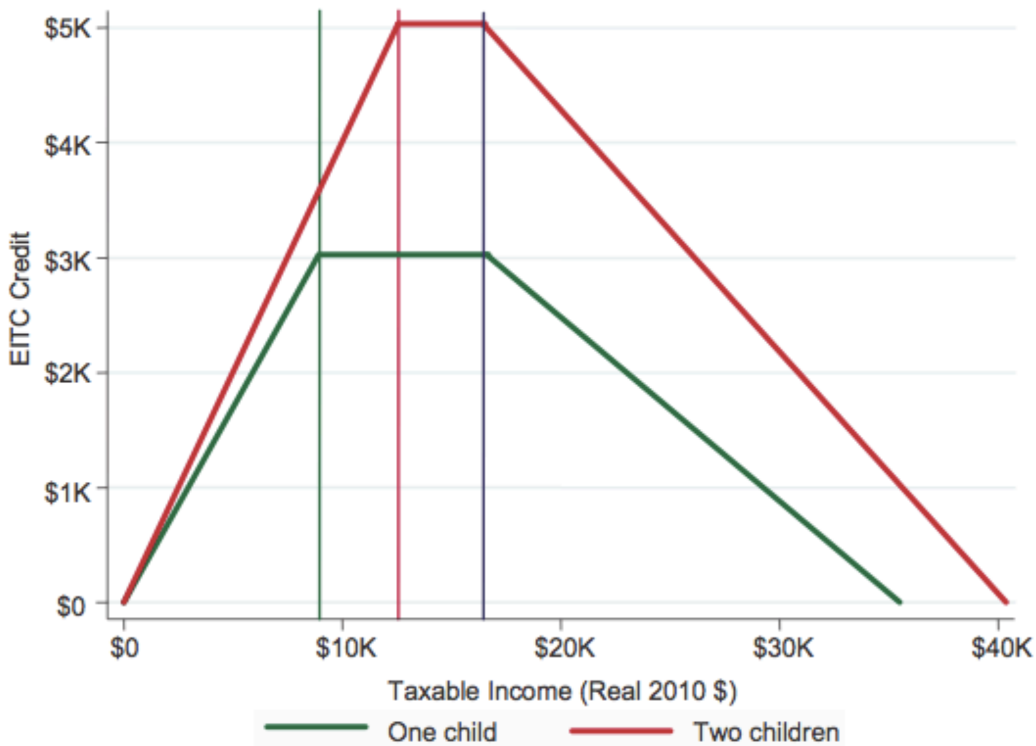


FIGURE 6

Average EITC Benefits Per Child

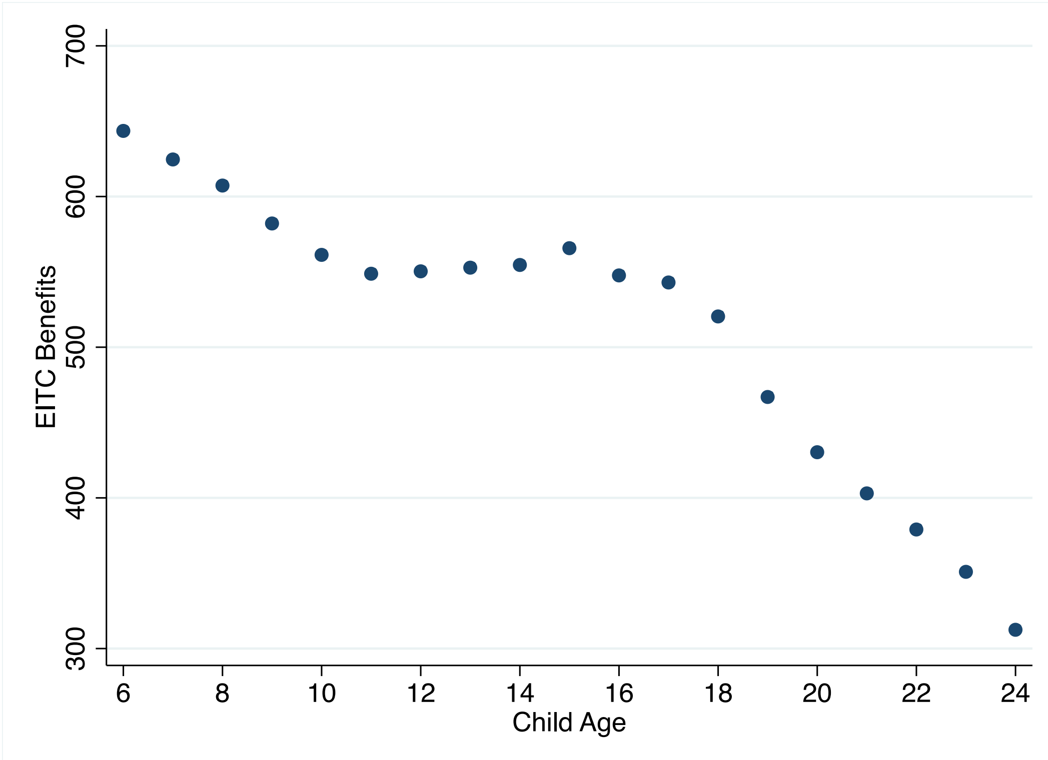


FIGURE 7

EITC Benefits as Earnings Insurance

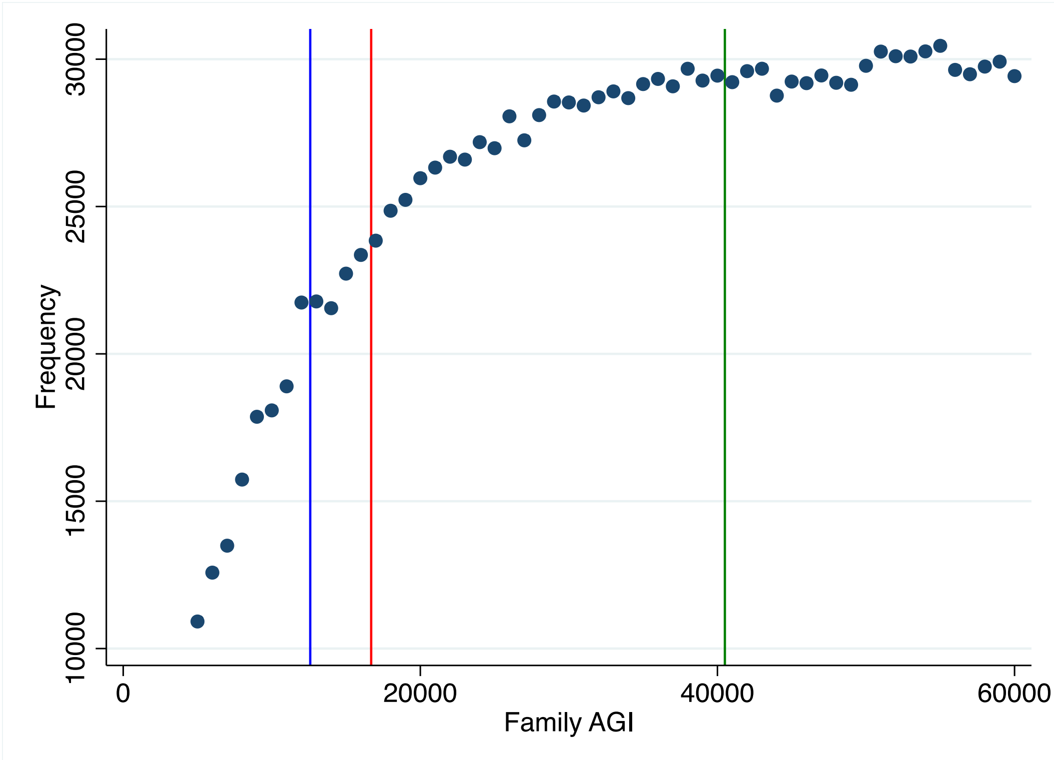


FIGURE 8

EITC Benefits and College Enrollment

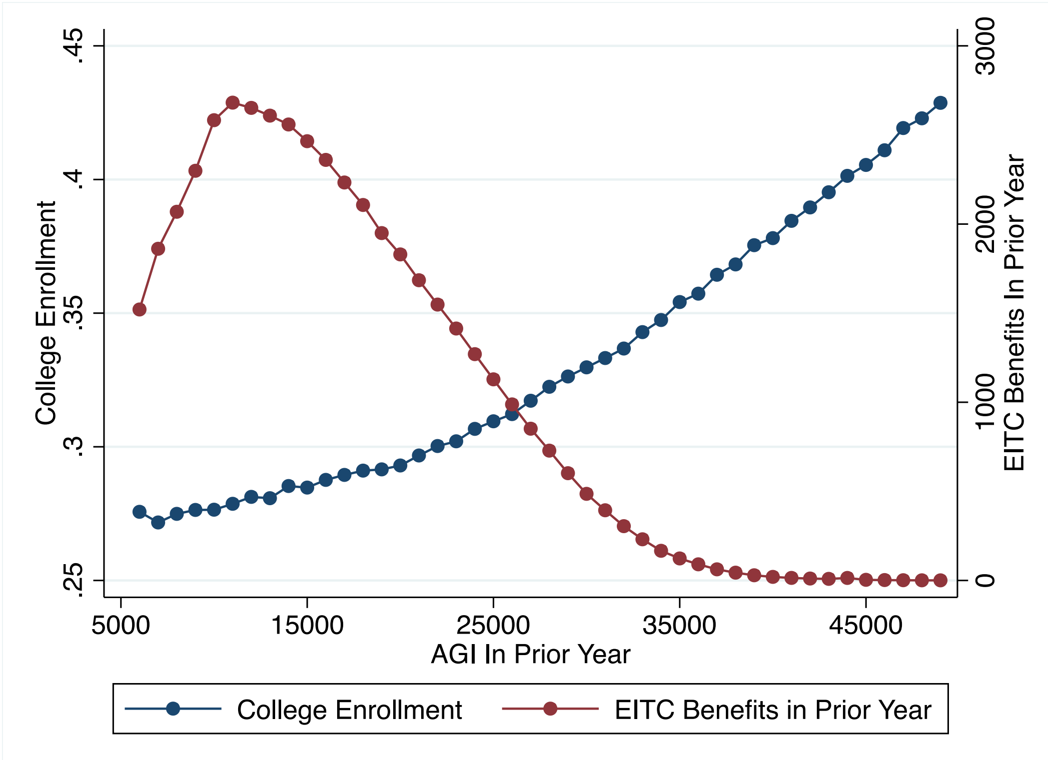
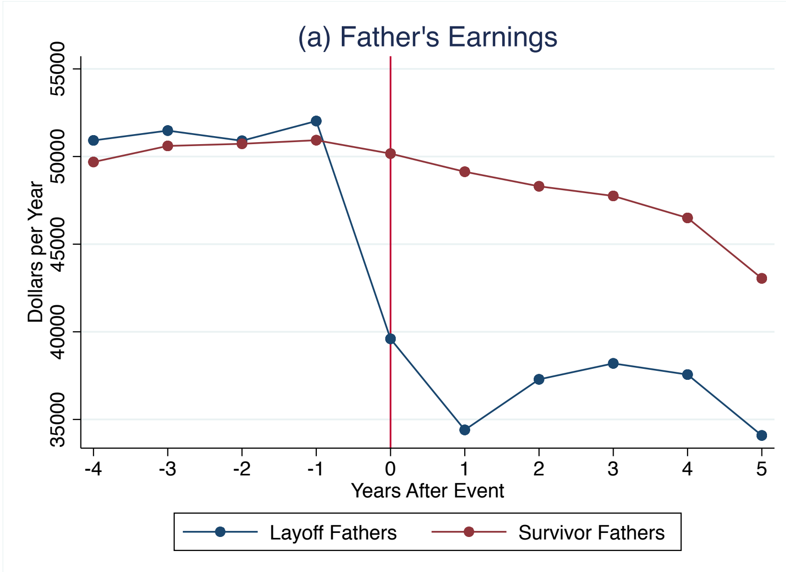


FIGURE 9

Effects of Layoffs on Parent Outcomes



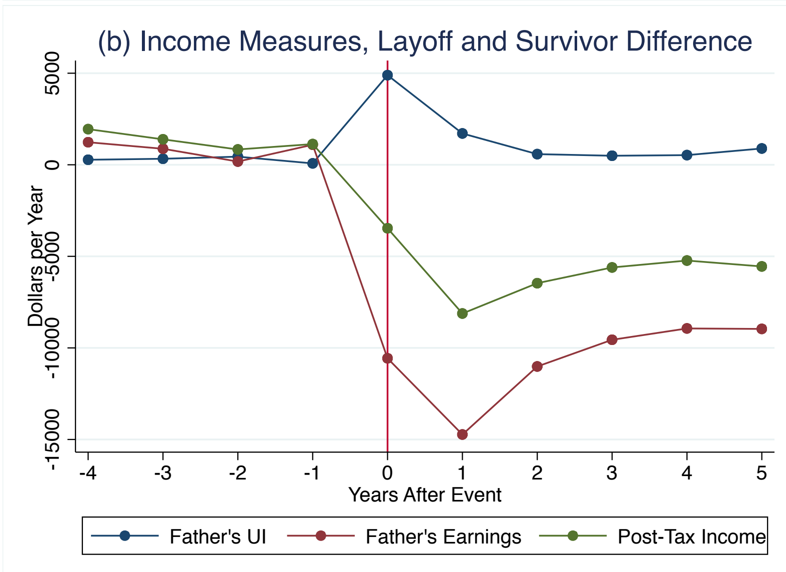


FIGURE 10

Effects of Layoffs on Parent Mobility and Housing Consumption

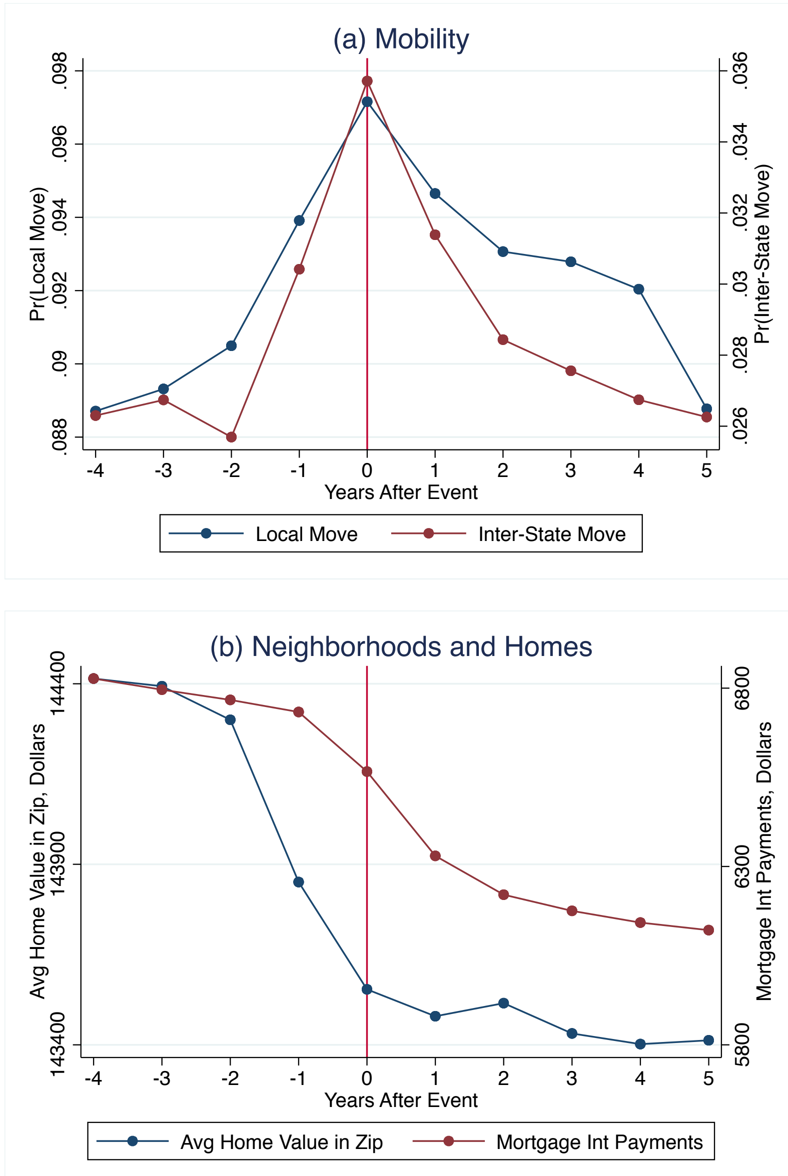


FIGURE 11

Event-Age Study Pooling Ages 18-22

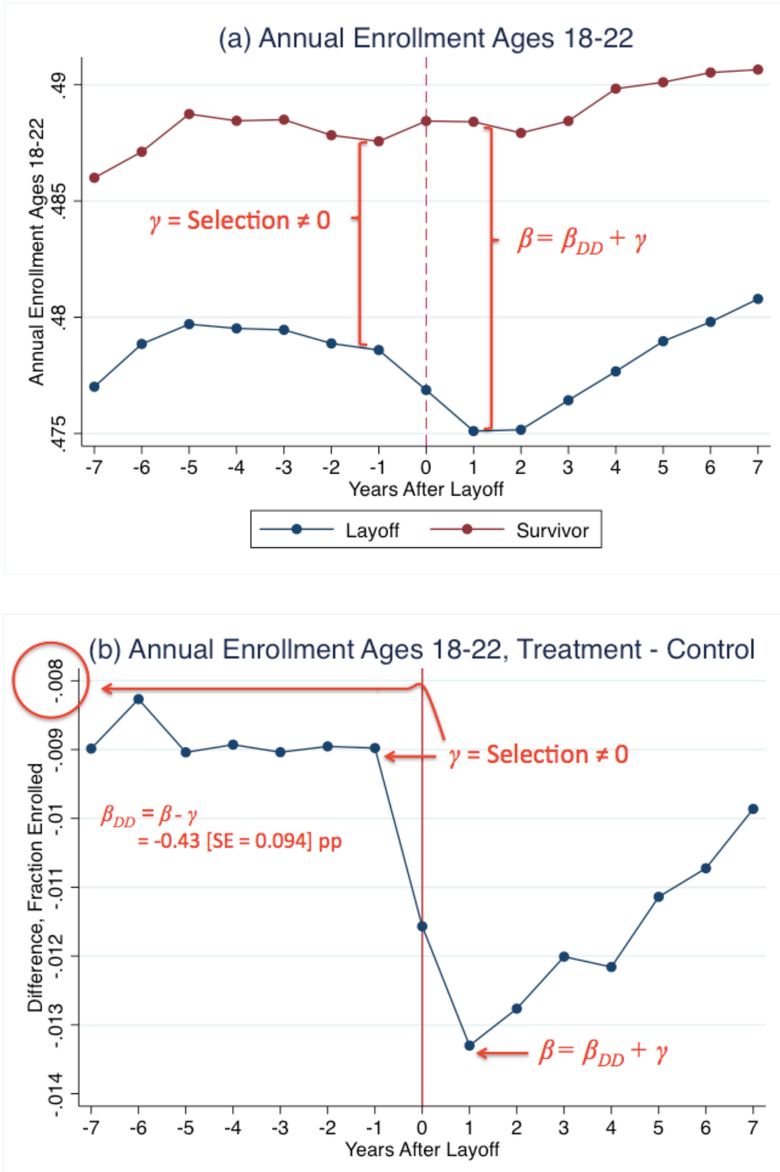


FIGURE 12

Treatment Effects by Family Income

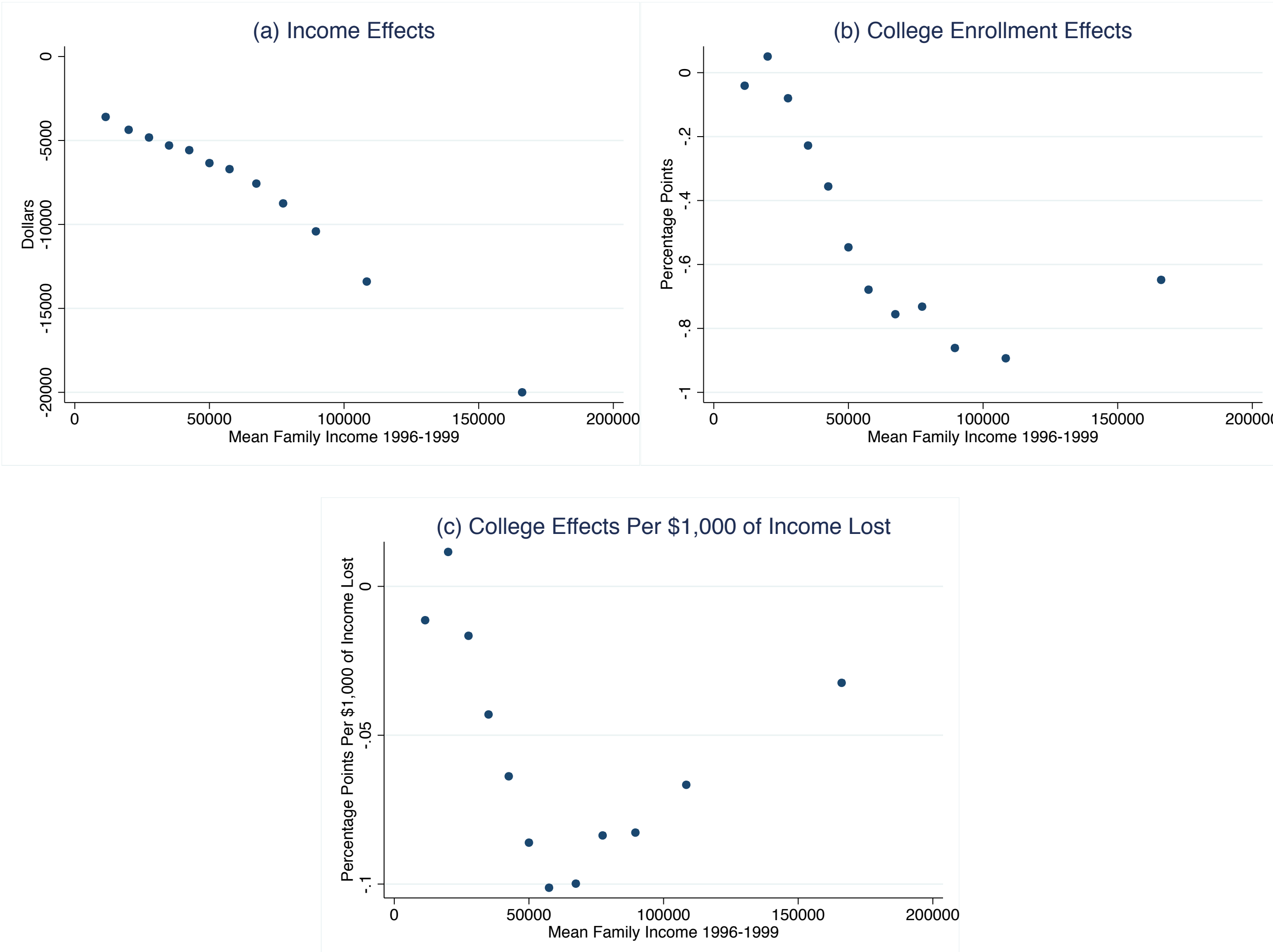


FIGURE 13

Treatment Effects by Father's Earnings Share: Variation from Mother's Earnings

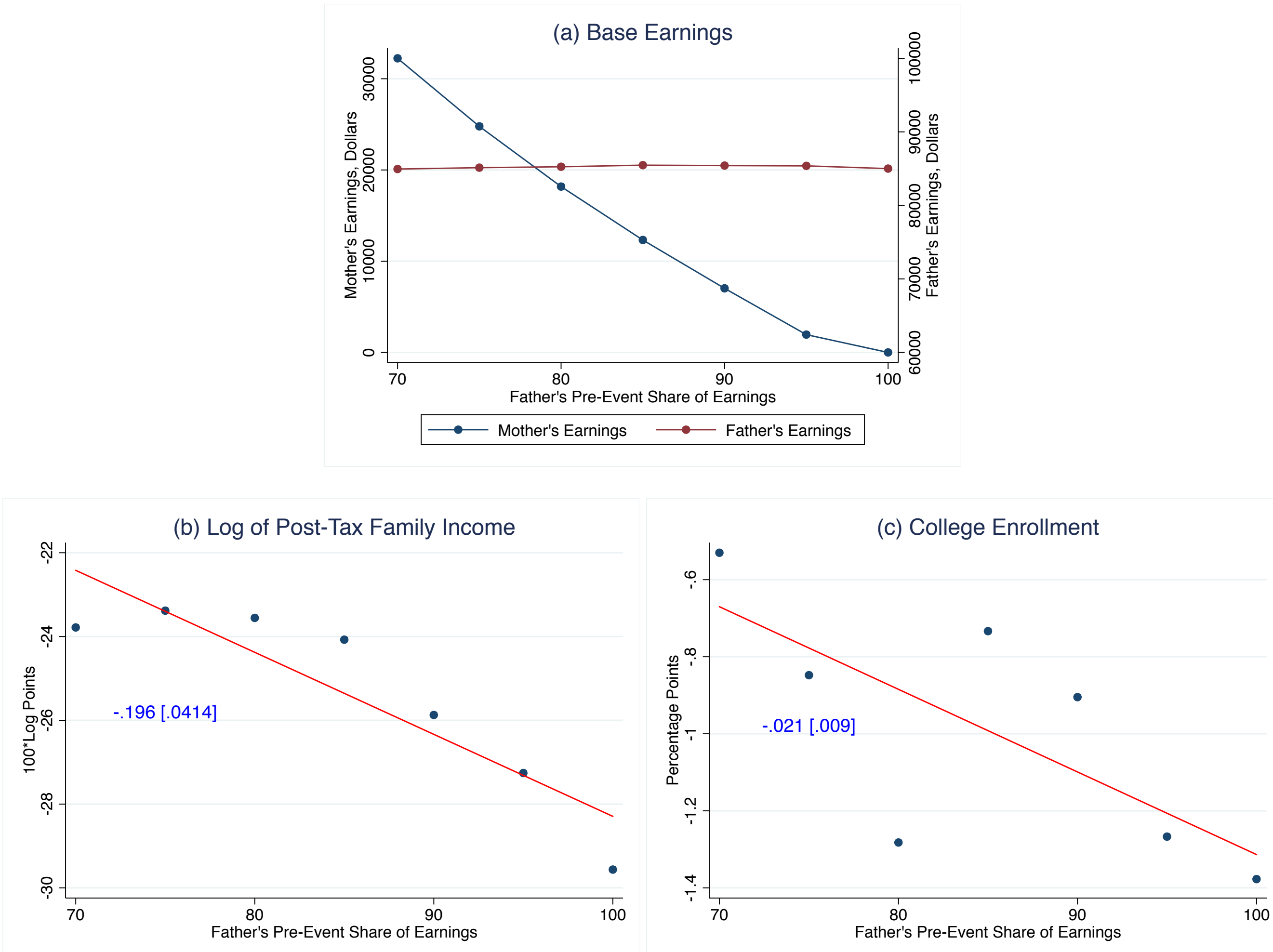


FIGURE 14

Treatment Effects by Father's Earnings Share: Variation from Father's Earnings

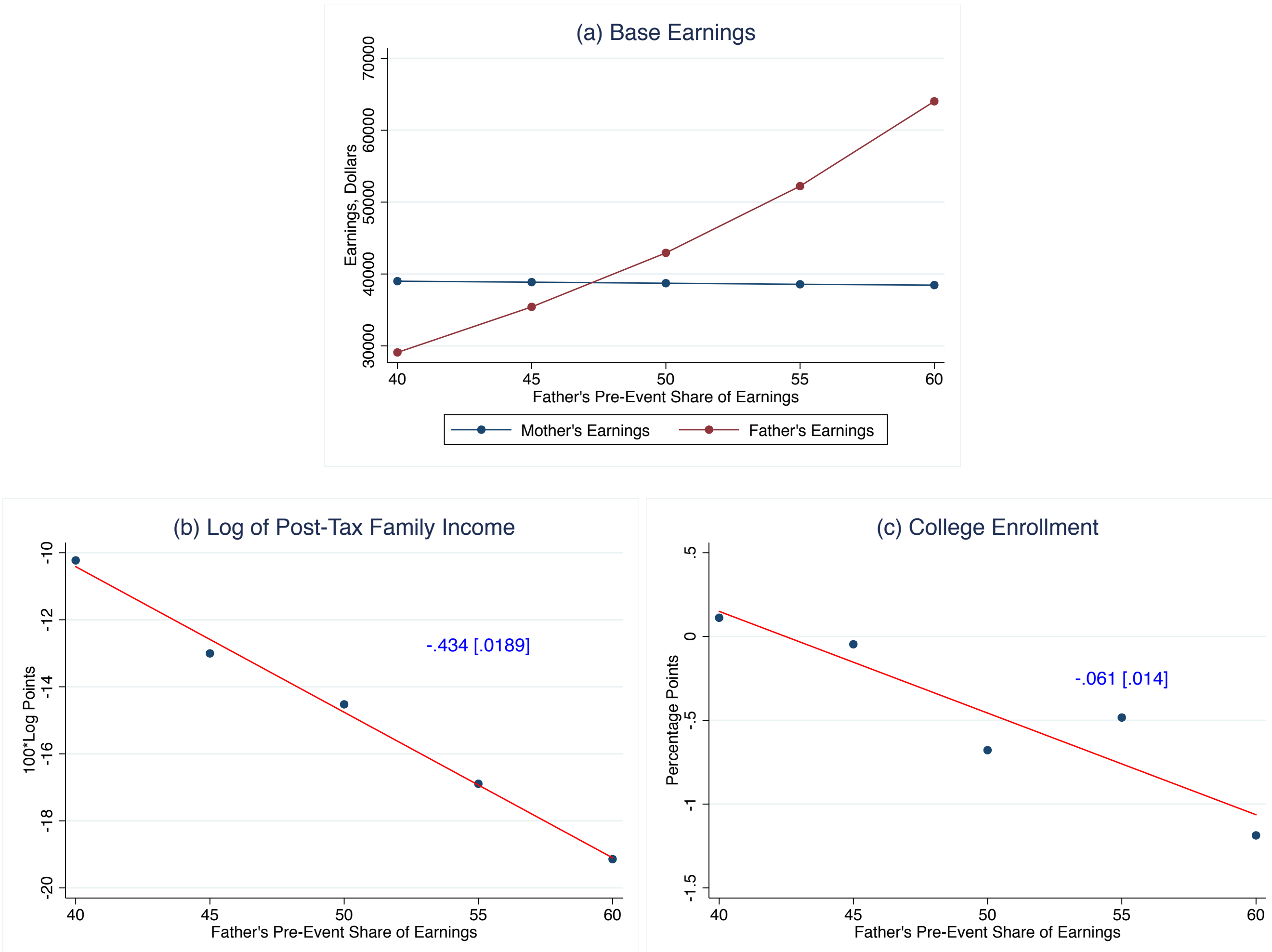


FIGURE 15

Treatment Effects by Predicted Percent Earnings Loss

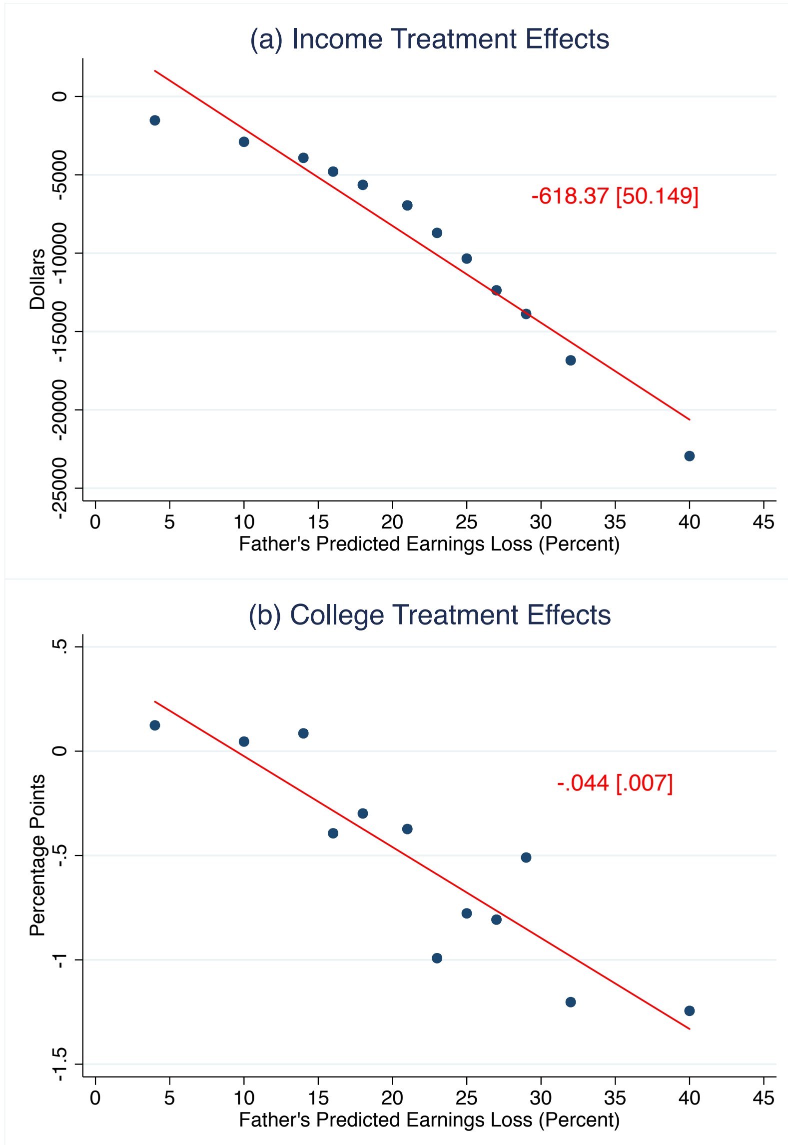


FIGURE 16

Event-Time Studies of Firm Closure

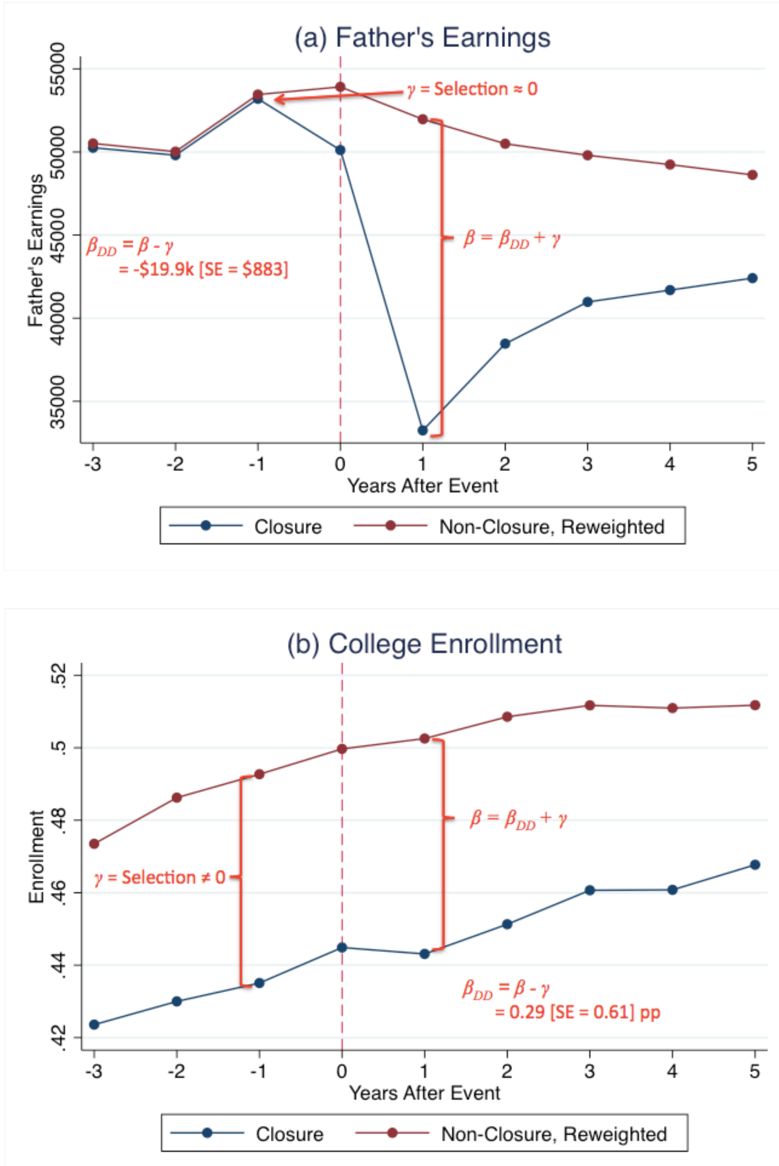


FIGURE 17

Estimated Effects of Firm Closure on Enrollment Ages 18-22 by Income Quartile

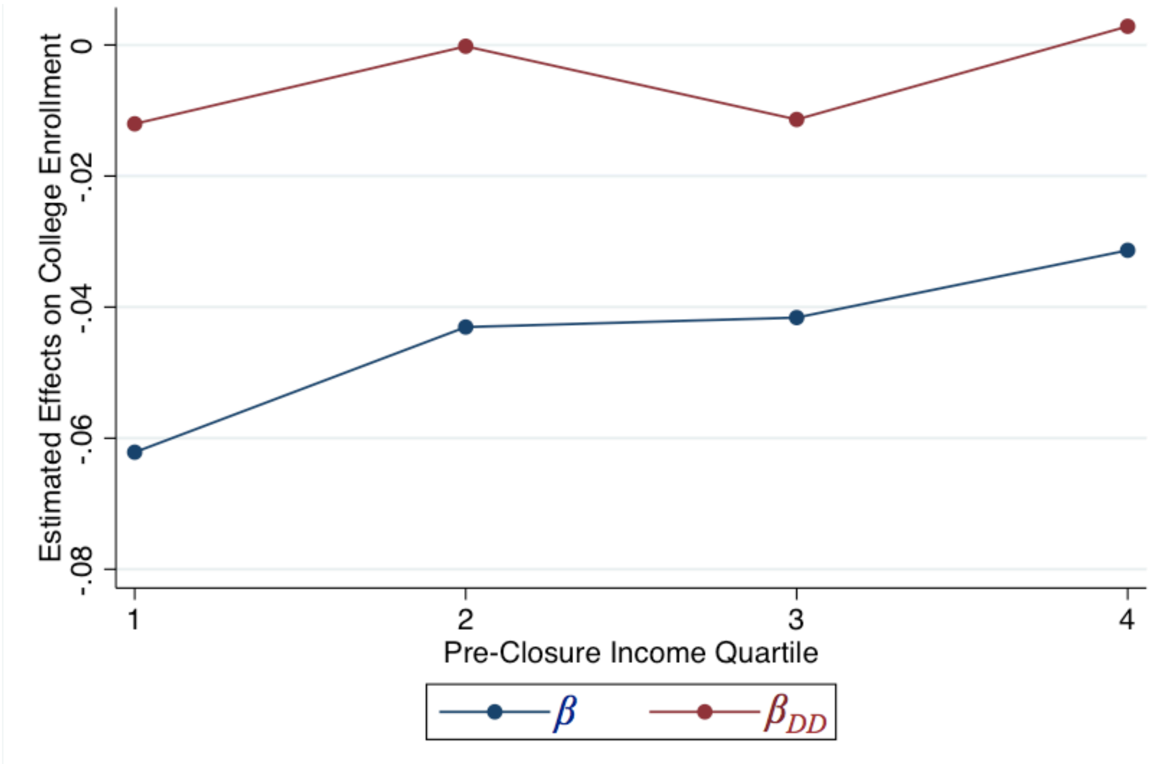
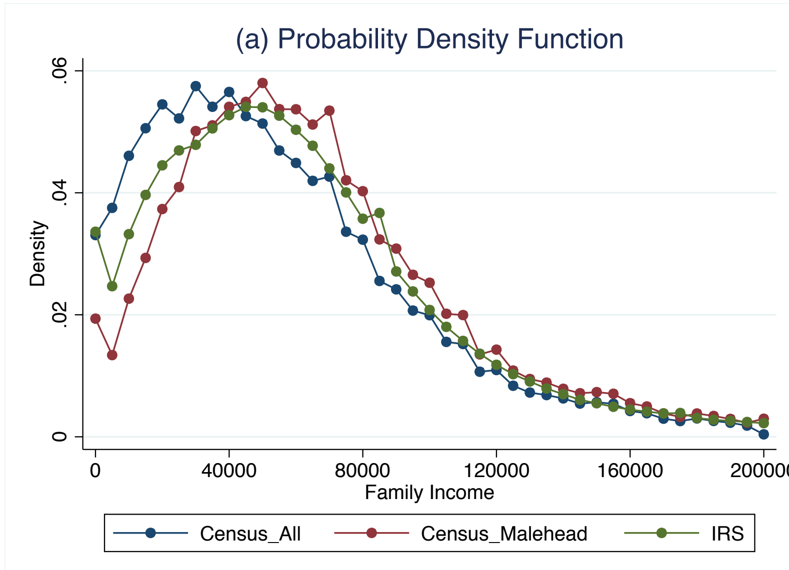


FIGURE A1.1

IRS Linked Sample vs Census

Family Income Distributions of Children Age 14-16 in 2001



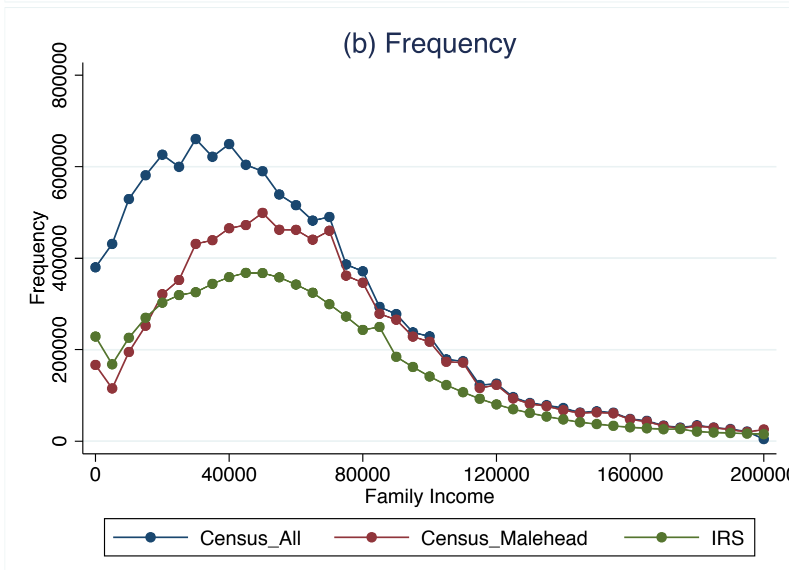


FIGURE A3.1

Outcomes at Age 19 by Event-Age, Layoff Sample

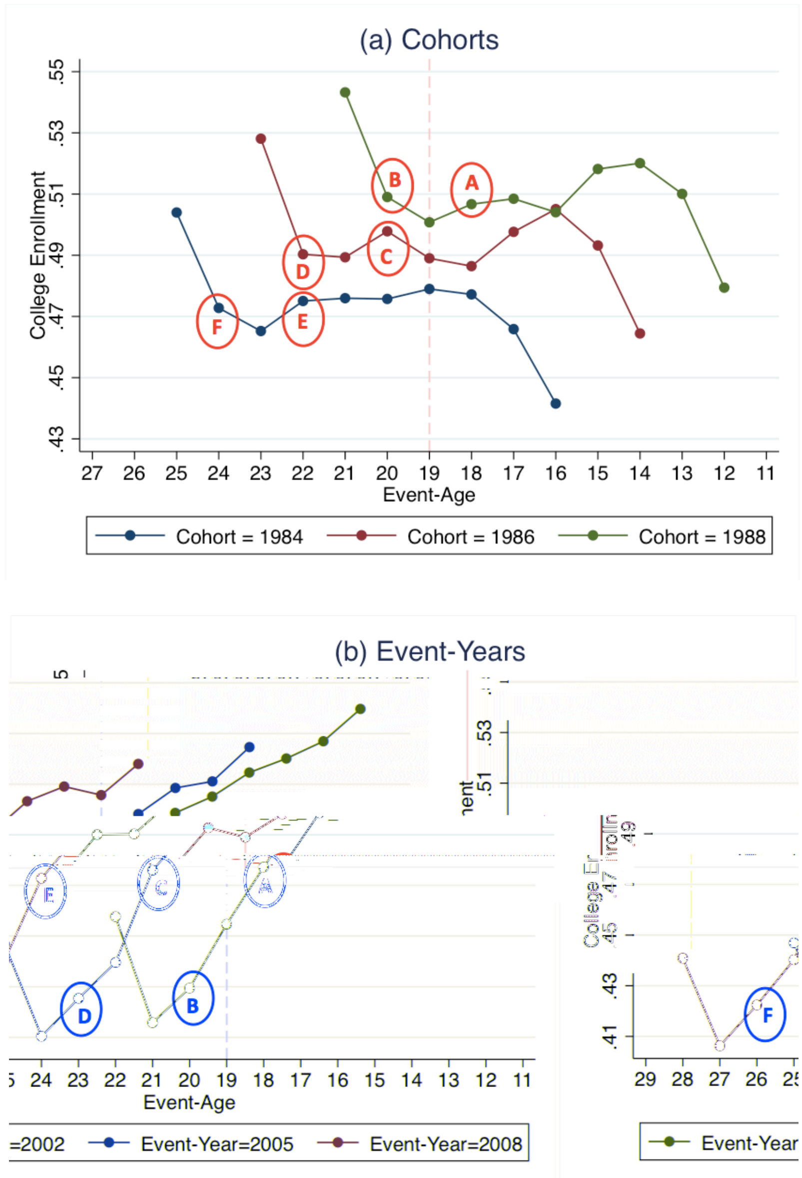


FIGURE A5.1

College Enrollment at Age 19 in 2002 by Family Income

Two Alternative Measures

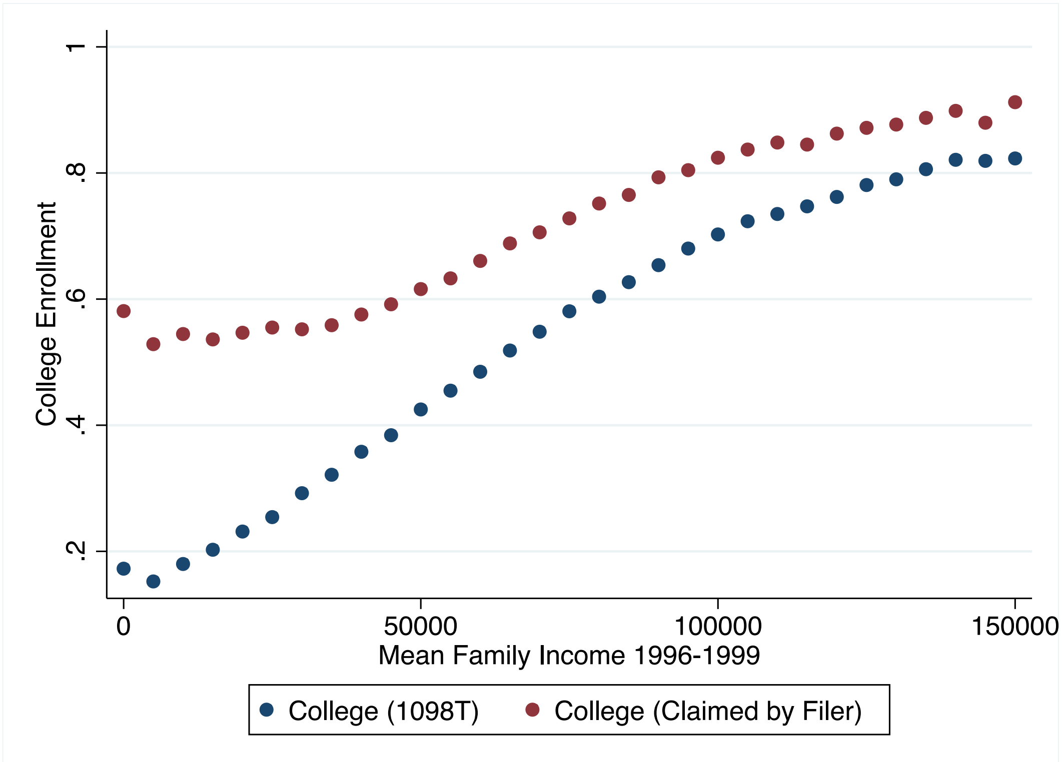


FIGURE A5.2

Event-Age Studies for Alternative College Enrollment Measures