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Author
Volkov, Eden

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Essays on Public Economics

By

EDEN VOLKOV
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Approved:

_____

Marianne Page, Co-Chair

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Marianne Bitler, Co-Chair

_____

Jenna Stearns

Committee in Charge

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ABSTRACT OF THE DISSERTATION

Essays on Public Economics

This thesis investigates how the economic resources available to children are impacted by changes in family structure and the public safety net (Chapters 1 & 2), and how health insurance access impacts the post-secondary school enrollment decisions of young adults (Chapter 3).

Chapter 1 examines the impact of parental union dissolution on the economic resources available to children. Using the 1968-2019 PSID, and multiple measures of economic well-being, I compare the dynamic consequences of parental separation before and after the 1996 welfare reform using an event study model with child fixed effects. My results suggest that welfare reform has contributed to worsening economic outcomes following parental union dissolution.

Using the 1968-2019 PSID, chapter 2 investigates the impact of the 1996 welfare reform and other safety net reforms in the 1990s on components of income and food expenditures available to children whose parents divorce or separate. Using an event study model with child fixed effects, I show changes in earnings, take-up of public assistance benefits, spending on food at home, restaurant meals, and food purchased with SNAP following divorce or separation. Comparing changes in these measures after parental union dissolution before and after the policy changes in the 1990s, for children born to mothers

with a high school degree or less, relative to those born to mothers with more than a high school degree, my results indicate that the policy reforms of the 1990's negatively impacted material well-being among children most impacted these reforms.

Chapter 3 studies the interaction of state and federal dependent health insurance mandates on young adult post-secondary education using data from the 2005-2015 October Supplement of the Current Population Survey, and a Differences-in-Differences-in-Differences (DDD) model. State dependent coverage mandates that pre-dated the 2010 Affordable Care required school enrollment as condition of eligibility, while the ACA did not. I find that the education restrictions of state laws had no effect on post-secondary school enrollment. The 2010 ACA Young Adult Coverage Mandate (YACM) provision ended education-based restrictions, leading to a persistent 4.1-6.3 percentage point reduction in enrollment in post-secondary education which was completely driven by reductions in full-time enrollment and enrollment at four-year colleges among those with state-mandated-access-to-group-insurance.

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Chapter 1: The Economic Consequences of Parental Union Dissolution

1.1 Introduction

Prior work has established that parental union dissolution leads to steep and long lived economic consequences for children (Duncan & Hoffman 1985 ; Page & Stevens (2004)). However, since 1996, there have been a number of economic and demographic changes which may have altered the economic costs of parental separation. These include a decline in marital childbearing, rising female labor force participation, and major reforms to the social safety net.

Using data from the Panel Survey of Income Dynamics (PSID) from 1968-2019, I show how the economic resources available to children following parental union dissolution have been affected in light of these developments. I improve upon the estimation strategy used by most studies that evaluate the economic consequences of parental union dissolution. Using a sample of children that experience parental union dissolution and an event study with child fixed effects, I use within child variation to estimate the dynamic path of economic resource changes following parental union dissolution. Most prior studies used a control group of children that do not experience union dissolution and don't control for child fixed effects which are needed to account for unobserved time-invariant selection into union dissolution. Duncan and Hoffman (1985a) track changes in pre-tax income up to five years after a parental union dissolution among households with children ages 0 to 5, compared to continuously married families from 1971-1977 using the PSID. However, children in this control group could have different pre-trends in resources than children that experience parental separation. For example, research by Charles & Stephens (2004)

shows that job loss often precedes divorce. By not utilizing this control group and relying on within child variation, I can better measure the effect of union dissolution on resources.

The only other paper that uses within child variation to estimate the dynamic consequences of parental union dissolution is Page and Stevens (2004). Page and Stevens (2004) use a similar methodology to Duncan and Hoffman (1985), but they control for child fixed effects, extend their time period through 1993 and track changes in pre-tax income and food expenditures for families with children 0-16 years old. Page and Stevens (2004) is the most comprehensive study to date on the dynamic economic consequences of union dissolution among families with children. I use the same variation as their paper to estimate my effects and contribute to the literature by estimating the dynamic consequences of parental union dissolution since 1996 welfare reform.

In particular, I contrast the experiences of children facing parental union dissolution before welfare reform (1970-1991) with children whose parents' unions ended after welfare reform (1999-2019). We would expect the economic costs to have changed since 1996 because of a compositional change in parental unions away from marriage and toward unmarried cohabiting unions, increasing maternal labor supply, and reforms of the public safety net starting in the 1990s. While Tach & Eads (2004) evaluated the income consequences of parental union dissolution in the early 2000's, they did not estimate dynamic effects. Using an event study approach, they only follow families one year out from the initial dissolution, and unlike the present study they do not control for child fixed effects which are needed to account for unobserved time-invariant selection into union dissolution. My paper uses an event study to track changes in resources up to six or more years following parental union dissolution and controls for child fixed effects. Estimating dynamic effects is important to account for remarriage and other longer run responses to union dissolution.

My paper also captures the role of an increasing share fathers who receive custody of their children on the economic consequences of union dissolution by using a sample of children whose mother *or* father gets custody.[1] This is in contrast to Tach & Eads (2015), who focus exclusively on children whose mother gets custody. Failing to account for increases in paternal custody could lead to upward bias in the losses associated with union dissolution post welfare reform, given prior work establishes men face significantly smaller consequences of union dissolution than women.[2]

Finally, this study uses a more comprehensive set of material well-being measures than prior studies by focusing on post-tax income, food expenditures, and poverty. Descriptive analyses highlight that parental union dissolution is a leading contributor to child poverty. In fact, almost half of American families experienced poverty following divorce from 1968-1982 (Heath 1992). The extent to which the public safety net buffers this poverty risk before and after welfare reform is thus far unanswered in the literature on the economic consequences of union dissolution, and this paper fills this gap in our knowledge. My post-tax income measure captures the role of taxes as well as cash and in-kind benefits, which is important in light of changes made to the cash and tax-based welfare system during the 1990s. Changes in food expenditures following union dissolution have not been considered in recent papers. Results from Meyer and Sullivan (2003) indicate that consumption is a better predictor of well-being than income for disadvantaged groups. Food expenditures is a measure of consumption that directly reflects well-being, and is an important outcome to consider given that union dissolution contributes to economic disadvantage among families with children.

[1] Among union dissolutions that occur from 1968-1996, 15% of fathers get custody. Among union dissolutions that occur from 1997-2019, 25% of fathers get custody.

[2] Hauser et al. (2018), de Vaus et al. (2017).

On aggregate, my event study estimates show that children facing union dissolution experience prolonged income consequences of union dissolution after welfare reform as compared to before welfare reform. In contrast, I find smaller percent declines and more recovery in food expenditures. Finally, I find that the change in the poverty gap following union dissolution is the same in both time periods.[3][4] The event study estimates show that percent declines in post-tax income within a year of union dissolution are similar before and after welfare reform. Specifically, income declines 19% before welfare reform and 26% after welfare reform. After welfare reform, income losses grow and do not recover following union dissolution, while prior to welfare reform there is more income recovery. Pre-reform, income remains 11% below its pre-dissolution level four to five years after union dissolution, while post reform, income is still 27% lower.[5] Immediate percent declines in food expenditures are smaller after welfare reform, and there is more recovery. I show that percent declines in food expenditures are smaller, even though percent declines in income are larger, because a larger share of children are born to cohabiting parents after welfare reform. Research by Deleire & Kalil (2005) shows that cohabiting parents spend a smaller share of their income on food than single parents. Meanwhile, married and divorced parents spend the same share of their income on food.[6] As cohabiting unions have become more commonplace since welfare reform, the percent declines in food expenditure following union dissolution are smaller.

Underlying the aggregate income and poverty results there is important heterogeneity that highlights the impact of the 1996 cash assistance reforms. In particular, I find less income recovery four to five years following parental union dissolution among children

[3] All income and expenditure-based measures are in real $2019.

[4] The poverty gap is defined as the difference between family unit post-tax income including cash and in-kind benefits and the anchored supplemental poverty threshold. Families that have income above the threshold, have a poverty gap of 0.

[5] This differences in magnitudes are not statistically significant.

[6] Authors estimates using the PSID confirm this.

born to mothers with a high school degree or less. Prior to 1996, I show suggestive evidence that AFDC reduced income losses following union dissolution. After the 1996 reform, the TANF program no longer reduces income losses following union dissolution. I show that the reduced responsiveness of the AFDC/TANF program over time is due to lower benefit generosity as well as lower take-up. I also find suggestive evidence that among children born to mothers with a high school degree or less, the depth of poverty following union dissolution is more severe. Despite worsening income-based outcomes for these children, changes in food expenditures following union dissolution have remained unchanged. This is because the rise in child-bearing in cohabiting unions has been most pronounced among this subset of families.

The rest of the paper is laid out as follows: Section 2 provides background information regarding major changes in family structure, female labor force participation and the public safety net. Section 3 describes the PSID data and presents descriptive statistics. Section 4 describes the event study model used this paper. Section 5 presents my results, and Section 6 details this paper's conclusions.

1.2 Background

Prior papers have established that prior to 1996, children experience steep and long lived economic consequences of union dissolution. Since 1996, there have been a number of economic and demographic changes which may have changed the economic costs of parental separation. These include a decline in marital childbearing, rising female labor force participation, and major reforms to the social safety net.

In 1996, the US Congress overhauled the cash safety net as part of the 1996 Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA). PROWRA only

affected the costs of union dissolution for low income children. This federal reform came in the wake of waivers granted to states starting in 1992. By the time the federal legislation passed, 27 states used these waivers to reform their AFDC programs in ways that were ultimately federalized as part of 1996 welfare reform. Waiver policies included family caps, time limits and work requirements. Blank (2002) presents a review of the research using variation in state waiver policies to evaluate the impacts of these policies on caseloads and work participation rates, among other outcomes. These papers find that waivers reduced caseloads and increased work participation rates among single mothers.

Once the largest cash assistance program available to single mothers, the Aid to Families with Dependent Children (AFDC) program was replaced by the Temporary Assistance for Needy Families (TANF) program in 1996. Unlike AFDC, TANF receipt is generally limited to a federal maximum of five years, although some states have time limits shorter than five years. The TANF program is also available to two parent families, whereas AFDC was generally limited to single parents. Finally, because a major goal of welfare reform was to encourage labor force participation, the non-working poor are generally ineligible for TANF.

TANF replaced the matching fund arrangement under AFDC with a block grant. The TANF block grant is fixed in nominal terms and the contribution for each state is determined by the federal AFDC matching grant contribution in the years before PRWORA. As a result, benefits paid to families have been falling in real terms. According to a Congressional Research Service Report, by 2021 the real value of the TANF block grant has fallen 40% since 1997. In addition to the nominal value of the benefit falling, cash assistance makes up a much smaller share of TANF spending than it did under AFDC, as states are using funds for in-kind benefits and services like child care, marriage promotion, and job training. In FY2020, the average state only spent 22% of their block grant on direct cash

assistance. [7]

De-coupling cash assistance receipt from single parenthood, the reduction in the value of cash assistance benefits through inflation erosion, allocation of TANF funds away from cash provision, and limiting the program for non-working families with children, could increase the economic costs of union dissolution after welfare reform, especially so for non-working parents. In fact, research by Bitler & Hoynes (2016) shows that TANF is unresponsive to labor market shocks.

While 1996 welfare reform restricted non-tax-based cash assistance, tax-based income support programs for working families expanded in the early 1990s. The 1993 expansion of the Earned Income Tax Credit (EITC) increased the maximum benefit available to families with one child by just over $1,000 (real $2019) and increased the maximum benefits available to families with two or more child by $2,300 ($2019) [8] Hoynes & Patel (2018) find a policy induced $1000 increase in the EITC leads to a 7.3 percentage point increase in employment and a 9.4 percentage point reduction in the share of families with after-tax and transfer income below 100% of poverty. The increased generosity of the EITC could lower the economic consequences of union dissolution though the mechanical effect of increased benefit generosity for already working parents. However, for all parents, the effect of EITC expansions on the income consequences of union dissolution are ambiguous because the maternal labor supply effects are ambiguous. In particular, research has shown that the EITC led to increased labor supply of single mothers, but decreased labor supply of married mothers. [9] In this paper's setting, roughly half the children in my sample,

[7] https://sgp.fas.org/crs/misc/RL32760.pdf

[8] Originally enacted in 1975, the maximum EITC credit was $400 and phased out between incomes of $4,000 and $8,000. The Revenue Act of 1978 increased the maximum credit to $500. The credit further increased to $550 in 1984, and then $800 in 1986. The Omnibus Reconciliation Act of 1990 increased the maximum credit for families with two or more children.

[9] Eissa and Hoynes (2004)

go from having a married mother to a single mother. The other half of children go from having a cohabiting mother to a single mother. If married mothers earn less as a result of EITC expansions, then after 1996, union dissolution should result in larger percent declines in earnings. However, if as a result of the EITC, these mothers start working and earning more when they are single, this could lead to similar or smaller percent declines in earnings following union dissolution over time. Thus, it is not immediately clear whether percent declines in total family income would be smaller or larger after the 1990's.

In addition to the expansions made to the EITC, the Child Tax Credit (CTC) was enacted as part of the Taxpayer Relief Act of 1997. The original CTC was a $500-per-child nonrefundable credit to provide tax relief to middle- and upper-middle-income families. Given the credit is non-refundable, only families with a tax liability are eligible. Since 1997, several laws have been enacted that expanded the availability of the benefit to more low-income families while also increasing the value of the tax credit.[10] Unlike the EITC, Hoynes & Rothstein (2016) find most of the benefits of the CTC go to higher income households. This suggests that this policy may only contribute to lowering the economic costs of union dissolution for this subset of families. Both the EITC and the CTC will also be less effective than the non-tax-based safety net at providing an immediate financial buffer following parental union, given increases in benefits won't occur till the tax year following the union dissolution. Furthermore, eligibility or benefits for these respective programs do not increase in response to union dissolution, so percent declines in post-tax income may be unaffected by the increasing generosity of these programs. However, even if these programs don't affect percent declines in income, they could still lower the poverty gap.

[10]The Economic Growth and Tax Relief Reconciliation Act of 2001 increased the amount of the credit over time to $1,000 per child and made it partially refundable under the earned income formula. The Emergency Economic Stabilization Act of 2008 and the American Recovery and Reinvestment Act of 2009 further expanded the availability and amount of the credit to taxpayers whose income was too low to either qualify for the credit or be eligible for the full credit. Finally, at the end of 2017, Congress enacted P.L. 115-97 which doubled the maximum amount of the credit.

Overall it is not immediately clear how these safety net reforms affect the economic consequences of union dissolution for low income families with children. By restricting benefits for the non-working poor, 1996 welfare reform may exacerbate the economic shock of union dissolution for children whose parents are unemployed. This is a salient concern given there is evidence that job loss leads to divorce (Charles & Stephens, 2004). Expanding categorical eligibility for public cash assistance to two parent families makes TANF less responsive to union dissolution than AFDC, thereby increasing the economic costs. Five year time limits could reduce income recovery following parental union dissolution if the likelihood of re-marriage is unaffected or reduced by the reform. Alternatively, if public benefits and spousal earnings are substitutes, five year time limits on benefit receipt could incentivize re-marriage, leading to more income recovery.[11] Finally, the maternal labor force participation effects of tax-based cash assistance programs are ambiguous in this setting, but the reforms to the EITC program and the CTC could at a minimum lower the poverty gap following union dissolution.

Starting in the 1970s, women entered the "quiet revolution" phase of female labor force participation (Goldin 2006). The "Quiet Revolution" is characterized by delayed marriage and increased labor force participation and educational attainment among women who plan life long careers over temporary jobs. I show descriptive evidence that among mothers that eventually divorce or separate, only those with more than a high school degree increased their labor supply since the 1990s (Figure 6). This suggests that children born to lower skilled mothers experience larger economic costs of union dissolution given these mothers do not increase their labor supply after 1996, and the public cash safety net is less generous for non-working parents. In contrast, increased labor supply among middle

[11]Bitler et al. (2004) find that implementation of TANF is associated with a 17-21 percent decline in marriage rates, relative to the AFDC program. However, affects on marriage may depend on marriage parity.

to high skill mothers implies that their children could experience smaller consequences of union dissolution since 1996.

Middle to high skilled mothers were not only more likely to be working since 1996, but they were also the group of mothers that continued to have children in marriage at similar rates as they did in 1970.[12] In contrast, there was a retreat from marital childbearing among low skilled mothers (Lundberg et al. 2016). In 2019, among children born to mothers with a high school degree or less, the share born to married parents has fallen by 40 percentage pints (57%) since 1970. The retreat from marital childbearing was accompanied by a rising share of children born into unmarried cohabiting unions.

Cohabiting unions are more economically disadvantaged and unstable than marriages. Children born to cohabiting parents in 1996 represented less than 30% of the children whose parents separated. By 2017, 50% of children whose parents separated were born to unmarried cohabiting parents. Tach & Eads (2015) document that the short term income losses associated with cohabiting union dissolution have grown since the 1990s, while those associated with divorce have slightly improved. This could be because children born into cohabiting unions were most impacted by the 1996 welfare reform. Since a growing share of children are born into these unions, understanding how public safety net reforms affect economic consequences following union dissolution is especially important.

Additionally, Deleire & Kalil (2005) show that unmarried parents with children spend a lower share of their income on food than do single parents, but married parents spend the same share of their income on food as divorced parents.[13] All else equal, as cohab-

[12]Among children born to mothers with more than a high school degree, 83% were born to married parents in 1970. In 2019, the corresponding share was 77%.

[13]To illustrate why a growing share of children born to cohabiting parents will lead to lower percent declines in food expenditures, consider the following example: A cohabiting couple spends 25% of their income on food and a newly single formerly cohabiting parent spends 30% of their income on food. If

iting unions become a larger share of all unions with children, percent declines in food expenditure following union dissolution should fall over time.

1.3 Data

I use data from the 1968-2019 Panel Study of Income Dynamics (PSID) to investigate how parental union dissolution affects children's access to economic resources. The PSID is a longitudinal survey administered by the University of Michigan's Institute for Social Research. The PSID data begins in 1968 and has detailed information on family and individual economic resources, demographics, and family structure. Importantly, the longitudinal structure of the PSID allows me to control for fixed effects.

The original PSID sample of roughly 18,000 people living in 5,000 households was made up of a nationally representative sample, called the Census sample, and an over-sample of low income families, called the Survey of Economic Opportunity (SEO) sample. The SEO sample was included to enhance research into poverty-related issues. Families from the Census and SEO samples and their split-offs constitute the Core PSID sample and were interviewed annually from 1968-1996 and biannually from 1997-2019. My analysis uses families in the Census and SEO samples to increase the precision of the estimates and because the low income sample is a high impact sample for my analysis. My estimates are weighted using the child longitudinal weight in the last year the child is observed to ensure a nationally representative sample. The child's weight is used because this analysis is based on the family resources available to a focal child.

income declines 30% following the parent's separation, food expenditures would decline 16%. In contrast, a married couple spends 25% of their income on food and a divorced parent spends 25% of their income on food. If income declines 30% following the parent's divorce, food expenditures would decline 30%. This simple example shows that all else equal, if the share of children born to cohabiting parents increases over time, the percent decline in food expenditures following union dissolution should fall.

After 1997, the PSID surveys families every other year. This means that before 1997, I have data on family resources every year, but starting in 1997, I only have data on family resources every other year. Because of the way PSID survey questions are asked, if a child's parents separated after 1996 in an even year, I will have data on income and poverty status available to the family in even event times and data on food expenditures in odd event times. If a child's parents separated after 1996 in an odd year, I will have data on income and poverty status available to the family in odd event times and data on food expenditures in even event times.[14] To ensure that any changes in the estimated affects of union dissolution since 1996 are not due to changes in survey design, I define my event times the same way when estimating effects before and after 1996. In particular, I create event time bins to capture resources -3 or more, -2 or -1, 0 or 1, 2 or 3, 4 or 5, and 6 or more years since the "event".[15]

1.4 Empirical Strategy

The basic approach taken in this paper is to use event study regression with child fixed-effects to control for time-invariant unobserved family characteristics that may be correlated with union dissolution. Given longitudinal data on family income, poverty thresholds, food expenditures and partner histories, the effects of union dissolution can be modeled by:

[14]For example, if the union dissolution occurred in 2002, then I will have data on resources after union dissolution in years 2003, 2005,..etc. In a given survey year, data on income and poverty status refers to the prior calendar year. Thus, data on income or poverty in 2003, refers to income or poverty in 2002. This corresponds to 0 years since the "event". Data on income or poverty in 2005 would refer to income or poverty in 2004, or 2 years since the "event". Data on food expenditures in 2003 refers to the value in 2003 and thus represents expenditure 1 year since the "event". Data on food expenditures in 2005 refers to the value in 2005 and thus represents expenditure 3 year since the "event".

[15]The results in Appendix Tables A6-A8 show that my main results are not sensitive to how I bin the event times.

$$(1)\quad \mathbf{Y}_{i,t} = \beta \mathbf{X}_{i,t} + \omega D_{-3} \mathbb{1}\{t - E_i^* \leq -3\} + \sum_{d=(0,1)}^{(4,5)} \theta_d D_i \mathbb{1}\{t - E_i^* = d\} + \gamma D_{6+} \mathbb{1}\{t - E_i^* \geq 6\} + \tau_t + \alpha_i + \varepsilon_{i,t}$$

where $\mathbf{Y}_{i,t}$ is a family unit resource measure for child i, in year t = 1968,..., 2019.[16] Income, poverty thresholds, and food expenditures are all expressed in real \$2019. τ_t are calendar year fixed effects which absorb national trends in outcomes over time. $\mathbf{X}_{i,t}$ is a set of time-varying child/family-specific control variables like dummies for family unit size, dummies for parent's age and child's age, and family food needs.[17]

The time-invariant child-specific effect, α_i captures anything about the child that is constant over time. Since most children in single-parent families live with their mothers, this variable will primarily pick up time-invariant characteristics of the child's mother that could be correlated with parental union dissolution and family income. For example, if mothers with higher earnings capacity are more susceptible to divorce, then estimates of divorce effects that fail to control for α_i will be biased toward finding smaller losses.

I use a binary indicator of "treatment", D_i, equal to 1 if child i experienced a parental union dissolution. The estimates characterizing the consequences of parental union dissolution are the coefficients on the interaction of D_i with event-year dummies, $\mathbb{1}\{t - E_i^* = d\}$, which are equal to 1 when the year of observation is d = -10,...,0,...,14 years from E_i^*, the year when the child's parents separated (d = (-2,-1) is omitted). I only observe resources up 10 years before and 14 years after

[16]For income and food expenditures my outcomes are specifically Log(Y). Logs are used to measure percent changes. Measuring level changes in income don't directly reflect changes before and after reform in the economic consequences of union dissolution because incomes are higher post welfare reform. Furthermore, the levels specification is not ideal for the main analysis because parental union dissolution is likely to have very different level effects on rich and poor families. Observations with an outcome value of 0 represent fewer than 0.3% of observations, so using Log(Y) as my outcome only results in a very small amount of data loss.

[17]The family food needs variable is constructed by the PSID to measure the caloric needs of the family, accounting for family size, sex, and the age of the family members. This control variable is only used when family unit food expenditures is the outcome.

parental union dissolution because I am only estimating effects for children who experience union dissolution from ages 2-10 and children are only observed from ages 0 to 16.

My sample only includes children who experienced a union dissolution from ages 2-10 because children who were older than 10 will not contribute to estimating all post event time coefficients. Children that experienced union dissolution before age 2 cannot contribute to estimating the base event time. Pre-period trends in outcomes are captured by the dummy, $\mathbb{1}\{t-E_i^* \leq -3\}$, and resource recovery 6 or more years after parental union dissolution is captured by the dummy, $\mathbb{1}\{t - E_i^* \geq 6\}$. The coefficients on θ_d describe the divergence in outcomes d years after union dissolution, relative to the pre-period, adjusting for model covariates. Standard errors are clustered at the family level.

Even though the estimation of the $\mathbb{1}\{t - E_i^* \leq -3\}$ dummy uses an unbalanced set of years, the figures and tables will show the estimated coefficient on this event time dummy to provide visual evidence of parallel pre-trends. In Appendix Table A10 I show that my income results are not sensitive to the choice of the number of pre-years included in the (-3+) bin.[18] The estimation of the $\mathbb{1}\{t - E_i^* \geq 6\}$ dummy also has an unbalanced set of years, and this coefficient does not directly reflect more or less resource recovery following union dissolution before and after welfare reform. As such my tables and figures will exclude the estimate the this coefficient.

To assess whether children face different economic consequences following parental union dissolution after 1996 welfare reform, I separately estimate the outcome dynamics post dissolution for children whose parents separate from 1970-1991 (before welfare reform) and 1999-2019 (after welfare reform).

[18] In particular, this coefficient is never statistically different from 0 and is close to 0 in magnitude. I have estimated models showing that the -3 estimate is not sensitive to the choice of number of pre-event years for any of my outcomes. These results are available from the author.

1.4.1 Estimation Sample

This paper's identification comes from comparing children's access to economic resources after a parental divorce (or the end of a non-marital cohabiting partnership), relative to before. As such, my sample consists of an unbalanced sample of children who are born into two biological parent families and are potentially followed from the year of birth until age 16.[19] The change in economic resources following the first parental union dissolution experienced by children born into two biological parent families directly reflects the economic consequences of going from a two parent to a one parent family. This analysis leaves out the experiences of children born to single mothers, and children born into two-parent families that never divorce or separate.[20]

A child is only followed up to age 16 because living with parents past age 16 could be an endogenous response to resources available in the family unit (Dey et al. (2014)). Children who were born before the 1968 survey are excluded from the sample because I cannot determine whether they were born into a two-parent or single-parent family.[21] I only use data on children who experienced a parental union dissolution from ages 2 to 10 years. This ensures that children's age at parental union dissolution is balanced across event times -2 to 6+. This represents 70% of children born into two-parent families who experience a parental union dissolution by age 16. I do not include children whose data come from adoption records because I cannot observe their resources at birth and I exclude cases where the union dissolution results from a parental death.

My main results use a sample of children whose mother *or* father gets custody after union dissolution. Using this sample allows me to capture the role of an increasing share of children whose father gets custody over time on the economic consequences of union dissolution for

[19] The adults that I observe living with a child in the birth year, are both the child's biological parents.

[20] Children born to single mothers may experience the end of the union between their biological mother and a step-parent. I do not capture the end of a step-parent union, because the pre-period will reflect years in which the child was living with a single mother, thus the event study estimates will not speak directly to how resources change when a two-parent family becomes a one parent family.

[21] Data on family structure is based on the current structure in a given year and the first year of the PSID is 1968.

children. I have also estimated results for a sample of children whose mother gets custody after union dissolution and for a sample whose father gets custody after union dissolution. These results indicate that the income consequences of parental union dissolution for children whose mother gets custody are similar to the broader sample. There is also suggestive evidence that income losses following parental union dissolution have grown for children whose father gets custody. Specifically, there are no family income losses after union dissolution before welfare reform. After welfare reform, income falls between 10 and 18% following union dissolution are less but these losses are not statistically different from zero given the small sample size. These estimates are available from the author. All sample restrictions and how they affect the final analysis sample are outlined in the Appendix.

After deleting observations for which the income and consumption measures are imputed or missing, my sample consists of 2,7994 children (28,620 child years). The parental union dissolution resulted in the mother getting custody in 81% of cases, while in 19% of cases the father got custody. These samples are slightly smaller when food expenditures or income including in-kind benefits are my dependent variables since the PSID is missing food expenditures and food stamp data in 1973, 1988, and 1989.[22]

Not all children are present from birth up to age 16. 4.3% of children that I observe from birth attrit from the survey before the child turns 16. 13.6% of children I observe from birth don't respond to the survey for a number of waves and eventually return.[23] I use all available data for children because in the year of the child's return I have data on whether a person moved in or out since the last year the family was surveyed, as well as the specific year a person moved in or out. This data is available for all members of the family unit. This data allows me to construct a measure of family size in the calendar year before the survey year and to know the exact year of

[22] I have left the outcome variable as missing so that an estimate of the event time corresponding to the missing year of data is not estimated. This shouldn't bias my estimates of the event time coefficients because the fact that a certain variable is missing in 1973, 1988, and 1989 has nothing to do with its hypothetical value or with the values of other variables. The variable's value is missing completely at random (MCAR).

[23] 95% of these children return between 2 and 4 years later.

the union dissolution if the child's data is missing in some years.[24]

My estimation model will include child fixed effects in order to control for time-invariant selection into attrition or non-response. However, the decision to include observations from children with missing data could bias my results if attrition or survey non-response in any time period is a function of time-varying shocks. If there is no time-varying selection into attrition or survey non-response, then the unbalanced results are unbiased estimates of the effect of parental separation on family level economic resources. To investigate whether time-varying selection into attrition or survey non-response is biasing my unbalanced panel results, I re-estimate my results using a balanced panel. The balanced panel results provide an internally consistent effect of union dissolution on family level economic resources. Given that my balanced and unbalanced panel results are statistically and substantively similar, this suggests that time-varying selection into attrition or survey non-response is not biasing my results in this setting. As such, I will use the unbalanced panel for increased precision.[25]

1.4.2 Summary Statistics

Table 1 is a cross-sectional comparison of the demographic and socioeconomic characteristics of different family types before and after welfare reform. Columns 2-5 & 7-10 of Table 1 show that even before union dissolution, average family resources are lower for children whose parents will eventually divorce or separate than for families that will remain intact. This suggests that part of the difference in resources could exist for reasons other than differences in family structure. Given families that separate already have lower resources before separation, cross-sectional analyses will overstate the resource consequences of going from a two-parent family to a one-parent family. My model will include child fixed effects to address this concern, by controlling for unobserved

[24]Given that data on income and food stamp receipt in a given survey year refers to their respective values in the prior calendar year, I need to construct a measure of family size in the calendar year prior to the survey year to temporally align the independent and dependent variables.

[25]Appendix Tables A3-A5 re-estimate my results using a balanced panel of children who are present in the survey from event time (-2,-1) to (4,5). I will further discuss the balanced panel construction and results in the section on robustness checks.

time-invariant selection into union dissolution.

This cross-sectional analysis also shows the share of children born into two-parent families, has remained constant over time. What has changed, is the stability of parental unions, as well as the composition of two-parent families. In particular, before and after welfare reform, 75% of children are born into two biological parent families. Among children born before welfare reform, 40% of children born into two-parent families experience parental union dissolution by age 16. Among children born after welfare reform, 33% of children born into two-parent families experience parental union dissolution by age 16. This reflects greater stability in parental unions over time.

An increasing share of children born in two-parent families that experience union dissolution are born to unmarried cohabiting parents. Before welfare reform, 25% of these children are born to unmarried cohabiting parents. After welfare reform, the share born to unmarried cohabiting parents rose to 46%. Using data from the CPS March Supplement, the Pew Research Center notes the share of unmarried cohabiting parents living with a child has risen from 2% in 1968 to 35% in 2016. This indicates a larger share of all children are living in cohabiting unions and not just children whose parents will eventually separate. One difference between my sample and the national statistics is that my sample has a larger of share or children born to cohabiting parents because I am specifically looking at children born into two-parent families whose parents divorce or separate. Parental union dissolution is more likely in cohabiting unions than in marriages, which is my sample over represents children in cohabiting unions, relative to the national average.

On average, children born into two-parent families that experience union dissolution are more socioeconomically advantaged over time, a falling share depend on the cash safety net, and a rising share depend on the in-kind and tax-based safety net. Furthermore, children in cohabiting unions utilize the safety net at higher rates than children of married parents. Figures 1-3 graphically summarize how the composition of families has changed from 1968-2019. Figure 4 indicates how

the composition of families in my sample has changed from 1968-2019. Given that the composition of separating families has changed, as have the types of resources accessed by these families, the economic consequences of union dissolution could be different after welfare reform.

1.4.3 Measures of Economic Resources

I use three measures of family level economic resources. My measure of income is family unit post-tax income including cash and in-kind transfers. I use a post-tax measure of income given that post-tax income better captures disposable income and because tax credits became increasingly important for many low-income families after the EITC expansions (Grogger 2004; Hoynes, 2009). I also include in-kind benefits given research highlighting the importance of these benefits among economically disadvantaged families (Fox et al. 2015). My income measure is expressed in real $2019, and is the sum of AFDC/TANF benefits, Social Security (including DI and OASI), SSI, child support, alimony, pensions, annuities, unemployment insurance, worker's compensation, help from relatives, labor market earnings, asset income, the dollar value of in-kind benefits like food stamps and energy assistance, the Earned Income Tax Credit (EITC), and the Child Tax Credit (CTC), net of federal and state taxes and social security contributions.[26] The advantage of this resource measure is that it captures a family's total spending capacity.

My second measure is family food expenditures. Family food expenditures are defined as the sum of money spent by the family unit on food consumed at home (excluding food purchased with food stamp benefits), food delivered to the home, food consumed away from home, and the dollar value of food stamp benefits. Yearly family food expenditures is expressed in real $2019. Several researchers argue that expenditure-based measures are preferable to income measures among poor households because these households under-report transfer income (Meyer, Mok and Sullivan (2015)). Food expenditures is the only form of expenditure available in the PSID that directly captures well-being as is available in most survey years. Furthermore, food expenditures

[26]Tax liabilities and subsidies are calculated using the NBER Taxsim27 program created by Daniel Feenberg and Elizabeth Coutts (http://www.nber.org/taxsim/).

is an important outcome to measure because research has shown that households are less able to smooth food expenditures when income shocks are more severe and unexpected (Cullen et al. 2004).[27] Last, the economic development literature suggests that consumption is preferred to income for measuring material well-being, particularly among poor households (Cutler & Katz 1991).

A disadvantage of using food expenditures is that one might expect to see less variation in food expenditures than in almost any other component of consumption. It's plausible that families may spend down their savings to maintain some threshold level of food expenditures, which would attenuate the observed consequences of union dissolution. Furthermore, parents may cut down disproportionately more on their food expenditures to ensure more consumption smoothing for their children. The latter point is not captured in the family unit level measures of food expenditures. Given that there is no longitudinal data set that would allow me to investigate this issue, this caveat should be kept in mind when interpreting my results. Finally, food expenditures could be lower than food consumption among very low income families given that I do not capture WIC, school meals, and pantries.[28] Therefore, the changes in food expenditures I capture could overstate the food consumption changes following parental union dissolution for very low income families.

My third measure is the poverty gap. This measure speaks directly to economic hardship. Specifically, I measure the gap between family post-tax and transfer income including in-kind benefits and the anchored Supplemental Poverty Measure threshold.[29] Using the anchored SPM

[27]While parental union dissolution may not be entirely unexpected, all evidence indicates this event is a large and persistent income shock.

[28]Free and reduced-price school lunches are available to low income children ages 5-18.

[29]The National Academy of Sciences (NAS) documented in 1995 report (Citro & Michael, 1995), that the Official Poverty Measure (OPM) understates the extent of poverty by using thresholds that are outdated and may not adjust appropriately for the needs of different types of individuals and households, in particular, families with children and the elderly. Furthermore, it overstates poverty and understates the role of government policies especially since the 1990s expansions to the EITC and the growth of the SNAP program starting in the 2000s (Blank, 2008). The anchored threshold fixes the SPM threshold at the 2019 level. This transforms the poverty measure into an absolute poverty standard. There are two advantages of using an absolute poverty standard, as opposed to a relative poverty threshold like the original SPM threshold.

will allow me to directly investigate how changes in income and transfer payments have affected the depth of poverty following union dissolution over time.

I construct anchored SPM thresholds and measure the poverty gap from 1968-2019 by approximating the approach taken by Wimer et al.(2016).[30] The income resource measure in Wimer et al.(2016) takes pre-tax income including cash transfers and adds in-kind benefits from food stamps, school lunches, the Women, Infants, and Children (WIC) program, housing subsidies, and LIHEAP as well as income tax credits. Taxes and medical and work/child care expenses are then subtracted. Their measure imputes housing subsidies before 1975, tax liabilities and subsidies using the Taxsim program, SNAP and school lunch before 1979, WIC before 2001, and medical and child care expenses for all years. I construct an income resource measure that includes many of these components but due to data limitations in the PSID I cannot capture additional spending capacity from housing subsidies, the school lunch program, and WIC.[31] I also cannot account for non-discretionary medical and work/child care expenses. While my measure of poverty is missing some resources, I do capture the contribution of the largest non-Medicaid social safety net programs helping families with children, namely, refundable tax credits, AFDC (pre-1996), Food Stamps, and SSI.

The first is setting an absolute threshold is more directly comparable to the approach taken by official poverty statistics. Second, this method better demonstrates the role of social policy, the labor market, and changing demographics in poverty rates over time, given that changes in the threshold are held constant. For example, if all families are poorer over time because of a weakened safety net, then a relative poverty measure won't pick up on this increase in poverty. By holding the standard constant at a fixed level and comparing a family's income to that fixed standard, this absolute standard will capture increases in poverty resulting from a weakened safety net.

[30] In particular, I use the three distinct published thresholds for families with two adults and two children in 2005. The three thresholds reflect whether the family rents, owns a home with a mortgage, or owns a home without a mortgage. I first adjust these thresholds for other family sizes and compositions. I then carry these thresholds backward and forwards historically by adjusting for inflation using the Consumer Price Index Research Series (CPI-U-RS), the Census Bureau's preferred series for overall changes in inflation over time. Last, I express the thresholds in $2019.

[31] While I do not have a direct measure of LIHEAP, there is a proxy variable available in the PSID. The PSID asks about the dollar amount of energy assistance received from *any* government program starting in 1980, the same year LIHEAP was introduced. I use this measure as a proxy for LIHEAP assistance given that the respondents are not required to give the name of the specific program they receive the assistance from.

There are caveats one should bear in mind when interpreting my poverty gap results. In particular, because my measure does not account for WIC, an important nutrition program for young children, I will understate available resources and therefore overstate poverty gaps for low income children ages 0-4.[32] Furthermore, I also cannot account for resources from the school lunch program and housing assistance. These programs cover fewer children than the programs that I do include, but their omission may lead me to understate the depth of poverty.

On the other hand, the housing assistance program aids a smaller share of families over time due to limited funding.[33]. By omitting housing assistance from my income resource measure, if I compare how much the poverty gap increases following union dissolution post-1996 to how much it increases following union dissolution pre-welfare reform, I would underestimate the increase in poverty gaps. Despite these limitations, a measure of income resources that captures in-kind benefits and taxes is superior to one that doesn't, given the increasing importance of tax-based and in-kind transfers after the 1996 welfare reform.

1.5 The Economic Consequences of Union Dissolution

In my main results, I evaluate how the economic consequences of parental union dissolution have changed since welfare reform for the average child whose parents divorce or separate. My pre-welfare reform sample includes all children born into two parent families whose parents separated from 1970-1991. My post welfare reform sample includes all children born into two parent families whose parents separated from 1999-2019.[34]

[32]Failing to account for food purchased through WIC could lead me to overestimate the economic consequences of union dissolution for low income children who experience a parental union dissolution before age 5. While 40% of separations occur among income eligible children before the child has aged out of WIC, take-up of WIC is only 50% on average.

[33]https://www.cbpp.org/research/housing/federal-rental-assistance-fact-sheets#US

[34]Children are between 2 and 10 years old when the parental separation occurs.

Family Post-Tax Income

The results in Table 2 illustrate the dynamic path of income resources following union dissolution before and after welfare reform. The estimates in Columns 1 show percent declines in post-tax income including cash and in-kind benefits following union dissolution before welfare reform. The estimates in Columns 2 show percent declines in post-tax income including cash and in-kind benefits following union dissolution after welfare reform.[35] The results in Table 2 show suggestive evidence of larger percent declines in income and less income recovery following union dissolution after welfare reform. After welfare reform, four to five years following union dissolution, income is 20 percentage points lower relative to the pre-dissolution level, than before welfare reform. While percent declines in income following union dissolution are larger after welfare reform, the differences over time are not statistically significant.

My estimates of percent declines in income following union dissolution prior to 1996 are smaller compared to the results in Page & Stevens (2004). Their estimates indicate that in the year following divorce, family income falls by 41% and five years following divorce income is still 24% lower than it would have been if the divorce had not occurred. I find estimated losses of 31% percent (one year) and 16% percent (five year). However, if I use the same analysis years (1968-1993), I find nearly identical results. In particular, I find estimated losses of 36% percent (one year) and 20% percent (five year). This suggests that the income costs of union dissolution were falling shortly before 1996 welfare reform .

[35]I also measure changes in income levels before and after welfare reform. Pre-welfare reform, income one to two years pre-dissolution is $53,000, while post reform it is $73,000. I find that income falls by $9,800 zero to one years following union dissolution before and after welfare reform. Pre-welfare reform, income is $10,000 below its pre-dissolution level two to three years following union dissolution. Post reform income is $20,000 below its pre-dissolution level two to three years following union dissolution. Finally, pre-reform, income is $5,000 below the pre-dissolution level four to five years following union dissolution, while post reform it is $15,000 below the pre-dissolution level four to five years following union dissolution. The levels specification is not my preferred specification because union dissolution is likely to have very different level effects on rich and poor families.

My results indicating larger percent declines and less recovery in income following union dissolution after 1996, are consistent with Hauser et al. (2018). These authors find that among women (with and without children), income losses are larger following union dissolution from 2005-2013 as compared to 1985-1993. They also find less income recovery five years later. The results in Table 2 are summarized graphically in Figure 7.

Family Poverty Gap

The results in Table 3 illustrate the dynamic path of poverty gaps after parental union dissolution before and after welfare reform for all children. The estimates in Columns 1 show changes in the poverty gap following union dissolution before welfare reform. The estimates in Columns 2 show changes in the poverty gap following union dissolution after welfare reform. The estimates in Table 3 show that the poverty gap is the same following union dissolution before and after welfare reform. There is also no recovery in the poverty gap following union dissolution before or after welfare reform. This suggests that children in families above the poverty threshold are driving the income recovery reported in Table 2. Zero to one years following parental union dissolution, the poverty gap falls by $1000-$1,200 before and after welfare reform. Four to five years following parental union dissolution, the poverty gap is still $1,200 below it's pre-dissolution level, before and welfare reform. The results in Table 3 are summarized graphically in Figure 8.

I have also estimated the consequences of parental union dissolution on poverty status in Appendix Tables A12 & A13. I show suggestive evidence that the deep poverty risk associated with union dissolution rises post welfare reform. In contrast, the poverty risk following union dissolution remains constant over time. A rising deep poverty risk indicates that economically vulnerable families are more adversely affected by union dissolution post welfare reform.

Family Food Expenditures

The results in Table 4 show changes in food expenditures following parental union dissolution before and after welfare reform for all children. The estimates in Columns 1 show changes in

food expenditures following union dissolution before welfare reform. The estimates in Columns 2 show changes in food expenditures following union dissolution after welfare reform. The results in Columns 1 & 2 of Table 4 suggest that the percent decline in food expenditures is smaller and there is more recovery following union dissolution since welfare reform. Before welfare reform, food expenditures initially fall 11% and are still 11% below its pre-dissolution level four to five years following parental union dissolution. After welfare reform, food expenditures initially fall 8% and recover to their pre-dissolution level two to three years following parental union dissolution. The results in Table 4 are summarized graphically in Figure 9.

These results are in line with past work finding a rise in deep poverty during the mid-1990s welfare waiver period, while also finding increases in consumption expenditure, even among very low-income single mothers with children (Haskins, 2001). These results are also consistent with recent work that shows consumption among single mothers with a high school degree or less has increased since welfare reform, while income has declined among the poorest single mothers (Meyer et al. 2021). In this setting, a plausible explanation for why percent declines in food expenditures are smaller, despite larger percent declines in income, comes from past work showing that cohabiting parents spend a smaller share of their income on food, as compared to single parents. In contrast married parents and divorced parents spend the same share of income on food.[36] Since a larger share of children are born to cohabiting parents, the corresponding percent decline in food expenditures following union dissolution should be smaller over time, all else equal.

In Appendix Table A14, I show that percent declines in food expenditures are smaller, despite larger percent declines in income, because a growing share of children are born to cohabiting parents. Columns 1 & 2 of Table A14 show that the percent declines in food expenditures following union dissolution have stayed the over time for children born to cohabiting parents. Columns 3 & 4 of Table A14 show that the percent declines in food expenditures following union dissolution

[36]Deleire & Kalil (2005)

have also stayed over time for children born to married parents. The percent declines in food expenditures following cohabiting union dissolution are smaller that those following divorce in both time periods, as expected. The evidence in this table shows that the smaller percent declines in food expenditures following union dissolution since welfare reform can be explained by the compositional change in the types of unions children are born into and don't reflect improvements in material well-being.

An alternative hypothesis is that smaller percent declines in food expenditures following union dissolution is in fact evidence against declines in material well-being, and percent declines in income are only larger after 1996 because transfer income is increasingly under-reported in survey data over time (Meyer and Sullivan 2021). According to Meyer, Mok, and Sullivan (2009), around 40% of AFDC dollars were missed using PSID data from 1970-1996. According to Meyer, Mok, and Sullivan (2015), approximately 60% of TANF dollars were missed using PSID data from 2000-2012. If I increase AFDC dollars by 40% from 1968-1996, and by 60% from 1997-2019, I still find larger percent declines income and less income recovery. This is evidence that since welfare reform material well-being has worsened following union dissolution.

Heterogeneity in Effects of Union Dissolution by Socioeconomic Status

I next explore whether there are differential impacts of parental union dissolution after the 1996 welfare reform by mother's educational attainment. Specifically, I evaluate how the economic costs of union dissolution have changed since the 1996 welfare reform among children born to mothers with a high school degree or less and how these costs have changed for children born to mothers with more than a high school degree. The former group of children is most affected by the 1996 welfare reform.

I find less income recovery following union dissolution after welfare reform among children born to mothers with a high school degree or less. I also find a larger poverty gap following union dissolution after 1996 among these children. Finally, even though income losses have increased for

these children, I find smaller percent declines and more recovery in food expenditures following union dissolution. In contrast, for children unaffected by welfare reform, there are no changes in the income based consequences of union dissolution over time. Food expenditures also more recover more for these families.

The estimates in the right panel of Figure 10 show less income recovery 4-5 years following union dissolution for children born to mothers with a high school degree or less.[37] Pre-reform, income is 10% below it's pre-dissolution level 4-5 years later. After reform, income is still 30% below it's pre-dissolution level 4-5 years later.[38] In contrast, the estimates in the left panel of Figure 10 show no change in income losses following union dissolution for children born to mothers with more than a high school degree.

The estimates in the right panel of Figure 11 show evidence of an increase in the depth of poverty after 1996 among children born to mothers with a high school degree or less. The poverty gap is between $500-$1,000 larger following union dissolution after welfare reform, but this difference is not statistically significant. While the poverty gap has grown for socioeconomically disadvantaged children, the estimates in the left panel of Figure 11 show that the poverty gap is smaller for children unaffected by welfare reform.

Despite the worsening income-based consequences of union dissolution for children affected by welfare reform, the estimates in the right panel of Figure 12 show that food expenditures decline 50-100% less following union dissolution after 1996. The estimates in the left panel of Figure 12

[37] Figure 5 indicates that this is unrelated to different re-partnership rates.

[38] In Appendix Table A11, I show that excluding states that implemented welfare reform waivers before the 1996 welfare reform was passed does not change my main conclusion; there is less income recovery after welfare reform. These waivers were implemented by 27 states and incorporated many of the program features of TANF policy. Excluding these states from my analysis allows me to directly compare changes in income following union dissolution under traditional AFDC policy to changes in income following union dissolution under TANF policy. These results indicate that among socioeconomically disadvantaged children, there is less income recovery 4-5 years following union dissolution under TANF policy than under traditional AFDC policy. This evidence further points to 1996 welfare reform as a contributing factor to worsening income outcomes.

show this is also true for children unaffected by welfare reform. [39] This can be attributed to a growing share of children being born to cohabiting parents.

In summary, only children affected by the 1996 welfare reform experience worse income-based outcomes following union dissolution after welfare reform. Furthermore, smaller percent declines food expenditures following union dissolution don't reflect improvements in material conditions since welfare reform. Rather, they are the result of a growing share of children being born into unions that spend a smaller share of their income on food, relative to single parents. These cumulative results strongly suggest that the federal reform of cash welfare increased the economic costs of parental union dissolution.

1.5.1 Robustness Checks

In my main analyses, I estimate the dynamic path of resource changes following union dissolution, both before and after welfare reform. To evaluate whether the consequences of union dissolution have changed after welfare, I compare the post event time dummies in the post reform model to those in the pre-reform model. There are two concerns with this approach. First, by splitting my sample in two, I may be under-powered to detect differences over time. Second, differences in my estimated lead coefficients between periods could be functions of differences in pre-trends.

To investigate whether my main results are robust to these concerns, in Appendix Table A9 I re-estimate my main results using an interacted model. This specification interacts the event time coefficients with an indicator that is equal to one after welfare reform. The results in Columns 1,2 & 3 of Appendix Table A9 show that my income, poverty, and food expenditures results are robust to the alternative model specification, respectively.

[39] The differences are not statistically significant over time.

The results from the interacted model support the findings in my main analyses and this model also has enough power to detect a statistically significant reduction in income recovery following union dissolution after welfare reform and a statistically significant improvement in the food expenditures consequences of union dissolution. In particular, the estimates in Column 1 indicate that after welfare reform, income recovers 11 percentage points (70%) less four to five years following union dissolution. The estimates in Column 3 indicate that after welfare reform, food expenditures recovers 12 percentage points (90%) more two to three years following union dissolution.

I also consider whether selection into attrition and non-response that is time-varying biases my estimates. The decision to include observations from children with missing data could bias my results if attrition or survey non-response in any time period is a function of time-varying shocks because the child fixed effect will not control for this time of selection. In Appendix Tables A3-A5 I re-estimate my main results using a balanced panel within each time period.[40] The coefficients on the event time bin dummies (0,1) to (4,5) are not statistically different from those using an unbalanced panel for any of the outcomes. The pattern of results in the balanced panel is the same as in the unbalanced panel as well. This shows that time-varying selection into attrition and non-response is not biasing my main findings.

1.6 Conclusion

In this paper, I present evidence on how the economic consequences of union dissolution have changed since welfare reform. Specifically, using an event study with child-fixed effects, I measure

[40] I only use odd years of data to mimic the data construction when the survey became biannual in 1997. To be in the balanced panel the child had to be observed once in each two-year binned event time (-2,-1), (0,1), (2,3), and (4,5). Pre-welfare reform, the earliest year a child can experience parental union dissolution is 1970 because in my sample children are at least 2 when their parents separate. I also impose that the last year a child can experience parental union dissolution is 1991, so that the last survey year resources can be measured in the (4,5) bin is 1996. Post-welfare reform, I impose the earliest year a child can experience parental union dissolution is 1999, so that the earliest survey year resources can be measured in the (-2,-1) bin is 1997. The latest year a child can experience parental union dissolution is 2015 to have resources measured four years after the split. Thus, in the balanced panel children experience union dissolution from 1970-1991 in the pre-reform sample, and they experience union dissolution from 1999-2015 in the post reform sample.

the dynamic path of income, the poverty gap, and food expenditures following union dissolution before and after the 1996 welfare reform. I find that on aggregate, there is less recovery in income resources over time. In contrast, percent declines in food expenditures following union dissolution are smaller over time. I show that smaller percent declines in food expenditures don't reflect improvements in material condition, and are instead the result of a larger share of these children being born to parents that spend a larger share of their income on food when they are single, than when they are cohabiting. Finally, despite the increased generosity of the tax-based safety net since 1996, the poverty gap remains the same. This highlights the negative effect welfare reform potentially had on income resources available to children whose parent's separate. These results are robust to several specification checks, including time-varying selection into attrition and non-response, and differences in outcome pre-trends before and after welfare reform.

Underlying these aggregate results is interesting and important heterogeneity. First, after welfare reform, income losses following union dissolution have only grown for socioeconomically disadvantaged families. Second, among children born to mothers with a high school degree or less, there is a larger poverty gap following union dissolution after welfare reform, while the poverty gap is smaller for children unaffected by welfare reform. Finally, declines in food expenditures following union dissolution are smaller over time. However, smaller percent declines in food expenditures don't reflect improvements in material condition, and are instead the result of a larger share of these children being born to cohabiting parents.

The growing income consequences of union dissolution since the 1996 welfare reform is an important new finding in the economics literature. Given the documented link between resources available in childhood and adult outcomes, these results underscore the need to increase resources to single-parent families.[41] Additional work is needed to understand how the changing economic costs of parental union dissolution relate to the safety net reforms in the 1990s.

[41] Almond et al. (2011), Hoynes et al. (2009)

Figures

Figure 1.1: All Children

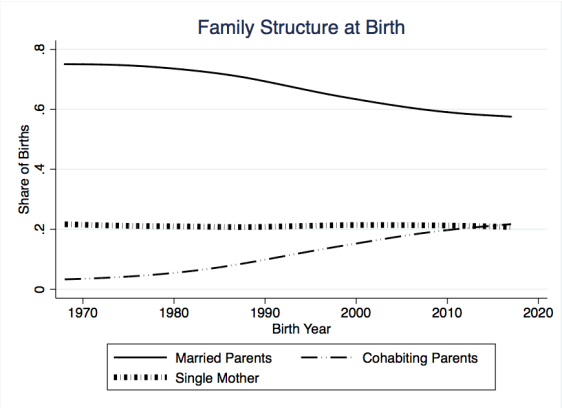


Figure 1.2: Children Born to Mothers with ≤ HS Degree

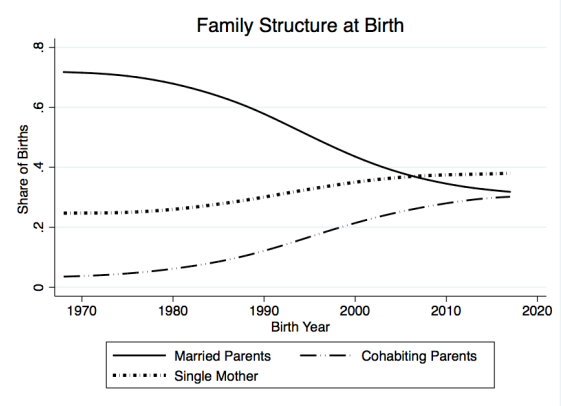
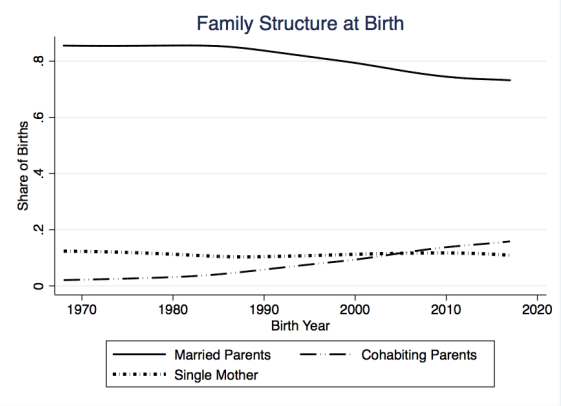


Figure 1.3: Children Born to Mothers with > HS Degree



Note: Figures 1.1-1.3 show changes in family structure at birth from 1968-2019 by mother's educational attainment. Changes are weighted using PSID longitudinal weights.

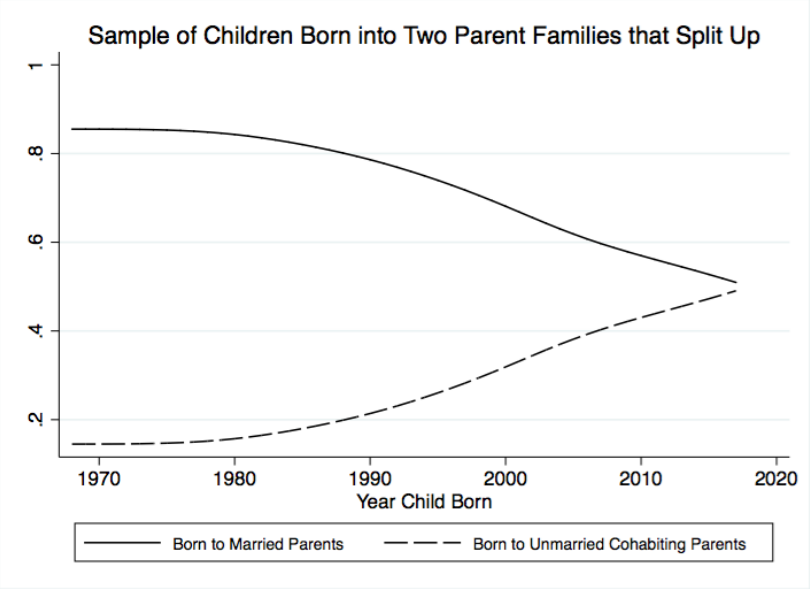


Figure 1.4: Figure shows changes in family structure at birth from 1968-2019 among children born into two-parent families that divorce or separate. Changes are weighted using PSID longitudinal weights.

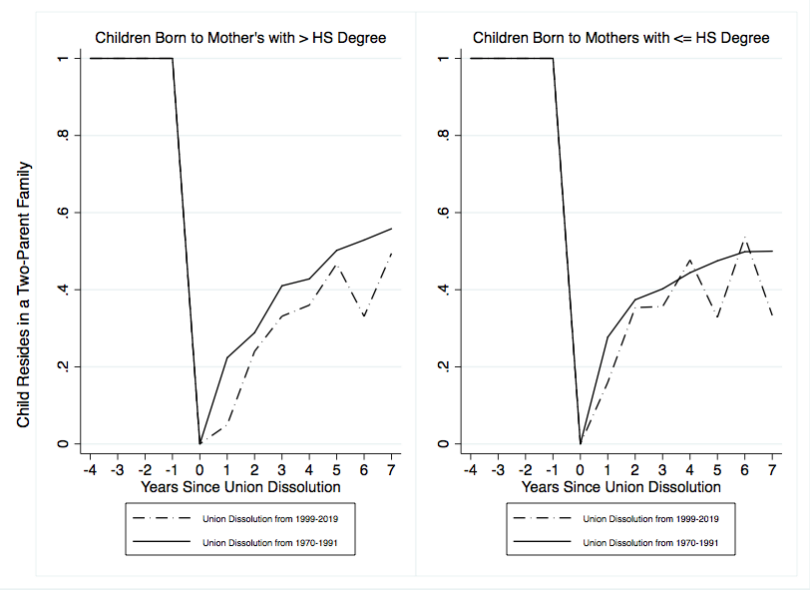


Figure 1.5: Figure shows changes in re-partnership dynamics before and after 1996 by mother's educational attainment at birth among children born into two-parent families that divorce or separate. Changes are weighted using PSID longitudinal weights.

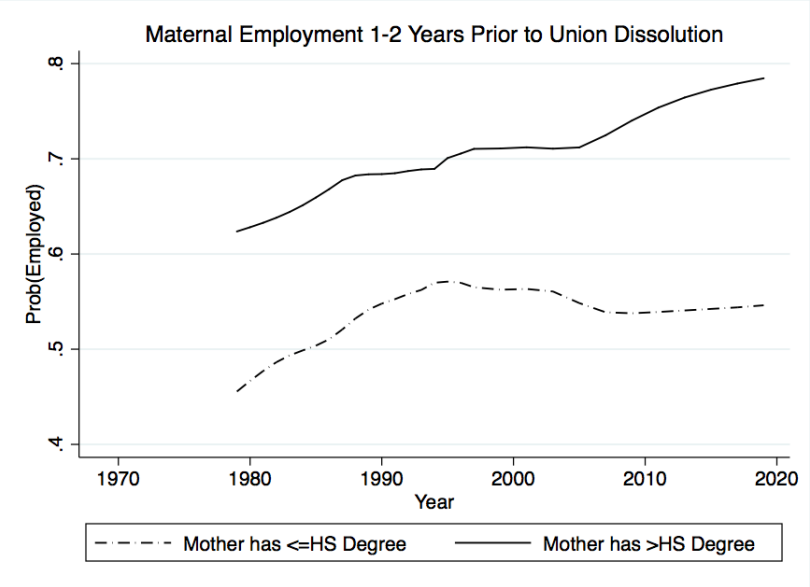


Figure 1.6: Figure shows changes in maternal labor supply for married or cohabiting mothers by educational attainment at birth among children born into two-parent families that divorce or separate. Changes are weighted using PSID longitudinal weights.

Main Analyses

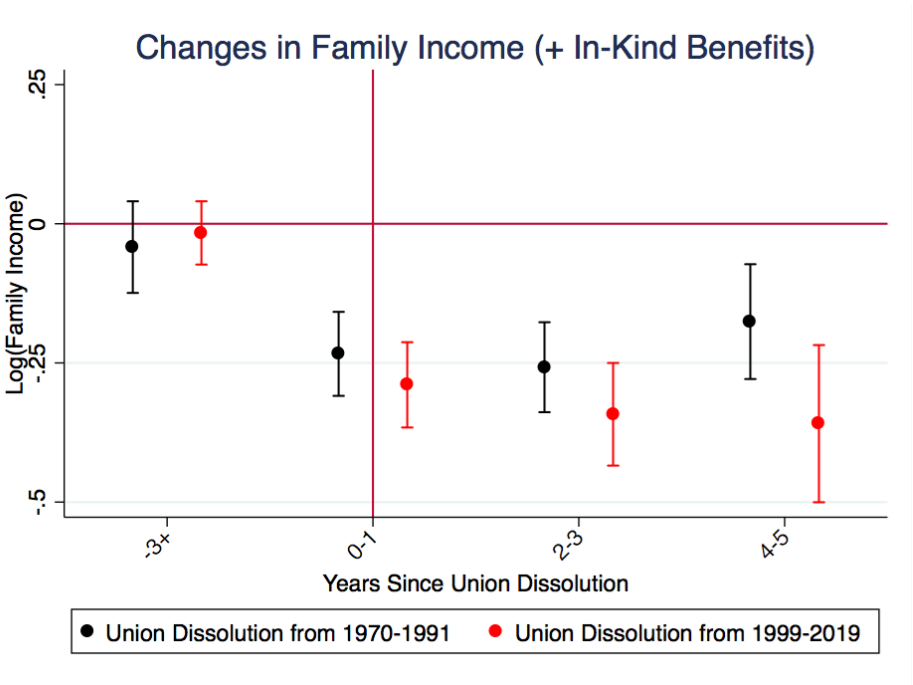


Figure 1.7: Outcome is Log(Family Post-Tax Income including cash and in-kind benefits). **Black** series refers to union dissolutions from 1970-1991 and resources in event times (-2,-1) to (4,5) are measured from 1968-1996. **Red** series refers to union dissolutions from 1999-2019 and resources in event times (-2,-1) to (4,5) are measured from 1997-2019. Weighted using the child longitudinal weight in the last year observed.

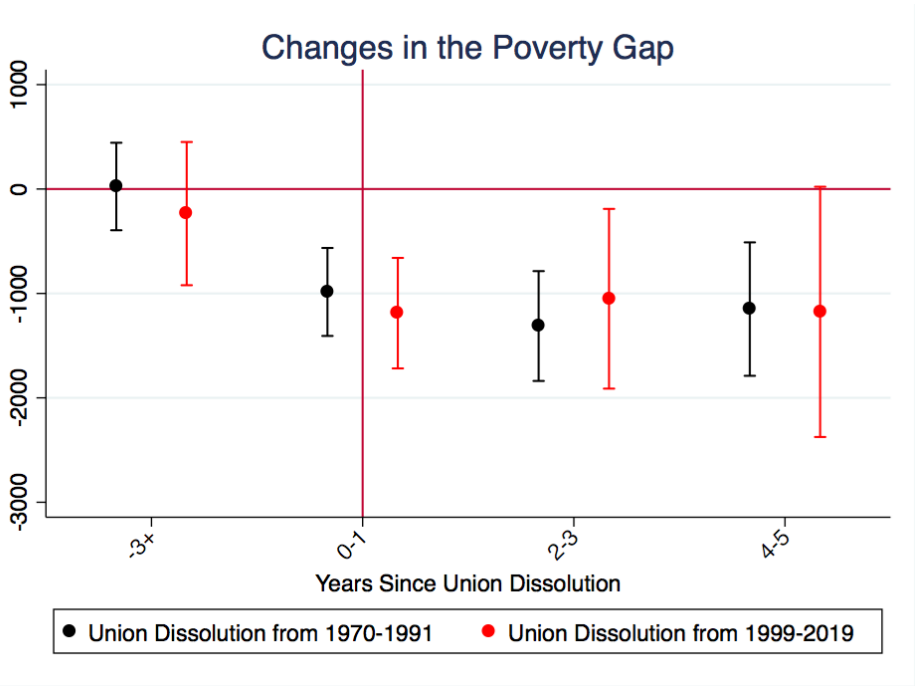


Figure 1.8: Outcome is the difference between family post-tax income including cash and in-kind benefits and the SPM threshold. Dollar values are in $2019. **Black** series refers to union dissolutions from 1970-1991 and resources in event times (-2,-1) to (4,5) are measured from 1968-1996. **Red** series refers to union dissolutions from 1999-2019 and resources in event times (-2,-1) to (4,5) are measured from 1997-2019. Weighted using the child longitudinal weight in the last year observed.

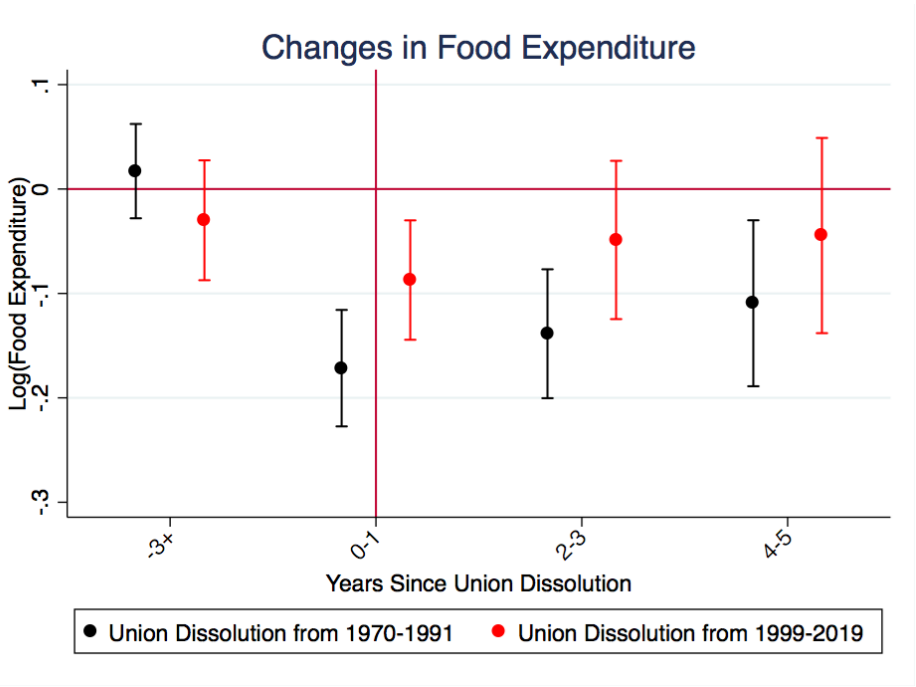


Figure 1.9: Outcome is Log(Family Food Expenditures). This is expenditure on food at home, delivered meals, food away from home, and food purchased with food stamps. **Black** series refers to union dissolutions from 1970-1991 and resources in event times (-2,-1) to (4,5) are measured from 1968-1996. **Red** series refers to union dissolutions from 1999-2019 and resources in event times (-2,-1) to (4,5) are measured from 1997-2019. Weighted using the child longitudinal weight in the last year observed.

Supplementary Analyses

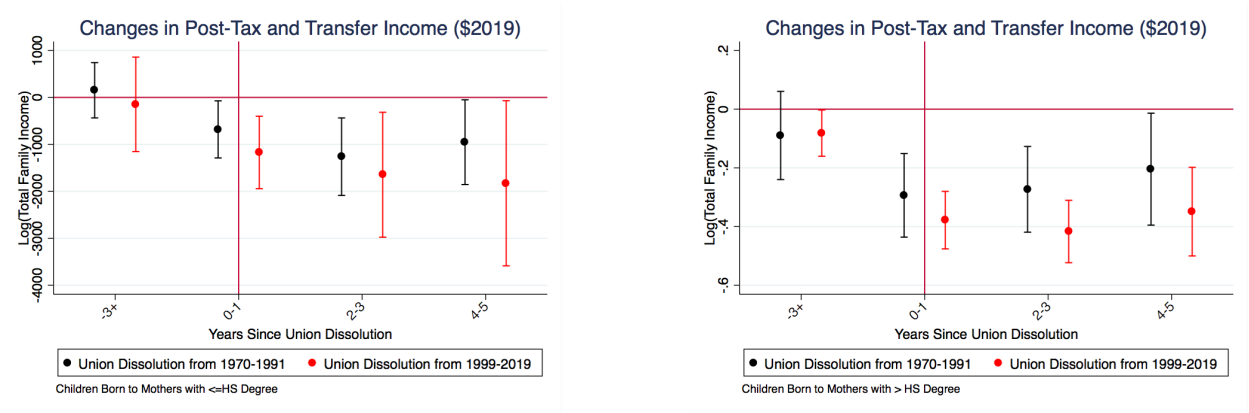


Figure 1.10: Outcome is Log(Family Post-Tax Income including cash and in-kind benefits). **Black** series refers to union dissolutions from 1970-1991 and resources in event times (-2,-1) to (4,5) are measured from 1968-1996. **Red** series refers to union dissolutions from 1999-2019 and resources in event times (-2,-1) to (4,5) are measured from 1997-2019. Weighted using the child longitudinal weight in the last year observed.

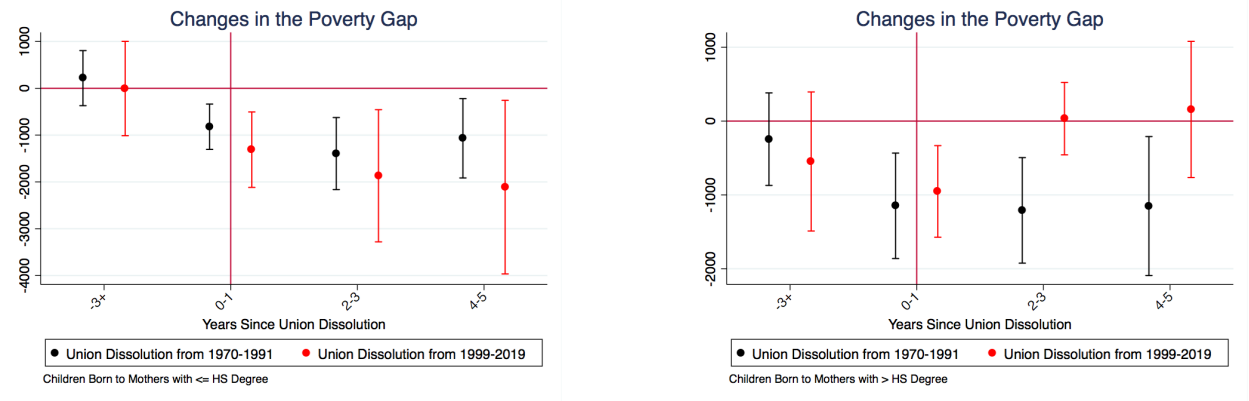


Figure 1.11: Outcome is the family poverty gap. **Black** series refers to union dissolutions from 1970-1991 and resources in event times (-2,-1) to (4,5) are measured from 1968-1996. **Red** series refers to union dissolutions from 1999-2019 and resources in event times (-2,-1) to (4,5) are measured from 1997-2019. Weighted using the child longitudinal weight in the last year observed.

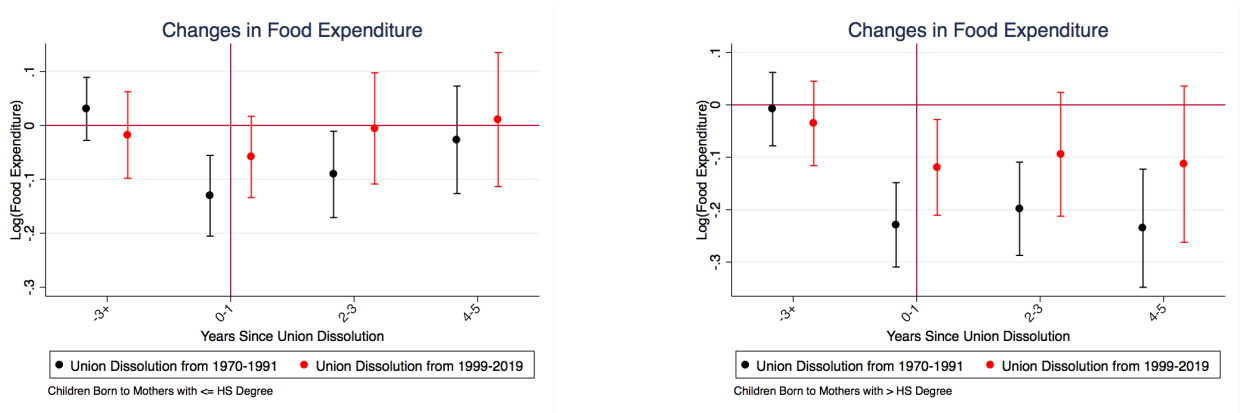


Figure 1.12: Outcome is Log(Family Food Expenditures). **Black** series refers to union dissolutions from 1970-1991 and resources in event times (-2,-1) to (4,5) are measured from 1968-1996. **Red** series refers to union dissolutions from 1999-2019 and resources in event times (-2,-1) to (4,5) are measured from 1997-2019. Weighted using the child longitudinal weight in the last year observed.

Tables

Table 1.1: *Families Characteristics in the Year of Birth*

Birth Year	1968-1996	1968-1996	1968-1996	1968-1996	1997-2019	1997-2019	1997-2019	1997-2019
Type:	Cohabiting (Stay Together) (1)	Cohabiting (Separate) (2)	Married (Stay Together) (3)	Married (Separate) (4)	Cohabiting (Stay Together) (5)	Cohabiting (Separate) (6)	Married (Stay Together) (7)	Married (Separate) (8)
Share of All Families:	3%	9%	42%	21%	11%	12%	39%	13%
Family Head is Black (%)	18.0	24.0	5.0	10.0	17.0	27.0	6.0	12.0
Family Head is Hispanic (%)	5.0	12.0	4.0	4.0	8.0	5.0	5.0	3.0
Family Head is White (%)	75.0	63.0	90.0	84.0	68.0	60.0	85.0	82.0
Mother's # of Partners	1.00	1.40	1.00	1.48	1.00	1.37	1.00	1.29
Mother's Age at Childbirth	23.56	21.78	26.18	24.01	25.58	22.45	29.22	26.48
PTT Income w/ In-Kind Benefits	$40,737	$39,993	$61,302	$52,163	$65,988	$45,205	$88,402	$ 66,194
Total Family Food Expenditure	$9,117	$8,215	$10,385	$9,838	$9,843	$8,152	$9,956	$8,777
Mother Had ≤ HS Degree (%)	70.0	81.0	51.0	64.0	49.0	73.0	18.0	46.0
PTT Income + In-Kind Benefits (%) ≤ 100% SPM	21.0	23.0	4.0	10.0	6.0	15.0	1.0	5.0
PTT Income + In-Kind Benefits (%) ≤ 50% SPM	8.0	9.0	1.0	3.0	2.0	6.0	0.0	3.0
Family Received AFDC/TANF (%)	11.0	10.0	1.0	4.0	3.0	3.0	0.0	1.0
Family Received SNAP (%)	16.0	22.0	4.0	9.0	20.0	29.0	3.0	10.0
Family Received Heat Assistance (%)	10.0	6.0	2.0	3.0	7.0	10.0	1.0	4.0
Family Received EITC (%)	34.0	36.0	15.0	22.0	37.0	55.0	19.0	32.0
Family Received CTC (%)	0.0	0.0	0.0	0.0	37.0	42.0	57.0	55.0
# of Children	357	618	4,482	1,988	497	407	1,346	420

Note: Weighted using PSID longitudinal family weights. Dollar amounts are in $2019. Children born to single mothers represent 25% of all children born before and after 1996. Percent variables are rounded to the nearest percent. Headers in Red refer to the estimation sample used for the present analysis.

Table 1.2: Estimated Income Changes After Union Dissolution

Coefficient on Event Time	(1)	(2)
- 3+ yrs	-0.044	-0.043
	(0.042)	(0.029)
0 to 1 yrs	-0.205***	-0.299***
	(0.039)	(0.038)
+ 2 to 3 yrs	-0.228***	-0.311***
	(0.039)	(0.048)
+ 4 to 5 yrs	-0.150***	-0.325***
	(0.050)	(0.072)
# of Children	1,092	1,453
# of Child Years	10,766	8,467

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: Outcome is family post-tax income including cash and in-kind benefits. Models presented are weighted least-squares estimates of Equation (1) using an unbalanced panel of children whose mother or father gets custody after union dissolution. Weighted using the child longitudinal weight in the last year the child was observed. Estimates in Column 1 refer to union dissolutions from 1970-1991. The (-2,-1) to (4,5) event time bins in Column 1 reflect resources measured from 1968 to 1996. Estimates in Column 2 refer to union dissolutions from 1999-2019. The (-2,-1) to (4,5) event time bins in Column 2 reflect resources measured from 1997 to 2019.

Table 1.3: Estimated Poverty Gap Changes After Union Dissolution

Coefficient on Event Time	(1)	(2)
- 3+ yrs	23.993	-235.647
	(213.562)	(349.602)
0 to 1 yrs	-985.913***	-1,188.918***
	(214.680)	(269.700)
+ 2 to 3 yrs	-1,312.276***	-1,051.104**
	(268.076)	(438.756)
+ 4 to 5 yrs	-1,150.139***	-1,176.576*
	(325.659)	(610.876)
# of Children	1,092	1,453
# of Child Years	10,782	8,490

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: Outcome is the difference between family post-tax income including cash and in-kind benefits and the SPM threshold. Models presented are weighted least-squares estimates of Equation (1) using an unbalanced panel of children whose mother or father gets custody after union dissolution. Weighted using the child longitudinal weight in the last year the child was observed. Estimates in Column 1 refer to union dissolutions from 1970-1991. The (-2,-1) to (4,5) event time bins in Column 1 reflect resources measured from 1968 to 1996. Estimates in Column 2 refer to union dissolutions from 1999-2019. The (-2,-1) to (4,5) event time bins in Column 2 reflect resources measured from 1997 to 2019.

Table 1.4: Estimated Changes in Food Expenditures After Union Dissolution

Coefficient on Event Time	(1)	(2)
- 3+ yrs	0.016	-0.030
	(0.023)	(0.029)
0 to 1 yrs	-0.156***	-0.086***
	(0.029)	(0.029)
+ 2 to 3 yrs	-0.126***	-0.048
	(0.032)	(0.039)
+ 4 to 5 yrs	-0.090**	-0.044
	(0.041)	(0.048)
# of Children	1,092	1,453
# of Child Years	10,898	9,060

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: Outcome is family food expenditures. This is expenditure on food at home, delivered meals, food away from home, and food purchased with food stamps. Models presented are weighted least-squares estimates of Equation (1) using an unbalanced panel of children whose mother or father gets custody after union dissolution. Weighted using the child longitudinal weight in the last year the child was observed. Estimates in Column 1 refer to union dissolutions from 1970-1991. The (-2,-1) to (4,5) event time bins in Column 1 reflect resources measured from 1968 to 1996. Estimates in Column 2 refer to union dissolutions from 1999-2019. The (-2,-1) to (4,5) event time bins in Column 2 reflect resources measured from 1997 to 2019.

Appendix

Table 1.A1: Sample Construction

Starting Sample: Children followed from birth up to age 16 that were born between 1968-2019 into a two co-residing parent family from the Core Sample that experienced a union dissolution	*this includes children born in even years after 1997, who only enter sample at age 1 (parents are observed pre-birth) *only children from SEO and Census HH's (92% of all HH's)	4,161 Children
Starting Sample:		41,546 Child-Years
Sample Restriction: Only study children whose parents separated when the child was 2-10 years old	* Ensures that all children can contribute to each event time from -2 to +6	2,994 (lost 1,167 children)
With Sample Restriction: Total # of Child Years		28,620 (lost 12,926 child-years)

Children not included in Main Sample	Children whose data come from adoption records, born to single mothers, born to parent's that do not separate, born before 1968, first obs at age>1, Children born into Immigrant or Latino Samples, Children whose parents separated before age 2 or after age 10	
Final Analysis Sample	2,994 Children (28,620 child-years)	

Table 1.A2: # of Children Contributing to Estimation By Event Time

Event time	Balanced	Unbalanced	Balanced	Unbalanced
Separations	from 1970-1991	from 1970-1991	from 1999-2015	from 1999-2019
(-2,-1)	719	1,057	938	1,371
(0,1)	719	1,057	938	1,320
(2,3)	719	777	938	1,184
(4,5)	719	773	938	989

Note: Numbers in table refer to the # of distinct children in each event time. Sample of children that live with mom or dad after union dissolution.

Robustness Checks

Table 1.A3: Estimated Income Changes After Union Dissolution (Balanced Panel)

Coefficient on Event Time	(1)	(2)
-3+ yrs	-0.100*	-0.057
	(0.061)	(0.035)
+ 0 to 1 yrs	-0.202***	-0.273***
	(0.061)	(0.047)
+ 2 to 3 yrs	-0.209***	-0.291***
	(0.054)	(0.056)
+ 4 to 5 yrs	-0.185***	-0.299***
	(0.068)	(0.082)
# of Children	719	938
# of Child Years	8,670	6,671

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: Outcome is family post-tax income including cash and in-kind benefits. Models presented are weighted least-squares estimates of Equation (1) using a balanced panel of children who were observed in event times (-2,-1), (0,1), (2,3), and (4,5) and whose mother or father gets custody after union dissolution. Weighted using the child longitudinal weight in the last year the child was observed. Estimates in Column 1 refer to union dissolutions from 1970-1991. The (-2,-1) to (4,5) event time bins in Column 1 reflect resources measured from 1968 to 1996. Estimates in Column 2 refer to union dissolutions from 1999-2015. The (-2,-1) to (4,5) event time bins in Column 2 reflect resources measured from 1997 to 2019.

Table 1.A4: Estimated Poverty Gap Changes After Union Dissolution (Balanced Panel)

Coefficient on Event Time	(1)	(2)
-3+ yrs	-149.320	-578.879
	(333.069)	(489.391)
+ 0 to 1 yrs	-874.329***	-1,251.529***
	(324.295)	(347.948)
+ 2 to 3 yrs	-1,184.492***	-1,107.857**
	(336.664)	(557.671)
+ 4 to 5 yrs	-1,093.044**	-1,236.660
	(476.131)	(775.773)
# of Children	719	938
# of Child Years	8,682	6,691

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: Outcome is the difference between family post-tax income including cash and in-kind benefits and the SPM threshold. Models presented are weighted least-squares estimates of Equation (1) using a balanced panel of children who were observed in event times (-2,-1), (0,1), (2,3), and (4,5) and whose mother or father gets custody after union dissolution. Weighted using the child longitudinal weight in the last year the child was observed. Estimates in Column 1 refer to union dissolutions from 1970-1991. The (-2,-1) to (4,5) event time bins in Column 1 reflect resources measured from 1968 to 1996. Estimates in Column 2 refer to union dissolutions from 1999-2015. The (-2,-1) to (4,5) event time bins in Column 1 reflect resources measured from 1997 to 2019.

Table 1.A5: Estimated Changes in Food Expenditures After Union Dissolution (Balanced Panel)

Coefficient on Event Time	(1)	(2)
-3+ yrs	0.034	-0.025
	(0.048)	(0.033)
+ 0 to 1 yrs	-0.139***	-0.085***
	(0.046)	(0.032)
+ 2 to 3 yrs	-0.165***	-0.042
	(0.054)	(0.042)
+ 4 to 5 yrs	-0.148**	-0.028
	(0.069)	(0.050)
# of Children	719	938
# of Child Years	8,916	7,239

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: Outcome is family food expenditures. This is expenditure on food at home, delivered meals, food away from home, and food purchased with food stamps. Models presented are weighted least-squares estimates of Equation (1) using a balanced panel of children who were observed in event times (-2,-1), (0,1), (2,3), and (4,5) and whose mother or father gets custody after union dissolution. Weighted using the child longitudinal weight in the last year the child was observed. Estimates in Column 1 refer to union dissolutions from 1969-1991. The (-2,-1) to (4,5) event time bins in Column 1 reflect resources measured from 1970 to 1996. Estimates in Column 2 refer to union dissolutions from 1999-2015. The (-2,-1) to (4,5) event time bins in Column 1 reflect resources measured from 1997 to 2019.

Table 1.A6: Estimated Income Changes After Union Dissolution

Coefficient on Event Time	(1)	(2)
-3+ yrs	0.003	-0.027
	(0.064)	(0.039)
-2 yrs	0.053	-0.020
	(0.052)	(0.042)
0 yrs	-0.042	-0.272***
	(0.052)	(0.055)
+1 yrs	-0.368***	-0.359***
	(0.044)	(0.053)
+2 yrs	-0.271***	-0.364***
	(0.043)	(0.056)
+3 yrs	-0.203***	-0.342***
	(0.053)	(0.057)
+4 yrs	-0.157***	-0.401***
	(0.055)	(0.079)
+5 yrs	-0.180***	-0.302***
	(0.059)	(0.076)
# of Children	1,092	1,453
# of Child Years	10,766	8,467

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: Outcome is family post-tax income including cash and in-kind benefits. Models presented are weighted least-squares estimates of Equation (1) using an unbalanced panel of children whose mother or father gets custody after union dissolution. Event times are not binned. Weighted using the child longitudinal weight in the last year the child was observed. Estimates in Column 1 refer to union dissolutions from 1970-1991. The (-2,-1) to (4,5) event time bins in Column 1 reflect resources measured from 1968 to 1996. Estimates in Column 2 refer to union dissolutions from 1999-2019. The (-2,-1) to (4,5) event time bins in Column 2 reflect resources measured from 1997 to 2019.

Table 1.A7: Estimated Poverty Gap Changes After Union Dissolution

Coefficient on Event Time	(1)	(2)
-3+ yrs	103.794	-168.201
	(268.720)	(485.882)
-2 yrs	39.893	113.957
	(212.173)	(304.742)
0 yrs	-681.743***	-1,468.455***
	(232.349)	(343.103)
+1 yrs	-1,379.873***	-355.912
	(248.130)	(287.916)
+2 yrs	-1,381.500***	-1,310.003***
	(288.435)	(432.558)
+3 yrs	-1,399.098***	63.467
	(348.324)	(385.292)
+4 yrs	-1,103.444***	-1,232.093**
	(358.879)	(570.753)
+5 yrs	-1,427.172***	-109.108
	(364.886)	(578.321)
# of Children	1,092	1,453
# of Child Years	10,782	8,490

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: 0utcome is the difference between family post-tax income including cash and in-kind benefits and the SPM threshold. Models presented are weighted least-squares estimates of Equation (1) using an unbalanced panel of children whose mother or father gets custody after union dissolution. Event times are not binned. Weighted using the child longitudinal weight in the last year the child was observed. Estimates in Column 1 refer to union dissolutions from 1970-1991. The (-2,-1) to (4,5) event time bins in Column 1 reflect resources measured from 1968 to 1996. Estimates in Column 2 refer to union dissolutions from 1999-2019. The (-2,-1) to (4,5) event time bins in Column 1 reflect resources measured from 1997 to 2019.

Table 1.A8: Estimated Changes in Food Expenditures After Union Dissolution

Coefficient on Event Time	(1)	(2)
-3+ yrs	-0.007	-0.040
	(0.029)	(0.030)
-2 yrs	-0.025	-0.030
	(0.024)	(0.039)
0 yrs	-0.054**	-0.206***
	(0.023)	(0.057)
+1 yrs	-0.244***	-0.068**
	(0.036)	(0.032)
+2 yrs	-0.229***	-0.039
	(0.043)	(0.065)
+3 yrs	-0.200***	-0.075*
	(0.044)	(0.040)
+4 yrs	-0.189***	-0.023
	(0.043)	(0.061)
+5 yrs	-0.167***	-0.071
	(0.045)	(0.051)
# of Children	1,092	1,453
# of Child Years	9,852	8,836

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: Outcome is family food expenditures. This is expenditure on food at home, delivered meals, food away from home, and food purchased with food stamps. Models presented are weighted least-squares estimates of Equation (1) using an unbalanced panel of children whose mother or father gets custody after union dissolution. Event times are not binned. Weighted using the child longitudinal weight in the last year the child was observed. Estimates in Column 1 refer to union dissolutions from 1970-1991. The (-2,-1) to (4,5) event time bins in Column 1 reflect resources measured from 1968 to 1996. Estimates in Column 2 refer to union dissolutions from 1999-2019. The (-2,-1) to (4,5) event time bins in Column 2 reflect resources measured from 1997 to 2019.

Table 1.A9: Results Robust to Alternative Model Specification

Outcome :	Income	Poverty Gap	Food Expenditures
Coefficient on Event Time	(1)	(2)	(3)
(- 3+ yrs)xYear>1996	-0.002	-30.650	-0.003
	(0.035)	(228.202)	(0.029)
(+0-1 yrs)xYear>1996	-0.075	-419.773	0.030
	(0.046)	(266.135)	(0.029)
(+2-3 yrs)xYear>1996	-0.077*	-264.182	0.119***
	(0.043)	(318.069)	(0.038)
(+4-5 yrs)xYear>1996	-0.112*	-328.260	0.061
	(0.059)	(402.574)	(0.042)
- 3+ yrs	-0.053	-102.822	-0.015
	(0.032)	(167.669)	(0.021)
+ 0 to 1 yrs	-0.212***	-872.070***	-0.102***
	(0.034)	(177.915)	(0.018)
+ 2 to 3 yrs	-0.206***	-992.913***	-0.169***
	(0.032)	(197.983)	(0.029)
+ 4 to 5 yrs	-0.163***	-1,068.634***	-0.131***
	(0.042)	(250.747)	(0.033)
# of Children	2,994	2,994	2,994
# of Child Years	24,063	25,953	25,141

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: Models presented are weighted least-squares estimates using an unbalanced panel of children whose mother or father gets custody after union dissolution. Weighted using the child longitudinal weight in the last year the child was observed. Model includes the binned event time (+6+) and the interaction term (+6+ yrs)xYear>1996

Table 1.A10: Income Results Robust to # of Years in -3+ Bin

Resources Measured in Event Times:	-10 to +14	-7 to +14	-4 to +14	-10 to +14	-7 to +14	-4 to +14
Coefficient on Event Time	(1)	(2)	(3)	(4)	(5)	(6)
- 3+ yrs	-0.044	-0.055	-0.044	-0.043	-0.039	-0.046
	(0.042)	(0.043)	(0.046)	(0.029)	(0.034)	(0.036)
+ 0 to 1 yrs	-0.205***	-0.189***	-0.167***	-0.299***	-0.303***	-0.297***
	(0.039)	(0.038)	(0.042)	(0.038)	(0.040)	(0.047)
+ 2 to 3 yrs	-0.228***	-0.201***	-0.153***	-0.311***	-0.316***	-0.291***
	(0.039)	(0.039)	(0.043)	(0.048)	(0.055)	(0.071)
+ 4 to 5 yrs	-0.150***	-0.111**	-0.051	-0.325***	-0.334***	-0.297***
	(0.050)	(0.047)	(0.054)	(0.072)	(0.081)	(0.106)
# of Children	1,061	1,061	1,061	1,401	1,401	1,401
# of Child Years	13,544	13,104	11,783	9,732	9,176	7,918

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: Models presented are weighted least-squares estimates of Equation (1) using an unbalanced panel of children whose mother or father gets custody after union dissolution. Weighted using the child longitudinal weight in the last year the child was observed. Estimates in Columns 1-3 refer to union dissolutions from 1970-1991. The (-2,-1) to (4,5) event time bins in Columns 1-3 reflect resources measured from 1969 to 1996. Estimates in Column 4-6 refer to union dissolutions from 1999-2019. The (-2,-1) to (4,5) event time bins in Columns 4-6 reflect resources measured from 1997 to 2019.

Table 1.A11: Changes in Income After Union Dissolution (including and excluding welfare waiver states)

Coefficient on Event Time	(1)	(2)	(3)	(4)
- 3+ yrs	-0.019	-0.023	0.009	0.017
	(0.030)	(0.044)	(0.041)	(0.060)
0 to 1 yrs	-0.200***	-0.196***	-0.232***	-0.147*
	(0.036)	(0.047)	(0.055)	(0.085)
+ 2 to 3 yrs	-0.261***	-0.247***	-0.293***	-0.200*
	(0.043)	(0.062)	(0.070)	(0.103)
+ 4 to 5 yrs	-0.172***	-0.206***	-0.361***	-0.367**
	(0.054)	(0.078)	(0.104)	(0.165)
# of Children	717	418	799	445
# of Child Years	7,005	3,728	4,789	2,367

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: Outcome is family post-tax income including cash and in-kind benefits. Models presented are weighted least-squares estimates of Equation (1) using an unbalanced panel of children whose mother or father gets custody after union dissolution **and whose mother has a high school of less at child birth**. Event times are not binned. Weighted using the child longitudinal weight in the last year the child was observed. Estimates in Column 1 refer to union dissolutions from 1970-1991. Estimates in Column 2 refer to union dissolutions from 1970-1991, excluding waiver states. The (-2,-1) to (4,5) event time bins in Columns 1 & 2 reflect resources measured from 1968 to 1996. Estimates in Column 3 refer to union dissolutions from 1999-2019. Estimates in Column 4 refer to union dissolutions from 1999-2019, excluding waiver states. The (-2,-1) to (4,5) event time bins in Column 3 & 4 reflect resources measured from 1997 to 2019.

Additional Analyses

Table 1.A12: Estimated Poverty Changes After Union Dissolution

Coefficient on Event Time	(1)	(2)
- 3+ yrs	0.024*	-0.009
	(0.015)	(0.016)
0 to 1 yrs	0.114***	0.098***
	(0.020)	(0.018)
+ 2 to 3 yrs	0.123***	0.075***
	(0.022)	(0.024)
+ 4 to 5 yrs	0.088***	0.094***
	(0.028)	(0.031)
# of Children	1,092	1,453
# of Child Years	10,782	8,490

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: Outcome is Prob(Income≤ 100% of SPM). Models presented are weighted least-squares estimates of Equation (1) using an unbalanced panel of children whose mother or father gets custody after union dissolution. Weighted using the child longitudinal weight in the last year the child was observed. Estimates in Column 1 refer to union dissolutions from 1970-1991. The (-2,-1) to (4,5) event time bins in Column 1 reflect resources measured from 1968 to 1996. Estimates in Column 2 refer to union dissolutions from 1999-2019. The (-2,-1) to (4,5) event time bins in Column 1 reflect resources measured from 1997 to 2019.

Table 1.A13: Estimated Deep Poverty Changes After Union Dissolution

Coefficient on Event Time	(1)	(2)
- 3+ yrs	-0.005	-0.006
	(0.008)	(0.010)
0 to 1 yrs	0.018*	0.041***
	(0.010)	(0.012)
+ 2 to 3 yrs	0.027**	0.035*
	(0.011)	(0.018)
+ 4 to 5 yrs	0.010	0.041*
	(0.013)	(0.022)
# of Children	1,092	1,453
# of Child Years	10,782	8,490

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: Outcome is Prob(Income≤ 50% of SPM). Models presented are weighted least-squares estimates of Equation (1) using an unbalanced panel of children whose mother or father gets custody after union dissolution. Weighted using the child longitudinal weight in the last year the child was observed. Estimates in Column 1 refer to union dissolutions from 1970-1991. The (-2,-1) to (4,5) event time bins in Column 1 reflect resources measured from 1968 to 1996. Estimates in Column 2 refer to union dissolutions from 1999-2019. The (-2,-1) to (4,5) event time bins in Column 1 reflect resources measured from 1997 to 2019.

Table 1.A14: Changes in Food Expenditures After Union Dissolution By Union Type at Childbirth

Coefficient on Event Time	(1)	(2)	(3)	(4)
- 3+ yrs	-0.001	-0.084	0.010	-0.092*
	(0.054)	(0.054)	(0.026)	(0.056)
0 to 1 yrs	0.040	-0.009	-0.141***	-0.138***
	(0.076)	(0.055)	(0.034)	(0.052)
+ 2 to 3 yrs	0.020	0.032	-0.053	-0.033
	(0.079)	(0.076)	(0.041)	(0.066)
+ 4 to 5 yrs	0.093	0.104	-0.001	0.019
	(0.123)	(0.090)	(0.054)	(0.077)
# of Children	260	672	822	837
# of Child Years	2,076	3,630	8,322	3,722

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: Outcome is family food expenditures. This is expenditure on food at home, delivered meals, food away from home, and food purchased with food stamps. Models presented are weighted least-squares estimates of Equation (1) using an unbalanced panel of children whose mother or father gets custody after union dissolution. Weighted using the child longitudinal weight in the last year the child was observed. Estimates in Column 1 refer to cohabiting union dissolutions from 1970-1991. Estimates in Column 3 refer to married union dissolutions (divorce) from 1970-1991. The (-2,-1) to (4,5) event time bins in Columns 1 & 3 reflect resources measured from 1968 to 1996. Estimates in Column 2 refer to cohabiting union dissolutions from 1999-2019. Estimates in Column 4 refer to married union dissolutions (divorce) from 1999-2019. The (-2,-1) to (4,5) event time bins in Columns 2 & 4 reflect resources measured from 1997 to 2019.

Chapter 2: The Impact of Safety Net Reforms on the Economic Consequences of Parental Union Dissolution

2.1 Introduction

The passage of the 1996 Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA), as well as the expansion and introduction of policies emphasizing work-based assistance like the Earned Income Tax Credit (EITC) and the Child Tax Credit (CTC), marked a major shift away from an entitlement based safety net, towards an in-work safety net. Prior work has shown that PRWORA led to a dramatic fall in welfare receipt, and increases in work and earnings[42] While welfare reform did increase average earnings, there is also evidence that the most vulnerable experienced losses in income and increases in poverty.[43] However, there is also evidence that these adverse outcomes are overstated due to increased under-reporting of transfer income in survey data over time, and by examining measured that are well reported like consumption, we should conclude that material well-being has increased for the most economically disadvantaged since welfare reform.[44] In addition, another strand of literature has unambiguously shown that expansions to the EITC led to increases in income and earnings, and reductions in poverty among single mothers.[45] In sum this prior literature indicates that safety net reforms of the 1990's improved material conditions for single mothers.

My first chapter contributed to this literature on material well-being after welfare reform by examining how the economic consequences of parental union dissolution have changed since 1996. I showed that on aggregate there is less recovery in income following parental separation after welfare reform. In contrast, the poverty gap remains the same, and there are smaller percent

[42]See Blank (2002) and Grogger and Karoly (2005) for reviews of the literature on the effect of welfare reform on earnings and welfare receipt.

[43]See Rebecca M. Blank (2002), Jeffrey Grogger and Lynn A. Karoly (2005), and Ziliak (2016) for reviews of this literature.

[44]Meyer & Sullivan (2009) ; Han, Meyer & Sullivan (2021)

[45]Hoynes (2019) provides summary of the economic literature of the effects of the EITC.

declines and more recovery in food expenditures. I also showed that the worse income-based outcomes were concentrated among children born to mothers with a high school degree or less; children most impacted by the 1996 welfare reform. A remaining question is how the reforms made to the safety net in 1990's impacted these outcomes explored in chapter one.

In this chapter, I highlight how the 1990's reforms contributed to these overall income and food expenditures effects by showing how components of income and food expenditures have changed after parental separation since the 1996 welfare reform, for children more and less impacted by this reform. In particular, using data from the Panel Survey of Income Dynamics (PSID) from 1968-2019, I show how receipt of AFDC/TANF, EITC, CTC, and earnings following union dissolution changes before and after the 1996 welfare reform for children born to mothers with a high school degree or less. These income sources are directly impacted by these reforms. Measuring changes in these components of income will show the distinct contribution of the 1996 welfare reform, and the tax safety net expansions separately on the changing aggregate income consequences of parental union dissolution after 1996.

In addition, I show how expenditures on food at home, restaurant meals, and food bought with food stamps (SNAP) change following union dissolution before and after the 1996 welfare reform for this same sample. This is done to comprehensively assess changes in material well-being, and to determine whether these reforms differentially impacted the various types of food spending. This will show whether the smaller percent declines in food expenditures following union dissolution found in chapter actually reflect improvements in material well-being resulting from the 1990s policy reforms, as opposed to increased reliance on in-kind benefits like SNAP. I contrast the experience of this population of children with those born to mothers with more than a high school in order to better understand the direct effect of the policy changes.

This is the first paper to show why the safety net reforms of the 1990's impacted the types of economic resources available to children following divorce or separation. In particular, I contrast

the ability of the safety net to mitigate the economic effects of union dissolution before the 1996 welfare reform. By contrasting changes in components of income following parental separation across these two periods, I extend work by Page & Stevens (2004). In their paper, they showed how earnings and AFDC receipt changed following union dissolution. I show changes in these income components, as well as changes in EITC and CTC benefits, before and after welfare reform. This allows me to assess how the role of the tax and non-tax cash safety net has changed over time. In addition, by examining how these components of income change following union dissolution over time, among children more and less affected by these reforms, I can more directly estimate the effect of the policy changes.

A second contribution of this paper is that it more comprehensively examines how 1996 welfare reform and the other safety net reforms of the 1990's impacted children's material well-being by also examining components of food expenditures. Meyer and Sullivan (2008) indicate that food expenditures are a measure of well-being that has not seen the sharp increase in under-reporting that transfer income has, and are more closely tied to well-being for poor households. By examining changes in spending on food at home versus spending on restaurant meals, my results will show whether shifting from an out-of-work to an in-work safety net had an effect on food spending patterns after divorce or separation. I will also examine changes in spending on food using food stamps (SNAP). This analysis will show whether the smaller percent declines in food expenditures following union dissolution after welfare reform, found in chapter one, are the result of increases in the generosity of SNAP from 2009-2014, or if these smaller percent declines are evidence of improved material conditions resulting from the safety net reforms of the 1990's.

My event study estimates show that the 1996 welfare reform and the other safety net policy reforms of the 1990's reduced the ability of the non-tax cash safety net to mitigate income losses following parental union dissolution for children born to mothers with a high school degree or less. My results also suggest that these policies increased earnings among mothers most impacted by the reform. The analysis in chapter one showed that after 1996, these children faced larger percent

declines and less recovery in total family income following union dissolution, but also experienced smaller percent declines in food expenditures. In this chapter I show that this is partly driven by increases in SNAP generosity. I find smaller percent declines in non-SNAP food at home spending and larger percent declines in spending on restaurant meals following union dissolution. This is evidence consistent with SNAP spending being a complement to purchases on food at home, but not restaurant meals. As families face larger income shocks from divorce or separation after 1996, they rely more on the SNAP program to smooth food expenditures.

Larger percent declines in income driven by reduced TANF generosity, coupled with SNAP driven improvements in food expenditures, is evidence consistent with material well-being worsening after welfare reform for children born to mothers with a high school degree or less. For children born to mothers with more than a high school degree, there are no changes over time in the responsiveness of the public safety net to union dissolution. In addition, percent declines in spending on food at home and restaurant meals are smaller following union dissolution since 1996, and this is not driven by increased SNAP generosity. This indicates that material well-being is no worse than before welfare reform for these families.

For children born to mothers with a high school degree or less, I find that within a year of union dissolution, TANF benefits increased 43% less after 1996 than AFDC benefits did prior to 1996. Four to five years after parental separation, TANF benefits are 50% lower than AFDC benefits were.[46] I also find that EITC is responsive to union dissolution for these children, but it's responsiveness has not changed since 1996. In contrast, CTC benefits decrease by 50-60% following divorce or separation. These decreases are driven by families losing eligibility.[47] Finally, there is some evidence of smaller declines in family earning following union dissolution, which is consistent with the documented maternal labor supply effects of welfare reform. Even though these changes lead to larger percent declines and less recovery in total family income following

[46] After parental union dissolution, among families that received TANF or AFDC, the average yearly TANF benefit is $7,500 (46%) lower than the average AFDC benefit.

[47] 20% of these children lost CTC eligibility after their parents separated.

union dissolution, non-SNAP expenditures on food at home falls 10-14% less since 1996. This is partially attributable to increased SNAP generosity since 1996 welfare reform; not welfare reform improving material well-being. By comparison, there is no change in the ability of the public safety net to buffer the economic costs of union dissolution net among children born to mothers with more than a high school degree. Furthermore, percent declines in expenditures on restaurant meals are 15-16% smaller, and there is suggestive evidence that percent declines in expenditures on meals eaten at home are smaller as well. This is all indicative of the 1990s safety net reforms playing no role in buffering the economic costs of union dissolution for children born to mothers with more than a high school degree.

The rest of the paper is laid out as follows: Section 2 provides background information regarding major changes in the public safety net during the 1990's. Section 3 describes the PSID data and presents descriptive statistics. Section 4 describes the event study model used in this paper. Section 5 presents my results, and Section 6 details this paper's conclusions.

2.2 Background

During the 1990s the U.S safety underwent substantial reforms which may have changed the economic costs of parental separation. The most significant reform was the 1996 Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA), which replaced entitlement based cash assistance (AFDC) with work based cash assistance (TANF). This federal reform came in the wake of waivers granted to states starting in 1992. By the time the federal legislation passed, 27 states used these waivers to reform their AFDC programs in ways that were ultimately federalized as part of 1996 welfare reform. Waiver policies included family caps, time limits and work requirements.

Once the largest cash assistance program available to single mothers, the Aid to Families with Dependent Children (AFDC) program was replaced by the Temporary Assistance for Needy Families (TANF) program in 1996. Unlike AFDC, TANF receipt is generally limited to a federal

maximum of five years, although some states have time limits shorter than five years. One of the principal goals of welfare reform was to encourage two-parental family formation among welfare eligible mothers, so the TANF program is also available to two parent families, whereas AFDC was generally limited to single parents. Another major goal of welfare reform was to encourage labor force participation, which is why the non-working poor are generally ineligible for TANF. Blank (2002) presents a review of the research using variation in state waiver policies to evaluate the impacts of these policies on caseloads and work participation rates, among other outcomes. These papers find that waivers reduced caseloads and increased work participation rates among single mothers.

TANF also replaced the matching fund arrangement under AFDC with a block grant. The TANF block grant is fixed in nominal terms and the contribution for each state is determined by the federal AFDC matching grant contribution in the years before PRWORA. As a result, benefits paid to families have been falling in real terms. According to a Congressional Research Service Report, by 2021 the real value of the TANF block grant has fallen 40% since 1997. In addition to the nominal value of the benefit falling, cash assistance makes up a much smaller share of TANF spending than it did under AFDC, as states are using funds for in-kind benefits and services like child care, marriage promotion, and job training. In FY2020, the average state only spent 22% of their block grant on direct cash assistance. [48]

De-coupling cash assistance receipt from single parenthood, the reduction in the value of cash assistance benefits through inflation erosion, allocation of TANF funds away from cash provision, and limiting the program for non-working families with children, would make the program less responsive to union dissolution at the intensive and extensive margin for children born to mothers with a high school degree or less. However, by leading to increases in maternal labor supply, we might expect to see smaller percent declines in family earnings following union dissolution, which could counteract declines in program responsiveness.

[48] https://sgp.fas.org/crs/misc/RL32760.pdf

This period also marked the beginning of expansions to and introduction of policies emphasizing work-based assistance like the Earned Income Tax Credit (EITC) and the Child Tax Credit (CTC). The 1993 expansion of the Earned Income Tax Credit (EITC) increased the maximum benefit available to families with one child by just over $1,000 (real $2019) and increased the maximum benefits available to families with two or more child by $2,300 ($2019) [49] In addition to the expansions made to the EITC, the Child Tax Credit (CTC) was enacted as part of the Taxpayer Relief Act of 1997. The original CTC was a $500-per-child nonrefundable credit to provide tax relief to middle- and upper-middle-income families. Given the credit is non-refundable, only families with a tax liability are eligible. Since 1997, several laws have been enacted that expanded the availability of the benefit to more low-income families while also increasing the value of the tax credit.[50]

Research has shown that the EITC led to increased labor supply of single mothers, but decreased labor supply of married mothers. [51] In this paper's setting, roughly half the children in my sample go from having a married mother to a single mother. The other half of children go from having a cohabiting mother to a single mother. If married mothers earn less as a result of EITC expansions, then after 1996, union dissolution should result in larger percent declines in earnings. However, if as a result of the EITC, these mothers start working and earning more when they are single, this could lead to similar or smaller percent declines in earnings following union dissolution over time. Thus, it is not immediately clear whether percent declines in total family earnings would be smaller or larger after the 1990's. It's also ambiguous whether EITC or CTC benefits would increase following union dissolution for children born to mothers with a high school degree or less because

[49]Originally enacted in 1975, the maximum EITC credit was $400 and phased out between incomes of $4,000 and $8,000. The Revenue Act of 1978 increased the maximum credit to $500. The credit further increased to $550 in 1984, and then $800 in 1986. The Omnibus Reconciliation Act of 1990 increased the maximum credit for families with two or more children.

[50]The Economic Growth and Tax Relief Reconciliation Act of 2001 increased the amount of the credit over time to $1,000 per child and made it partially refundable under the earned income formula. The Emergency Economic Stabilization Act of 2008 and the American Recovery and Reinvestment Act of 2009 further expanded the availability and amount of the credit to taxpayers whose income was too low to either qualify for the credit or be eligible for the full credit. Finally, at the end of 2017, Congress enacted P.L. 115-97 which doubled the maximum amount of the credit.

[51]Eissa and Hoynes (2004)

EITC and CTC receipt is not tied to family structure. Additionally, unlike the EITC, Hoynes & Rothstein (2016) find most of the benefits of the CTC go to higher income households. This suggests that by lowering family income, parental union dissolution should lower CTC receipt. Unlike TANF, the behavioral effect of these programs on earnings is ambiguous. The mechanical effect of union dissolution on EITC receipt is ambiguous, and the mechanical effect on CTC receipt is negative.

It is not immediately evident how the safety net reforms of the 1990's impacted components of food expenditures following union dissolution. If more single mothers are employed, we should expect to see smaller percent declines in restaurant meals following union dissolution over time, because working mothers have less time to prepare meals at home. However, if percent declines in total income are larger over time, the income effect should lead to families substituting away from restaurant meals and towards cooking at home, which could lead smaller declines in expenditures on food consumed at home and larger declines in restaurant expenditures.

In addition to the major reforms made to the public safety net in the 1990's, the composition of two-parent families has changed drastically over time. Specifically, there was a retreat from marital childbearing among low skilled mothers (Lundberg et al. 2016). By 2019, among children born to mothers with a high school degree or less, the share born to married parents had fallen by 40 percentage pints (57%) since 1970. The retreat from marital childbearing was accompanied by a rising share of children born into unmarried cohabiting unions. Cohabiting unions are more economically disadvantaged and unstable than marriages. Children born to cohabiting parents in 1996 represented less than 30% of the children whose parents separated. By 2017, 50% of children whose parents separated were born to unmarried cohabiting parents. Tach & Eads (2015) document that the short term income losses associated with cohabiting union dissolution have grown since the 1990s, while those associated with divorce have slightly improved. This could be because children born into cohabiting unions were most impacted by the 1996 welfare reform. Since a growing share of children are born into these unions, understanding how public safety net

reforms affect economic consequences following union dissolution is especially important.

Importantly, changes in food expenditures following union dissolution could be different over time because of this compositional shift in parental unions. Deleire & Kalil (2005) show that unmarried parents with children spend a lower share of their income on food than do single parents, but married parents spend the same share of their income on food as divorced parents.[52] In particular, Deleire & Kalil (2005) show that as a share of total income, cohabiting parents spend 13% less on food consumed at home, as compared to single parents. All else equal, as cohabiting unions become a larger share of all unions with children, percent declines in expenditures for food consumed at home should be smaller over time.

2.3 Data

I use data from the 1968-2019 Panel Study of Income Dynamics (PSID) to investigate how parental union dissolution affects children's access to economic resources. The PSID is a longitudinal survey administered by the University of Michigan's Institute for Social Research. The PSID data begins in 1968 and has detailed information on family and individual economic resources, demographics, and family structure. Importantly, the longitudinal structure of the PSID allows me to control for fixed effects.

The original PSID sample of roughly 18,000 people living in 5,000 households was made up of a nationally representative sample, called the Census sample, and an over-sample of low income families, called the Survey of Economic Opportunity (SEO) sample. The SEO sample was included to enhance research into poverty-related issues. Families from the Census and SEO samples and

[52]To illustrate why a growing share of children born to cohabiting parents will lead to lower percent declines in food expenditures, consider the following example: A cohabiting couple spends 25% of their income on food and a newly single formerly cohabiting parent spends 30% of their income on food. If income declines 30% following the parent's separation, food expenditures would decline 16%. In contrast, a married couple spends 25% of their income on food and a divorced parent spends 25% of their income on food. If income declines 30% following the parent's divorce, food expenditures would decline 30%. This simple example shows that all else equal, if the share of children born to cohabiting parents increases over time, the percent decline in food expenditures following union dissolution should fall.

their split-offs constitute the Core PSID sample and were interviewed annually from 1968-1996 and biannually from 1997-2019. My analysis uses families in the Census and SEO samples to increase the precision of the estimates and because the low income sample is a high impact sample for my analysis. My estimates are weighted using the child longitudinal weight in the last year the child is observed to ensure a nationally representative sample. The child's weight is used because this analysis is based on the family resources available to a focal child.

After 1997, the PSID surveys families every other year. This means that before 1997, I have data on family resources every year, but starting in 1997, I only have data on family resources every other year. Because of the way PSID survey questions are asked, if a child's parents separated after 1996 in an even year, I will have data on income and poverty status available to the family in even event times and data on food expenditures in odd event times. If a child's parents separated after 1996 in an odd year, I will have data on income and poverty status available to the family in odd event times and data on food expenditures in even event times.[53] To ensure that any changes in the estimated affects of union dissolution since 1996 are not due to changes in survey design, I define my event times the same way when estimating effects before and after 1996. In particular, I create event time bins to capture resources -3 or more, -2 or -1, 0 or 1, 2 or 3, 4 or 5, and 6 or more years since the "event".

2.4 Empirical Strategy

The basic approach taken in this paper is to use event study regression with child fixed-effects to control for time-invariant unobserved family characteristics that may be correlated with union dissolution. Given longitudinal data on family income, poverty thresholds, food expenditures and

[53]For example, if the union dissolution occurred in 2002, then I will have data on resources after union dissolution in years 2003, 2005,..etc. In a given survey year, data on income and poverty status refers to the prior calendar year. Thus, data on income or poverty in 2003, refers to income or poverty in 2002. This corresponds to 0 years since the "event". Data on income or poverty in 2005 would refer to income or poverty in 2004, or 2 years since the "event". Data on food expenditures in 2003 refers to the value in 2003 and thus represents expenditure 1 year since the "event". Data on food expenditures in 2005 refers to the value in 2005 and thus represents expenditure 3 year since the "event".

partner histories, the effects of union dissolution can be modeled by:

$$(2) \quad \mathbf{Y}_{i,t} = \beta \mathbf{X}_{i,t} + \omega D_{-3} \mathbb{1}\{t - E_i^* \leq -3\} + \sum_{d=(0,1)}^{(4,5)} \theta_d D_i \mathbb{1}\{t - E_i^* = d\} + \gamma D_{6+} \mathbb{1}\{t - E_i^* \geq 6\} + \tau_t + \alpha_i + \varepsilon_{i,t}$$

where $\mathbf{Y}_{i,t}$ is a family unit resource measure for child i, in year t = 1968,..., 2019.[54] Components of income and food expenditures are all expressed in real \$2019. τ_t are calendar year fixed effects which absorb national trends in outcomes over time. $\mathbf{X}_{i,t}$ is a set of time-varying child/family-specific control variables like dummies for family unit size, dummies for parent's age and child's age, and family food needs.[55]

The time-invariant child-specific effect, α_i captures anything about the child that is constant over time. Since most children in single-parent families live with their mothers, this variable will primarily pick up time-invariant characteristics of the child's mother that could be correlated with parental union dissolution and family income. For example, if mothers with higher earnings capacity are more susceptible to divorce, then estimates of divorce effects that fail to control for α_i will be biased toward finding smaller losses.

I use a binary indicator of "treatment", D_i, equal to 1 if child i experienced a parental union dissolution. The estimates characterizing the consequences of parental union dissolution are the coefficients on the interaction of D_i with event-year dummies, $\mathbb{1}\{t - E_i^* = d\}$, which are equal to 1

[54]For the income components and for food stamps the outcomes are specifically Log(1+Y). For the food expenditures components excluding food stamps the outcomes are Log(Y). Logs are used to measure percent changes. Measuring level changes in income don't directly reflect changes before and after reform in the economic consequences of union dissolution because incomes are higher post welfare reform. Furthermore, the levels specification is not ideal for the main analysis because parental union dissolution is likely to have very different level effects on rich and poor families. I use of Log(1+Y) to mitigate against data loss when using the income and food stamp variables. 5% of families report 0 earnings, 90% of families report 0 AFDC/TANF income, 70% of families report SNAP benefits of 0, 60% of families report 0 EITC benefits, and 70% of families report CTC benefits of 0. Less than 0.5% of families report 0 expenditures on food at home or at restaurants, so I am not concerned about data loss using the Log(Y) for these outcomes.

[55]The family food needs variable is constructed by the PSID to measure the caloric needs of the family, accounting for family size, sex, and the age of the family members. This control variable is only used when the components of family unit food expenditures are the outcomes.

when the year of observation is d = -10,...,0,...,14 years from E_i^*, the year when the child's parents separated (d = (-2,-1) is omitted). I only observe resources up 10 years before and 14 years after parental union dissolution because I am only estimating effects for children who experience union dissolution from ages 2-10 and children are only observed from ages 0 to 16.

My sample only includes children who experienced a union dissolution from ages 2-10 because children who were older than 10 will not contribute to estimating all post event time coefficients, and children that experienced union dissolution before age 2 cannot contribute to estimating the base event time. This is primarily done to ensure the average age of the child is comparable across event time, thereby ensuring that treatments effects are not a function of differences in the ages of children at each event time. The advantage of using this sample instead of less restrictive age sample, is that this sample yields internally consistent coefficient estimates. The disadvantage is that estimates derived using this sample are not representative of the effects that would occur for the average child. However, over 80% of parental union dissolutions occur between ages 2 and 10, so this sample does represent a large majority of divorces or separations. Pre-period trends in outcomes are captured by the dummy, $\mathbb{1}\{t - E_i^* \leq -3\}$, and resource recovery 6 or more years after parental union dissolution is captured by the dummy, $\mathbb{1}\{t - E_i^* \geq 6\}$. The coefficients on θ_d describe the divergence in outcomes d years after union dissolution, relative to the pre-period, adjusting for model covariates. Standard errors are clustered at the family level.

Even though the estimation of the $\mathbb{1}\{t - E_i^* \leq -3\}$ dummy uses an unbalanced set of years, the figures and tables will show the estimated coefficient on this event time dummy to provide visual evidence of parallel pre-trends. The estimation of the $\mathbb{1}\{t - E_i^* \geq 6\}$ dummy also has an unbalanced set of years, and this coefficient does not directly reflect more or less resource recovery following union dissolution before and after welfare reform. As such my tables and figures will exclude the estimate the this coefficient.

To assess the role of the safety net reforms of the 1990's on the family income and food resources available to children whose parents divorce or separate, I separately estimate the outcome dynamics post dissolution for children whose parents separate from 1970-1991 (before welfare reform) and 1999-2019 (after welfare reform).[56]

2.4.1 Estimation Sample

This paper's identification comes from comparing children's access to economic resources after a parental divorce (or the end of a non-marital cohabiting partnership), relative to before. As such, my sample consists of an unbalanced sample of children who are born into two biological parent families and are potentially followed from the year of birth until age 16.[57] The change in economic resources following the first parental union dissolution experienced by children born into two biological parent families directly reflects the economic consequences of going from a two parent to a one parent family. This analysis leaves out the experiences of children born to single mothers, and children born into two-parent families that never divorce or separate.[58]

A child is only followed up to age 16 because living with parents past age 16 could be an endogenous response to resources available in the family unit (Dey et al. (2014)). Children who were born before the 1968 survey are excluded from the sample because I cannot determine whether they were born into a two-parent or single-parent family.[59] I only use data on children who experienced a parental union dissolution from ages 2 to 10 years. This ensures that children's age at parental union dissolution is balanced across event times -2 to 6+. This represents 70% of children born into two-parent families who experience a parental union dissolution by age 16. I

[56]I exclude children whose parents divorced or separated from 1992-1998, because the years used to estimate the event time coefficients would be a mix of pre- and post 1996 welfare reform years, thereby obscuring differences in effects before and after welfare reform.

[57]The adults that I observe living with a child in the birth year, are both the child's biological parents.

[58]Children born to single mothers may experience the end of the union between their biological mother and a step-parent. I do not capture the end of a step-parent union, because the pre-period will reflect years in which the child was living with a single mother, thus the event study estimates will not speak directly to how resources change when a two-parent family becomes a one parent family.

[59]Data on family structure is based on the current structure in a given year and the first year of the PSID is 1968.

do not include children whose data come from adoption records because I cannot observe their resources at birth and I exclude cases where the union dissolution results from a parental death.

My main results use a sample of children whose mother *or* father gets custody after union dissolution. Using this sample allows me to capture the role of an increasing share of children whose father gets custody over time on the economic consequences of union dissolution for children. All sample restrictions and how they affect the final analysis sample are outlined in the Appendix.

After deleting observations for which the income and consumption measures are imputed or missing, my sample consists of 2,7994 children (28,620 child years). On average, 44% of the children in this sample were born to mothers with more than a high school degree, and 56% were born to mothers with a high school degree or less. Additionally, the parental union dissolution resulted in the mother getting custody in 81% of cases, while in 19% of cases the father got custody. The share of children living with their fathers after divorce or separation has been rising over time, highlighting the importance of accounting for all parental living arrangements.[60] These samples are slightly smaller when components of food expenditures are my dependent variables since the PSID is missing food expenditures and food stamp data in 1973, 1988, and 1989.[61]

Not all children are present from birth up to age 16. 4.3% of children that I observe from birth attrit from the survey before the child turns 16. 13.6% of children I observe from birth don't respond to the survey for a number of waves and eventually return.[62] Even though I need to control for the family size the year before the survey, I can still use data from children that attrit. This is because I have data on whether a person moved in or out since the last year the family was surveyed, as

[60] Among union dissolutions that occur from 1968-1996, 15% of fathers get custody. Among union dissolutions that occur from 1997-2019, 25% of fathers get custody.

[61] The outcome variable is left as missing so that an estimate of the event time corresponding to the missing year of data is not estimated. This shouldn't bias my estimates of the event time coefficients because the fact that a certain variable is missing in 1973, 1988, and 1989 has nothing to do with its hypothetical value or with the values of other variables. The variable's value is missing completely at random (MCAR).

[62] 95% of these children return between 2 and 4 years later.

well as the specific year a person moved in or out. This data is available for all members of the family unit. This data allows me to construct a measure of family size in the calendar year before the survey year and to know the exact year of the union dissolution if the child's data is missing in some years.[63]

My estimation model will include child fixed effects in order to control for time-invariant selection into attrition or non-response. However, the decision to include observations from children with missing data could bias my results if attrition or survey non-response in any time period is a function of time-varying shocks. If there is no time-varying selection into attrition or survey non-response, then the unbalanced results are unbiased estimates of the effect of parental separation on family level economic resources. To investigate whether time-varying selection into attrition or survey non-response is biasing my unbalanced panel results, I re-estimate my results using a balanced panel. The balanced panel results provide an internally consistent effect of union dissolution on family level economic resources. Given that my balanced and unbalanced panel results are statistically and substantively similar, this suggests that time-varying selection into attrition or survey non-response is not biasing my results in this setting. As such, I will use the unbalanced panel for increased precision.[64]

2.4.2 Summary Statistics

Table 1 is a cross-sectional comparison of the demographic and socioeconomic characteristics of different family types before and after welfare reform. Columns 2-5 & 7-10 of Table 1 show that even before union dissolution, average family resources are lower for children whose parents will eventually divorce or separate than for families that will remain intact. This suggests that part of the difference in resources could exist for reasons other than differences in family structure. Given

[63]Given that data on income and food stamp receipt in a given survey year refers to their respective values in the prior calendar year, I need to construct a measure of family size in the calendar year prior to the survey year to temporally align the independent and dependent variables.

[64]Appendix Tables A3-A4 re-estimate my results using a balanced panel of children who are present in the survey from event time (-2,-1) to (4,5). I will further discuss the balanced panel construction and results in the section on robustness checks.

families that separate already have lower resources before separation, cross-sectional analyses will overstate the resource consequences of going from a two-parent family to a one-parent family. My model will include child fixed effects to address this concern, by controlling for unobserved time-invariant selection into union dissolution.

This cross-sectional analysis also shows the share of children born into two-parent families has remained constant over time. What has changed is the composition of two-parent families at child birth. In particular, before and after welfare reform, 75% of children are born into two biological parent families.[65] An increasing share of these children are born to unmarried cohabiting parents. Before welfare reform, 25% of these children are born to unmarried cohabiting parents. After welfare reform, the share born to unmarried cohabiting parents rose to 46%. Figure 1 shows that over 50 years, among children born to parents that eventually divorce or separate, the share born to cohabiting parents has risen from 5% in 1969 to 50% in 2019. Figure 2 augments these statistics by showing that the rise in cohabiting unions is most pronounced among mothers with a high school degree or less at child birth. Among children whose parents divorce or separate, by 2019, 75% of children born to mothers with a high school degree or less are born to cohabiting parents.[66]

The average child born into two-parent families that experience union dissolution is more socioeconomically advantaged over time. Family earnings have increased for those born to cohabiting and married parents. Married and cohabiting parents also spend less of their food budget on food at home and more on meals consumed outside the home after 1996. This is consistent with rising maternal labor force participation rates over time.

A falling share of children in this sample depend on the cash safety net, and a rising share depend on the in-kind and tax-based safety net. The TANF program is less generous than AFDC, but CTC, EITC, and SNAP benefits have risen since 1996. Finally, children in cohabiting unions

[65] Biological parent identification is based on self-reports.

[66] By comparison, in this same sample, 40% of children born to mothers with more than a high school degree are born to cohabiting parents.

utilize the safety net at higher rates than children of married parents. These summary statistics show that the types of safety net programs families use, and the types of families that use them has drastically changed since welfare reform. This indicates that income resources and by extension expenditures on food could be different following union dissolution over time.

The event study analysis in this paper will formally show the role of the 1996 welfare reform and other safety net reforms in the 1990's on the economic consequences of union dissolution, by comparing changes in components of income and food expenditures following union dissolution, before and after welfare reform, among children more and less affected by the 1996 welfare reform.

Components of Income

To highlight how the 1996 welfare reform and the other safety net reforms of the 1990's impacted the economic consequences of parental union dissolution, I show how four components of income change following parental union dissolution before and after 1996. These components represent 90% of total family income and are those that we would expect to be directly affected by these reforms.[67] The main family level income components measured in this analysis include AFDC/TANF benefits, Earned Income Tax Credit (EITC) benefits, Child Tax Credit (CTC) benefits, and earnings. These income components are expressed in real $2019.[68]

Components of Food Expenditures

I complement my analysis of changes in income components following parental union dissolution by also looking at changes in the components of food expenditures for three main reasons. First, several researchers argue that expenditure-based measures are preferable to income measures for assessing changes in material well-being among poor households over time because these

[67]Child support constitutes 5% of family income after divorce or separation. In a separate analysis, I show that percent increases in child support payments are very similar before and after welfare reform, for children more and less affected by the reform.

[68]Tax subsidies are calculated using the NBER Taxsim27 program created by Daniel Feenberg and Elizabeth Coutts (http://www.nber.org/taxsim/).

households increasingly under-report transfer income (Meyer, Mok and Sullivan (2015)). For this analysis, I examine components of food expenditures because these expenditures are available in the PSID across the analysis period, and they directly capture well-being. Second, the economic development literature suggests that consumption is preferred to income for measuring material well-being, particularly among poor households (Cutler & Katz 1991). Thus, examining which of these components is changing over time helps explain whether the 1996 welfare reform and the other safety net reforms of the 1990's positively or negatively impacted material well-being.

The main family level components of food expenditures measured in this analysis include expenditures on food consumed at home (excluding food purchased with food stamp benefits), food consumed away from home (excluding food purchased with food stamp benefits), and the dollar value food purchased using food stamp benefits. If more single mothers are employed, we should expect to see smaller percent declines in restaurant meals following union dissolution over time, because working mothers have less time to prepare meals at home. However, if overall income falls more over time, the income effect should lead to families substituting away from restaurant meals and towards cooking at home, which could lead smaller declines in expenditures on food consumed at home and larger declines in restaurant expenditures. Finally, if smaller percent declines in food expenditures are driven by increases in spending on food using SNAP, this indicates welfare reform did reduce material well-being, but these effects were counteracted by SNAP policy. These components of food expenditures are expressed in real $2019.

2.5 Results

The event study estimates in Table 2 show that the 1996 welfare reform and the other safety net policy reforms of the 1990's reduced the ability of the safety net to mitigate the effects of union dissolution responsiveness of the non-tax cash safety net to parental union dissolution, and contributed to increased earnings among mothers with a high school degree or less at child birth. Among children born to mothers with more than a high school degree at child birth, there is no

change in the role of the public safety net after 1996.

The estimates in Column (1) of Panel B, show that pre-1996, among children born to mothers with a high school degree or less, AFDC benefits increased by 0.902-1.360 log points (150-300%) following divorce or separation. The estimates in Column (1) of Panel A show that TANF benefits are 0.524-0.759 log points (43-55%) lower following union dissolution relative to AFDC. Among families that receive these benefits, this represents a $7,500 dollar decline in yearly benefits. Figure 12 shows that TANF is less able to buffer the economic costs of union dissolution than AFDC because a smaller share of these children receive the benefits after their parents separate, and the ones that do receive a smaller amount. In particular, receipt of non-tax cash assistance following union dissolution fell 50%, and benefits received per person fell 40% after welfare reform. By comparison, non-tax cash benefits are much less responsive to union dissolution for children born to mothers with more than a high school degree than for children born to mothers with a high school degree or less, as expected. The estimates in Column (2) of Panel B show that AFDC benefits increase by 0.432 log points (56%) within a year of union dissolution, but return to their pre-dissolution levels two years after. The estimates in Column (2) of Panel A show changes in TANF mirror changes in AFDC benefits, indicating that these children were relatively unaffected by welfare reform, as expected. [69]

The estimates in Column (3) of Panels A and B show that the EITC does play a role in reducing the economic consequences of union dissolution for children born to mothers with a high school degree or less. There is also suggestive evidence that EITC benefits increase by 40-50% more following union dissolution after welfare reform, but this effect is not statistically significant. Prior to welfare reform, EITC benefits increase by 1.204 log points (230%) within a year of union dissolution, and are still 0.533 log points (70%) higher 2-3 years later. The estimates in Column (4) of Panel B show that the ability of the EITC to mitigate the effects of union dissolution is higher among children born to mothers with more than a high school degree as compared to those born to

[69] These estimates are summarized graphically in Figure 5.

mothers with a high school degree or less. EITC benefits go up by 1.297 log points (265%) within a year of union dissolution, and are still 0.604 log points (83% higher) 2-3 years later. In addition, the estimates in Column (4) of Panel A show that the role of the EITC following parental separation has not changed since welfare reform. [70]

The estimates in Columns (5) and (6) of Panel A show that the CTC benefits decrease following parental union dissolution.[71] Column (5) of Panel A shows that among children born to mothers with a high school degree or less benefits fall by 0.722-1.025 log points (50-65%) after divorce or separation. These declines in average CTC receipt are driven by 20% of children losing eligibility.[72] These children lost eligibility because for many years after the CTC was introduced because eligibility was limited to families earning $10,000 at minimum. Prior to losing eligibility, these families were receiving an average benefit of $1,550 a year. This high threshold for eligibility was not lowered till 2009. The estimates in Column (6) show that among children born to mothers with more than a high school degree, CTC benefits fall 0.741 log points (50%) within a year of divorce or separation, but return to their pre-dissolution levels within two years. This can be explained by recovery in earnings among this subset of families that will be addressed when discussing the results in Column (8). [73]

The estimates in Column (7) of Panel B show that pre-1996, among children born to mothers with a high school degree or less, family earnings fell 0.985-1.415 log points (62-76%) after union dissolution. The estimates in Column (7) of Panel A show suggestive evidence that family earnings fall less following union dissolution.[74] The left panel of Figure 13 indicates that this is driven by increases in maternal employment. This figure shows that maternal employment increases 4-10

[70]These estimates are summarized graphically in Figure 6.

[71]There are no results in Panel B because the CTC was not enacted till 1997, so there is no CTC receipt before or after union dissolution prior to the 1996 welfare reform.

[72]See Figure 12.

[73]These estimates are summarized graphically in Figure 7. In Figure 7, the estimates for the pre-welfare period (1968-1996) are estimated as precise zeroes because the CTC was not introduced till 1997.

[74]Combining all of the post event years together I find that family earnings decline 25% post union dissolution after welfare, but this difference over time is not statistically different from zero.

percentage points more following union dissolution after welfare reform relative to before. This is consistent with prior work showing that the 1996 welfare reform and the EITC expansions increased employment of single mothers.[75] By comparison, the estimates in Column (8) of Panel B show that after welfare reform, there is no change in the percent declines in earnings following union dissolution among children born to mothers with a high school degree or less.

The estimates in Column (8) of Panel A show that pre-1996, earnings fall 0.919 log points (60%) within a year of union dissolution. Two to three years later, earnings are 0.835 log points (50%) lower relative to their pre-dissolution level, and are 0.565 log points (40%) below their pre-dissolution level four to five years later.[76] The pattern of earnings recovery mirrors the pattern of CTC recovery evident in the right panel of Figure 7, indicating that as earnings recover, these families regain eligibility for the CTC. The identical percent declines in earnings following union dissolution before and after welfare reform can be explained with evidence from Figures 4 & 13. Figure 4 shows that among mother's with a high school degree or more, employment have been consistently increasing since 1980. The right panel shows of Figure 13 shows that before 1996, these mothers are 10-20 percentage points more likely to work after divorce or separation, but after 1996, there is no change in their employment following union dissolution. Taken together this shows that before 1996 a lower share of mothers worked prior to union dissolution, but they increased their earnings and employment afterward. In contrast, after 1996, mothers were 10 percentage points more likely to be working prior to divorce or separation, and they remained working afterward. Because the same share of mothers with more than a high school degree were working after divorce or separation, in both time periods, there is no change in percent declines in earnings over time.[77]

[75]The right panel of Figure 3 also indicates that smaller percent declines in earnings are not the result of increased re-partnership.

[76]These estimates are summarized graphically in Figure 8.

[77]80% of mothers with more than a high school degree were working after divorce or separation before and after 1996.

To summarize, since welfare reform, the ability of the non-tax cash safety net to reduce income losses following union dissolution has drastically fallen for children born to mothers with a high school degree or less. There is suggestive evidence that their mothers increase their labor supply after divorce or separation, contributing to smaller percent declines in family earnings. The EITC is responsive to union dissolution, while CTC benefits fall after these children's parent divorce or separate. In contrast, there is no change in the percent declines in family earnings, or the role of the public safety net for children born to mothers with more than a high school degree.

In my first chapter I show that among children born to mothers with a high school degree or less, there are larger percent declines and less recovery in total family income following union dissolution after 1996. This is despite the increases in maternal employment outlined above. The evidence in Table 2 clearly shows this is due to the non-tax cash safety net being much less able to reduce income losses following union dissolution since 1996.

Even though these changes lead to larger percent declines and less recovery in total family income following union dissolution among children born to mothers with a high school degree or less, they did not result in larger percent declines in food expenditures. The estimates in Column (1), Panel B, of Table 3 show that the smaller percent declines in food expenditures occur solely for food consumed at home. After welfare reform, food at home expenditures fall 0.107-0.133 log points (10-14%) less following union dissolution, relative to before welfare reform. This is consistent with these families driving the increases in the share of children being born to cohabiting parents. These parents spend a smaller share of their income on food consumed at home, than do single mothers. The estimates in Columns (2) of Panel B suggest that children born to mothers with more than a high school degree also experience smaller percent declines in food at home expenditures, but the differences over time are not statistically significant.[78] There is also suggestive evidence of larger percent declines in spending on restaurant meals after welfare reform among children born to mothers with a high school degree or less. While the estimates are not statistically significant, the

[78]The increasing share of children born to unmarried cohabiting parents over time has been substantially less pronounced among this subset of families.

estimates in Column (3) of Panel B show that among children born to mothers with a high school degree or less, expenditures on restaurant meals fall 4% more following union dissolution after welfare reform.

In contrast, children born to mothers with more than a high school degree experience smaller percent declines in expenditures on restaurant meals. The estimates in Columns (4) of Panel B show that percent declines in spending on restaurant meals fall 0.135-0.147 log points (15-16%) less after welfare reform reform. Finally, smaller percent declines in food expenditures among children born to mothers with a high school degree or less are partly due to increases in SNAP generosity after welfare reform. The estimates in Column (5) of Panel B show that SNAP benefit increase by 0.653 log points (92%) more within a year of union dissolution after welfare reform relative to before. SNAP is a complement with food at home, and a substitute with meals eaten out, which also explains why expenditures on food at home fall less over time and expenditures on restaurant meals fall more for children born to mothers with a high school degree or less. The estimates in Column (6) of Panel B show that SNAP does not play a role in mitigating percent declines in food expenditures following union dissolution among children born to mothers with more than a high school degree, as expected.

Larger percent declines in income driven by reduced TANF generosity, coupled with substitution away from eating out at restaurants towards home cooking, is evidence consistent with material well-being worsening for socioeconomically disadvantaged families after welfare reform. For children born to mothers with more than a high school degree, percent declines in spending on eating out are smaller following union dissolution since 1996, and there is no change in percent declines in total income, indicating that material well-being is no worse than before welfare reform for these families.

Another explanation for why smaller percent declines in food expenditures following union dissolution after welfare reform is not evidence of improvements of material well-being comes

from research by Deleire & Kalil (2005). They show that cohabiting parents spend a smaller share of their income on food at home than single parents. As these types of unions become an increasing share of all parental unions, percent declines in food expenditures could be smaller over time. Further evidence in support of this comes from the fact that percent declines in food expenditures fall the most among children born to mothers with a high school degree or less; the subset of children whose parents have been increasingly likely cohabit instead of marry.

An alternative hypothesis is that smaller percent declines in food expenditures following union dissolution is in fact evidence against declines in material well-being, and percent declines in income are only larger after 1996 because transfer income is increasingly under-reported in survey data over time (Meyer and Sullivan 2021). While transfer income is increasingly under-reported, I show in my first chapter that the increase in under-reporting would not fully explain the 55% drop in reported AFDC/TANF income received after union dissolution since welfare reform. Cumulatively, the evidence in this paper strongly indicates that after 1996, divorce or separation results in larger declines in material well-being for economically vulnerable children.

2.5.1 Robustness Check

In a robustness check I test whether selection into attrition and non-response that is time-varying biases my estimates. The decision to include observations from children with missing data could bias my results if attrition or survey non-response in any time period is a function of time-varying shocks because the child fixed effect will not control for this time of selection. In Appendix Tables A3 & A4 I re-estimate my main results for components of income and food expenditures respectively, using a balanced panel.[79] The coefficients on the event time bin dummies (0,1) to

[79]I only use odd years of data to mimic the data construction when the survey became biannual in 1997. To be in the balanced panel the child had to be observed once in each two-year binned event time (-2,-1), (0,1), (2,3), and (4,5). Pre-welfare reform, the earliest year a child can experience parental union dissolution is 1970 because in my sample children are at least 2 when their parents separate. I also impose that the last year a child can experience parental union dissolution is 1991, so that the last survey year resources can be measured in the (4,5) bin is 1996. Post-welfare reform, I impose the earliest year a child can experience parental union dissolution is 1999, so that the earliest survey year resources can be measured in the (-2,-1) bin is 1997. The latest year a child can experience parental union dissolution is 2015 to have resources measured

(4,5) are not statistically different and do differ in direction from those using an unbalanced panel for any of the outcomes. This shows that time-varying selection into attrition and non-response is not biasing my main findings.

2.6 Conclusion

The cumulative evidence in this paper strongly indicates that the 1996 welfare reform and the other safety net reforms of the 1990's negatively impacted income resources among children born to mothers with a high school or less by reducing TANF cash assistance receipt following parental union dissolution.[80] While the EITC does play a role in reducing the income consequences of parental union dissolution, the effect is not statistically different over time. In contrast to the EITC, the CTC does not buffer income losses following union dissolution, and instead increases these losses. The declining ability of the cash safety net to reduce the income consequences of parental union dissolution does not result in larger declines in food expenditures following parental union dissolution. Due in part to the increased reliance on SNAP benefits, families are better able to smooth food expenditures following divorce or separation. These benefits allow families to reduce their expenditures on food eaten at home by a smaller amount over time. In contrast, families cut back more on spending on restaurant meals following union dissolution after 1996. The results for children born to mothers with more than a high school degree are in stark contrast to the findings for children born to mothers with a high school degree or less. In particular, the ability of the safety net to mitigate the economic consequences of parental union dissolution for these children has not changed over time. In addition, these families face smaller percent declines in expenditures food eaten at home and expenditures on restaurant meals, indicating the material well-being is no worse for these families after 1996. This paper shows that the safety net is less able to mitigate the economic consequences of parental union dissolution for children born to mothers with a high school degree or less, but not for children born to mothers with more than a

four years after the split. Thus, in the balanced panel children experience union dissolution from 1970-1991 in the pre-reform sample, and they experience union dissolution from 1999-2015 in the post reform sample.

[80] All the results are robust to accounting for time-varying selection into attrition and non-response.

high school. This is a significant driver of the larger percent declines in income following union dissolution for socioeconomically disadvantaged children found in chapter one.

Figures

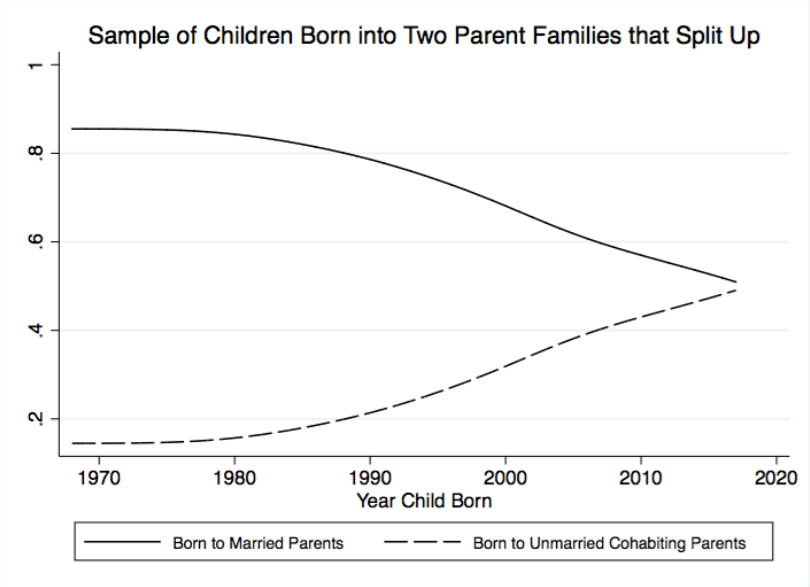


Figure 2.1: Figure shows changes in family structure at birth from 1968-2019 among children born into two-parent families that divorce or separate. Changes are weighted using PSID longitudinal weights.

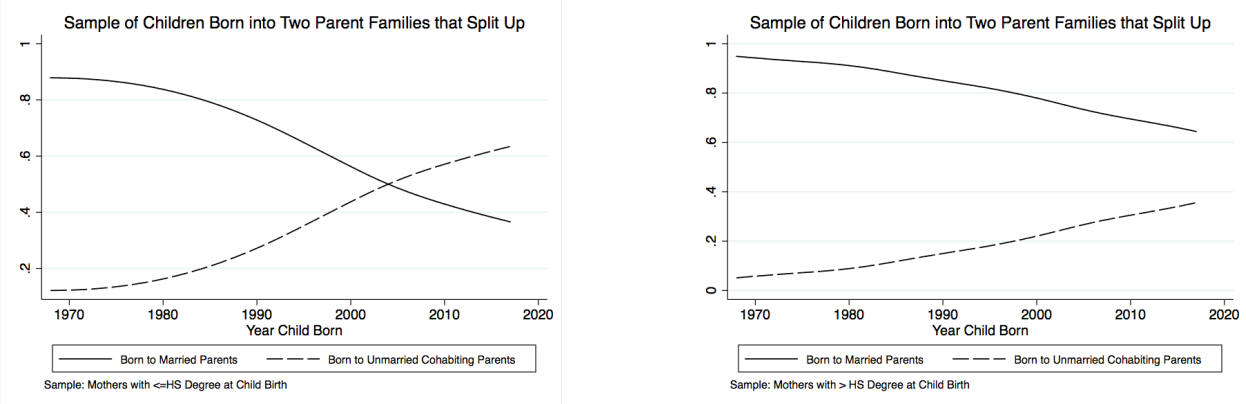


Figure 2.2: Figure shows changes in family structure at birth from 1968-2019 among children born into two-parent families that divorce or separate by mother's educational attainment at child birth. Changes are weighted using PSID longitudinal weights.

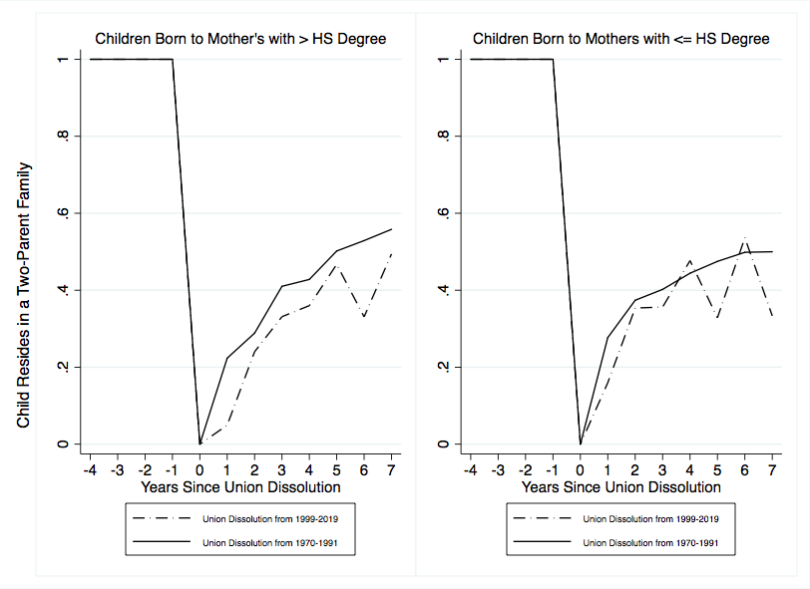


Figure 2.3: Figure shows changes in re-partnership dynamics before and after 1996 by mother's educational attainment at birth among children born into two-parent families that divorce or separate. Changes are weighted using PSID longitudinal weights.

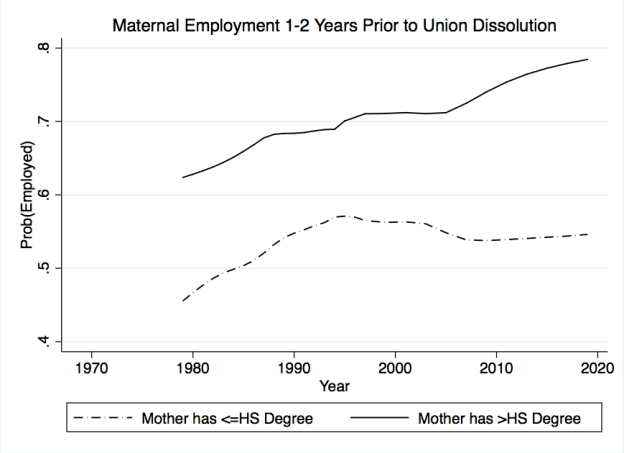


Figure 2.4: Figure shows changes in maternal labor supply for married or cohabiting mothers by educational attainment at birth among children born into two-parent families that divorce or separate. Changes are weighted using PSID longitudinal weights.

Main Analyses

Components of Income

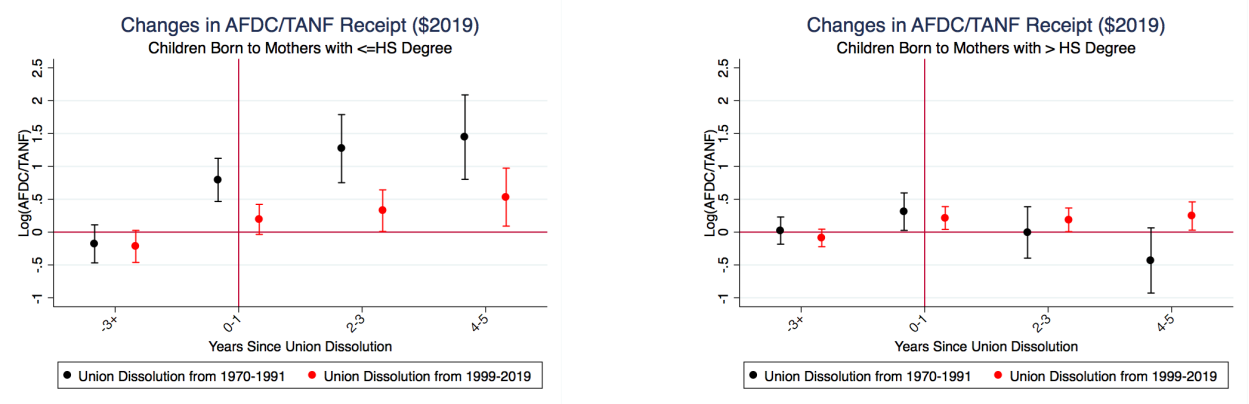


Figure 2.5: Outcome is Log(1+Family AFDC/TANF Income). **Black** series refers to union dissolutions from 1970-1991 and resources in event times (-2,-1) to (4,5) are measured from 1968-1996. **Red** series refers to union dissolutions from 1999-2019 and resources in event times (-2,-1) to (4,5) are measured from 1997-2019. Weighted using the child longitudinal weight in the last year observed.

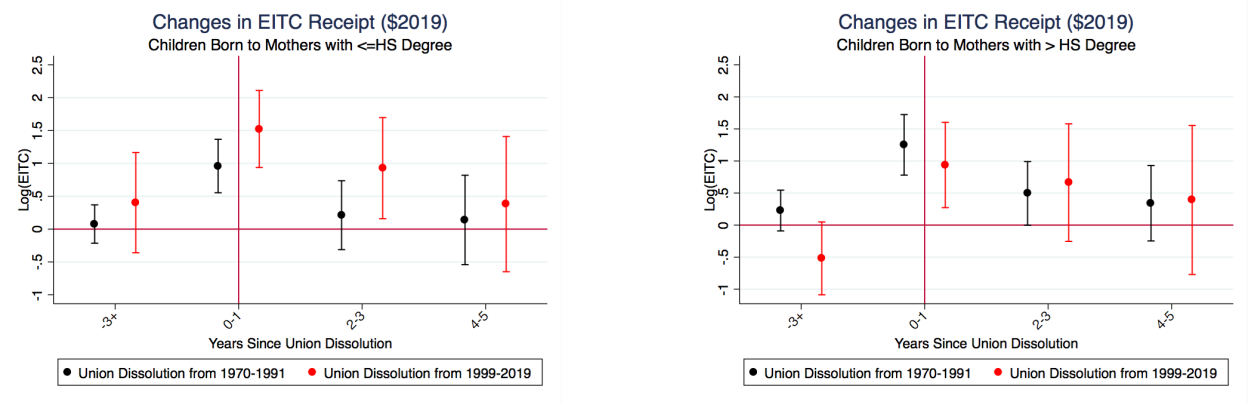


Figure 2.6: Outcome is Log(1+Family EITC Income). **Black** series refers to union dissolutions from 1970-1991 and resources in event times (-2,-1) to (4,5) are measured from 1968-1996. **Red** series refers to union dissolutions from 1999-2019 and resources in event times (-2,-1) to (4,5) are measured from 1997-2019. Weighted using the child longitudinal weight in the last year observed.

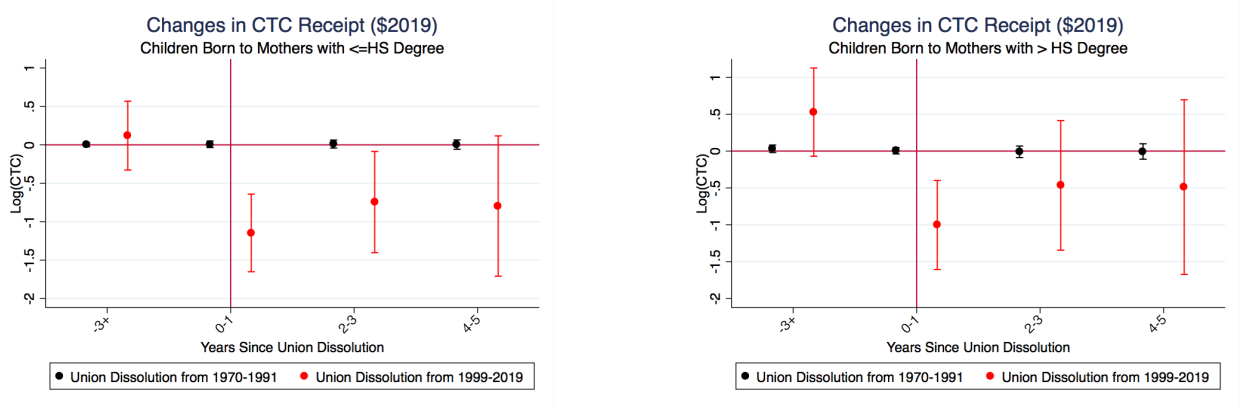


Figure 2.7: Outcome is Log(1+Family CTC Income). **Black** series refers to union dissolutions from 1970-1991 and resources in event times (-2,-1) to (4,5) are measured from 1968-1996. **Red** series refers to union dissolutions from 1999-2019 and resources in event times (-2,-1) to (4,5) are measured from 1997-2019. Weighted using the child longitudinal weight in the last year observed.

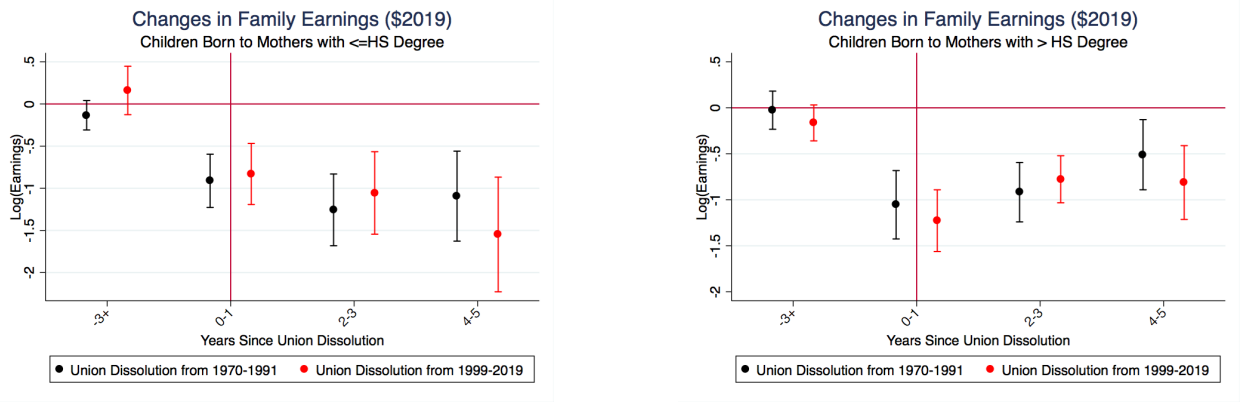


Figure 2.8: Outcome is Log(1+Family Earnings). **Black** series refers to union dissolutions from 1970-1991 and resources in event times (-2,-1) to (4,5) are measured from 1968-1996. **Red** series refers to union dissolutions from 1999-2019 and resources in event times (-2,-1) to (4,5) are measured from 1997-2019. Weighted using the child longitudinal weight in the last year observed.

Components of Food Expenditures

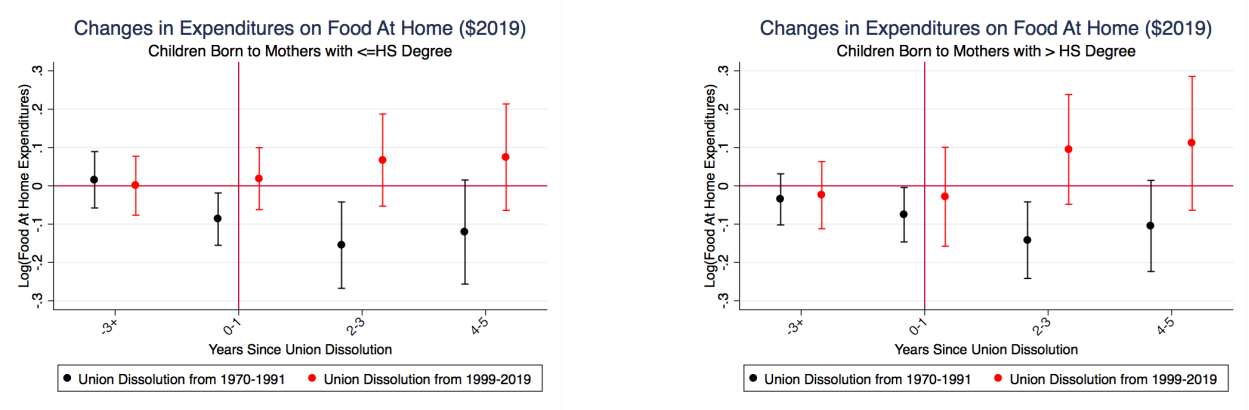


Figure 2.9: Outcome is Log(Family Expenditures on Food At Home). **Black** series refers to union dissolutions from 1970-1991 and resources in event times (-2,-1) to (4,5) are measured from 1968-1996. **Red** series refers to union dissolutions from 1999-2019 and resources in event times (-2,-1) to (4,5) are measured from 1997-2019. Weighted using the child longitudinal weight in the last year observed.

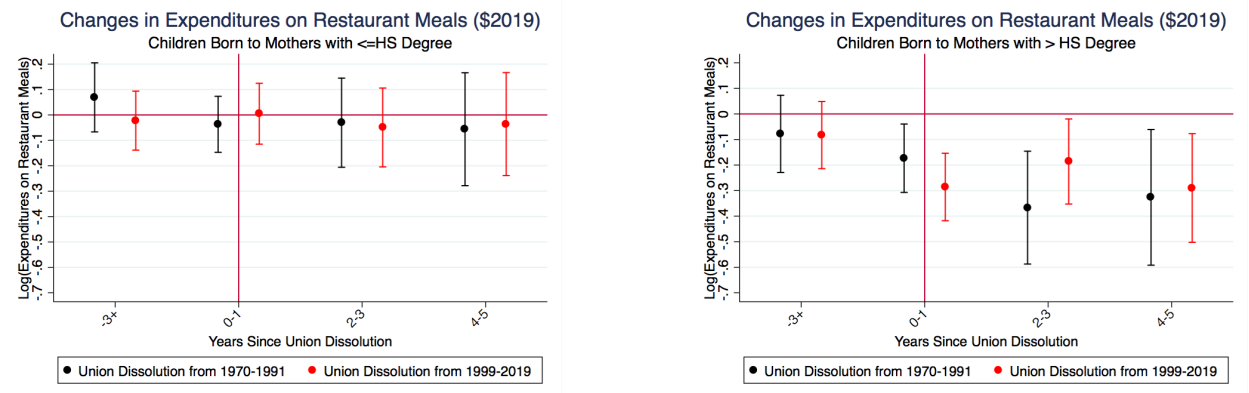


Figure 2.10: Outcome is Log(Family Expenditures on Restaurant Meals). **Black** series refers to union dissolutions from 1970-1991 and resources in event times (-2,-1) to (4,5) are measured from 1968-1996. **Red** series refers to union dissolutions from 1999-2019 and resources in event times (-2,-1) to (4,5) are measured from 1997-2019. Weighted using the child longitudinal weight in the last year observed.

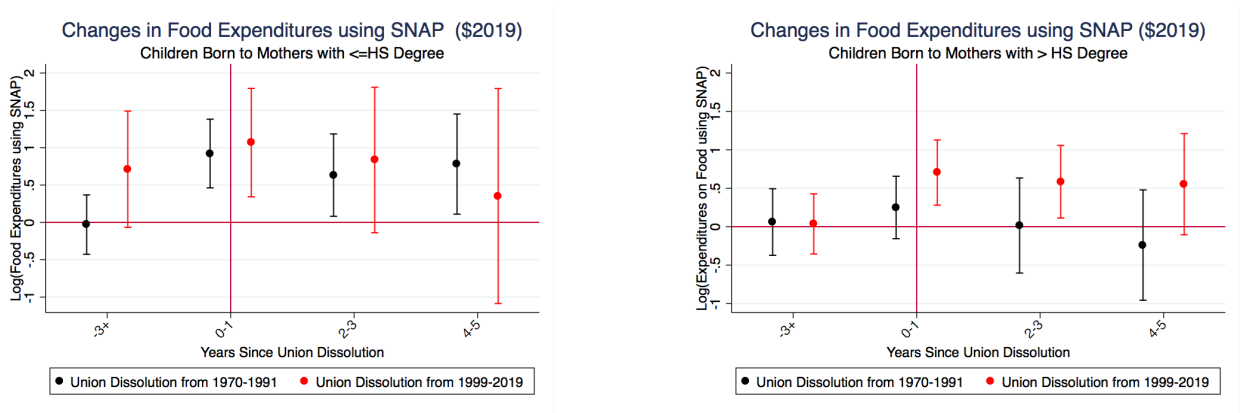


Figure 2.11: Outcome is Log(1+Family Food Expenditures using SNAP). **Black** series refers to union dissolutions from 1970-1991 and resources in event times (-2,-1) to (4,5) are measured from 1968-1996. **Red** series refers to union dissolutions from 1999-2019 and resources in event times (-2,-1) to (4,5) are measured from 1997-2019. Weighted using the child longitudinal weight in the last year observed.

Supplementary Analyses

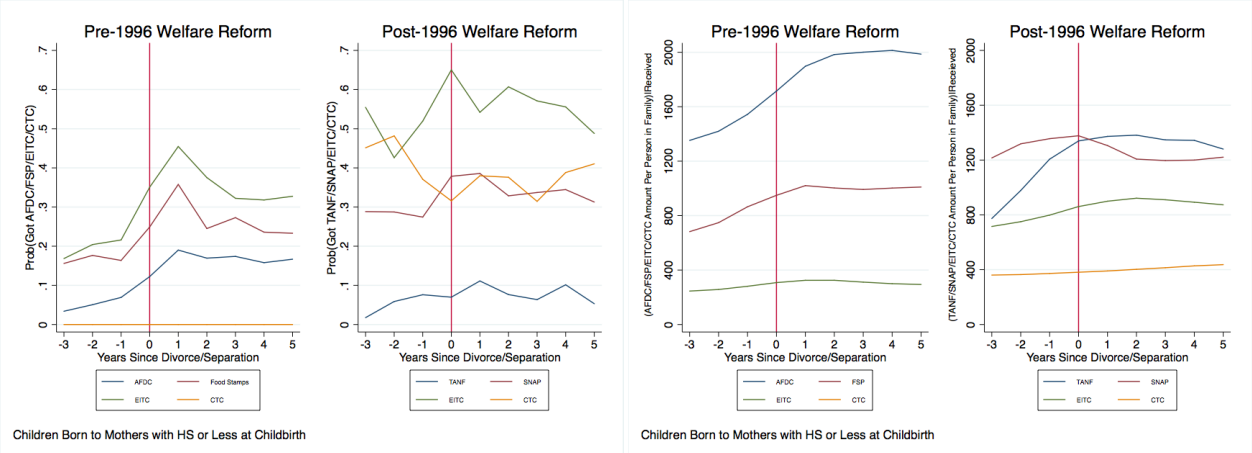


Figure 2.12: Program Take-Up and Generosity Among Children Born to Mothers with ≤ HS degree. Weighted using PSID longitudinal individual weights.

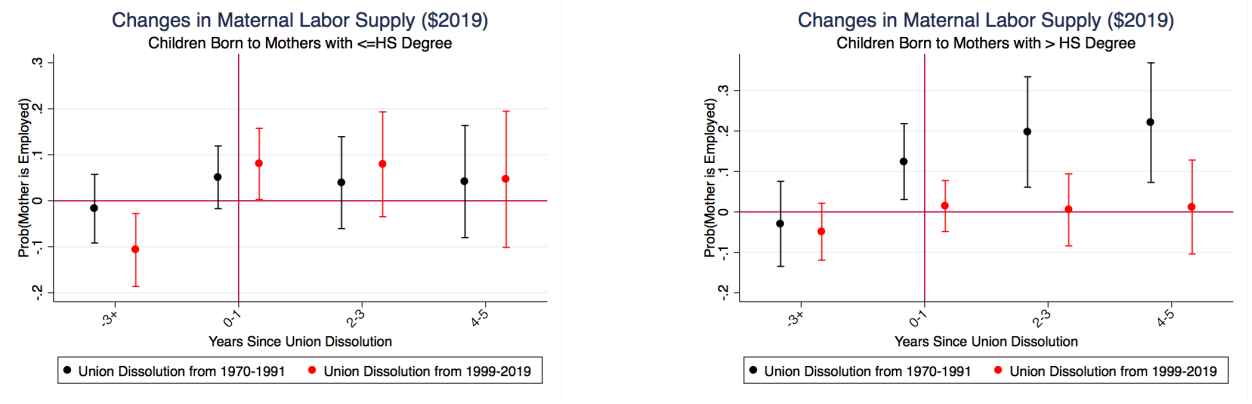


Figure 2.13: Outcome is Prob(Mother is Employed). **Black** series refers to union dissolutions from 1970-1991 and resources in event times (-2,-1) to (4,5) are measured from 1968-1996. **Red** series refers to union dissolutions from 1999-2019 and resources in event times (-2,-1) to (4,5) are measured from 1997-2019. Weighted using the child longitudinal weight in the last year observed.

Tables

Table 2.1: *Families Characteristics in the Year of Birth*

Birth Year	1968-1996	1968-1996	1968-1996	1968-1996	1997-2019	1997-2019	1997-2019	1997-2019
Type:	Cohabiting (Stay Together)	Cohabiting (Separate)	Married (Stay Together)	Married (Separate)	Cohabiting (Stay Together)	Cohabiting (Separate)	Married (Stay Together)	Married (Separate)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Share of All Families:	3%	9%	42%	21%	11%	12%	39%	13%
Family Head is Black (%)	18.0	24.0	5.0	10.0	17.0	27.0	6.0	12.0
Family Head is Hispanic (%)	5.0	12.0	4.0	4.0	8.0	5.0	5.0	3.0
Family Head is White (%)	75.0	63.0	90.0	84.0	68.0	60.0	85.0	82.0
Mother's Age at Childbirth	23.56	21.78	26.18	24.01	25.58	22.45	29.22	26.48
Mother Had ≤ HS Degree (%)	70.0	81.0	51.0	64.0	49.0	73.0	18.0	46.0
Food at Home Expenditures ($2019)	7,352	8,790	8,572	8,245	7,983	6,560	7,646	7,092
Food away from Home Expenditures ($2019)	1,997	1,371	1,936	1,693	2,806	2,139	2,765	2,206
Family Earnings ($2019)	49,972	44,576	82,990	65,921	81,838	42,717	114,751	80,873
Value of SNAP benefits ($2019)	507	794	86	235	999	1,359	235	557
AFDC/TANF ($2019)	473	921	62	251	77	212	6	105
Total CTC ($2019)	0	0	0	0	1,862	709	1,160	1,071
Total EITC ($2019)	127	174	26	58	996	1,100	380	585
Family Received AFDC/TANF (%)	11.0	10.0	1.0	4.0	3.0	3.0	0.0	1.0
Family Received SNAP (%)	16.0	22.0	4.0	9.0	20.0	29.0	3.0	10.0
Family Received Any EITC (%)	34.0	36.0	15.0	22.0	37.0	55.0	19.0	32.0
Family Received Any CTC (%)	0.0	0.0	0.0	0.0	37.0	42.0	57.0	55.0
# of Children	357	618	4,482	1,988	497	407	1,346	420

Note: Weighted using PSID longitudinal family weights. Dollar amounts are in $2019. Children born to single mothers represent 25% of all children born before and after 1996. Percent variables are rounded to the nearest percent. Headers in Red refer to the estimation sample used for the present analysis.

Table 2.2: Estimated Changes in Income Components After Union Dissolution

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mother's Education at Childbirth:	≤ HS Degree	> HS Degree	≤ HS Degree	> HS Degree	≤ HS Degree	> HS Degree	≤ HS Degree	> HS Degree
Outcome is Log of:	AFDC/TANF	AFDC/TANF	EITC	EITC	CTC	CTC	Earnings	Earnings
Panel A: Differences Over Time in Log Point Changes in Income Resources								
(- 3+ yrs) x Year>1996	-0.232	-0.0219	0.390	-0.315	-0.0220	0.177	0.0394	-0.116
	(0.152)	(0.0970)	(0.283)	(0.278)	(0.200)	(0.211)	(0.137)	(0.108)
(+ 0-1 yrs) x Year>1996	-0.524***	-0.156	0.330	-0.543	-1.025***	-0.741***	0.159	-0.260
	(0.172)	(0.154)	(0.316)	(0.376)	(0.214)	(0.247)	(0.204)	(0.204)
(+ 2-3 yrs) x Year>1996	-0.759***	-0.145	0.415	-0.345	-0.722***	-0.115	0.440*	0.0272
	(0.215)	(0.131)	(0.349)	(0.387)	(0.233)	(0.251)	(0.242)	(0.147)
(+ 4-5 yrs) x Year>1996	-0.638**	0.158	-0.0503	-0.687	-0.795***	0.0327	0.0128	-0.213
	(0.263)	(0.133)	(0.409)	(0.422)	(0.269)	(0.279)	(0.276)	(0.197)
Panel B: Log Point Changes in Income Resources Prior to Welfare Reform								
(- 3+ yrs)	-0.133	0.0257	0.179	0.0461			-0.0184	-0.00632
	(0.112)	(0.0781)	(0.135)	(0.167)			(0.0746)	(0.0762)
(+ 0-1 yrs)	0.902***	0.432***	1.204***	1.297***			-0.985***	-0.919***
	(0.142)	(0.125)	(0.194)	(0.281)			(0.141)	(0.138)
(+ 2-3 yrs)	1.281***	0.216	0.533**	0.604*			-1.415***	-0.835***
	(0.209)	(0.140)	(0.225)	(0.313)			(0.180)	(0.121)
(+ 4-5 yrs)	1.360***	-0.125	0.364	0.541			-1.299***	-0.565***
	(0.265)	(0.156)	(0.280)	(0.358)			(0.215)	(0.158)
# of Children	1,787	1,171	1,787	1,171	1,787	1,171	1,787	1,171
# of Child-Years	16,512	10,668	15,191	8,660	15,191	9,864	16,508	10,652

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Models presented are weighted least-squares estimates of Equation (1) using an unbalanced panel of children whose mother or father gets custody after union dissolution. Weighted using the child longitudinal weight in the last year the child was observed. Estimates in Column 1 refer to union dissolutions from 1970-1991. The (-2,-1) to (4,5) event time bins in Column 1 reflect resources measured from 1968 to 1996. Estimates in Column 2 refer to union dissolutions from 1999-2019. The (-2,-1) to (4,5) event time bins in Column 2 reflect resources measured from 1997 to 2019. Estimates reflect log points.

Table 2.3: Estimated Changes in Components of Food Expenditures After Union Dissolution

	(1)	(2)	(3)	(4)	(5)	(6)
Mother's Education at Childbirth:	≤ HS Degree	> HS Degree	≤ HS Degree	> HS Degree	≤ HS Degree	> HS Degree
Outcome is Log of:	Food at Home	Food at Home	Restaurant Meals	Restaurant Meals	SNAP	SNAP
Panel A: Differences Over Time in Log Point Changes in Food Expenditures						
(- 3+ yrs) x Year>1996	0.0110	0.0251	0.0160	-0.0175	0.291	-0.185
	(0.0448)	(0.0559)	(0.0664)	(0.0732)	(0.231)	(0.185)
(+ 0-1 yrs) x Year>1996	0.107**	0.0265	0.0715	0.135*	0.653**	0.181
	(0.0499)	(0.0653)	(0.0689)	(0.0733)	(0.257)	(0.218)
(+ 2-3 yrs) x Year>1996	0.133**	0.141***	-0.0494	0.147*	0.274	-0.0285
	(0.0540)	(0.0533)	(0.0768)	(0.0860)	(0.303)	(0.242)
(+ 4-5 yrs) x Year>1996	0.119**	0.0849	-0.0445	0.0336	0.175	0.214
	(0.0563)	(0.0623)	(0.0898)	(0.101)	(0.327)	(0.277)
Panel B: Log Point Changes in Food Expenditures Prior to Welfare Reform						
(- 3+ yrs)	-0.0209	-0.0309	-0.00237	-0.0551	0.0852	0.287**
	(0.0289)	(0.0271)	(0.0492)	(0.0539)	(0.145)	(0.120)
(+ 0-1 yrs)	-0.0631*	-0.0856**	-0.104**	-0.169***	0.574***	0.563***
	(0.0340)	(0.0367)	(0.0457)	(0.0505)	(0.158)	(0.149)
(+ 2-3 yrs)	-0.105**	-0.115***	-0.0345	-0.325***	0.913***	0.553***
	(0.0446)	(0.0408)	(0.0643)	(0.0775)	(0.223)	(0.203)
(+ 4-5 yrs)	-0.0790	-0.0611	-0.0196	-0.299***	0.812***	0.198
	(0.0518)	(0.0521)	(0.0860)	(0.0947)	(0.249)	(0.249)
# of Children	1,787	1,171	1,787	1,171	1,787	1,171
# of Child-Years	15,195	9,919	12,792	8,247	15,325	9,949

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Models presented are weighted least-squares estimates of Equation (1) using an unbalanced panel of children whose mother or father gets custody after union dissolution. Weighted using the child longitudinal weight in the last year the child was observed. Estimates in Column 1 refer to union dissolutions from 1970-1991. The (-2,-1) to (4,5) event time bins in Column 1 reflect resources measured from 1968 to 1996. Estimates in Column 2 refer to union dissolutions from 1999-2019. The (-2,-1) to (4,5) event time bins in Column 2 reflect resources measured from 1997 to 2019. Estimates reflect log points.

Appendix

Table 2.A1: Sample Construction

Starting Sample: Children followed from birth up to age 16 that were born between 1968-2019 into a two co-residing parent family from the Core Sample that experienced a union dissolution	*this includes children born in even years after 1997, who only enter sample at age 1 (parents are observed pre-birth) *only children from SEO and Census HH's (92% of all HH's)	4,161 Children
Starting Sample:		41,546 Child-Years
Sample Restriction: Only study children whose parents separated when the child was 2-10 years old	* Ensures that all children can contribute to each event time from -2 to +6	2,994 (lost 1,167 children)
With Sample Restriction: Total # of Child Years		28,620 (lost 12,926 child-years)

Children not included in Main Sample	Children whose data come from adoption records, born to single mothers, born to parent's that do not separate, born before 1968, first obs at age>1, Children born into Immigrant or Latino Samples, Children whose parents separated before age 2 or after age 10	
Final Analysis Sample	2,994 Children (28,620 child-years)	

Table 2.A2: # of Children Contributing to Estimation By Event Time

Mother's Education at Childbirth:	≤ HS Degree	> HS Degree	≤ HS Degree	> HS Degree
Separations from:	1970-1991	1970-1991	1999-2015	1999-2019
Event times (-2,-1)	701	365	796	605
Event times (0,1)	701	366	785	564
Event times (2,3)	515	283	714	489
Event times (4,5)	503	285	627	377

Note: Numbers in table refer to the # of distinct children in each event time. Sample of children that live with mom or dad after union dissolution.

Robustness Checks

Table 2.A3: Estimated Changes in Income Components After Union Dissolution (Balanced Panel)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mother's Education at Childbirth:	≤ HS Degree	> HS Degree	≤ HS Degree	> HS Degree	≤ HS Degree	> HS Degree	≤ HS Degree	> HS Degree
Outcome is Log of:	AFDC/TANF	AFDC/TANF	EITC	EITC	CTC	CTC	Earnings	Earnings
Panel A: Differences Over Time in Log Point Changes in Income Resources								
(- 3+ yrs) x Year>1996	-0.147	-0.0614	0.438	-0.270	-0.328	-0.0418	-0.0715	-0.0501
	(0.193)	(0.121)	(0.325)	(0.327)	(0.233)	(0.286)	(0.161)	(0.146)
(+ 0-1 yrs) x Year>1996	-0.600***	-0.147	0.348	-0.196	-1.052***	-0.718**	0.255	-0.334
	(0.205)	(0.196)	(0.355)	(0.442)	(0.245)	(0.308)	(0.229)	(0.264)
(+ 2-3 yrs) x Year>1996	-0.740***	-0.170	0.559	-0.211	-0.856***	0.0360	0.455*	0.127
	(0.240)	(0.146)	(0.389)	(0.421)	(0.256)	(0.303)	(0.265)	(0.168)
(+ 4-5 yrs) x Year>1996	-0.625**	0.178	0.0451	-0.569	-0.951***	0.165	0.0488	-0.152
	(0.289)	(0.152)	(0.443)	(0.458)	(0.287)	(0.322)	(0.295)	(0.221)
Panel B: Log Point Changes in Income Resources Prior to Welfare Reform								
(- 3+ yrs)	-0.164	0.0353	0.223	-0.0263			-0.0505	-0.0355
	(0.145)	(0.0936)	(0.164)	(0.189)			(0.0883)	(0.0861)
(+ 0-1 yrs)	1.045***	0.500***	1.269***	1.531***			-1.134***	-0.994***
	(0.166)	(0.144)	(0.219)	(0.292)			(0.160)	(0.158)
(+ 2-3 yrs)	1.390***	0.238	0.496**	0.771**			-1.553***	-0.862***
	(0.233)	(0.157)	(0.250)	(0.325)			(0.193)	(0.133)
(+ 4-5 yrs)	1.490***	-0.131	0.291	0.788**			-1.460***	-0.579***
	(0.298)	(0.181)	(0.312)	(0.381)			(0.236)	(0.181)
# of Children	1,211	749	1,211	749	1,211	749	1,211	749
# of Child-Years	12,922	8,178	11,948	6,762	11,948	7,570	12,925	8,162

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Models presented are weighted least-squares estimates of Equation (1) using a balanced panel of children who were observed in event times (-2,-1), (0,1), (2,3), and (4,5) and whose mother or father gets custody after union dissolution. Weighted using the child longitudinal weight in the last year the child was observed. The (-2,-1) to (4,5) event time bins in Column 1 reflect resources measured from 1968 to 1996. Estimates in Column 2 refer to union dissolutions from 1999-2015. The (-2,-1) to (4,5) event time bins in Column 2 reflect resources measured from 1997 to 2019. Estimates reflect log points.

Table 2.A4: Estimated Changes in Components of Food Expenditures After Union Dissolution (Balanced Panel)

	(1)	(2)	(3)	(4)	(5)	(6)
Mother's Education at Childbirth:	≤ HS Degree	> HS Degree	≤ HS Degree	> HS Degree	≤ HS Degree	> HS Degree
Outcome is Log of:	Food at Home	Food at Home	Restaurant Meals	Restaurant Meals	SNAP	SNAP
Panel A: Differences Over Time in Log Point Changes in Food Expenditures						
(- 3+ yrs) x Year>1996	-0.0119	0.0695	-0.0385	0.0700	0.248	-0.324
	(0.0540)	(0.0740)	(0.0780)	(0.0886)	(0.270)	(0.237)
(+ 0-1 yrs) x Year>1996	0.0997*	0.0261	0.0539	0.158*	0.863***	0.176
	(0.0602)	(0.0796)	(0.0806)	(0.0857)	(0.302)	(0.273)
(+ 2-3 yrs) x Year>1996	0.137**	0.169***	-0.0806	0.156	0.346	-0.00966
	(0.0589)	(0.0609)	(0.0855)	(0.0983)	(0.332)	(0.283)
(+ 4-5 yrs) x Year>1996	0.130**	0.0971	-0.0700	0.0698	0.157	0.0560
	(0.0606)	(0.0690)	(0.0977)	(0.114)	(0.351)	(0.319)
Panel B: Log Point Changes in Food Expenditures Prior to Welfare Reform						
(- 3+ yrs)	-0.0101	-0.0594*	0.0364	-0.0767	0.137	0.260*
	(0.0349)	(0.0326)	(0.0585)	(0.0602)	(0.175)	(0.145)
(+ 0-1 yrs)	-0.0518	-0.0891**	-0.143**	-0.246***	0.486**	0.591***
	(0.0421)	(0.0446)	(0.0559)	(0.0564)	(0.192)	(0.179)
(+ 2-3 yrs)	-0.108**	-0.119**	-0.0470	-0.403***	0.926***	0.640***
	(0.0505)	(0.0483)	(0.0742)	(0.0855)	(0.247)	(0.225)
(+ 4-5 yrs)	-0.0902	-0.0632	-0.0306	-0.425***	0.797***	0.321
	(0.0590)	(0.0614)	(0.0983)	(0.105)	(0.275)	(0.290)
# of Children	1,211	749	1,211	749	1,211	749
# of Child-Years	11,975	7,425	10,082	6,784	12,061	7,454

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Models presented are weighted least-squares estimates of Equation (1) using a balanced panel of children who were observed in event times (-2,-1), (0,1), (2,3), and (4,5) and whose mother or father gets custody after union dissolution. Weighted using the child longitudinal weight in the last year the child was observed. Estimates in Column 1 refer to union dissolutions from 1970-1991. The (-2,-1) to (4,5) event time bins in Column 1 reflect resources measured from 1968 to 1996. Estimates in Column 2 refer to union dissolutions from 1999-2015. The (-2,-1) to (4,5) event time bins in Column 2 reflect resources measured from 1997 to 2019. Estimates reflect log points.

Chapter 3: The Effect of Health Insurance Coverage Mandates on the Education of Young Adults: Evidence from the ACA

3.1 Introduction

There is minimal causal evidence on the effect of health insurance coverage on educational attainment among young adults in the United States. State dependent coverage laws and the 2010 Affordable Care Act (ACA) dependent coverage expansion provide a unique opportunity to study the impacts of a health insurance intervention specific to young adults, the age group with the highest uninsured rate. Figure 1 shows that in 2009, the insured rate was 70% among individuals ages 19–25, compared to 90% for those 18 and under, 80% for those 26-64, and over 99% for those above 65.[81] By 2013, the insured rate averaged 76% among individuals ages 19–25, implying a 6 percentage point increase in insurance coverage (Figure 1).

Establishing whether health insurance is a determinant of education is important because the amount of schooling attained has broad implications ranging from health to earnings (Krueger et al., 1991 ; Carneiro et al. , 2011 ; Clark & Royer, 2013 ; Brunello, Giorgio, et al., 2016 ; Buckles et al., 2016). Jung et al. (2013) show that the availability of dependent coverage increases the likelihood of being a student by twenty percentage points. However, this study does not use exogenous variation in insurance access to estimate effects, so these findings are only correlational. Health insurance coverage is often tied to full time universities at four year colleges and universities.[82] In a 2008 report, the Government Accountability Office (GAO) estimated that about 30 percent of four-year colleges nationwide required full-time students to have health insurance in academic

[81] Following the passage of the 2010 ACA, approximately 5.5 million young adults gained parental insurance coverage (Ellison et al. (2020))

[82] Community colleges only have these requirements for international students.

years 2007-2008.[83] By 2014, a report by the American College Health Association found that 48.7% of four-year colleges require health insurance for full-time students.[84]

Given that health insurance tends to be requirement for full time students at four year colleges or universities, and dependent coverage is more affordable that university sponsored health plans, it is unsurprising that there is a positive relationship between dependent coverage and full-time college enrollment.[85] However, this relationship may not be causal. In fact, a recent paper by Heim et al. (2018) found that increases in insurance access caused by the 2010 Affordable Care Act had negligible impacts on post-secondary educational attainment among young adults. This paper used an age-time Differences-in-Differences model to examine the effect of the 2010 YACM on overall post-secondary school enrollment, full-time enrollment, and graduate school enrollment, using a treated group of young adults ages 24-25, and a control group of ages 27-29.

An important caveat to the findings in Heim et al. (2018) is that pre-2010 heterogeneity in insurance access and educational incentives among ACA eligible young adults is not addressed. By not incorporating this pre-2010 heterogeneity, this paper does not consider how the 2010 YACM changed schooling incentives for young adults that were age eligible for coverage via state mandates with explicit requirements for school enrollment. By eliminating the school enrollment requirement for this subset of young adults, the 2010 YACM reduces educational incentives for 19-25 year olds that had state-mandated-access-to-group-insurance, but does not change incentives for 19-25 year old young adults that did not.[86] By pooling all 2010 YACM age-eligible (24-25) young adults together into a single treatment group, Heim et al. (2018) potentially miss this important heterogeneity in effects.

[83] https://www.gao.gov/products/gao-08-389

[84] https://www.acha.org/documents/resources/SHIP_Adapting_to_the_Environment_Post_ACA.pdf

[85] The same 2008 Government Accountability Office (GAO) report found that 67% of students were covered through employer-sponsored plans, 7% were covered through other private health insurance plans (such as student health insurance plans), 6% were covered by public programs, and 20% were uninsured.

[86] Here and throughout this paper, state-mandated-access-to-group-insurance specifically means age eligible for dependent coverage via state mandates that explicitly required school enrollment as a condition of eligibility.

State policymakers were the first to mandate health insurers to expand the ages through which children could remain on their parents' group health coverage (Figure 2). While research shows that these state-based expansions were successful in increasing the number of adult children on their parents' insurance plans, they also placed restrictions on coverage that came with direct implications for educational decisions of young adults (Levine, McKnight, and Heep (2011); Monheit et al. (2011); Cantor, Belloff, et al. (2012)). First, 11 states with mandates had an educational enrollment requirement that often required full time enrollment, thereby encouraging post-secondary educational enrollment at the full-time level. Second, 9 of these 11 states provided coverage through at least age 24, encouraging enrollment in 4-year degree programs.[87]. Thus, state mandates created clear incentives for post-secondary school enrollment, particularly at the full-time level.

The Young Adult Coverage Mandate (YACM) provision of the Patient Protection and Affordable Care Act (ACA) was implemented in 2010. This provision expanded insurance coverage for dependent young adults by requiring all health plans to provide coverage eligibility to policyholder children under age 26 without educational or marital status based restrictions. This law not only led to large increases in dependent coverage among individuals ages 19-25, but it also changed incentives for educational attainment among those eligible under state mandates (Figure 3).[88] While there has been research examining the impact of this law on a wide range of second order effects like labor supply, health, and health care utilization, there has been limited work studying educational attainment.

Furthermore, there is no work to date addressing whether the 2010 YACM interacted with prior state dependent coverage mandates in ways that impacted education decisions. Prior to 2010, there is also substantial cross state variation in employer sponsored health insurance (ESHI) coverage among parents. As few as 71% of parents had (ESHI) in New Mexico, and as many as

[87]2014-2015 Data from the National Student Clearinghouse Research Center indicates that Bachelor's degree earners take 5.1 academic years, on average to complete their degree.

[88]Cantor, Monheit, et al. 2012; Akosa Antwi, Moriya, and Simon 2013; Sommers, Decker, et al. 2013

91% of parents had (ESHI) in Massachusetts .[89] Dependent coverage under an ESHI plan serves as a tuition subsidy because this type of coverage can be subsisted for college provided insurance plans. These plans are roughly 10% of tuition costs and typically required at four year colleges and universities. Young adults may be more likely to go to college if their parents' employer sponsored health insurance plans covers them beyond age 18, regardless of whether their state policy requires them to be students to be eligible or not. It is also possible that prior to the YACM, some young adults that were eligible for dependent coverage based on their age, via state mandates with educational enrollment requirements, only attended college because they had to in order to remain dependent coverage eligible.

Upon the passage of the 2010 YACM, all 19-25 became eligible for dependent coverage regardless of student status. These separate policies at the state and federal levels thereby interact and create incentives for educational enrollment in opposite directions for young adults that already had state-mandated-access-to-group-insurance. If some young adults were only enrolling in school for dependent coverage, by eliminating the educational enrollment requirement, the federal policy could lead to reductions in educational enrollment, specifically full-time enrollment. In addition, by eliminating the requirement that young adults remain unmarried as a condition for eligibility, the 2010 YACM did increase marriage rates among young adults eligible under state mandates.[90] This could lead to lower school enrollment, as more young adults get married and become eligible for spousal insurance coverage. Alternatively, if young adults' enrollment decisions are not driven by insurance access, the state mandates and the YACM should not impact their schooling decisions.

Leveraging age, time, and state variation in dependent coverage eligibility via state mandates and the 2010 YACM, this paper uses a Differences-in-Differences-in-Differences (DDD) model to study the effect of the 2010 YACM on the educational attainment of young adults previously coverage eligible via state mandates that had school enrollment requirements for eligibility. My data sources come from the October Supplement of the Current Population Survey (CPS) from 2005-

[89] Author calculations using 2005-2009 ASEC.
[90] Barkowski & McLaughlin (2020)

2015, as well as a data set compiled by Barkowski & McLaughlin (2020) on state-level dependent-coverage mandates before and after the 2010 ACA. I estimate educational enrollment effects for young adults using within state-by-age variation in group coverage eligibility from a subset of state mandates that required school enrollment as a condition of eligibility.

Prior to 2010, I find no differences in school enrollment outcomes for young adults with and without state-mandated-access-to-group-insurance. However, the passage of the YACM reduced post-secondary education enrollment among young adults with state-mandated-access-to-group-insurance. Specifically, following the implementation of the 2010 YACM, enrollment persistently fell 4.1-6.3 percentage points among those with state-mandated-access-to-group-insurance. These declines in enrollment were completely driven by reductions in full-time enrollment and enrollment at four-year universities. Furthermore, the overall enrollment declines were driven by reduced college entrance. The elimination of school enrollment requirements for dependent coverage eligibility as part of the 2010 YACM could have either had no effect or a negative effect on enrollment rates of state-mandated-access-to-group-insurance young adults. The large and persistent negative effect found in this paper shows that young adults' education decisions are very responsive to insurance coverage.

This paper makes several contributions to the existing literature on the effects of health insurance access for young adults. First, it shows how the 2010 YACM interacted with prior state dependent coverage mandates to impact educational enrollment of young adults. These results add to work by Heim et al. (2018), who found that the ACA mandate had negligible impacts on post-secondary educational attainment among young adults. I find that there is important heterogeneity to the effect of the ACA mandate that depends on state-mandated-access-to-group-insurance. Second, I examine how YACM-mandated-access-to-group-insurance interacted with state-mandated-access-to-group-insurance to impact flows into and out of post-secondary schooling, as well as the decision to enroll in four-year versus two-year programs. Examining effects by type of institution is important because community colleges don't have student health insurance

requirements for domestic students, while a non-trivial share of four-year colleges and universities do. By showing how effects differ by type of educational institution, this paper helps explain the mechanism underlying the causal link between health insurance coverage and educational attainment.

Last, this paper provides an improved identification strategy to the literature that evaluates the YACM, by using within state by age variation in eligibility for state insurance mandate laws in order to estimate the effects of the 2010 YACM provision. Prior papers studying the second order effects of the YACM have used an age-time Differences-in-Differences approach, comparing outcomes before and after 2010 for an age eligible treatment group (typically 19-25, 22-25, or 24-25), and an age ineligible group (typically 27-29) (Antwi et al. (2013); Abramowitz (2015); Barbaresco et al. (2015); Heim et al. (2018)). This approach has the drawback of comparing people in disparate stages of life. In contrast, I use variation in coverage eligibility and educational incentives among 19-25 year olds. This variation is based on age, state, and year, and is generated by state mandate and ACA mandate rules. This allows me to contrast educational attainment trends among young adult that are more similar in age, holding constant their state of residence, thereby utilizing a more similar control group.

The rest of the paper is laid out as follows: Section 2 provides background information regarding the state dependent coverage laws and the federal 2010 YACM. Section 3 describes the CPS data and the data on state-level dependent coverage mandates. Section 4 outlines my estimation sample and outcomes of interest. Section 5 describes the DDD model used this paper and presents descriptive statistics. Section 6 presents my results, and Section 7 details this paper's conclusions.

3.2 Background

Prior to the enactment of the ACA, 33 states already had a dependent coverage law which mandated insurers to raise "limiting ages", or the ages at which young adults age out of eligibility for dependent coverage under parents' plans. This approach began in Louisiana in 1974, which

raised its limiting age to 24, and was followed over time by many other states. This extended eligibility at the state-level often came with restrictions on marriage and/or requirements that recipients be enrolled in school. 28 of these 33 states had restrictions on eligibility based on marital status or educational enrollment. Of the 28 laws, 11 required school enrollment. Among the states that had an enrollment requirement, 8 also required recipients of dependent coverage to be unmarried and the other 3 had no such restriction. Additionally, state mandates varied in their limiting ages across states and time, from as low as 21 in Idaho and Illinois, to 31 in New Jersey, and even no age limit in Iowa and Rhode Island for students. Research has consistently shown that these mandates increased dependent coverage among young adults, with mixed results regarding overall coverage. Prior work has also shown that these mandates reduced labor supply, reduced marriage, and raised wages and educational attainment for those over 25 (Hahn and Young, 2014 ; Dillender, 2014 ; Depew, 2015 ; Barkowski & McLaughlin, 2020). The empirical strategy used in this paper is most similar to Depew (2015), who used an age-state-time triple difference to estimate effects of state-mandated-access-to-group-insurance on employment. Using this same approach, and an updated data set on state mandate policies, I will estimate effects on education before and after the YACM.

One of the first provisions enacted under the Patient Protection and Affordable Care Act (PPACA) was the Young Adult Coverage Mandate (YACM), which took effect on September 23, 2010. Under this provision, individuals up to age 26, can remain on their parents' employer provided health insurance plan. This federal policy effectively superseded more restrictive state limiting ages and eliminated educational incentives and marriage disincentives embedded in the state policies. Furthermore, the YACM applied to all insurance plans in the country, whereas state mandates did not apply to employer-provided policyholders that had self-funded plans.[91] Using an age-time Differences-in-Differences approach, Sommers, Buchmueller, et al. (2013), show that by 2012 the YACM had decreased uninsured rates of young adults by up to 21 percent. The 2010 law also had far-reaching second order effects which are summarized by French et al. (2016).

[91]The 2010 YACM was one of the few provisions of the ACA that applied to grandfathered plans.

Of significance for this analysis, Barkowski & McLaughlin (2020) show that the YACM eliminated the difference in marriage rates between 19-25 year old young adults with and without state-mandated-access-to-group-insurance. A key distinction between their study and my analysis is how state-mandated-access-to-group-insurance is defined. In contrast to this paper, these authors treat young adults that are age eligible for dependent coverage under *any* state mandate as those with state-mandated-access-to-group-insurance. In my paper, I limit my definition of those with state-mandated-access-to-group-insurance to young adults that are age eligible for dependent coverage under state mandates that require educational enrollment as condition of eligibility. The decision to have a broader definition of state-mandated-access-to-group-insurance is suitable for the analysis done by Barkowski & McLaughlin (2020) because they study marriage, and 28 of the 33 states have a marriage restriction as part of the eligibility requirements.

I use a more restricted definition of state-mandated-access-to-group-insurance because only 11 of the 33 states with a mandate explicitly require school enrollment. In Appendix Table A4, I show that using the broader definition of state-mandated-access-to-group-insurance used by Barkowski & McLaughlin (2020), I still find statistically significant reductions in any post-secondary school enrollment and reductions in 4-year program enrollment, but these effects are smaller than the estimates using the narrower definition of state-mandated-access-to-group-insurance used in this paper. This indicates that using the broader definition of insurance access mutes the effects of state-mandated-access-to-group-insurance via laws requiring school enrollment, as expected.

In their paper Barkowski & McLaughlin (2020) show that a marriage gap existed prior to 2010 because many state dependent coverage mandates restricted marriage as a condition of eligibility. The 2010 YACM eliminated this marriage restriction. According to these authors, prior to the 2010 YACM, young adults with state-mandated-access-to-group-insurance were 2 percentage points less likely to be married than young adults without access. They also show that this marriage gap was eliminated by the 2010 YACM. Their work provides the first evidence that there is important heterogeneity in the effect of the 2010 YACM on young adult's decisions, based on prior state-

mandated-access-to-group-insurance.

While Barkowski & McLaughlin (2020) studied the way eligibility for the 2010 YACM interacts with state-mandated-access-to-group-insurance to affect marriage among young adults, I study educational enrollment effects. In my analysis, I focus exclusively on the educational enrollment effect of the 2010 YACM among young adults with state-mandated-access-to-group-insurance (using the narrower definition discussed above). Since 8 of the 11 state mandates with student requirements for dependent coverage also had a marriage restriction, I control for these marriage restrictions, given the prior evidence that they drive marriage behavior.[92] My results therefore isolate the interaction effect of the state mandate rules requiring enrollment with the 2010 YACM rules that eliminated this requirement on education.

3.3 Data

This paper uses data from the 2005-2015 October Supplement of the Current Population Survey (CPS) as well as as a data set compiled by Barkowski & McLaughlin (2020) on state-level dependent-coverage mandates.

3.3.1 The CPS October Supplement

The CPS is a nationally representative probability sample of households. The survey is conducted in 50,000–60,000 dwelling units monthly. Dwelling units are in-sample for 4 successive months, out-of-sample for the next 8 months, and then return to the sample for the following 4 months. An adult member of each household provides information for all members of the household. The October Supplement provides information on the school enrollment status and educational attainment of household members 3 years old and over. Importantly for this analysis, the October Supplement has data on type of schooling organization offering instruction for each

[92]Since the interaction of these state rules and the YACM impacted marriage, they may have indirectly impacted education. Specifically, increases in marriage after 2010 could lead to lower school enrollment, as more young adults get married and become eligible for spousal insurance coverage.

member of the household and level of enrollment for each member of the household. One limitation of using the CPS is that I only have data on the young adult's state of residence and not their parent's state, which is the applicable state for the state-mandate policy. This would attenuate my estimated effects of state mandates on enrollment if young adults that enroll in college as a result of their state mandate, enroll out of state.

The young adults' state of residence is the same as their parent's if they are living with their parents, living in their home state without their parents, or live in on-campus student housing.[93]. Using data from the Current Population Survey, the Pew Research Center reports that 44% of all 18-29 year olds lived with any parent in 2010. By using data on the mother's and father's location in the child's household, I find that 66% of 19-25 year olds that are enrolled in post-secondary education live with a parent. By comparison, 43% of 19-25 year olds that are not enrolled in post-secondary education live with a parent. The comparable figures for 19-28 year olds are 61 and 36%, respectively. These are the minimum shares of young adults whose coded state of residence in the CPS reflects the state whose mandate rules apply to the focal young adult. Some young adults could be living in the same state as their parents but not in the same household, while others could live in on-campus student housing, but their coded state of residence still reflects their parent's state of residence. In particular, according to data from the the National Post-secondary Student Aid Study (NPSAS), 16% of college undergraduates lived on campus from 2010-2016. As a robustness check, I show in Table A5 that if I proxy for current state of residence with state of residence 1 year ago, the results are nearly identical to my main results.[94]

[93] According to the Census Bureau

[94] These results are obtained using data from the 2005-2015 ASEC. The state of residence one year prior to the survey is a proxy for where the young adults' parents currently reside, and therefore the relevant state when determining the limiting age and enrollment requirements. The state of residence one year ago is most likely to reflect the parent's state of residence if the young adult is a college freshman, but is less likely to reflect where the young adults' parents currently reside if the young adult is in a later year of college or graduate school. Despite this limitation, using the variation in group coverage eligibility based on the state of residence one year ago, is still a useful way to show that the education effects are not the result of measurement error. In addition, as will be discussed in the results section, I show that enrollment effects are driven by reduced college entrance (being a college freshman). Since the results in Table A3 are very similar to the main results, we should not be concerned that the main results which are derived using the current state to proxy for the state whose rules apply to focal child, are driven by measurement error in the

3.3.2 State-Level Dependent-Coverage Mandates Data Set

Following a careful review of the histories of the state statutes, Barkowski & McLaughlin (2020) compiled a new data set that records the state mandates back to their original enactment dates for all states starting in 2000 and ending in 2015. This data set reconciled discrepancies in prior work that performed analyses using state mandate variation. I use this data to determine which states had dependent coverage laws, these law's limiting ages, their requirements for eligibility, and in which years they were in effect. I use this information to code eligibility. In this study, for a given year, a young adult is coded as eligible if they are at or below the state's limiting age and the state dependent coverage law has a student requirement.

3.4 Estimation Samples

For my analyses I have two age samples and rely on age-state-year variation in mandated-access-to-group-insurance coming from states that adopted and got rid of their policies at various points from 2005-2009.[95] The main DDD results in this paper rely on within state-by-age variation in mandated-access-to-group-insurance that comes from the enactment of a policy that affected all states in the same year; the 2010 YACM. This eliminates concerns about biased treatment effects when using a staggered treatment design (Baker et al., 2022). My main age sample is similar to the sample used by Barkowski & McLaughlin (2020), in that I restrict my main sample to respondents ages 19 to 25, given this is the group targeted by the 2010 YACM.[96] This is in contrast to other work that has adopted the framework used by Akosa Antwi, Moriya, and Simon (2013), and Sommers, Buchmueller, et al. (2013), comparing young adults up to age 25 and covered by the ACA mandate to those 26 or older and not covered. I supplement my main analysis, using a secondary age sample of young adults ages 19-25, and 27-28, to utilize additional within-state-be age variation

treatment variable.

[95]States that got rid of their mandate policies prior to 2010, never re-introduced these policies, and states that adopted their policies later in the pre-period, kept their policies throughout the pre-period, until these policies were superseded by the 2010 YACM.

[96]I exclude young adults that are reported as having a disability, since young adults with a disability are always eligible for dependent coverage. 0.33% of young adults are reported to be disabled.

from states with limiting ages of 25.

Restricting the first sample to those ages 19-25 was guided by the fact that there is within and across state by age variation in eligibility for dependent coverage via state mandates among this age group. Furthermore, the YACM limiting age of 25 applied uniformly across the country, therefore generating natural "treated" and "control" groups of young adults based on their age, state of residence, and year of interview.[97] The results from this sample indicate how the easing of educational enrollment requirements for young adults with prior dependent coverage access impacted their enrollment decisions, net of changes in educational enrollment for similarly aged young adults that did not have access to dependent coverage prior to 2010. This is my preferred sample because the eligible and ineligible groups in this sample are similar in age (they are all ages 19-25), making them more comparable to each other than younger "treated" (typically 19-25) and older "control" (typically 27-29) groups used in other studies studying effects of the YACM.[98]

The drawback of this age sample is that this sample limits the within state by age variation in mandate eligibility that can be leveraged. This is because 2 out of the 11 states with education requirements had a limiting age of 25 in most or all years before the YACM was implemented.[99] To address this disadvantage, I use a second sample of young adults ages 19-25, and 27-28.[100]

[97] Only one state (SD) had a limiting age above 25 in all years from 2005-2010. The age limit for dependent coverage for students in SD is 29. In a second state, IA, there was no age limit for dependent coverage for students from 2007-2010. These respective age limits stayed the same from implementation through 2015. A third state, RI, had no age limit for students from 2005-2006, but this was lowered to 24 in 2007.

[98] 27-29 year olds are not the ideal control group for 19-25 year olds in this context because they are in very different stages of life. The older control group is more likely to be married and working, providing them with other types of insurance, relative to the younger treatment group. This could mean that their decisions around education in response to dependent coverage are quite different from the treatment group.

[99] One example is North Dakota. In ND the age limit for dependent coverage has always been 25. Thus, all young adults in ND are eligible for dependent coverage pre-2010 and post 2010; yielding no within state by age variation in eligibility for young adults in ND. Another example is Georgia. In GA the limiting age was 24 in 2005, but starting in 2006, the limiting age was 25.

[100] Age 26 is excluded because age is reported in October, indicating some individuals could be in the post ACA treatment group (under 26) in some months of the year and in the control group (26 and over) in the other months.

[101] This sample allows me to use more within state by age variation to estimate my effects.[102] One disadvantage of this sample is that the treatment effect is less easily interpreted. The DDD treatment effect using this sample measures changes in (1) enrollment for mandate eligible young adults, some of whom lose educational incentives because they are eligible for the 2010 YACM (19-25), and others (27-28) who don't because they are ineligible for the 2010 law from 2010-2015, *subtracting* (2) changes in enrollment for YACM eligible (19-25) and ineligible (27-28) young adults, both of which did not have dependent coverage prior to 2010. Another disadvantage with this sample, that is not unique to this paper, is that this sample has less similar age profiles between those with and without

My main outcomes of interest, denoted as Y, are indicator variables for whether an individual was (1) enrolled in any post-secondary degree granting educational institution, (2) enrolled full time, (3) enrolled in a 4-year degree program, and (4) enrolled in a 2-year degree program at the time of the interview. These outcomes reflect extensive and intensive margin responses to insurance provision that could reasonably be affected by eliminating the education based restrictions of state dependent coverage laws.

In supplementary analyses, I also evaluate flows into and out of post-secondary education using data on current and last year's enrollment among respondents by grade level. This is done to identify whether the overall enrollment effects are driven by reduced college or graduate school entrance, or by young adults discontinuing their college studies.

Specifically, I use variables exclusive to the October Supplement that indicate whether the respondent was enrolled in school last year, and in which grade level, to examine (1) Pr(respondent

[101] Generally a DDD model includes the main effect which is the three-way interaction of age, state, and year of mandate, as well as a full set of double interactions. For the 27-28 year olds in the secondary sample, there is no within state-by-age variation in group coverage eligibility after the YACM because this group is not eligible for the YACM.

[102] Using the ND example: there is no change in coverage for the 27-28 year old ineligible group, and therefore no change in schooling incentives. For the 19-25 year olds eligible group, coverage may go up because the ACA applies to more insurance plans, and educational incentives change.

is enrolled in the 1st year of college | not enrolled or enrolled in HS last year), (2) Pr(respondent is enrolled in the 2nd year of college | enrolled in 1st year of college last year), (3) Pr(respondent is enrolled in the 3rd year of college | enrolled in 2nd year of college last year), and (4) Pr(respondent is enrolled in the 1st year of graduate school | not enrolled or enrolled in grade < 1st year graduate school last year).

3.5 Empirical Strategy

To estimate how the 2010 YACM interacted with state dependent coverage laws requiring student status, I estimate the following Difference-in-Differences-in-Differences (DDD) model:

$$\mathbf{Y}_{iast} = \beta_1 Elig_{ast} \cdot YACM_t + \beta_2 Elig_{ast} + \omega \mathbf{X}_{iast} + \phi \mathbf{Z}_{ast} + \alpha_a + \delta_{st} + \sigma_{ta} + \gamma_{sa} + \varepsilon_{iast} \tag{3}$$

Y_{iast} indicates the educational outcome of respondent i of age a living in state s in year t= 2005,..., 2015, while $Elig_{ast}$ is a binary variable identifying individuals who are age eligible for dependent coverage via a state mandate that require school enrollment as a condition for coverage. In particular, $Elig_{ast}$ is an two interaction based on the three sources of variation; respondents' age, state of residence, and year of interview.[103] For a given year, a young adult is coded as eligible if he or she is living in a state with a mandate requiring school enrollment in effect and is younger than the mandate limiting age. This in contrast to the approach taken by Barkowski & McLaughlin (2020) who define a young adult as eligible so long as they are living in a state with *any* mandate and they are younger than the mandate limiting age. The decision to restrict the eligible group in my analysis to this narrower group was guided by the fact that of the 33 states that had a mandate, only 11 of these states had a mandate that explicitly required school enrollment. Treating young adults living in *any* mandate states as the eligible group would thereby mute the effect of the 2010 YACM eliminating the school enrollment requirement.

[103]Explicitly $Elig_{ast}$ interacts an indicator variable equal to 1 if the young adult is age eligible for any state policy (which varies at the state age and year level) with an indicator variable equal to 1 if a state mandate requiring school enrollment is in effect (varies at the state and year level)

As an example, Louisiana's mandated limiting age was 23 for students. Using the notation in Equation (1), 19-23 year-olds living in Louisiana would have $Elig_{ast}$ equal to 1 in all years from 2005-2010, $Elig_{ast}xYACM_t$ equal to 0 from 2005-2009, and $Elig_{ast}xYACM_t$ equal to 1 from 2010-2015. In the primary sample, 24 and 25 year-olds living in Louisiana would have $Elig_{ast}$ equal to 0 in all years from 2005-2010, $Elig_{ast}xYACM_t$ equal to 0 from 2005-2009, and $Elig_{ast}xYACM_t$ equal to 0 from 2010-2015. In the secondary sample, 24, 25, and 27-28 year-olds living in Louisiana would have $Elig_{ast}$ equal to 0 in all years from 2005-2010, $Elig_{ast}xYACM_t$ equal to 0 from 2005-2009, and $Elig_{ast}xYACM_t$ equal to 0 from 2010-2015.

Individuals in states without mandates requiring school enrollment are coded as ineligible regardless of age.[104] Age, state-by-year, state-by-age and year-by-age fixed effects are denoted by α_a, δ_{st}, σ_{ta}, and γ_{sa} respectively, and ε_{iast} is an term. The state-by-year interaction fixed effect flexibly controls for state specific time trends, which is important given that the Great Recession coincided with the implementation of the YACM. It also controls for any early ACA policies that are not age dependent. The year-by-age interaction FE flexibly controls for year specific age differences in education. Finally, the state-by-age interaction FE flexible controls for state specific age differences in education.

Additional individual-level demographic controls are contained in the vector $\mathbf{X}_{iast}$. These controls include dummies for race, Hispanic ethnicity, citizenship status, gender, and the full interaction between age and gender.[105] The vector $\mathbf{Z}_{ast}$ includes two interactions to capture the effects of the marriage restrictions embedded in the state dependent coverage polices. The first a two-way interaction between an indicator equal to one if the young adult is living in a state with a depen-

[104]This includes age-eligible young adults living in states with mandates that don't require school enrollment. For example, prior to 2010, 19-24 year olds were age eligible for dependent coverage in Colorado. However, these young adults didn't have to be enrolled in school, instead they had to be unmarried. Since the focus of this paper is whether schooling incentives embedded in insurance laws impact education, I don't consider 19-24 year olds in CO "treated", since their education incentives were not explicitly impacted by the 2010 YACM. The effects of state laws with marriage restrictions are also controlled for in the regression. This is explained further when the $\mathbf{Z}_{ast}$ term is described.

[105]Legal permanent residents are eligible for employer sponsored health insurance as are their dependents.

dent coverage mandate that had a marital status restriction and no education requirement, and an indicator equal to 1 if the young adult is age eligible for dependent coverage under the state mandate. The second is a three-way interaction between an indicator equal to one if the young adult is living in a state with a dependent coverage mandate that had a marital status restriction and no education requirement, an indicator equal to 1 if the young adult is age eligible for dependent coverage under the state mandate, and an indicator equal to 1 if the year is ≥ 2010.[106] In addition, I do not control for marital status since this has been shown to be affected by eligibility criteria in state mandates and is likely to be jointly determined with educational attainment.[107]

The main interaction effect, β_1 captures the remaining variation in educational attainment, which is within state by age, before and after 2010. Using the primary sample, the DDD effect is captured using within state by age variation, before and after 2010, from 9 states. These states had limiting ages under 25, allowing for within state by age variation in coverage eligibility, prior to 2010, when using the 19-25 year old age sample. Using the secondary sample, the DDD effect is captured using within state by age variation from 10 states.[108] β_2 captures the effect of state mandates that required school enrollment on education for eligible ages prior to 2010.

Another important consideration for this analysis is that it relies on variation from states that do not always have their dependent coverage mandates. In particular, not all eleven states had their mandates in all years from 2005-2010. This has implications for the effects being estimated. Six of

[106]Barkowski & McLaughlin (2020) show that the interaction of these state rules regarding marital status and the YACM impacted marriage. By impacting marriage, these mandate rules may have indirectly impacted education. Specifically, increases in marriage after 2010 could lead to lower school enrollment, as more young adults get married and become eligible for spousal insurance coverage. To isolate the effect of mandates with education-based requirements, I control for the state mandates regarding marriage.

[107]In separate analyses, I re-estimated Equation (1), controlling for marital status and the results were unchanged. I also do not control for the unemployment rate because research has shown that YACM eligible young adults reduced their employment. In a separate analyses, I re-estimated Equation (1) for all the outcomes, controlling for the age-state-year unemployment for all ages (19-28) and the average state year unemployment rates for 26-30 year olds, and the results were unchanged.

[108]The 11th state, Iowa, adopted its policy in 2007 (effective 2008),and covered students indefinitely. Therefore, everyone in the secondary sample residing in Iowa is coded as having state-mandated-access-to-group-insurance from 2008-2010. Prior to 2008 everyone residing in IA, in the primary and secondary sample, is coded as ineligible for state-mandated-access-to-group-insurance.

the eleven states with student mandates changed their mandates between 2005 and 2010. CT, MN, and WV got rid of the school enrollment requirement in their state mandates in 2007. Therefore, there is only within state by age variation in coverage eligibility among young adults in these states from (2005-2006). ID, IA, KY implemented their dependent coverage laws from 2008-2009, and these laws required school enrollment as a condition for eligibility. There is only within state by age variation in coverage eligibility from these states from 2008-2010.

Assuming the young adults living in states that adopted their policies right before the YACM was enacted would have smaller state-mandate-ACA-interaction (β_1) treatment effects, would imply that that using variation from states that adopted policies later in the pre-period would attenuate the measured effects of the YACM on educational enrollment. In a robustness check, I estimate my main model only using the five states that had their policies from in all years from 2005-2015, and show that the effects are in fact larger.

In a supplementary analysis, I also present estimates from a model with additional parameters that are similar to the ones estimated by Heim et al. (2018), except I separate the ACA effect into state eligible and ineligible versions.[109] The sample is young adults ages 24-29 to be directly comparable to the sample used by Heim et al. (2018). The model used to estimate these effects is of the form:

$$\begin{aligned}(4) \qquad \mathbf{Y}_{iast} = \beta_1 Elig_{ast} \cdot YACM_t + \beta_2 Elig_{ast} + \beta_3 Elig_{ast} \cdot YACM_t xOV26_a + \beta_4 YACM_t \cdot OV26_a \\ + \beta_5 Elig_{ast} \cdot OV26_a + \omega \mathbf{X}_{iast} + \phi \mathbf{Z}_{ast} + \alpha_a + \delta_{st} + \sigma_{ta} + \gamma_{sa} + \varepsilon_{iast}\end{aligned}$$

β_1 and β_2 have the interpretations they had in Equation (1), but the expansion introduces two

[109] In their paper, Heim et al. (2018) use a Differences-in-Differences model to study the effect of the 2010 YACM on post-secondary school enrollment, graduate school enrollment, and full-time enrollment using data from tax returns. Their treatment group is 24-25 year olds, and their control group is 27-29. Their DD effect pools the effect for state-mandated-access-to-group-insurance and ineligible young adults.

additional terms of interest: β_4, the DD parameter for the ACA's enrollment effects for the state ineligible, and β_3-β_4, the same but for the eligible. In both cases, the treatment group is those under age 26 while the control group is those over 26. These additional parameters are similar to ones estimated by Heim et al. (2018), except I separate the YACM effect into state eligible and ineligible versions. The effect of the YACM on the state-mandate ineligible (β_4) is likely positive, given evidence that YACM increased insurance coverage and reduced work hours. This could mean that previously coverage ineligible young adults have more time to devote to school, and by having dependent coverage can avoiding having to pay for their college's insurance plan; making college more affordable. [110]

On the other hand, because the 2010 YACM eliminates the enrollment requirement embedded in state mandates, we should expect that the effect of the YACM on those with state-mandated-access-to-group-insurance (β_3-β_4) is negative. The weighted average of (β_4) and (β_3-β_4), where the weights are these group's respective share of the population, should come close to or equal the DD effect reported by Heim et al. (2018). The results from Table S3 confirm the findings in Heim et al. (2018) ; namely, that the overall DD effect of the YACM on education is negligible.[111]

3.5.1 Sample Characteristics

The main (19-25) sample used to estimate equation (1) consists of 126,924 observations of young adults, and the secondary sample (19-25, 27-28), consists of 164,215 observations of young adults. In the primary sample, 11% of these young adults were interviewed in states with dependent coverage mandate that required school enrollment and were at or below the limiting age for the policy, while the corresponding figure is 9% in the secondary sample. All sample statistics and regression models are estimated using the education supplement weight, and all standard errors account for state-level clustering (Bertrand, Duflo, and Mullainathan 2004).[112]

[110] College insurance plans are typically 10% of tuition costs.
[111] These results will be discussed further in Section 6.
[112] The education supplement weight is a weight specific to the Education Supplement, and should be used for all analyses using variables from this supplement.

Table 1 presents descriptive statistics before and after the 2010 YACM by mandate eligibility status, for the primary sample (19-25) and the secondary sample (19-25, 27-28). These summary statistics show that there are cross-sectional differences in demographics among young adults with and without state-mandated-access-to-group-insurance, highlighting the importance of controlling for these factors. Eligible young adults are approximately 0.5 years younger than ineligible young adults in main sample, and 1.5 years younger in the secondary sample. They are also less likely to be Hispanic, more likely to be Black, and more likely to be citizens.

Another important difference between the groups is that prior to 2010, the young adults with state-mandated-access-to-group-insurance were more likely to reside in state that prohibited marriage as a condition for dependent coverage eligibility. While 25% of young adults without state-mandated-access-to-group-insurance were subject to the marriage restriction pre-YACM, nearly 50% of those with state-mandated-access-to-group-insurance were subject to this restriction. Given that Barkowski & McLaughlin (2020) found that easing this restriction as part of the 2010 YACM did impact marriage rates, and marriage decisions could plausibly impact schooling decisions, I control for the effects of this state mandate requirement.

In addition, across both samples, young adults with and without state-mandated-access-to-group-insurance are more likely to live with their parents after 2010. This trend over time is strongest for young adults without state-mandated-access-to-group-insurance, who are 6 percent points (15%) more likely to live with their parents after 2010. This coincides with rising enrollment rates among those without state-mandated-access-to-group-insurance after 2010, which is consistent with young adults that are enrolled in post-secondary education being more likely to live with their parents. Table 1 makes evident that pre-2010, those with state-mandated-access-to-group-insurance had higher rates of enrollment, full time enrollment, and enrollment in 4-year programs as compared to the ineligible group. However, after 2010 these outcome rates fell, while rising among those without state-mandated-access-to-group-insurance. Thus, there is already evidence from the basic summary statistics that the 2010 YACM had an impact on enrollment rates.

3.6 Results

Table 2 presents the interaction effects of state mandate-YACM coverage eligibility on dependent coverage. This exercises shows changes in dependent coverage post 2010 among 19-25 year olds that were previously eligible for coverage and faced education incentives, relative to 19-25 year olds that didn't face education incentives. A share of the ineligible group in this analysis was eligible for coverage pre-ACA, but via states mandates that didn't require educational enrollment, while another share was ineligible for coverage altogether. These effects are important to establish because a priori they are unclear, and these results will inform the discussion of the second order effects on education. Importantly, a null first order take-up of dependent coverage effect does not imply there is no identifying variation, rather this would show that there was no disparate coverage increases post-2010 among those previously coverage eligible and those ineligible. This would indicate that the changes in enrollment post 2010 are driven by previously coverage eligible young adults losing the incentive to be enrolled in school.

The results in Columns (1)-(2) indicate that relative to those without state-mandated-access-to-group-insurance, there was no difference in dependent coverage take-up among those with state-mandated-access-to-group-insurance after the 2010 YACM. Figures 4 & 5 show that this lack of an interaction effect is due to the fact that dependent coverage increases among those with and without state-mandated-access-to-group-insurance post 2010 were very similar. The results in Table 2 indicate that as a result of the 2010 YACM, dependent coverage increased approximately 6 percentage points among those with and without state-mandated-access-to-group-insurance. The share of young adults with state-mandated-access-to-group-insurance that had dependent coverage increased after 2010 because the YACM applied to all insurance plans in the country, whereas state mandates did not apply to employer-provided policyholders that had self-funded plans. Young adults with state-mandated-access-to-group-insurance that already had dependent coverage lost the incentive to enroll in school, while the newly covered young adults, with and without state-mandated-access-to-group-insurance, could be more likely to enroll in school. Pre-

vious work has shown that young adults that gained coverage through the 2010 YACM reduced their work hours, which could translate to increases in post-secondary education enrollment.[113]

As young adults cut back on their work hours, they may have more time for school. Furthermore, by gaining insurance coverage through their parents, college becomes a more affordable option, as young adults don't having to pay for their college provided health insurance policy. Young adults that were age eligible and covered lost the incentive, while young adults that were age eligible and not covered may have gained the incentive to enrolled in school. Therefore, the state mandate-ACA interaction effect on educational enrollment is ambiguous. The results in Table 3, report these state mandate-ACA interaction treatment effects.

Table 3 presents the main results from Equation (1). Columns (1) and (2) show estimates for the effect of the student mandate pre-2010, $Elig_{ast}$, and the interaction effect $Elig_{ast}xYACM_t$, on post-secondary school enrollment, for the primary and secondary sample, respectively. These same estimates are presented in Columns (3)-(4), (5)-(6), and (7)-(8), for the remaining three outcomes; full time post-secondary enrollment, enrollment in a four-year degree program, and enrollment in a two-year degree program, respectively. In all columns, the estimates were produced with a model controlling for state-specific, linear time trends in the outcomes, year specific age differences in the outcome, and state specific age differences in the outcome.

In all cases, the estimate on $Elig_{ast}$, β_2, is not statistically different from zero, indicating that there were no differences in educational enrollment outcomes among young adults with and without state-mandated-access-to-group-insurance from 2005-2009.[114] For the coefficient on $Elig_{ast}xYACM_t$, β_1, the estimate is negative and statistically significant in 6 out of 8 columns, indicating that the 2010

[113] Antwi et al. (2013)

[114] To study the labor supply effects of these mandates Depew (2015) used data from 2001-2009. Using the same DDD specification, and a sample of 19-29 year olds, this paper also showed that the mandates had no effect on student status (coefficient of 0.0032). This effect is attributed to the fact that firms already had a tax incentive to cover dependent students through age 23, even without these state mandates. In Table A7, I include these additional pre-ACA years (2001-2004) and show that the coefficient on β_2 is unaffected, and β_1 is still statistically indistinguishable from 0, but is now even closer to 0 in magnitude.

YACM reduced post-secondary educational enrollment. Figures 6 & 7 show that these negative effects are driven by enrollment falling in absolute terms among the state mandate eligible.[115]

The estimates for β_1, in Columns (1)-(2) indicate that enrollment fell 4.1-6.3 percentage points (11% to 20%). Prior to the YACM, young adults with and without state-mandated-access-to-group-insurance were equally likely to enroll in school because these laws were not typically binding (e.g firms already had an tax incentive to cover students through age 23). After the YACM, firms have to cover dependents through age 25 without requiring student enrollment. Since firms can't require it anymore, 19-23 year old young adults become less likely to go to college. This is also the group that generally had state-mandated-access-to-group-insurance. However, 24-25 and 27-28 year olds were typically not age eligible for the state mandates, firms didn't cover them, and they are near completion or done with college, so their enrollment does not change after 2010.[116] The overall DDD effect on enrollment, β_1, is negative because it is comparing changes in enrollment among those with state-mandated-access-to-group-insurance (typically 19-23) to those without state-mandated-access-to-group-insurance (typically 24-25 and 27-28 year olds) before and after the ACA within the same state.

The differences in these coefficients is related to the source of the within state by age variation in mandate-ACA eligibility. The results in Column (1) use the 19-25 age sample. The variation using this sample comes from states that adopted their policies later in the pre-period (2005-2009). [117] Using these states as the primary source of within state by age identifying variation attenuates the treatment effects. This is in contrast to the results in Column (2), which use the 19-25, 27-28 age sample. This sample leverages more identifying variation from states that had their policies

[115]Results from a simple DDD model are shown in Appendix Table A5. This model does not include fixed effects or demographic/state policy controls. Interestingly, the β_2 estimate is positive and significant in most cases using this model, indicating that pre-2010 differences in enrollment were driven by demographic differences between mandate eligible and ineligible young adults. The estimates for β_1 are very similar to the main model, indicating that differences in the control variables between mandate eligible and ineligible young adults don't explain the post 2010 enrollment effects.

[116]In addition the 27-28 year olds are unaffected by the YACM, because they are age ineligible.

[117]Namely, states with limiting ages below 25 were more likely to be the states did not have their policies in all pre-period years.

since 2005, meaning these young adults lived in states that had student requirements as part of their mandate polices in all year from 2005-2010. This is what leads to larger estimated treatment effects for this sample.

The results in Columns (3)-(6) show that declines in these post-secondary schooling are driven by reductions in full time enrollment and enrollment in four-year degree programs. In particular, full-time enrollment fell 3.7 to 3.5 percentage points (11%), and enrollment in a 4-year degree program fell 3.5-4.2 pp (12-14%). The results in Columns (7)-(8) indicate that there was none to little change in 2-year program enrollment. This is consistent with a non-trivial share of 4-year educational institutions requiring full-time students to have health insurance coverage.[118] In contrast, community colleges only have this requirement for international students.

19-23 year old young adults typically had state-mandated-access-to-group-insurance or were covered by their parent's firm even without a state mandate. This age group effectively got a tuition subsidy if they were enrolled full time at 4-year institutions. This could induce selection into full-time enrollment at 4-year colleges or universities among 19-23 year olds with and without state-mandated-access-to-group-insurance. [119] This is a subsidy that slightly older and typically those without state-mandated-access-to-group-insurance did not get. At community colleges those with state-mandated-access-to-group-insurance did not this advantage over those without state-mandated-access-to-group-insurance, because health insurance is not required. Thus, the reductions in full time enrollment and enrollment at four year universities after the YACM is indicative of selection into full time enrollment and enrollment at four year universities prior to 2010, among all 19-23 year olds. On the other hand, the fact that there is no change in enrollment in community colleges, indicates that there was no selection into community college enrollment among 19-23 year olds prior to 2010, as expected.

[118]The American College Health Association reports that the average health insurance plan for students through their universities costs $1500-$2500 annually, or 10% of tuition costs.

[119]Most full time students get health insurance through a parent's firm because this is a more affordable option. 67% of full time students were covered through employer-sponsored plans, while only 7% were covered under the university health plan (2008 GAO report)

Though minimal work has estimated the causal impact of health insurance on educational choices, Jung et al. (2018) find a strong positive correlation between parental insurance coverage and full-time post secondary school enrollment.[120] Their results show that from 2001-2008, the availability of parental health insurance increases the probability of being a full-time student by 20.0 percentage points and increase the probability of enrolling in college by 19.4 percentage points among 17-23 year olds. They say this is because states tied dependent coverage eligibility to enrollment, and typically full time enrollment from 2001-2008. Using these point estimates they state that if all 17-23 year old young adults received parental health insurance coverage without having to enroll or enroll full time (as the YACM policy does), full time enrollment and overall enrollment should fall by 20 percentage points. My coefficient estimate for the decline in full time enrollment is 4 percentage points. According to Figure 3, an additional 20% of the young adults gained dependent coverage through this policy, implying a treatment effect on the treated of 20 percentage points. This estimate is identical to the calculation done by Jung et al. (2018).

These results indicate large enrollment changes in response to dependent coverage, in contrast to the findings by Heim et al. (2018), who find extremely small effects on post-secondary school enrollment, full time enrollment, and enrollment as a graduate student.[121] Using a treated sample of young adults ages 24-25, and a control group of those ages 27-29, they estimate DD effects of the YACM on education. The key distinction is that they did not utilize pre-2010 variation in state dependent coverage policies.

By not incorporating this pre-2010 heterogeneity, this paper did not consider how the 2010 YACM changed schooling incentives for young adults that were age eligible for coverage via state mandates with explicit requirements for school enrollment. By eliminating the school enrollment

[120] These authors evaluate the choice to (i) enroll full time, (ii) enroll part time, or (iii) not enroll, as a function of having dependent coverage, using a multinomial logit model. They also control for individual and parental demographic and socioeconomic characteristics.

[121] Specifically, they find post-secondary enrollment decreased 0.2 percentage points and graduate school enrollment increased 0.2 percentage points. They also find full time enrollment increased 0.01 percentage points, but the effect is not statistically significant.

requirement for this subset of young adults, the 2010 YACM reduced educational incentives for 19-25 year olds that had state-mandated-access-to-group-insurance prior to 2010, but didn't change incentives for 19-25 year old young adults that did not.

Columns (1)-(3) in Table S4 present estimates for Equation (4). Column (1) shows that enrollment in post-secondary education increased 1.6 percentage points among the state mandate ineligible after 2010, while enrollment decreased 2.4 percentage points among those state mandate eligible. Column (2) shows that enrollment in full time post-secondary education increased 2.1 percentage points among the state mandate ineligible after 2010, while enrollment decreased 5.4 percentage points among the state mandate eligible. Finally, Column (3) shows that enrollment in graduate school increased 2.24 percentage points among the state mandate ineligible after 2010, while enrollment decreased 4.2 percentage points among the state mandate eligible. The overall DD effects when pooling the mandate eligible and ineligible, are very similar to those in Heim et al. (2018), confirming that the YACM had negligible effects on educational enrollment among 24-25 year olds. These results also highlight the importance of examining the heterogeneous treatment effect of the 2010 YACM on educational decisions among those with and without state-mandated-access-to-group-insurance, by showing that the effects for those with and without state-mandated-access-to-group-insurance are of opposite signs.

3.6.1 Supplementary Analyses

To provide support for my primary results, I perform several additional analyses investigating related aspects of the main research question.

Analysis of Enrollment Flows

In the first of these additional analyses, I evaluate how the main enrollment effects are driven by reduced flows into enrollment and increased flows out of enrollment by level of enrollment. I use the same DDD model outlined in Equation (1), but redefine my samples to reflect flows into and out of enrollment. In particular, the results in Table S1 show how the 2010 YACM interacted

with state dependent coverage mandates to impact college and grad school entrance, and the probability of continuing in progress college degrees among young adults ages 19-25.[122]

The results in Column (1) of Table S1 show that the main enrollment effects are driven by reduced college entrance. In particular, the probability of entering the first year of college among young adults either not enrolled or enrolled in high school the prior year fell 6.2 percentage points as a result of the 2010 YACM.[123] The evidence from Columns (2)-(4) of Table S1 show that the main enrollment declines reported in Table 3 do not stem from young adults being less likely to continue their college studies or being less likely to enroll in graduate school.

In Table S2, I provide further evidence that post-secondary enrollment declines are driven by young adults in high school with state-mandated-access-to-group-insurance. By matching young adults to their parents in the Annual Socioeconomic Survey (ASEC), I determine whether their parents have ESHI. I interact this time varying individual level indicator variable with the two main variables of interest in Equation (1), $Elig_{ast}xYACM_t$ and $Elig_{ast}$. The results in Table S2, show heterogeneity in enrollment effects by whether the young adult's parent's have ESHI. I also show how these heterogeneous treatment effects vary by level of enrollment. The estimates in Column (1) of Table S2 show that the overall enrollment declines resulting from the 2010 YACM, are 16 percentage points larger if the child's parents have ESHI. The estimates in Columns (2) and (3) show that this larger effect is only seen for young adults with at most a high school education. The estimates in Column (2) indicate that enrollment declines resulting from the 2010 YACM, among those with at most a high school degree, are 19 percentage points larger if the child's parents has ESHI. The estimates in Column (3) indicate that there are no differential enrollment declines among young adults in college with and parental ESHI, resulting from the 2010 YACM. This provides further evidence the overall enrollment effects in Table 3 are driven by young adults in high school that had state-mandated-access-to-group-insurance.

[122] I have also re-estimated these results using the secondary sample, and the conclusions are unchanged. These results are available upon request.

[123] 55% of young adults enrolled in the first year of college were in 12th grade (HS) the previous year. Only 4% of young adults enrolled in the first year of college were not in school the previous year.

Analysis of Time-Varying Treatment Effects

In my second supplementary analysis, I show how the way in which the YACM interacted with prior state mandates has changed since 2010, using an event study design of the form:

$$\mathbf{Y}_{iast} = \beta \mathbf{X}_{iast} + \sum_{y=-5}^{-2} \omega_y Elig_{ast} \cdot \mathbb{1}\{t - 2010 = y\} + \sum_{y=0}^{5} \theta_y Elig_{ast} \cdot \mathbb{1}\{t - 2010 = y\} + \alpha_a + \delta_{st} + \sigma_{ta} + \gamma_{sa} + \varepsilon_{iast} \quad (5)$$

The covariates used in Equation (5) are identical to those in Equation (1). ω_y, where y= (-5,-4,-3,-2), are evidence of parallel pre-trends. The main coefficients of interest in Equation (3) are those on the interactions of $Elig_{ast}$ with event-year dummies, $\mathbb{1}\{t - E_i^* = y\}$, which are equal to 1 when the year of observation is y = -5,...,0,...,5 years from 2010 (y = -1 is omitted). These coefficients, θ_y, where y= (0,1,2,3,4,5), characterize the effect of the 2010 law on young adults with state-mandated-access-to-group-insurance, relative to those without access. Assuming young adults take more than a month make enrollment decisions based on finding out they can stay on their parents' insurance plan on September 23,2010, I would expect the main enrollment effects to first be measured in 2011 (coefficient θ_1). If state-mandated-access-to-group-insurance young adults that were planning to enroll in 2011 to stay on their parent's plan, but ultimately never enrolled because of the YACM, this could lead to immediate and persistent effects. Furthermore, if other cohorts are affected as knowledge of the policy spreads, this could lead to effects that grow over time, as these cohorts may drop out. I would expect the immediate effects to persistent, but not grow, because it is more likely than a young adult's decision to enroll is more sensitive to changing educational incentives, than their decision to drop out.

I estimate Equation (5) using the secondary sample and show results with and without excluding states that did not always have their dependent coverage policies. This age sample is used because

states that always had their coverage polices from 2005-2009, had higher limiting ages. Using the more restricted set of states ensures that any change in schooling from 2010-2015, relative to the pre-period, is due to the YACM and not changes in state mandates pre-2010. The states that always had their polices include GA, LA, ND, RI, and SD. Their limiting ages range from 23-25. Using the 19-25 year old sample only LA contributes to the identifying variation in all pre-years. GA had a limiting age of 25 since 2006, so there is no variation in state coverage eligibility in most years. ND had a limiting age of 25 in all years. RI covers students indefinitely, and SD expanded coverage up to age 29 in 2008. Thus, limiting my sample to 19-25 years yields very little within state by age identifying variation; highlighting the need to expand the sample to include 27-28 year olds. [124] In Figures S1-S4, I present event study graphs for the four outcomes. I plot ω_y, where y= (-5,-4,-3,-2), and θ_y, where y= (0,1,2,3,4,5), binning event times -5 to -4 and 4 to 5, to provide visual evidence of parallel pre-trends, and the dynamic treatment effects. The left panels of Figures S1-S4, show results using the unrestricted set of states, while the right panels show results using the restricted set.

Figures S1-S4 show the estimates of the lag and lead coefficients where the outcomes are (1) post-secondary school enrollment, (2) full time enrollment, (3) enrollment in a four-year degree program, and (4) enrollment in a two-year degree program, respectively.

The estimates for ω_y, where y= ((-5,-4),-3,-2) are generally statistically equal to 0, and in most cases have a magnitude of 0, using the restricted and unrestricted set of states to estimate effects.[125] This shows that enrollment outcomes were not trending differently pre-2010 among young adults with and without state-mandated-access-to-group-insurance. The plot of ω_y, where y= ((-5,-4),-3,-2) provides a visual test in support of the parallel trends assumption needed for DDD estimation.

[124]The 11 states that didn't have their policies in all years include CT, GA, ID, IA, KY, LA, MN, NH, ND, RI, SD, and WV.

[125]The exception being the estimates on $\omega_{(-5,-4)}$ when evaluating 4-year enrollment using the restricted set of states. This is not a main concern for my results because if I exclude years 2005 and 2006 from my analysis, I obtain estimates that are very similar to my main results using this set of states.

Figures S1-S4 also confirm the main conclusions drawn from the static DDD model results in Table 3. Namely, after the 2010 YACM is implemented, enrollment declines among young adults with state-mandated-access-to-group-insurance, and these declines are driven by reductions in full-time enrollment and enrollment at four-year degree programs. There are also additional insights from examining the lead coefficients θ_y, where y= (0,1,2,3,(4,5)). In particular, these coefficients show that the effects of the policy occurred one year after implementation. Starting in 2011 post-secondary school enrollment declined 10 percentage points. This level of enrollment decline was persistent. Full time enrollment and enrollment in a 4-year degree program also persistently fell 10 percentage points starting in 2011. There was no change in enrollment in 2-year degree programs.

Analysis of Treatment Effect Heterogeneity: Share of State Employees Covered by Fully-Insured Plans

State level dependent coverage mandates are not binding for all firms. The 1974 Employee Retirement Income Security Act (ERISA) set self-insured employers under federal jurisdiction, meaning that state mandates do not apply to self-insured firms. In 2008, 55 percent of workers with health insurance were covered by a self-insured firm and 89 percent of workers in a firm with 5,000 or more employees were covered by a self-insured firm, indicating that state dependent coverage mandates did not apply to a large share of the workforce. (Pierron and Fronstin (2008)). In contrast, the YACM applied to all employer provided health plans, including at self-insured firms. This implies that the education effects should be stronger in states that have more insured individuals in plans under their regulatory authority before and after the ACA.

In my third supplementary analysis, I follow an approach used by Depew (2015) and Barkowski & McLaughlin (2020), by interacting a state-by-year-level variable measuring the share of employees covered by fully-insured health plans with the two main variables of interest in Equation (1), $Elig_{ast}xYACM_t$ and $Elig_{ast}$, which comes from the Agency for Healthcare Research and Qual-

ity.[126] Given that states' regulatory authority is an increasing function of the share of the workforce employed at fully-insured firms, I should observe estimates from models incorporating the fully-insured-share variable to have the same sign pattern and larger magnitudes as those using Equation (1). The model used to estimate this treatment intensity by share of employees covered by fully-insured health plans is as follows:

$$\begin{aligned}(6) \qquad \mathbf{Y}_{iast} = {} & \beta_1 Elig_{ast} \cdot YACM_t \cdot ShareFullyInsur_{ast} + \beta_2 Elig_{ast} \cdot ShareFullyInsur_{ast} \\ & + \beta_3 ShareFullyInsur_{ast} + \omega \mathbf{X}_{iast} + \phi \mathbf{Z}_{ast} + \alpha_a + \delta_{st} + \sigma_{ta} + \gamma_{sa} + \varepsilon_{iast}\end{aligned}$$

Columns (1) through (8) of Table S3 present estimates based on specification outlined in Equation (6). Since the estimates for β_1 and β_2 have the same sign patterns and larger magnitudes as the main results using Equation (1), this shows that the eliminating of enrollment incentives as part of the 2010 YACM, had the larger effects on enrollment among adults living in states with a higher degree of regulatory authority over dependent coverage.

The results in Column (1)-(8) indicate that if all health plans were fully insured by third party companies, state dependent coverage mandates still would not have changed enrollment rates. The state mandate-ACA interaction effects are now larger and most are statistically significant, showing that the state mandates' effects are proportional to the strength of states' regulatory authority. In particular, the results in Columns (1)-(2) indicate that if all state health plans were fully insured, enrollment would fall 4-12 percentage points as a result of the 2010 YACM. The results in Columns (3)-(4) show that full-time enrollment would fall 6-8 percentage points. The results in Columns (5)-(6) indicate that 4-year program enrollment would fall 5-8 percentage points. Finally, the results in Columns (7)-(8) indicate that 2-year program enrollment would fall 1-6 percentage points.

[126] This data is available from 2000-2006 and 2008-2015.

3.6.2 Robustness Checks

In my first robustness check, I test whether the enrollment effect estimated using Equation (1) may be a result of unobserved trends in post-secondary educational enrollment. This robustness check only uses young adults sample ages 27 to 28, to check whether effects of the YACM are observed for this group. Not only does this robustness check allow me to test whether the enrollment effects are driven by unobserved trends in mandate versus non-mandate states, but I can also directly show that 27-28 year olds in my secondary sample with state-mandated-access-to-group-insurance do not have a treatment effect from the 2010 YACM. Since some state mandates applied to ages above 26, I could observe effects of the mandates for this group. However, I should not observe effects from the ACA because this policy did not extend to these individuals. If my main results were not generated by unobserved trends, the estimated coefficients on $Elig_{ast}xYACM_t$ should be 0 for these groups. If the effects are non-zero we should be concerned about including the 27-28 year olds in the event study results and the main results using the secondary sample.

Table A1 presents results from placebo tests that show that the enrollment effects are not driven by unobserved trends and 27-28 year olds in my secondary sample with state-mandated-access-to-group-insurance do not have a treatment effect from the 2010 YACM. The estimate on $Elig_{ast}$ is positive and statistically significant in Columns (1)-(3), indicating that state mandates requiring education enrollment increased post-secondary enrollment, full-time enrollment, enrollment in 4-year programs, and enrollment in 2-year programs among 27-28 years olds. The estimate for $Elig_{ast}xYACM_t$ is 0 for all outcomes, showing that the YACM did not affect 27-28 year olds, as expected.

In my second robustness check, I check the robustness of my results to excluding states that introduced or got rid of their dependent coverage policies from 2005-2009.[127] Using variation from states that adopted their policies later in the pre-period should attenuate my results because eligible young adults in states that adopted their policies closer to 2010 should have smaller

[127] CT,MN, and WV got rid of their polices in 2007. ID, IA, and KT introduced their policies from 2008-2010.

treatment effects. This could be the case if young adults in ID, IA, and KT were less aware of, and therefore less likely to be making enrollment decisions based on their state policies, compared to young adults living in states that their policies for many more years prior to the enactments of the YACM. Thus, I expect to find larger estimates of treatment effects excluding states that did not have their dependent coverage in all years from 2005-2009.

Table A2 presents estimates from the model outlined in Equation (1) using the secondary sample and using states that had their dependent coverage policies in all years from 2005-2015 (as well the states with no such policies). The estimates from this analysis are larger than those using all states, indicating that including states that adopted or got rid of their polices from 2005-2010, attentuates my results, and the main results are in fact conservative estimates of treatment effects. In particular, the estimates in Columns (1)-(2) are 15-30% larger than my main results.

In a final analysis, I provide further evidence for the validity of my main results by showing that young adults with state-mandated-access-to-group-insurance are less likely to be in school and have dependent coverage after the 2010 YACM, but there is no change in the probability that young adults with state-mandated-access-to-group-insurance are in school and have another source of private coverage. This shows that young adults that had state-mandated-access-to-group-insurance and had dependent coverage are the group that is driving the enrollment declines.

Table A6 presents estimates from the model outlined in Equation (1). The estimates in Column (1) show that the probability of being enrolled in post-secondary education and having dependent coverage falls by 2.9 percentage points, while the estimates in Column (2) show no change in the probability of being in enrolled in post-secondary education and some private coverage other than dependent coverage. This is evidence in support of the conclusion that the YACM led to enrollment declines among young adults that already had state-mandated-access-to-group-insurance and had dependent coverage.

3.7 Conclusion

In this study, I showed that state mandates to increase insurance coverage among young adults did not affect post-secondary school enrollment. However, an unintended consequence of the 2010 Young Adult Coverage Mandate was that this policy reduced enrollment rates among young adults with state-mandated-access-to-group-insurance. These results are robust to using states that did and did not have their policies in all years pre-2010, and a placebo treatment group. The enrollment reduction was driven by a falling share of young adults with state-mandated-access-to-group-insurance enrolling full-time and at four-year universities. The main channel for these effects was through reduced college entrance. This paper also showed that these effects persisted through at least 2014. Finally, by documenting how the 2010 YACM interacts with state mandates, I add to the findings by Heim et al. (2018), by showing that the 2010 YACM had heterogeneous impacts on enrollment decisions of young adults which are masked by abstracting away variation from state dependent coverage policies.

This cumulative evidence indicates that young adults' education decisions are very responsive to health insurance access. This is a new and important finding. Even though young adults are a relatively healthy subset of the population, health insurance availability still drives their behavior around educational attainment. Future research is needed to investigate the effects of other components of the ACA, like the individual mandate and the Medicaid expansion, on decision making of young adults.

Figures

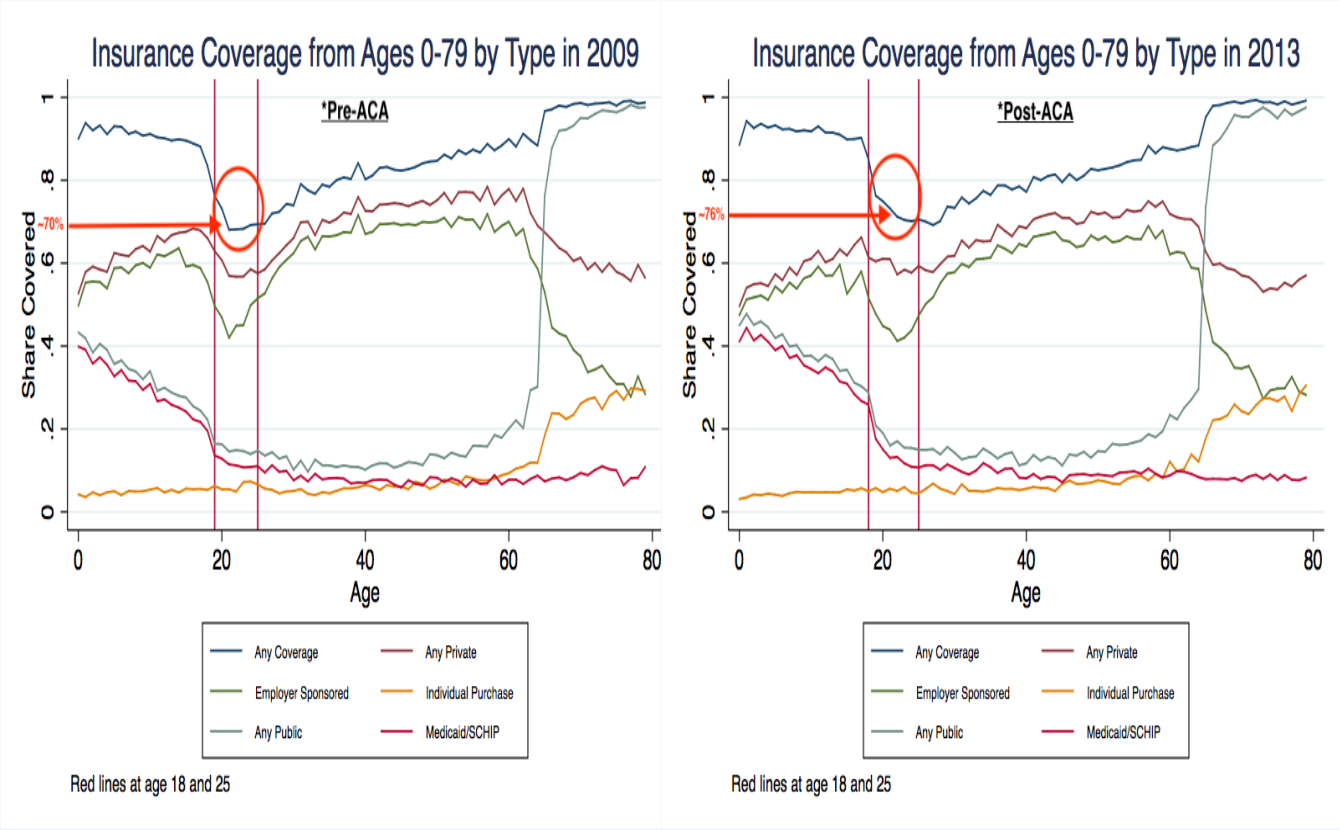
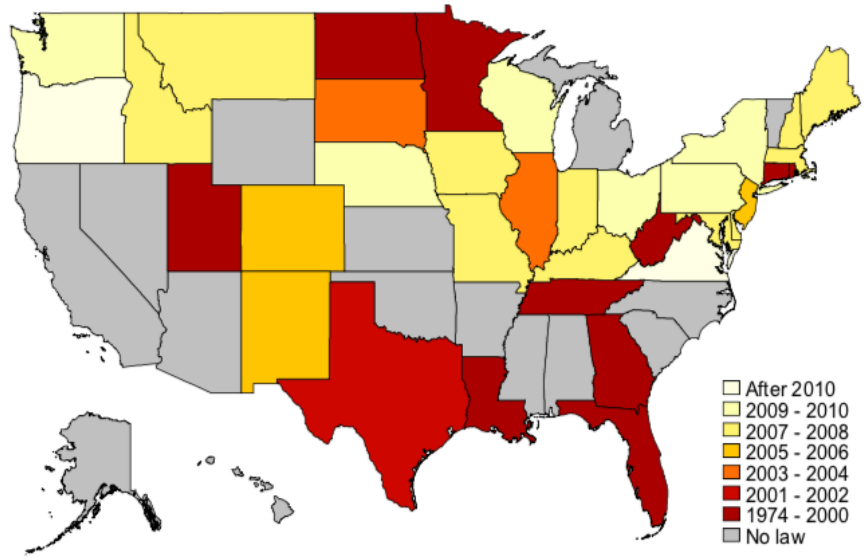


Figure 3.1: Changes in Insurance Coverage by Age from 2009 to 2013. Data from CPS ASEC.



Note: No state mandates were introduced for the first time after 2011.

Figure 3.2: Map of State Mandate Implementation (Source: Barkowski & McLaughlin (2020)) *Includes all State Mandates. *Only CT, GA, ID, IA, KY, LA, MN, ND, RI, SD, and WV have mandates requiring school enrollment.*

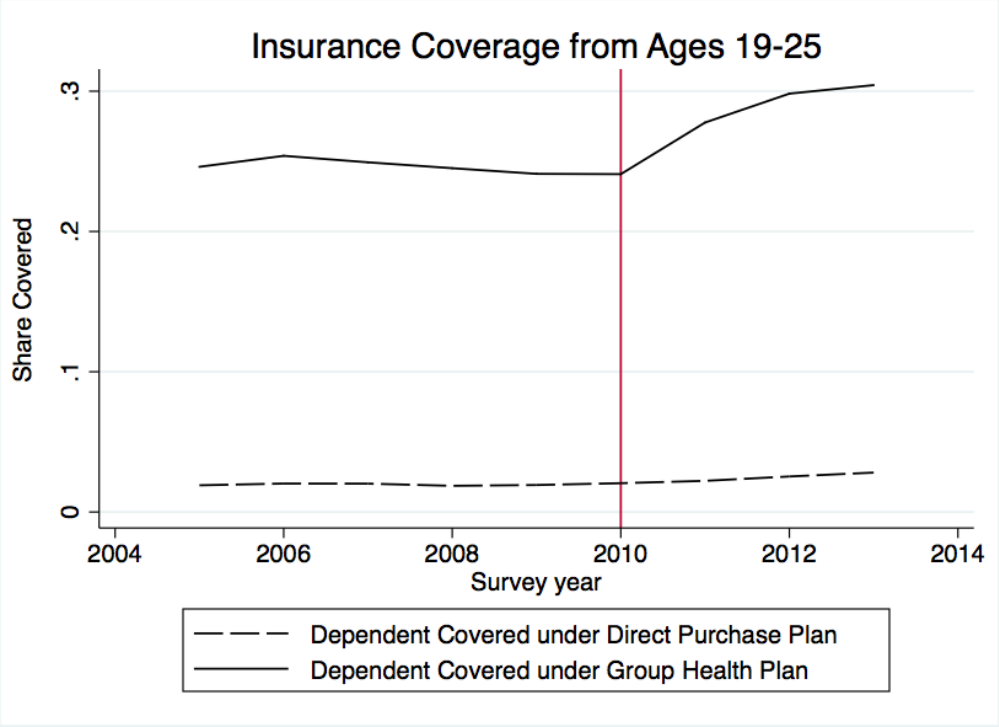


Figure 3.3: Dependent Coverage Changes from 2005-2015 Among Young Adults Ages 19-25: CPS ASEC

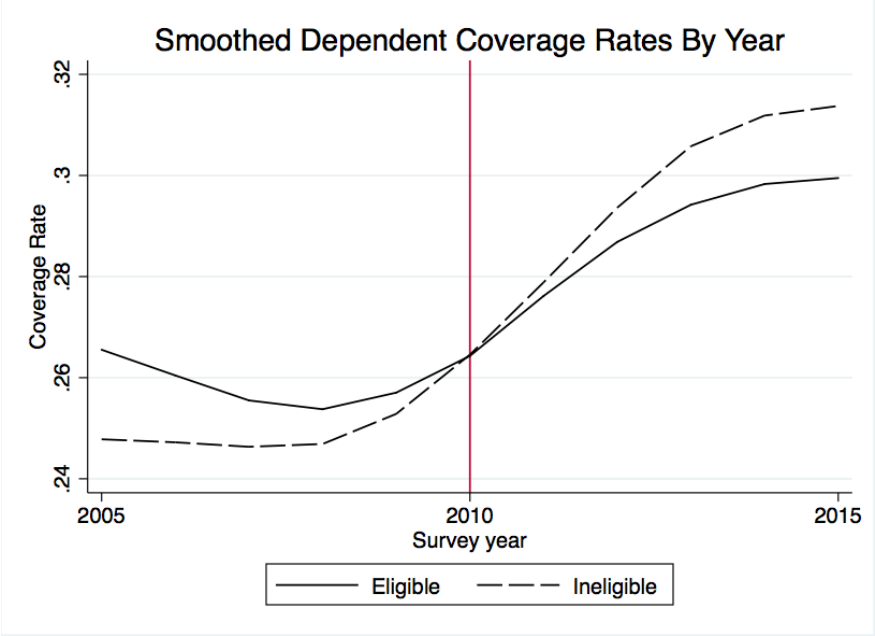


Figure 3.4: Smoothed Dependent Coverage Changes from 2005-2015 By State Coverage Eligibility Status. (Young Adults Ages 19-25: CPS ASEC)

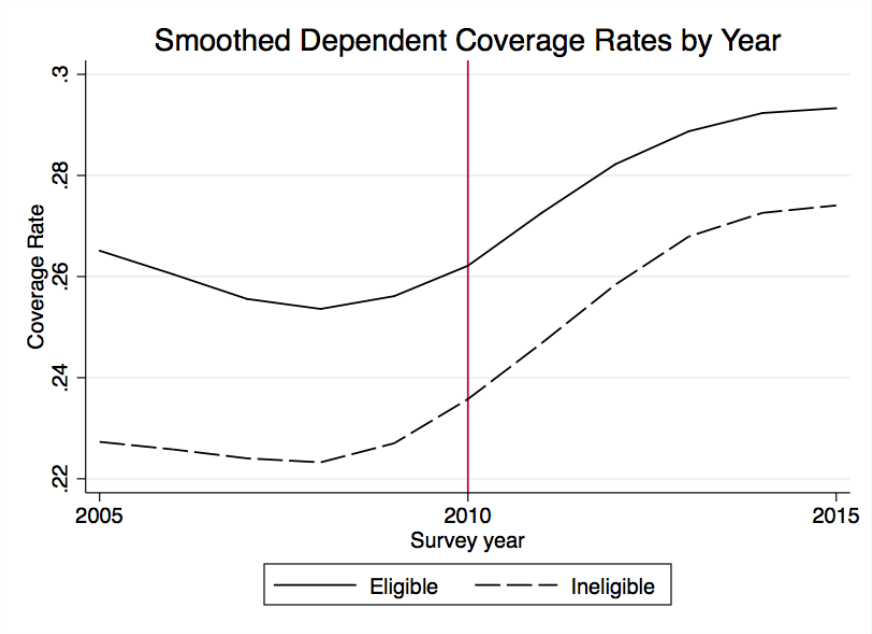


Figure 3.5: Smoothed Dependent Coverage Changes from 2005-2015 By State Coverage Eligibility Status. (Young Adults Ages 19-25, 27-28: CPS ASEC)

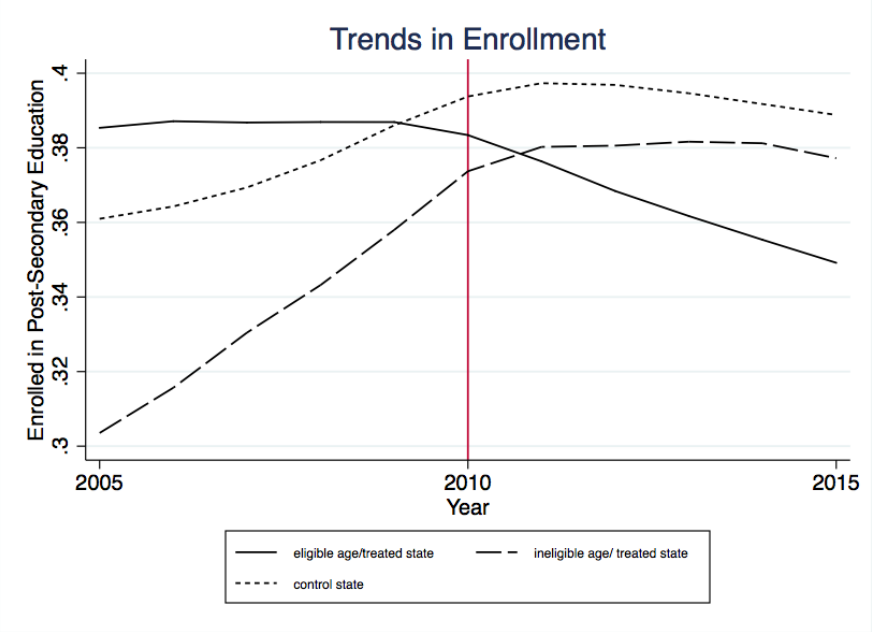


Figure 3.6: Smoothed Enrollment Changes from 2005-2015 By State Coverage Eligibility Status. (Young Adults Ages 19-25)

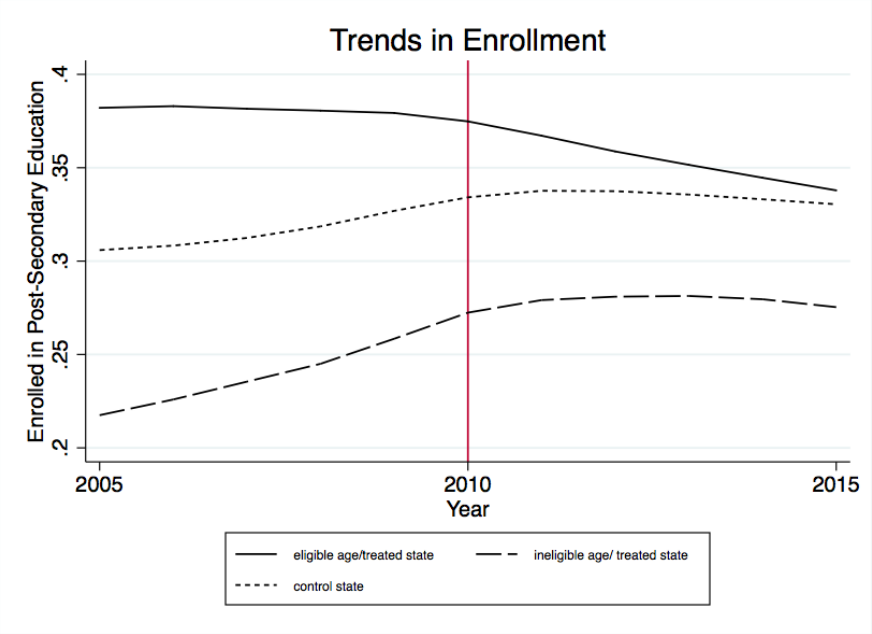


Figure 3.7: Smoothed Enrollment Changes from 2005-2015 By State Coverage Eligibility Status. (Young Adults Ages 19-25, 27-28)

Tables

Table 3.1: *Summary Statistics*

Treatment Status:	Ineligible	Ineligible	Eligible	Eligible		Ineligible	Ineligible	Eligible	Eligible
Years:	2005-2009	2010-2015	2005-2009	2010-2015		2005-2009	2010-2015	2005-2009	2010-2015
Primary:					Secondary:				
Sample:	19-25	19-25	19-25	19-25	Sample:	19-25, 27,28	19-25, 27,28	19-25, 27,28	19-25, 27,28
Lives with Any Parent	48.3	54.2	47.1	48.4	|	40.8	46.3	46.4	46.6
Age	22.0	22.0	21.6	21.8	|	23.3	23.3	21.7	22.1
Female (%)	49.6	49.6	51.5	50.0	|	49.7	49.6	51.4	49.8
Black (%)	14.3	15.5	22.6	24.3	|	14.2	15.4	22.2	23.4
Hispanic (%)	19.0	22.1	8.0	7.2	|	19.2	21.7	8.0	7.2
Citizen (%)	89.2	91.6	9.3	9.5	|	88.4	90.6	92.5	9.5
Unmarried Requirement (%)	24.9	58.2	47.3	52.8	|	25.3	58.3	47.9	54.0
Enrolled (%)	37.0	39.4	38.0	36.2	|	31.0	33.3	37.3	35.2
FT (%)	29.4	31.2	31.6	29.6	|	24.0	25.7	31.0	28.6
4-Year Program (%)	26.2	27.2	29.1	27.0	|	21.8	23.0	28.6	26.3
2-Year Program (%)	9.2	10.3	7.2	7.3	|	7.8	8.7	7.2	7.0
Observations	41,855	49,185	4,860	6,657		55,134	65,248	5,112	7,182

Note: Age 26 is excluded because age is reported in October, indicating some individuals could be in the post ACA treatment group (under 26) in some months of the year and in the control group (26 and over) in the other months. Weighted using CPS October Supplement Weight. Eligible young adults are those interviewed in states with a dependent coverage mandate that required school enrollment, who are at or below the limiting age for the policy.

Main Results

Table 3.2: First Stage Estimates of Mandate-ACA Interaction Effect on Dependent Coverage

	(1)	(2)
Sample:	19-25	19-25, 27-28
Eligible x Post-2010	-0.0141	-0.0156
	(0.00902)	(0.00987)
Eligible	-0.0191	-0.0186
	(0.0122)	(0.0113)
Post-2010	0.0607***	0.0607***
	(0.0189)	(0.0191)
Pre-2010 Outcome Mean Among Eligible (%)	36.4	39.1
Observations	187,836	243,453
State by Year FE	X	X
Year by Age FE	X	X
State by Age FE	X	X

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Data from 2005-2015 ASEC. Outcome is whether the young adult is covered as a dependent under a group plan. Eligible is a binary variable equal to 1 if the young adult is age eligible for dependent coverage via a state mandate that requires school enrollment as a condition of eligibility, and 0 otherwise. Eligibility is based on the state of residence and the year of interview. Post-2010 is a binary variable equal to 1 if the year is $\geq$ 2010, and 0 otherwise. Standard errors reported in parentheses are clustered at the state level. All estimates produced using ASEC weight. All models also include dummy variables for state, year, age, race, gender, and ethnicity, plus the full interaction between age and gender. Also included are the two-way interactions between age eligibility and state mandates with marriage restrictions and no education requirements being in effect, and the three-way interactions between age eligibility, state mandates with marriage restrictions and no education requirements being in effect, and the YACM being in effect.

Table 3.3: Estimates of Mandate-ACA Interaction Effects on Education

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Enrolled	Enrolled	Full Time	Full Time	4-Year Program	4-Year Program	2-Year Program	2-Year Program
Sample:	19-25	19-25, 27,28	19-25	19-25, 27,28	19-25	19-25, 27,28	19-25	19-25, 27,28
Eligible x YACM	-0.0407**	-0.0628***	-0.0373***	-0.0346***	-0.0346***	-0.0420***	-0.00610	-0.0208
	(0.0153)	(0.0140)	(0.0121)	(0.00865)	(0.0115)	(0.00764)	(0.00765)	(0.0135)
Eligible	-0.0533	-0.0366	-0.0468	-0.0438	-0.0317	-0.0280	-0.0216	-0.00863
	(0.0352)	(0.0222)	(0.0336)	(0.0374)	(0.0269)	(0.0244)	(0.0145)	(0.0131)
2005-2009 Outcome Mean Among Eligible (%)	38.0	36.0	33.0	31.0	30.0	29.0	7.4	7.3
Observations	111,784	145,034	111,784	145,034	111,784	145,034	111,784	145,034
State by Year FE	X	X	X	X	X	X	X	X
Year by Age FE	X	X	X	X	X	X	X	X
State by Age FE	X	X	X	X	X	X	X	X

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Eligible is a binary variable equal to 1 if the young adult is age eligible for dependent coverage via a state mandate that requires school enrollment as a condition of eligibility, and 0 otherwise. Standard errors reported in parentheses are clustered at the state level. All estimates produced using CPS October Supplement Weights. All models also include dummy variables for state, year, age, race, gender, and ethnicity, plus the full interaction between age and gender. Also included are the two-way interactions between age eligibility and state mandates with marriage restrictions and no education requirements being in effect, and the three-way interactions between age eligibility, state mandates with marriage restrictions and no education requirements being in effect, and the YACM being in effect.

Supplementary Analyses

Table 3.S1: Post-Secondary Education Flow Analysis

Enrolled in:	(1) 1st Year College	(2) 2nd Year College	(3) 3rd Year College	(4) 1st Year Grad School
Eligible x YACM	-0.0617***	-0.0483	-0.260	0.00802
	(0.0163)	(0.114)	(0.190)	(0.00729)
Eligible	0.00423	-0.150	0.325*	-0.00531
	(0.0149)	(0.104)	(0.164)	(0.00775)
2005-2009 Outcome Mean Among Eligible (%)	8.0	68.9	75.0	1.6
Observations	66,629	11,927	9,719	64,860
State by Year FE	X	X	X	X
Year by Age FE	X	X	X	X
State by Age FE	X	X	X	X

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Sample is 19-25 year old young adults. Eligible is a binary variable equal to 1 if the young adult is age eligible for dependent coverage via a state mandate that requires school enrollment as a condition of eligibility, and 0 otherwise. Column (1) results are conditional on young adult not being enrolled or enrolled in HS last year. Column (2) results are conditional on young adult being enrolled in 1st year of college last year. Column (3) results are conditional on young adult being enrolled in 3rd year of college last year. Column (4) results are conditional on young adult not being enrolled or enrolled in some grade below the 1st year of graduate school last year. Standard errors reported in parentheses are clustered at the state level. Statistically significant estimates for two-tailed tests at the one, five, and ten-percent levels are indicated by ***, **, and *, respectively. All models also include dummy variables for state, year, age, race, gender, and ethnicity, plus the full interaction between age and gender. Also included are the two-way interactions between age eligibility and state mandates with marriage restrictions and no education requirements being in effect, and the three-way interactions between age eligibility, state mandates with marriage restrictions and no education requirements being in effect, and the YACM being in effect.

Table 3.S2: Heterogeneous Effects on Post-Secondary Education Enrollment By Parental ESHI Coverage

Level of Educational Attainment:	(1) Full Sample	(2) ≤ HS Degree	(3) Some College
Eligible x YACM x Parents Have ESHI	-0.159**	-0.187***	-0.0253
	(0.0642)	(0.0523)	(0.0577)
Eligible x Parents Have ESHI	0.0990***	0.103**	0.0425
	(0.0177)	(0.0432)	(0.0376)
Observations	46,097	13,234	23,479
State by Year FE	X	X	X
Year by Age FE	X	X	X
State by Age FE	X	X	X

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Estimates using 2005-2015 ASEC. Sample is 19-24 year old young adults. Eligible is a binary variable equal to 1 if the young adult is age eligible for dependent coverage via a state mandate that requires school enrollment as a condition of eligibility, and 0 otherwise. Educational enrollment is only consisted measured for 19-24 year olds. All estimates produced using ASEC Weights. Column (1) results are estimated using all 19-24 year olds. Column (2) results are estimated using 19-24 year olds with at most a high school degree. Column (3) results are estimated 19-24 year olds with some college attendance. Standard errors reported in parentheses are clustered at the state level. Statistically significant estimates for two-tailed tests at the one, five, and ten-percent levels are indicated by ***, **, and *, respectively. All models also include dummy variables for state, year, age, race, gender, and ethnicity, plus the full interaction between age and gender. Also included are the two-way interactions between age eligibility and state mandates with marriage restrictions and no education requirements being in effect, and the three-way interactions between age eligibility, state mandates with marriage restrictions and no education requirements being in effect, and the YACM being in effect.

Table 3.S3: Estimates of Mandate-ACA Interaction Effects on Education By Treatment Intensity

Sample:	(1) Enrolled 19-25	(2) Enrolled 19-25, 27,28	(3) Full Time 19-25	(4) Full Time 19-25, 27,28	(5) 4-Year Program 19-25	(6) 4-Year Program 19-25, 27,28	(7) 2-Year Program 19-25	(8) 2-Year Program 19-25, 27,28
Eligible x YACM								
x share of fully-insured plans	-0.0399	-0.122**	-0.0593*	-0.0806**	-0.0510	-0.0765**	-0.0109	-0.0632*
	(0.0449)	(0.0490)	(0.0313)	(0.0364)	(0.0408)	(0.0320)	(0.0286)	(0.0375)
Eligible x share of fully-insured plans	-0.140	-0.0597	-0.0941	-0.0604	-0.112	-0.0758	-0.0275	0.0185
	(0.0875)	(0.0563)	(0.0569)	(0.0366)	(0.0756)	(0.0465)	(0.0323)	(0.0280)
Observations	100,407	130,319	100,407	130,319	100,407	130,319	100,407	130,319
State by Year FE	X	X	X	X	X	X	X	X
Year by Age FE	X	X	X	X	X	X	X	X
State by Age FE	X	X	X	X	X	X	X	X

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Eligible is a binary variable equal to 1 if the young adult is age eligible for dependent coverage via a state mandate that requires school enrollment as a condition of eligibility, and 0 otherwise. No data available in 2007. Standard errors reported in parentheses are clustered at the state level. All estimates produced using CPS October Supplement Weights. All models also include dummy variables for state, year, age, race, gender, and ethnicity, plus the full interaction between age and gender. Also included are the two-way interactions between age eligibility and state mandates with marriage restrictions and no education requirements being in effect, and the three-way interactions between age eligibility, state mandates with marriage restrictions and no education requirements being in effect, and the YACM being in effect.

Table 3.S4: DD Effect of the ACA by State Coverage Eligibility Status

	(1)	(2)	(3)
	Post-Secondary Enrollment	Full Time Enrollment	Graduate School Enrollment
Sample:	24-25, 27-29	24-25, 27-29	24-25, 27-29
Under age 26			
Eligible x YACM	-0.0103	-0.00817	-0.0205
	(0.0150)	(0.0107)	(0.0154)
Eligible	0.0194	0.0215	0.00687
	(0.0242)	(0.0230)	(0.0166)
ACA Difference-in-Differences			
Effect on the state ineligible	0.0161	0.0212	0.0224*
(96% of Population)	(0.0139)	(0.0129)	(0.0127)
Effect on the state eligible	-0.0238	-0.0540**	-0.0416***
(4% of Population)	(0.0183)	(0.0198)	(0.0174)
Observations	83,067	83,067	75,613
State by Year FE	X	X	X
Year by Age FE	X	X	X
State by Age FE	X	X	X

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Standard errors reported in parentheses are clustered at the state level. All estimates produced using CPS October Supplement Weights. All models also include dummy variables for state, year, age, race, gender, and ethnicity, plus the full interaction between age and gender, as well as the interactions between age and state mandates with marriage restrictions.

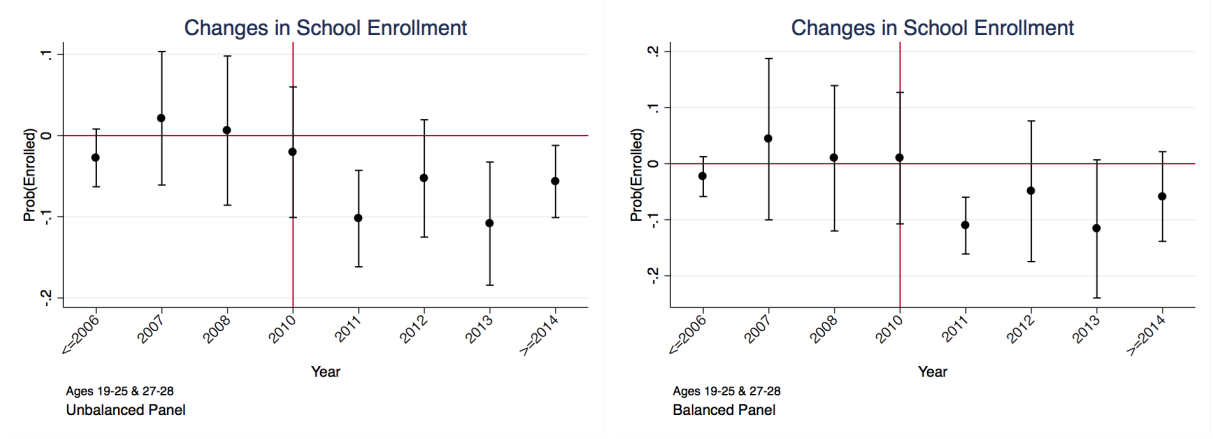


Figure 3.S1: Outcome is any school enrollment. Weighted using the CPS October supplement weight. Standard errors are clustered at the state level.

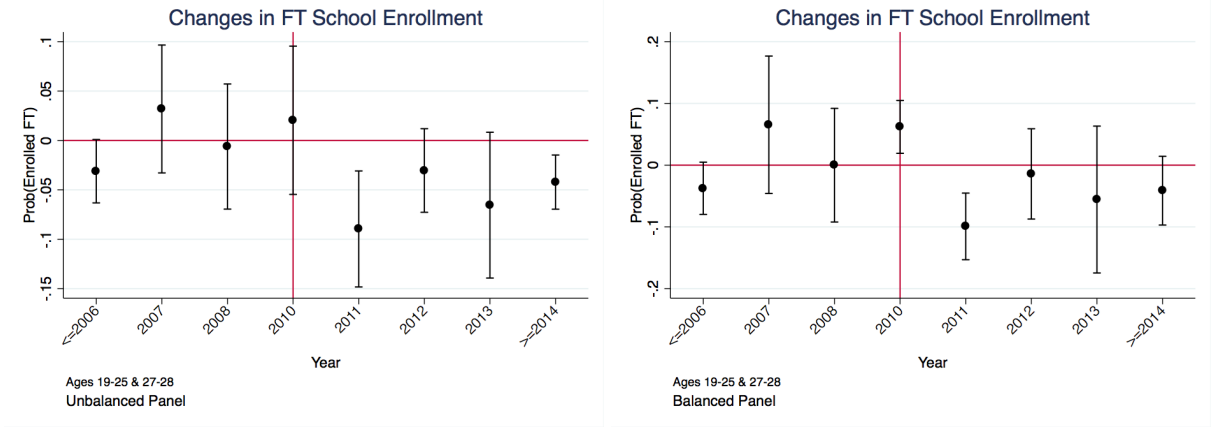


Figure 3.S2: Outcome is full time enrollment. Weighted using the CPS October supplement weight. Standard errors are clustered at the state level.

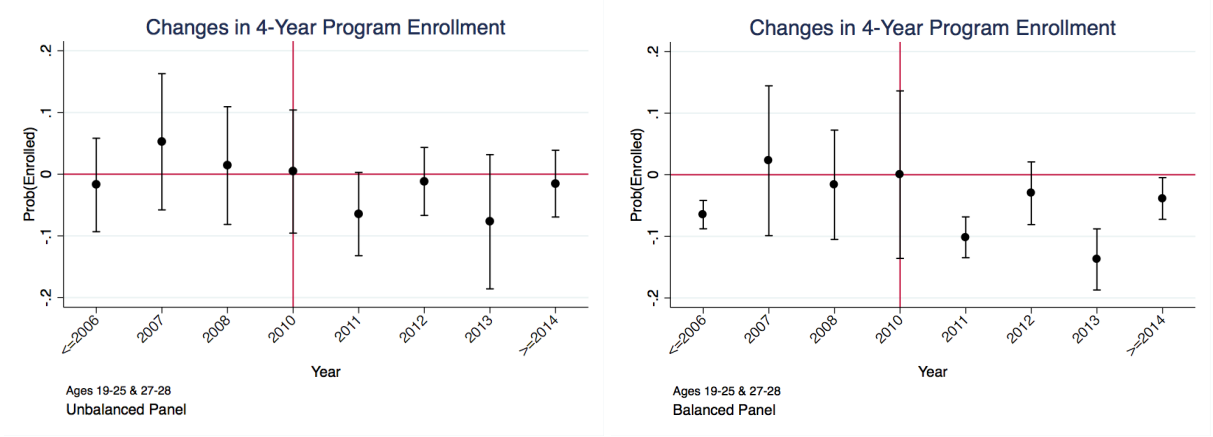


Figure 3.S3: Outcome is enrollment in a 4-year program. Weighted using the CPS October supplement weight. Standard errors are clustered at the state level.

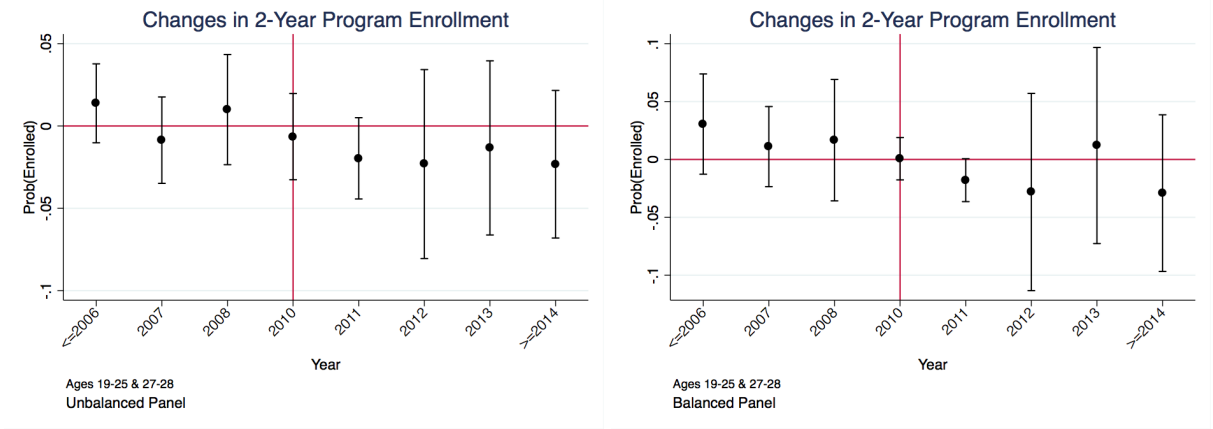


Figure 3.S4: Outcome is enrollment in a 2-year program. Weighted using the CPS October supplement weight. Standard errors are clustered at the state level.

Appendix

Robustness Checks

Table 3.A1: Placebo Tests

	(1) Enrolled	(2) Enrolled FT	(3) 4-Year Program	(4) 2-Year Program
Eligible x YACM	0.00194	-0.0141	0.00700	-0.00505
	(0.00725)	(0.05093)	(0.00611)	(0.00342)
Eligible	0.0928***	0.0783***	0.0563***	0.0364***
	(0.00911)	(0.0115)	(0.00825)	(0.00551)
Observations	26,750	26,750	26,750	26,750
State by Year FE	X	X	X	X
Year by Age FE	X	X	X	X
State by Age FE	X	X	X	X

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Model estimated using Equation (1). Sample is young adults ages 27-28. Eligible is a binary variable equal to 1 if the young adult is age eligible for dependent coverage via a state mandate that requires school enrollment as a condition of eligibility, and 0 otherwise. All models also include dummy variables for state, year, age, race, gender, and ethnicity, plus the full interaction between age and gender. Also included are the two-way interactions between age eligibility and state mandates with marriage restrictions and no education requirements being in effect, and the three-way interactions between age eligibility, state mandates with marriage restrictions and no education requirements being in effect, and the YACM being in effect.

Table 3.A2: Estimates of Mandate Interaction Effects on Education Excluding States that Did Not Have a Mandate (with a student requirement) in All Years From 2005-2009

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample:	Enrolled 19-25	Enrolled 19-25, 27,28	Full Time 19-25	Full Time 19-25, 27,28	4-Year Program 19-25	4-Year Program 19-25, 27,28	2-Year Program 19-25	2-Year Program 19-25, 27,28
Eligible x YACM	-0.0537***	-0.0788***	-0.0459***	-0.0508***	-0.0534***	-0.0455***	-0.000593	-0.0346**
	(0.0173)	(0.0126)	(0.0116)	(0.00842)	(0.0106)	(0.0107)	(0.00807)	(0.0146)
Eligible	-0.0276	-0.00830	-0.0356	-0.0162	-0.0270	-0.0101	0.000209	0.00520
	(0.0330)	(0.0348)	(0.0338)	(0.0368)	(0.0187)	(0.0210)	(0.0184)	(0.0170)
2005-2009 Outcome Mean Among Eligible (%)	36.4	36.2	31.8	31.6	29.3	29.1	7.2	7.1
Observations	98,805	128,307	98,805	127,835	988,05	130,210	100,618	130,210
State by Year FE	X	X	X	X	X	X	X	X
Year by Age FE	X	X	X	X	X	X	X	X
State by Age FE	X	X	X	X	X	X	X	X

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Eligible is a binary variable equal to 1 if the young adult is age eligible for dependent coverage via a state mandate that requires school enrollment as a condition of eligibility, and 0 otherwise. Dropping states that did not have a policy in all years from 2009-2010. Treated states are GA, LA, ND, RI, SD. Standard errors reported in parentheses are clustered at the state level. All estimates produced using CPS October Supplement Weights. All models also include dummy variables for state, year, age, race, gender, and ethnicity, plus the full interaction between age and gender. Also included are the two-way interactions between age eligibility and state mandates with marriage restrictions and no education requirements being in effect, and the three-way interactions between age eligibility, state mandates with marriage restrictions and no education requirements being in effect, and the YACM being in effect.

Table 3.A3: Estimates of Mandate-ACA Interaction Effects on Education (using state of residence 1 year ago)

	(1)	(2)
	Enrolled	Enrolled
Sample:	19-25	19-25, 27-28
Eligible x YACM	-0.0497*	-0.0578***
	(0.0286)	(0.0205)
Eligible	0.0371	-0.0026
	(0.0632)	(0.0476)
2005-2009 Outcome Mean Among Eligible (%)	40.2	39.8
Observations	79,708	98,763
State by Year FE	X	X
Year by Age FE	X	X
State by Age FE	X	X

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Eligible is a binary variable equal to 1 if the young adult is age eligible for dependent coverage via a state mandate that requires school enrollment as a condition of eligibility, and 0 otherwise. Estimates using 2005-2015 ASEC. Standard errors reported in parentheses are clustered at the state level. All estimates produced using CPS October Supplement Weights. All models also include dummy variables for state, year, age, race, gender, and ethnicity, plus the full interaction between age and gender. Also included are the two-way interactions between age eligibility and state mandates with marriage restrictions and no education requirements being in effect, and the three-way interactions between age eligibility, state mandates with marriage restrictions and no education requirements being in effect, and the YACM being in effect.

Additional Analyses

Table 3.A4: Estimates of Mandate-ACA Interaction Effects on Education Using Barkowski & McLaughlin (2020) Treatment Definition

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Enrolled	Enrolled	Full Time	Full Time	4-Year Program	4-Year Program	2-Year Program	2-Year Program
Sample:	19-25	19-25, 27,28	19-25	19-25, 27,28	19-25	19-25, 27,28	19-25	19-25, 27,28
Eligible x YACM	-0.0355**	-0.0144	-0.0232	-0.00939	-0.0379**	-0.0186	0.00240	0.00428
	(0.0176)	(0.0158)	(0.0180)	(0.0122)	(0.0163)	(0.0126)	(0.0101)	(0.0101)
Eligible	-0.0218	-0.0189*	-0.0303**	-0.0248***	-0.0123	-0.0107	-0.00951	-0.00825
	(0.0140)	(0.0105)	(0.0130)	(0.00676)	(0.0145)	(0.00883)	(0.00866)	(0.00568)
2005-2009 Outcome Mean Among Eligible (%)	38.8	37.8	31.3	30.5	27.9	27.2	9.0	8.8
Observations	123,096	159,697	123,096	159,697	123,096	159,697	123,096	159,697
State by Year FE	X	X	X	X	X	X	X	X
Year by Age FE	X	X	X	X	X	X	X	X
State by Age FE	X	X	X	X	X	X	X	X

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: A young adults is coded as eligible if they are living in any state with a dependent coverage mandate and they are at or below the state limiting age. Standard errors reported in parentheses are clustered at the state level. All estimates produced using CPS October Supplement Weights. All models also include dummy variables for state, year, age, race, gender, and ethnicity, plus the full interaction between age and gender. Also included are the two-way interactions between age eligibility and state mandates with marriage restrictions and no education requirements being in effect, and the three-way interactions between age eligibility, state mandates with marriage restrictions and no education requirements being in effect, and the YACM being in effect.

Table 3.A5: Estimates of Mandate-ACA Interaction Effects on Education, No FEs or controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Enrolled	Enrolled	Full Time	Full Time	4-Year Program	4-Year Program	2-Year Program	2-Year Program
Sample:	19-25	19-25, 27,28	19-25	19-25, 27,28	19-25	19-25, 27,28	19-25	19-25, 27,28
Eligible x YACM	-0.0506**	-0.0541***	-0.0467**	-0.0516**	-0.0398*	-0.0430*	-0.0108	-0.0111
	(0.0191)	(0.0192)	(0.0192)	(0.0203)	(0.0217)	(0.0216)	(0.00848)	(0.00724)
Eligible	0.0174	0.0715***	0.0304	0.0814***	0.0371**	0.0774***	-0.0197**	-0.00587
	(0.0193)	(0.0173)	(0.0196)	(0.0175)	(0.0184)	(0.0172)	(0.00892)	(0.00761)
Post YACM	0.0257***	0.0242***	0.0219***	0.0210***	0.0145**	0.0146***	0.0112***	0.00964***
	(0.00722)	(0.00592)	(0.00629)	(0.00530)	(0.00667)	(0.00530)	(0.00400)	(0.00322)
2005-2009 Outcome Mean Among Eligible (%)	38.0	36.0	33.0	31.0	30.0	29.0	7.4	7.3
Observations	111,784	145,034	111,784	145,034	111,784	145,034	111,784	145,034

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Eligible is a binary variable equal to 1 if the young adult is age eligible for dependent coverage via a state mandate that requires school enrollment as a condition of eligibility, and 0 otherwise. Standard errors reported in parentheses are clustered at the state level. All estimates produced using CPS October Supplement Weights.

Table 3.A6: Estimates of Mandate-ACA Interaction Effects on Education & Insurance

	(1) In School & Has Dependent Coverage	(2) In School & Has Other Private Coverage
Eligible x YACM	-0.0285***	-0.00489
	(0.00523)	(0.00635)
Eligible	0.0389	-0.0309
	(0.0257)	(0.0230)
Observations	160,585	160,585
State by Year FE	X	X
Year by Age FE	X	X
State by Age FE	X	X

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Data from 2005-2015 ASEC. 19-24 year old sample. Educational enrollment is only consisted measured for 19-24 year olds. Eligible is a binary variable equal to 1 if the young adult is age eligible for dependent coverage via a state mandate that requires school enrollment as a condition of eligibility, and 0 otherwise. Standard errors reported in parentheses are clustered at the state level. All estimates produced using ASEC Weights. All models also include dummy variables for state, year, age, race, gender, and ethnicity, plus the full interaction between age and gender. Also included are the two-way interactions between age eligibility and state mandates with marriage restrictions and no education requirements being in effect, and the three-way interactions between age eligibility, state mandates with marriage restrictions and no education requirements being in effect, and the YACM being in effect.

Table 3.A7: Estimates of Mandate-ACA Interaction Effects on Education Using Extended Pre-Period: Analysis Years (2001-2015)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample:	Enrolled 19-25	Enrolled 19-25, 27,28	Full Time 19-25	Full Time 19-25, 27,28	4-Year Program 19-25	4-Year Program 19-25, 27,28	2-Year Program 19-25	2-Year Program 19-25, 27,28
Eligible x YACM	-0.0618***	-0.0669***	-0.0704***	-0.0415***	-0.0575***	-0.0477***	-0.00424	-0.0191*
	(0.0140)	(0.0136)	(0.00943)	(0.0147)	(0.00881)	(0.00826)	(0.00792)	(0.0108)
Eligible	-0.0221	-0.0182	-0.0151	-0.0136	-0.0109	-0.0188	-0.0113	0.000614
	(0.0199)	(0.0131)	(0.0120)	(0.0116)	(0.0135)	(0.0139)	(0.0129)	(0.0120)
2005-2009 Outcome Mean Among Eligible (%)	39.0	37.0	32.0	32.0	30.0	29.0	7.5	7.4
Observations	111,784	145,034	111,784	145,034	111,784	145,034	111,784	145,034
State by Year FE	X	X	X	X	X	X	X	X
Year by Age FE	X	X	X	X	X	X	X	X
State by Age FE	X	X	X	X	X	X	X	X

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Eligible is a binary variable equal to 1 if the young adult is age eligible for dependent coverage via a state mandate that requires school enrollment as a condition of eligibility, and 0 otherwise. Standard errors reported in parentheses are clustered at the state level. All estimates produced using CPS October Supplement Weights. All models also include dummy variables for state, year, age, race, gender, and ethnicity, plus the full interaction between age and gender. Also included are the two-way interactions between age eligibility and state mandates with marriage restrictions and no education requirements being in effect, and the three-way interactions between age eligibility, state mandates with marriage restrictions and no education requirements being in effect, and the YACM being in effect.

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