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Author
Lippold, Kye

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Essays on Tax and Transfer Programs Affecting Low-Income Households

A dissertation submitted in partial satisfaction of the
requirements for the degree
Doctor of Philosophy

in

Economics

by

Kye Lippold

Committee in charge:

Professor Julie Berry Cullen, Chair
Professor Gordon Dahl
Professor Itzik Fadlon
Professor Lane Kenworthy
Professor Craig McIntosh

2020

The dissertation of Kye Lippold is approved, and it is acceptable in quality and form for publication on microfilm and electronically:

Chair

University of California San Diego

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DEDICATION

To Emily, who made my dreams come true.

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Chapters 1 and 2 are currently being prepared for submission for publication of the material. Kye Lippold was the primary investigator and sole author of this material.

Chapter 3 is coauthored with Chelsea Swete, and is currently being prepared for submission for publication of the material. Kye Lippold and Chelsea Swete shared writing and analysis responsibilities equally for this chapter.

VITA

2010	Bachelor of Arts in Public Policy (*magna cum laude*), Hamilton College
2010-2012	Research Assistant, Urban Institute
2013-2014	Research Associate II, Urban Institute
2014-2019	Teaching Assistant / Reader, University of California San Diego
2017	Master of Arts in Economics, University of California San Diego
2017	Summer Associate, Congressional Budget Office
2017-2018	Research Assistant, Professors Jim Rauch and Marc Muendler
2020	Ph. D. in Economics, University of California San Diego
2020	Financial Economist, Office of Tax Analysis, U.S. Treasury

PUBLICATIONS

Giannarelli, Linda, Kye Lippold, Elaine Maag, C. Eugene Steuerle, Nina Chien, and Suzanne Macartney (2019). *Estimating Marginal Tax Rates Using a Microsimulation Model*. Washington, DC: Urban Institute.

Lippold, Kye (2015). *Reducing Poverty in the United States: Results of a Microsimulation Analysis of the Community Advocates Public Policy Institute Policy Package*. Washington, DC: Urban Institute.

Giannarelli, Linda, Kye Lippold, Sarah Minton, and Laura Wheaton (2015). *Reducing Child Poverty in the US: Costs and Impacts of Policies Proposed by the Children's Defense Fund*. Washington, DC: Urban Institute.

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Nichols, Austin, Elaine Sorensen, and Kye Lippold (2012). *The New York Noncustodial Parent EITC: Its Impact on Child Support Payments and Employment*. Washington, DC: Urban Institute.

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ABSTRACT OF THE DISSERTATION

Essays on Tax and Transfer Programs Affecting Low-Income Households

by

Kye Lippold

Doctor of Philosophy in Economics

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Professor Julie Berry Cullen, Chair

In three essays, I develop causal evidence on how taxes and transfers affect the behavior of low-income households. Chapter 1 discusses the Child Tax Credit (CTC), a major US tax provision that has received relatively little research attention. I identify the effects of the CTC on labor supply using a difference-in-discontinuities design, exploiting the fact that parents lose eligibility for a child's credit when that child turns 17. Focusing on lower-income households in the Survey of Income and Program Participation, I find that loss of the credit leads to an 8.4 percentage point reduction in the probability a child's parents are employed. The implied elasticity is at the upper bound of previous studies, consistent with a response to a temporary tax change.

Traditional transfer programs in the United States provide few benefits to childless adults, so little is known about the effects of these policies on able-bodied adults without dependents (ABAWDs). In Chapter 2, I examine a novel source of variation in the SNAP (Food Stamps) program, in which unemployed ABAWDs have differential eligibility based a discontinuity in their local area's unemployment rate. I find causal evidence that removing SNAP work requirements decreases hours worked, but also reduces homelessness and property crimes. These findings help inform debates about basic income programs and related policies.

In Chapter 3 (joint work with Chelsea Swete) we examine taxation of diapers, usually seen as an inelastic health product, and find substantial income heterogeneity in responsiveness to taxes using retail scanner data. Exploiting changes to sales tax exemptions for diapers in New York and Connecticut, we find that diaper sales rise by 5.4% in low-income areas when taxes are removed, accompanied by reduced spending on children's pain medications. These results imply that sales tax exemptions for diapers can have positive spillover effects on health and well-being.

Taken together, this research helps inform policymakers about the broader effects of taxes and safety net programs on low-income Americans.

Chapter 1

The Effects of the Child Tax Credit on Labor Supply

1.1 Introduction

The Child Tax Credit (CTC) has been a consistently favored tax policy for over twenty years, with the Clinton, Bush, Obama and Trump administrations all expanding the credit. Both Republicans and Democrats proposed increases to the credit during 2017 tax reform negotiations, and California legislators passed a similar credit in 2019 for families with young children (Matthews 2017, 2019). However, relatively little research has examined the effects of the CTC, despite its similarity as a wage subsidy to the Earned Income Tax Credit (EITC), a tax policy that has attracted considerable interest for its effects on labor supply, health, and educational outcomes (Nichols and Rothstein 2016; Hotz and Scholz 2003; Hoynes, Miller, and Simon 2015; Dahl and Lochner 2012). In this paper, I examine the labor supply effects of the CTC as an initial step towards understanding how this policy has affected behavior.

Because the EITC and CTC have similar policy designs, my results on the CTC provide further evidence of the effectiveness of tax policies structured to encourage work. Policymakers have expressed interest in continuing to expand credits like the EITC that subsidize earnings, given their positive effects on a variety of dimensions, but most studies of the effects of the EITC were performed using data from the 1980s and 1990s (Eissa and Liebman 1996; Meyer and Rosenbaum 2001; Grogger 2003; Eissa and Hoynes 2004; Nichols and Rothstein 2016). There have been considerable changes in the economic and policy environment since that period, including welfare reform and the Great Recession, and some evidence suggests labor supply responses have declined for more recent tax reforms (Heim 2007; Kumar and Liang 2016). Furthermore, some recent research has questioned the previous literature on the labor supply effects of the EITC (Kleven 2019). Thus, it is valuable for policymakers to know whether labor supply responses to expansions of refundable tax credits have remained as strong as has been found in the past. Since the CTC subsidizes wages similarly to the EITC, it can provide evidence on these responses using new econometric techniques.

The CTC also provides evidence relevant to an ongoing debate in labor economics regarding the strength of labor supply responses. While most microeconomic evidence of labor supply responses to taxes find extensive margin elasticities in the range of 0.2 to 0.4 (Chetty et al. 2013), such results are typically identified from shocks that induce permanent changes in wages, and thus capture steady-state elasticities rather than the intertemporal substitution elasticities most useful for calibrating macroeconomic business cycle models. The CTC change I use reflects an anticipated temporary wage change, equivalent to the loss of a wage subsidy in a single year, so provides an ideal setting to measure intertemporal substitution effects from taxes for a large, low-income subpopulation.

To identify the labor supply effects of the CTC, I exploit the fact that credit eligibility is based on the number of children under age 17 in a tax unit as of the end of the tax year. For example, a family with a child born on January 1st, 1994 will receive a wage subsidy through the CTC for the entire year of 2010, while a family with a child born one day earlier on December 31st, 1993 will receive no wage subsidy for 2010. Other tax and transfer policies related to children typically change at ages 16 or 18, not age 17. If other characteristics of parents evolve smoothly around the age 17 cutoff, comparing the employment rates of parents with children just above and below the age 17 cutoff through a standard regression discontinuity (RD) design identifies the effect of the CTC on labor supply.

In practice, characteristics of children born in January and December differ due to seasonal factors, decisions related to timing of births, total years of exposure to the CTC policy, and incentives related to school starting ages (Bound and Jaeger 1996; Buckles and Hungerman 2013; LaLumia, Sallee, and Turner 2015), complicating a standard RD design. However, as long as these differences are consistent over time, it is possible to factor out such patterns through a difference-in-regression discontinuities (DiRD) approach (Grembi, Nannicini, and Troiano 2016). This method combines a traditional RD and a difference-in-differences model, using changes in treatment over time between the treated and control groups (in this case, children above and below the December birth month cutoff) to identify causal effects.

I estimate a DiRD design using data from the Survey of Income and Program Participation (SIPP, U.S. Census Bureau 2019), focusing particularly on households in 2001 to 2016 with prior year incomes below $20,000 (the range most subsidized by the CTC). My main results suggest an 8.4 percentage point fall in employment among low income households with children over the age 17 cutoff, giving an extensive margin elasticity with respect to the return to work of 1.04 for employment and 0.59 for labor force participation. These values are upper bounds due to intertemporal substitution effects, but a rough calibration exercise suggests these elasticities are consistent with steady-state values of 0.43 and 0.47, respectively. Thus, my results suggest that tax credits promoting employment for low-income workers continue to have strong labor supply effects.

Section 2 discusses the history of the CTC and reviews prior literature on the credit. Section 3 examines the CTC's theoretical effects on labor supply. Section 4 describes the identification strategy and data. Section 5 provides results, and Section 6 concludes.

1.2 Background

1.2.1 History of the CTC

The Child Tax Credit arose out of tax reform in the 1980s (Crandall-Hollick 2018). Members of Congress believed that the falling real value of the dependent exemption no longer adjusted taxable income in line with a family's ability to pay, violating principles of equity; furthermore, observers noted that most other developed nations provided some form of child benefit to families (National Commission on Children 1991). Accordingly, several proposals were floated in the early 1990s to provide new tax deductions for families with children, including an $800 per child credit in Bill Clinton's 1992 presidential platform (Woodward 1994)[1] and a $500 per child credit in the Republican party's famous 1994 Contract with America (Crandall-Hollick 2018). After the parties failed to reach an agreement in 1995, the issue remained on the Washington agenda, and the CTC became law as part of the Taxpayer Relief Act of 1997, which provided a credit of $500 per child under age 17.[2] The original credit was effectively nonrefundable (i.e., unavailable to families that did not owe taxes), as it could only be claimed up to the amount of tax liability.[3]

A notable element of the introduction of the credit was the limitation of the tax break to families with children under 17. Negotiators in the House, Senate, and Clinton administration had proposed various age cutoffs for the credit, ranging from age 13 to age 18, but the final proposal adopted age 17 as a compromise (Esenwein and Taylor 1997; Stevenson 1997). Importantly, this age 17 cutoff is unlike the other age cutoffs for child benefits in the tax code; parents can claim the dependent exemption and the EITC until their children turn 19 (or turn 24 when the child is a full-time student). This institutional detail is key to my identification of the credit's effects.

Four years after its enactment, the CTC expanded substantially as part of the Bush tax cuts of the early 2000s (Crandall-Hollick 2018). The credit value was increased to $600 in 2001 and $1,000 in 2003, and the 2001 tax cut also gave the CTC a refundable component increasing its potential subsidy to work. The credit's refundable component, officially called the "Additional Child Tax Credit" (but referred to as the "refundable portion" of the CTC in this paper), was adjusted so that taxpayers owing taxes less than their maximum CTC could claim the remaining

[1] Clinton's campaign proposed this credit, but it was not pursued after his election due to budgetary concerns (Woodward 1994, p. 31 and 154). However, Clinton's Treasury department proposed a $300 CTC in response to the Republican credit (Crandall-Hollick 2018), suggesting the issue was not forgotten by either party.

[2] In the initial year of 1998, the credit was $400, but could be claimed early through reduced withholding (Michel and Ahmad 2012).

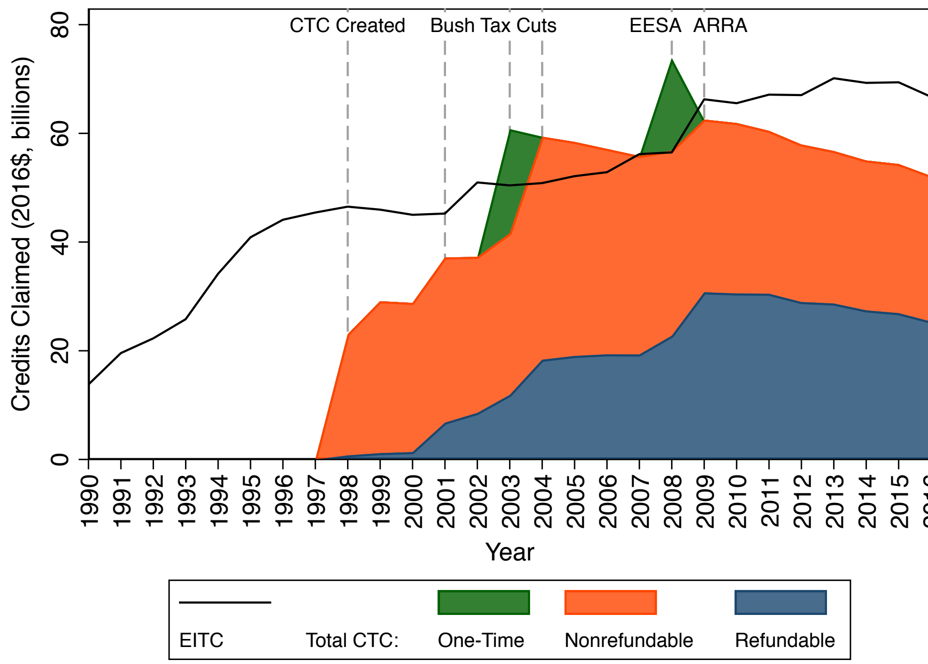
[3] The original CTC was refundable for families with three or more children, but only to the extent that their payroll taxes exceeded the family's EITC. Accordingly, refundable CTC amounts were only 3-5% of total CTC expenditures in the first few years of the credit (author's calculations from Internal Revenue Service 2018). This alternate payment formula still exists in current law, but as a family with three or more children will almost always have EITC exceeding payroll taxes at low levels of income (as the EITC subsidizes earnings at a 34 to 45% rate, whereas payroll taxes are 7.65%), this formula is rarely used and is not a focus in this or other studies of the CTC.

CTC amount up to 10% of earnings above $10,000, with the threshold adjusted for inflation.[4] One rationale for this initial threshold was to counteract high marginal tax rates from the EITC at low levels of income; the policy intention was to have the CTC phase in as the EITC phased out, lowering overall work disincentives (Sawhill and Thomas 2001; Tax Policy Center 2019b). Further expansions in this vein followed; the subsidy amount was increased to 15% in 2004, and the nominal threshold amount was lowered to $3,000 in 2009 as part of economic stimulus legislation during the Great Recession (Crandall-Hollick 2018). The 2017 Trump tax bill lowered the threshold slightly, to $2,500, and increased the total amount of the credit available to $1,400 per child (or $2,000 when nonrefundable). Thus, the CTC has subsidized income at low levels in a manner similar to the EITC since 2001. (The main CTC parameters are listed in Table A.1).

Because the maximum value of the CTC has never been indexed for inflation, the real value of the credit has fallen over time. Figure 1.1 presents total spending on the CTC in the years of its existence, as compared to the EITC; the discrete jumps in the level of the the CTC with legislated expansions are clear, as well as the gradually declining value of the credit in real terms. Notably, the total dollars distributed through the CTC surpassed the EITC briefly in 2003, and have remained at comparable levels ever since, indicating the importance of the CTC in the tax and transfer system.[5]

[4]As Steuerle (2004) notes, this proposal effectively adopted the CTC policy proposed by Sawhill and Thomas (2001).

[5]The calculations for total CTC received in this figure differ in 2003 and 2008 from those in other sources, such as from the Tax Policy Center (2019a) and Hoynes and Rothstein (2016), because the IRS Statistics of Income series on which such figures are based do not include either 1) the 2003 CTC expansion, which was distributed as a rebate to taxpayers based on their 2002 incomes in advance of the usual tax filing period (Internal Revenue Service 2005, p. 12), and 2) the Recovery Rebate Credit enacted by the Economic Stimulus Act of 2008, which gave a $300 tax credit to nearly all taxpayers for each CTC eligible child (based on 2007 tax information), and was thus effectively a one-time expansion of the CTC. The 2003 one-time rebate is further discussed by Johnson, Parker, and Souleles (2009) and Michel and Ahmad (2012), while the 2008 rebate is discussed by Parker et al. (2013) and Shapiro and Slemrod (2009). Thus, Figure 1.1 adds the amount of CTC distributed early ($14.7 billion 2003 dollars, as reported by the IRS (2004, 2004, p. 16)) to the CTC in 2003, and the amount of the Recovery Rebate Credit that I calculate as accruing to families with children (computed as the total base CTC times 0.3; this is a slight underestimate, as the credit also included more generous phase-ins and phase-outs than the regular CTC) to the CTC in 2008.



Source: Author's calculations from Internal Revenue Service (2018, Table A) and 2004 (2004).
Notes: EESA refers to the 2008 tax year stimulus bills (both the Economic Stimulus Act and the Emergency Economic Stabilization Act), and ARRA is the 2009 Obama stimulus package (American Recovery and Reinvestment Act). "One-time" areas refer to expansions of the credit that lasted for only one year.

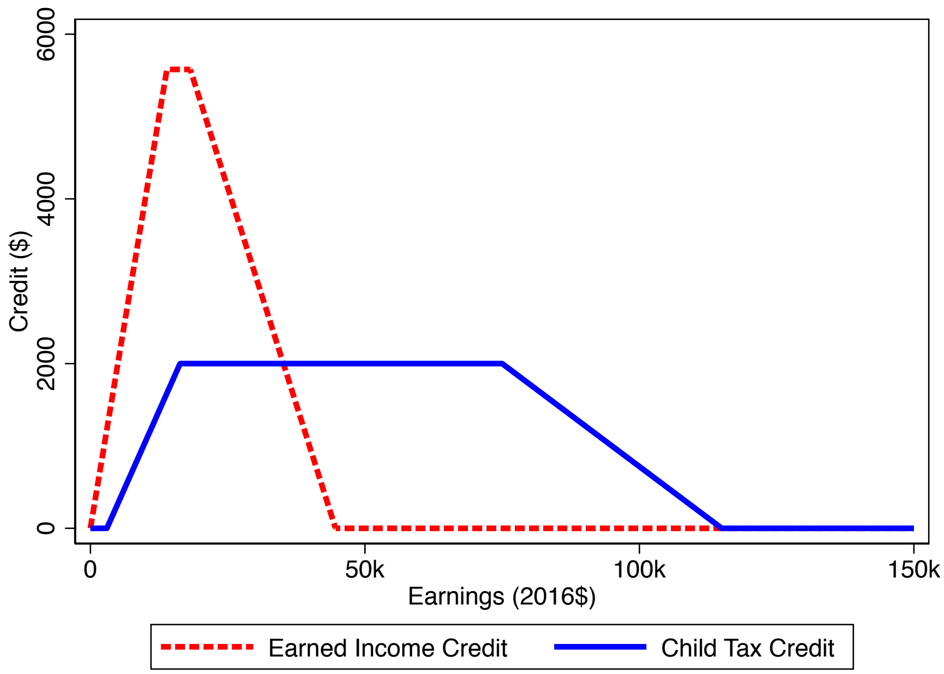
Figure 1.1: Expenditures on the CTC and EITC over Time

1.2.2 Design of the CTC

To illustrate how the Child Tax Credit affects a family's tax liability during the period of my study, Figure 1.2 compares the 2016 amount of the CTC to the EITC for a single parent with two children and all income from wages, salaries, or self-employment (earned income), computed using the Taxsim model (Feenberg and Coutts 1993).[6] The two credits have virtually identical designs in practice: each subsidizes earned income at a given rate (40% for a two-child EITC, and 15% for the CTC) up to a maximum level, remains constant with income for a plateau period, then is phased out (at a 21.06% and 5% rate, respectively) with increasing income. The CTC is smaller in value than the EITC, but extends to higher income levels, meaning it provides its

[6]Results are very similar for 1) married taxpayers, who face the same slopes but begin the phase-out for both the EITC and CTC at a higher income level, and 2) differing numbers of children, which rescales the maximum level of the credits but does not change the slopes.

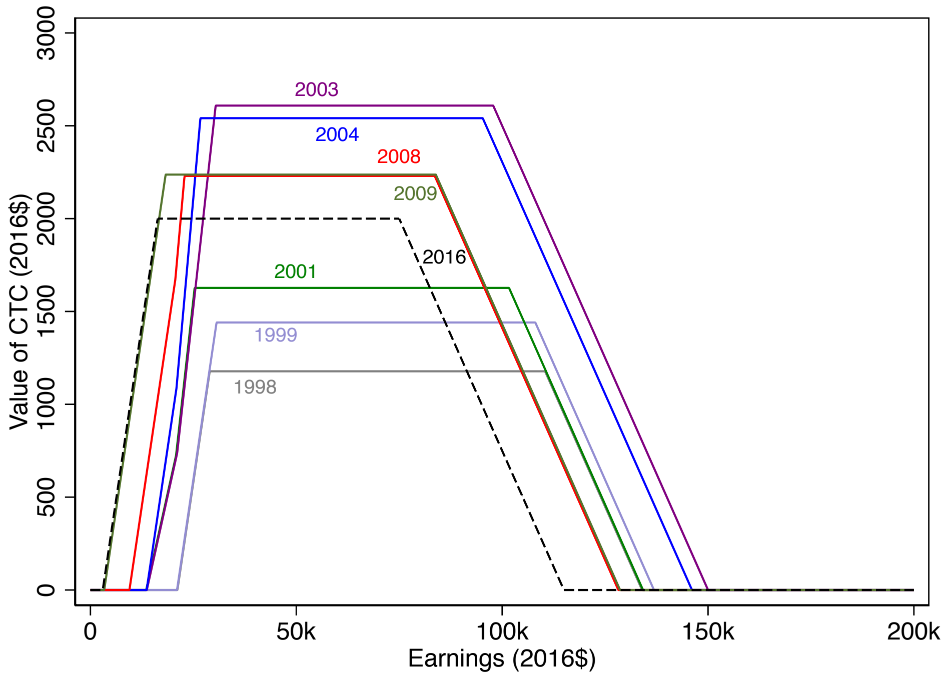
subsidy to most families in the population; the CTC also subsidizes earnings over $3,000 (or $2,500 since 2018), rather than subsidizing from the first dollar as for the EITC.



Source: Author's calculations using Taxsim. Calculations assume no unearned income or itemized deductions.

Figure 1.2: Amount of CTC and EITC for a Single Parent with Two Children, 2016 Tax Law

I next illustrate in Figure 1.3 the changes to the CTC's design over time, with all values adjusted to 2016 dollars. A few key factors stand out. First of all, the CTC has maintained a similar structure, with the value of the credit phasing in and out at similar rates; this held true even prior to the enactment of the refundable CTC, because the credit offsets taxes in the lowest rate bracket. Secondly, the credit has fluctuated in value over time; its real value was lowest at enactment in 1998, rose in the mid 2000s due to the Bush tax cuts, and has fallen since 2003 due to inflation. Thirdly, a large percentage of the credit's benefits have shifted from wealthier families to low-income families, due to both 1) legislated reductions in the point where the refundable credit phases in for lower incomes, and 2) inflation lowering the real income level where the credit phases out ($75,000 in nominal dollars for a single parent). The schedule of credits for other family arrangements are similar, and are shown in Appendix Figure A.3.[7]



Source: Author's calculations using Taxsim. Calculations assume no unearned income or itemized deductions. Years listed are those where the main CTC parameters were changed by law, as well as 2016.

Figure 1.3: Amount of CTC for a Single Parent with Two Children, Tax Laws for Various Years

An implication of these calculations is that the bulk of the CTC for most families comes through the refundable CTC formula of 15% of earnings over $3,000. While it is possible to receive a nonrefundable CTC that offsets taxes on unearned income, the standard deduction and exemptions for most taxpayers reduce taxable income to zero in the range where the CTC phases in, meaning there is no tax to offset. Nonrefundable credits are thus of little value to families with income under $30,000, and most such families receive a CTC that is directly based on earnings.[8]

Overall, the CTC functions very similarly to the EITC, providing a directly subsidy to earnings above a low threshold level. Theoretically, this means the CTC should encourage

[7]Note that after my period of study, the 2018 credit was expanded considerably by the Trump tax reform bill; however, the dependent exemption was also eliminated by that bill, so the new CTC is better compared to total tax benefits from the CTC plus dependent exemption in earlier years.

[8]For example, Williams (2013) finds over 99.5% of tax units with children and incomes under $30,000 owe no income tax in 2013. Those figures include the CTC, but my calculations with my 2001-2016 SIPP panel and Taxsim confirm that 96.0% of such families owe no income tax in this dataset, and 94.3% of such families would owe no tax even without the nonrefundable CTC.

increased labor supply, as discussed in Section 1.3.

1.2.3 Prior Research on Labor Supply Effects of the CTC and Other Tax Credits

As noted by Marr et al. (2015), the effects of the CTC by itself are typically not identified in the literature; this occurs in part because the CTC and EITC phase in over similar income ranges, making it difficult to disentangle their effects.

The most relevant study on the CTC comes from Feldman, Katuščák, and Kawano (2016), which uses the loss of the CTC when children turn 17 as a source of exogenous variation (in the same manner as this paper). However, this study focuses on higher income households, for whom the loss of the CTC is a lump sum change in taxes (and thus should generate only an income effect on labor supply). The authors find that loss of the CTC leads to a 0.5 percentage point reduction in earnings growth for the *next* year (an implied intensive margin elasticity of 0.3), but no change in earnings in the year for which credit eligibility is lost. They interpret this effect as taxpayer "confusion" between marginal and average tax rates.[9] While this is suggestive of no intensive margin response to the loss of the CTC, their paper only examines taxpayers with prior year income from $30,000 to $100,000, who are unlikely to be on the extensive margin. My study therefore expands on Feldman and coauthors by including lower income taxpayers and studying extensive margin changes, where EITC studies have shown stronger effects.[10] Because extensive margin responses are less subject to frictions (Chetty 2012), it is plausible that the intensive

[9]Notably, the authors do not discuss the potential role of average tax rates on labor supply; while they interpret their finding as a reduction in labor supply that is irrational, because marginal rates have not changed, the theoretical results presented in Section 1.3 suggest that it can be optimal to change labor supply in response to a change in *average* rates in the presence of fixed costs of work. Feldman et al.'s robustness checks also show that the effect on labor income is strongest for households with lower incomes (the $30,000-$55,000 range), which could be consistent with extensive margin responses by a portion of this group. Furthermore, because they examine labor supply in the year *after* CTC eligibility changes, their use of variation in eligibility for the CTC cannot be separated from the effect of children turning 18 in the tax year, which could have effects on labor supply through channels other than the CTC.

[10]These authors showed similar results to their 2016 study using SIPP data in a previous working paper (Feldman and Katuščák 2012); this study expands on those results by examining the extensive margin.

margin elasticities found in Feldman et al.'s paper will be a lower bound for the extensive margin effect of the credit.

Looney and Singhal (2006) examine changes in marginal tax rates associated with the loss of a dependent as children age (relative to families that do not lose a dependent).[11] While their method is similar to the one used in this study, exploiting changes in children's ages as a source of identification, their exogenous change in taxes uses the loss of dependent status rather than the loss of the CTC. A limitation of this approach is that children can be claimed as dependents until age 19 or age 24 if a full-time student; thus, the loss of the dependent exemption for a household can be correlated with endogenous college enrollment decisions. Also, their data only include the first two years of the CTC, when the credit was not refundable, and focuses on changes in the intensive margin (using marginal tax rates) rather than the extensive margin (with average tax rates). Their intensive margin elasticity estimate of 0.75 is close to those reported in studies of the EITC; this estimate includes individual fixed effects, and is thus an intertemporal labor supply elasticity (and therefore similar to the temporary wage variation I identify).

Another recent paper examines the effects of child tax benefits (including the CTC) on the labor supply of mothers immediately following their children's births, also using a discontinuity in child age around the end of the tax year (Wingender and LaLumia 2017). The authors find that an additional $1,000 of child tax benefits from having a December rather than a January birth leads to a 5-6 percentage point reduction in maternal labor supply in the third month following the birth. My study examines a different margin, the change in *prior year* incomes based on *fully anticipated* variation in the tax schedule. To the extent that the exact timing of a new birth could fall in either December or January, one would not necessarily expect mothers to anticipate their eligibility for tax benefits and alter their labor supply during pregnancy; indeed, as Wingender and LaLumia (2017) show in their Figure 2, mothers of newborns do *not* anticipate the change in eligibility for subsidies, as there is no discontinuity in their earnings in the year prior to birth.

[11] Dokko (2007) provides a similar study that examines changes in labor supply in response to the aging of dependents.

Thus, my result illustrates how parents respond to a fully anticipated tax change rather than an unanticipated income shock. Additionally, mothers of newborn children tend to have low levels of labor supply and unique needs in terms of childcare, meaning their responsiveness to taxes may be less representative of the overall population than the older parents that I study.

By comparison, prior research on the EITC has typically found strong labor supply elasticities, around 0.7 on the extensive margin and 0.1 on the intensive margin (Hotz and Scholz 2003; Nichols and Rothstein 2016; Meyer and Rosenbaum 2001). As a representative example, Grogger (2003) finds that a $1,000 increase in the EITC is associated with a 3.6 percentage point increase in employment for single mothers, which roughly corresponds to an elasticity of 0.64.[12] However, these estimates are typically identified from difference-in-differences approaches, which rely on unanticipated, permanent changes to the tax system (and thus identify steady-state elasticities, in the terminology of Chetty et al. (2013)); as intertemporal substitution Frisch elasticities are bounded below by steady-state Hicksian elasticities (Browning 2005), the results from this study may have higher magnitudes than the EITC results.

A recent strand of research has questioned some of the earlier findings of strong extensive margin effects of the EITC. Chetty, Friedman, and Saez (2013) find a smaller effect using a more plausibly exogenous identification strategy based on differences in knowledge across neighborhoods; their extensive margin elasticity is around 0.19 (but 0.6 in the top decile of knowledge about the EITC). Quite recently, Kleven (2019) criticizes the inconsistent responses to the EITC in difference-in-differences studies and provides evidence that the extensive margin elasticity is closer to zero. More generally, Heim (2007) and Kumar and Liang (2016) have found declining population-wide labor supply elasticities over time; these may be related to increasing participation of women in the labor force and the effects of welfare reform, both of which could leave fewer workers on the extensive margin. Thus, it will be of interest to see if the measured

[12] This elasticity is computed based on an average income of $12,300 in 1998 dollars and employment rate of 69 percent (per Table 1 in Grogger (2003)), and thus is somewhat overstated because it uses gross income rather than the return to work (Chetty et al. 2013).

effects of the CTC in a later time period and using a different identification strategy are of similar magnitude to those found for the EITC.

1.3 Theoretical Effects of the CTC on Labor Supply

The effects of the CTC on labor supply have not been thoroughly examined; as Marr et al. (2015) note, the CTC has not been studied extensively, due to its relatively recent introduction to the tax code. In this section I discuss the theoretical effects of the CTC on labor supply, drawing on a standard labor supply model, the fixed cost model of Eissa, Kleven, and Kreiner (2008), and a new dynamic fixed cost model.

1.3.1 Static Intensive Margin Effects

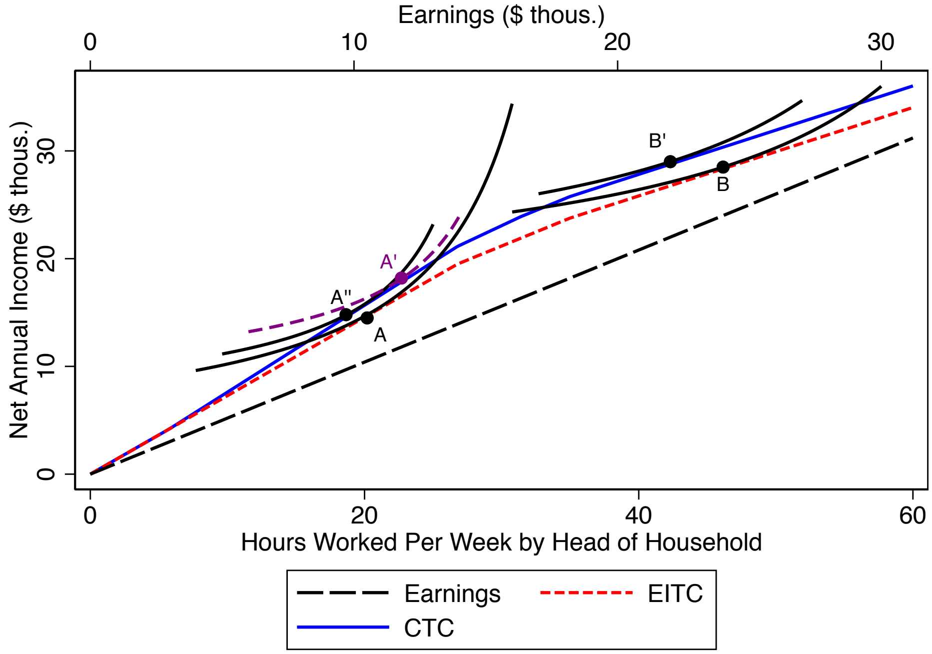
I first examine the CTC through the lens of a standard labor-leisure tradeoff, considering households that maximize utility $U(c,l)$ over bundles composed of hours of leisure l and consumption c. Household budget sets are given by earned income (an exogenous wage w times hours of work h), less (possibly nonlinear) taxes $T(wh)$, yielding

$$c = wh - T(wh)$$

while hours of leisure are equal to a time endowment e less hours of work h.

Notably, in the presence of an EITC and CTC, the budget set takes on the familiar "hump" shape seen in Nichols and Rothstein (2016, Figure 2.9). This shape is depicted in Figure 1.4. To generate empirically relevant estimates, I compute results for a single worker with two children using Taxsim, assuming 52 weeks of work per year at a wage of $10.00 per hour. Since hours of work reduce leisure, indifference curves are increasing to the upper left of this figure.

The EITC (red dashed line) generates a "hump" on the simple linear budget set wh (black



Source: Author's calculations using Taxsim. Household consists of a single parent with two children, working for 52 weeks at $10.00 per hour.

Figure 1.4: Budget Set with EITC and CTC for a Single Worker with Two Children, 2016 Policies

line), first subsidizing income in the phase-in region, then remaining constant as a plateau, and finally phasing out with additional hours of work. The CTC (blue solid line) augments the effects of the EITC by adding a new wage subsidy above $3,000 in earnings, which continues phasing in until about 31 hours of work per week. At this point, the CTC becomes a lump sum transfer; because the CTC does not phase out until a high income level, a low wage worker sees no increase in marginal tax rates at plausible values of hours worked.

Thus, the CTC effectively increases the subsidy provided by the EITC, providing differential effects on hours worked depending on whether a worker is in the subsidy or plateau region. In the subsidy region, the effect on labor supply is ambiguous, because the positive substitution effect towards work could be counterbalanced by a negative income effect. If the substitution effect dominates, the worker would move to an indifference curve with higher work hours, as from point A to point A'; if the reverse is true, the worker would move to an indifference curve that decreases work hours, such as A''. However, in the plateau and phase-out regions, the labor supply effect of the CTC is theoretically predicted to be negative. On the plateau, the consumer faces the same wage rate as without a CTC, so experiences a pure income effect increasing leisure. The consumer should thus shift from an indifference curve like B to a curve like B' and reduce hours of work. For the phase-out range, this process is amplified by a negative substitution effect on hours.

1.3.2 Static Extensive Margin Effects

Given prior research on the EITC that finds the strongest effects on the extensive margin, it is important to simulate extensive margin behavior in detail to get an accurate picture of the CTC's effects. Therefore, I consider a static model incorporating fixed costs of work, following Eissa, Kleven, and Kreiner (2008). As these authors note, fixed costs of work are important for tax reforms to generate welfare effects on the extensive margin; if extensive margin responses arise only from continuous movements across a reservation wage distribution, any tax revenue (and thus welfare) effects will be infinitesimal for a small change in after-tax wages. This detail, along with the fact that low hours of work are rarely observed empirically, suggests a model with fixed costs is more appropriate than the standard model to assess the CTC.

To illustrate such a model graphically, I augment the simplified budget set in Figure 1.4 by adding fixed costs of work to the model. I also add the value of transfer programs and taxes, using the Urban Institute's Net Income Change Calculator (NICC, 2016).[13] I use NICC to

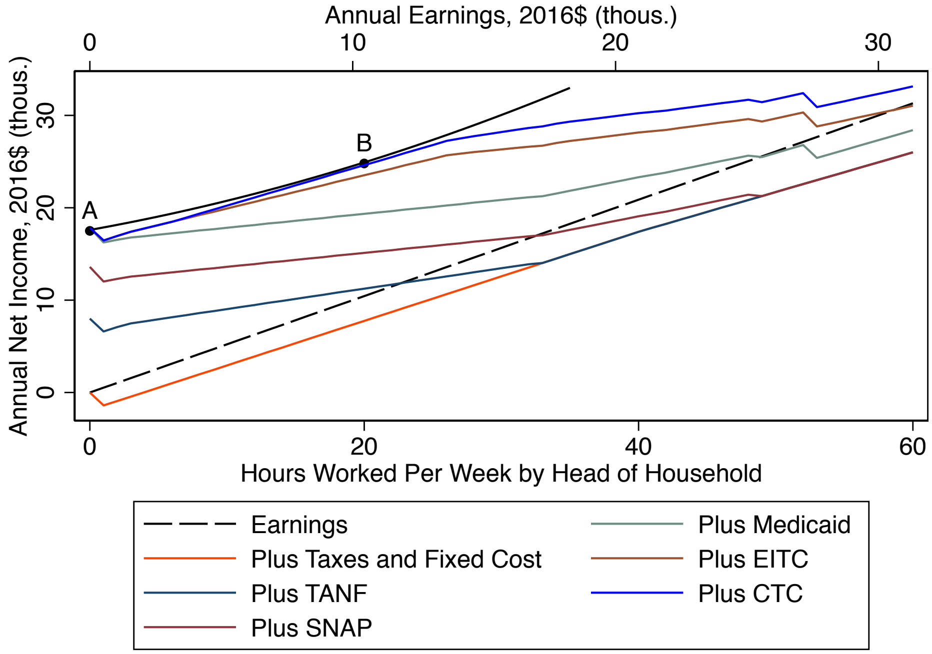
[13]The NICC is a tool that allows users to input family characteristics and receive computations of the levels of taxes and transfer programs for those families. It has been used previously in the tax literature by Maag et al. (2012). The NICC uses 2012 tax and transfer rules, but to make the results more comparable to the present day, I augment the NICC calculations by adjusting SNAP benefits to remove the temporary increases of 2009 stimulus legislation, remove the temporary payroll tax cut in place from 2011 to 2012, and add the 2014 value of Medicaid and CHIP under the Affordable Care Act (valuing at the cost per enrollee from Kaiser Family Foundation (2019), multiplied by 0.5 to reflect valuation of Medicaid below cost, as discussed in Finkelstein, Hendren, and Luttmer (2019)). Thus, the transfer system depicted in the figures roughly reflects the taxes and transfer system in 2016.

I assume that the example family lives in California, has no income or assets besides labor income, pays \$600 in monthly rent, and works for 52 weeks at a wage of \$9.60 per hour in 2012 dollars (which makes each hour increase in work increase annual income by roughly \$500, and is roughly equal to the \$9.57 deflated value of the \$10 2016 compute the value of welfare benefits (Temporary Assistance for Needy Families (TANF) and the Supplemental Nutrition Assistance Program (SNAP, formerly food stamps)) and income and payroll taxes for the example household, and assume that the worker will incur $150 per month in fixed costs when working positive hours (which generates extensive margin effects). The results in Figure 1.6a indicate that the CTC should have positive extensive margin labor supply effects for single parents. While the budget set has numerous kinks and cliffs, the CTC (the topmost blue line) offsets fixed costs; as shown in the figure, a household located at a corner solution with no work (point *A*) could be induced to work positive hours (point *B*) by the CTC's subsidy.

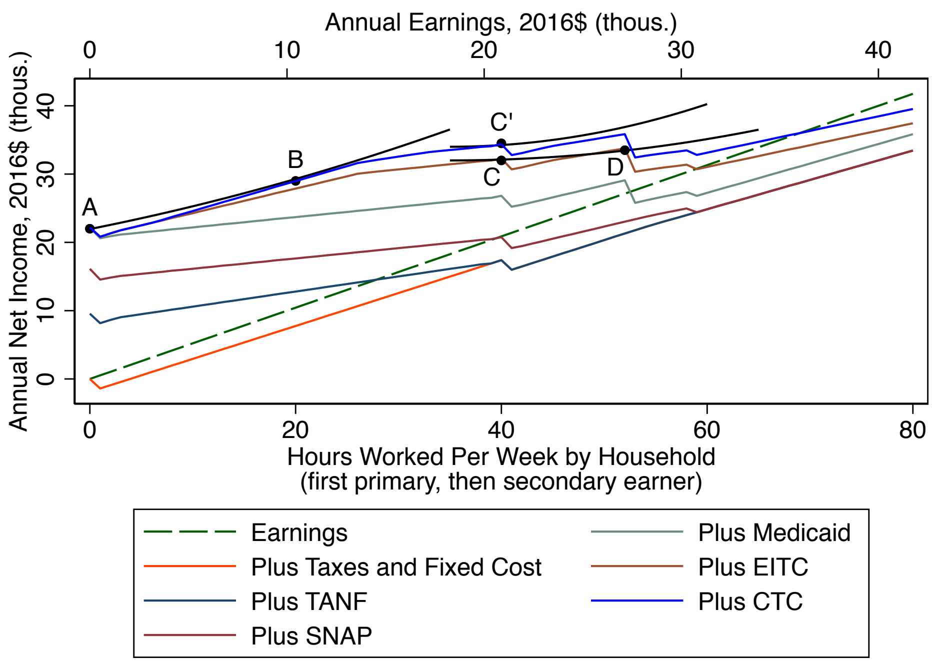
I also illustrate a similar budget set for a married couple with two children in Figure 1.6b. This model assumes both individuals work at the same wage, and that one individual (the primary earner) first works up to 40 hours per week, after which the secondary earner begins work up to 40 hours per week. Work expenses are incurred after each individual begins to work. In this scenario, the CTC (topmost blue line) again provides a positive substitution effect on the primary earner's labor supply, potentially moving the family from no work (point *A*) to work by the primary earner (point *B*). However, the CTC is fully phased in when the secondary earner begins to work, and thus only has an income effect for this worker. This raises the reservation wage, and could potentially move a secondary earner who was indifferent about working (between points *C* and *D*, so working enough to locate at the Medicaid cliff described in Yelowitz (1995)) to not working at all (point C'). Since this response only comes through an income effect, the magnitude is likely small (as income effects tend to be dominated by substitution effects for secondary earners, who have positive compensated elasticities (Blundell and Macurdy 1999)). Thus, the implication of this model is that the CTC should encourage labor supply by the primary earner in low-income families, but have little or negative effects for secondary earners in those families.

Thinking intuitively about these findings, the CTC lowers the average tax rate of working

California minimum wage). Note that TANF and Medicaid policies in California are typically more generous than in other states, although the other tax and transfer policies used for this exercise are mostly consistent across the country.



(a) Single Worker with Two Children



(b) Married Couple with Two Children

Source: Urban Institute (2016) and author's calculations (see text for details). Household consists of a single parent or married couple with two children, working for 52 weeks at roughly $10.00 per hour. Assumes $150 per month in work expenses, incurred once when the primary earner begins to work, and again when the secondary earner begins to work (after the primary earner works 40 hours).

Figure 1.5: Budget Sets with Fixed Costs Illustrating Extensive Margin Effects

for primary earners but not secondary earners (as it has typically already phased in for secondary earners). As Eissa, Kleven, and Kreiner (2008) discuss, the relevant parameter for extensive margin labor supply decisions is the average tax rate rather than the marginal tax rate. Because the CTC lowers average tax rates on work, it makes work unambiguously more attractive to primary earners.

1.3.3 A New Model of Dynamic Extensive Margin Effects

The nature of the variation in this study means I must also consider intertemporal substitution effects. My design compares families with children just over age 17 to those just under age 17 at the end of a tax year. Thus, the "control group" children in the natural experiment of the RD design will turn 17 and lose the credit next year; the two groups only face different tax rates for the one year period of "treatment." This design is similar to that used by Fehr and Goette (2007), who randomly increased the wages of Swiss bicycle messengers in two alternating periods to investigate intertemporal substitution. In both cases, a group with a net wage change is compared to a control group that will have a similar wage change in the next period, creating transitory and anticipated income variation. My identification strategy thus provides estimates of an intertemporal substitution elasticity.

It is well known that for intensive margin elasticities with additively separable utility, one can link the steady-state (static) elasticity to the intertemporal substitution elasticity using the formula (Browning, Deaton, and Irish 1985; Chetty 2012):

$$\varepsilon_S = \varepsilon_I - \rho\,(\omega)^2\,\frac{A}{wh} \tag{1.1}$$

which relates the compensated steady-state intensive elasticity ε_S to the intertemporal substitution intensive elasticity ε_I via the elasticity of intertemporal (consumption) substitution ρ, the income effect ω, the level of assets A, and earnings wh. Note that the intertemporal substitution elasticity is (weakly) greater than the steady-state elasticity.

To understand how the elasticity estimates from this study compare to steady-state estimates from other studies, I derive a similar formula to equation (1.1) relating the two elasticities in the extensive margin case. To do so, I develop a dynamic model of discrete labor force participation extending the work of Eissa, Kleven, and Kreiner (2008). A summary of the model follows; full details are given in Appendix A.2.

Consider a continuum of households who live for τ periods indexed by $t = 1, \ldots, \tau$ and maximize a concave, continuous utility function $U[C_t]$ over consumption C_t to solve

$$V[A_t, \theta_t] = \max_{p_t, A_{t+1}} U[C_t] - F p_t + \beta E_t [V[A_{t+1}, \theta_{t+1}]]$$

$$\text{s.t. } A_{t+1} = (1 + r_t)(A_t + Y_t - C_t)$$

$$Y_t = N_t + G_t + p_t w_t (1 - a_t)$$

where p_t is an indicator for labor force participation, A_t are assets, Y_t is non-asset income, and β is a discount rate. Wages w_t, non-labor income N_t, government transfers G_t, and interest rates r_t are given exogenously in each period. The parameter $\theta_t = \{w_t, G_t, N_t, \nu_t, r_t\}$ is a vector of the exogenous state variables.

The key extensions of the model relative to a standard dynamic labor supply model (such as Macurdy 1981) are the inclusion of fixed costs of work and taxes. Households are identical except for a varying disutility of participating in work F distributed according to a generic CDF $\phi[F]$, drawn once and then held fixed for each household. I define a_t as the average tax rate faced when going to work (due to taxes paid and transfers forgone). Given these parameters, households choose whether to participate in work and how much they will save for the next period.

To simplify notation, I define variables with a superscript as those evaluated at the superscript level of p_t. For example, U^1 (or U^0) is utility evaluated at the working (nonworking) state in that period. I then define $\Delta x = x^1 - x^0$ as the difference between variables in the work and non-work states.

The solution to the model involves an Euler equation pinning down an optimal savings level in each work state, $A_{t+1}^{*p_t}$ (depending on marginal utility in each state λ^{p_t}) and a cutoff condition

$$\Delta V^{*} = \Delta U + \beta E_t \left[\Delta V_{t+1} \right] \geq F \tag{1.2}$$

As noted by Blundell and Macurdy (1999), extensive margin labor supply models in a dynamic setting do not typically have simple closed-form solutions, because the amount of assets saved in each period will depend on past and future work decisions (unlike models of intensive margin labor supply, where the savings decision and work decision can be considered independently). To make the model tractable, I consider the case where individuals are making decisions in response to small changes in tax rates. Thus, we can approximate optimal savings with a first order Taylor expansion,

$$\Delta A_{t+1}^{*} = \alpha \Delta Y_t = \alpha w_t \left(1 - a_t\right)$$

where $\alpha = \frac{\partial A_{t+1}^{0*}}{\partial Y_t^0}$ is the marginal propensity to save out of current period income when not working.

I then simplify the model further by taking a second order Taylor expansion of equation (1.2). (Note that a second order expansion is needed, rather than first order, to account for consumption smoothing.) This approximation leads to an expression for the probability of labor force participation in each period:

$$P_t = E\left[p_t\right] = \phi\left[\Delta V^{*}\right]$$

and gives the intertemporal elasticity of substitution ε_I with respect to the next-of-tax rate (for an anticipated temporary tax change) as

$$
\begin{aligned}
\varepsilon_I &= \frac{\partial P_t}{\partial (1-a_t)} \cdot \frac{1-a_t}{P_t} \\
&= \frac{\phi' [\Delta V^*]}{\phi [\Delta V^*]} \lambda^0 w_t (1-a_t) \left(1 + \frac{\lambda_c^0}{\lambda^0} w_t (1-a_t) \left(1 - \frac{2\alpha}{1+r_t} + \frac{(2+r_t)\alpha^2}{(1+r_t)^2} \right) \right)
\end{aligned}
$$

where the last term (depending on α) reflects the degree of desired consumption smoothing as a function of the marginal propensity to save and return on assets.

The steady-state elasticity ε_S (for an anticipated permanent tax change) will involve no consumption smoothing motive, so $\alpha = 0$ in this case. Using this fact, and recognizing that many of the terms can be rewritten in terms of empirically measurable parameters (which can be taken as constant over a small tax change), I compute the relationship between the two elasticities as

$$
\varepsilon_I \approx \left(\frac{1 - \frac{\gamma W_t}{1-s_t} \left(1 - \frac{2\alpha}{1+r_t} + \frac{(2+r_t)\alpha^2}{(1+r_t)^2} \right)}{1 - \frac{\gamma W_t}{1-s_t}} \right) \varepsilon_S \tag{1.3}
$$

This gives an expression for the ratio of the two elasticities that depends on the coefficient of relative risk aversion γ, the marginal propensity to save α, the interest rate on assets r_t, the savings rate s_t, and the percentage change in post-tax income when working W_t. I then use these results to compare my elasticity estimates to steady-state values from other studies in Section 1.5.4.

Note that for plausible values of the parameters (as shown in Figure A.2), the value of the multiplier term in equation (1.3) is positive, meaning the intertemporal substitution elasticity will be larger than the steady-state elasticity (consistent with the intensive margin case discussed above).

1.3.4 Summary

Overall, Figure 1.4 indicates that the CTC could increase intensive margin labor supply relative to the EITC, depending on the relative magnitude of the income and substitution effects for each worker. Importantly, prior research on the EITC has suggested substitution effects of tax changes strongly outweigh income effects in magnitude (Nichols and Rothstein 2016); this would suggest the CTC's labor supply effects are positive on net for low-wage workers. As the model in Section 1.3.2 further shows, the extensive margin labor supply effects of the CTC are unambiguously positive for primary earners, as the CTC always reduces average tax rates and thus lowers reservation wages. Secondary earners should see little effect. Lastly, the model in Section 1.3.3 shows that the effects I find should be larger than those found in prior studies, reflecting the short-term change in incentives identified in this study that leads to intertemporal substitution.

1.4 Method

1.4.1 Data

I use the 1984 to 2014 panels of the Survey of Income and Program Participation (U.S. Census Bureau 2019), focusing on the 2001 to 2016 period where the refundable CTC was in effect. The SIPP is ideal for my purposes because it provides a large nationally representative panel of US households, is available both before and after the CTC policy was enacted, and provides information on month of birth for children, allowing construction of the running variable in an RD design. While the data collected by the SIPP has changed over the years (particularly for the 1996 and 2014 panels), it is broadly consistent for most key variables.

After importing the SIPP data (using Nichols 2008), I collapse all SIPP monthly characteristics to an annual level, using December characteristics for demographic information and rolling up income across months of the year.[14] I follow Looney and Singhal (2006) in imputing incomes for missing months based on the average for the rest of the year; this allows me to approximate characteristics of households as they are treated by the tax system, which uses calendar year incomes and determines marital status and age cutoffs based on December values. I generate information on tax liabilities and the amount of CTC received by each tax unit using the NBER Taxsim calculator (Feenberg and Coutts 1993), subject to several assumptions detailed in Appendix A.1. My main income measure is post-tax income, which includes labor income, non-labor income, taxes, and cash transfers, but I also use Adjusted Gross Income from the tax code (AGI, which excludes most cash transfers and taxes) to identify households most subsidized by the credit. I standardize ages within panels, using household and person IDs to compare ages for each record, and take the earliest reported (non-imputed) modal age where values differ.

I define the broad analysis sample as all children who are dependents of a tax unit and have ages within 6 months of turning age 14 to 17 at the end of the year. I also require at least one of the children's parents to be observed in the SIPP for 8 or more months in both the current and prior year (to ensure tax liabilities and labor supply reflect full year values).[15]

My primary sample is a low-income subset of the overall SIPP. While results in the broader population provide some information about labor supply responses, most households with children have incomes that place them beyond the phase-in region of the CTC, where labor supply effects should theoretically be strongest. Thus, to focus on families who are likely making extensive margin decisions, I exploit the panel nature of the data and focus on the subsample of children with prior year parental AGI of $0 to $20,000 in 2016 dollars (roughly the bottom 25% of all households with age 14-17 children). While current year income is endogenous to CTC eligibility, the use of prior year income helps isolate low income households using a predetermined characteristic. This subsample thus consists of lower income families who face

[14] In the case of attrition before December, I use characteristics from the last observed month in the year.

[15] I exclude two other small groups: children whose parents had no tax dependents (due to inconsistencies in the SIPP data) or had invalid ages (negative or only imputed values).

the greatest incentive to work due to the CTC, and functions similarly to the restrictions to single parents or workers with low education in studies of the EITC's effects (Meyer and Rosenbaum 2001; Grogger 2003; Eissa and Hoynes 2004; Kleven 2019).

I focus on two primary outcomes. The first is whether either of a child's parents are employed during the calendar year, as measured by an indicator for positive labor income (wages or self-employment, which are the income sources subsidized by the CTC). This measure is comparable to previous studies of the effects of tax credits on labor supply (Eissa and Liebman 1996; Meyer and Rosenbaum 2001; Eissa and Hoynes 2004), which typically use indicators for any work last year. However, while such employment measures are often referred to as "labor force participation" (LFP) in the existing literature, a more comprehensive definition of participation would include unemployed parents, who are willing to supply labor (as measured by looking for work) but have not found jobs. To the extent that unemployment reflects time for a search and matching process to take place, the CTC should subsidize both work and efforts to find work (as it raises the expected value of working). I thus also examine whether children's parents are ever in the labor force (employed or unemployed) during the year.

The unit of analysis for this study is a child, as each child's age determines their parents' eligibility for that child's CTC subsidy. A child's parents' tax unit can be composed of either two individuals (if parents are married) or one individual (if the child lives with only one parent or guardian, or if the parents are unmarried). My measures of outcomes examine behavior for the "first mover" in each tax unit (i.e., the presence of any employment or participation), following the analysis of Figure 1.6b, which suggested secondary earners will typically not face greater incentives to work due to the CTC.

Table 1.1 presents descriptive statistics for all households with children in the studied age range of 13.5 - 17.5 years (as detailed below), and those in the primary sample of lower-income households (with average income of about $19,600 in 2016 dollars). As the table shows, the primary sample reflects a disadvantaged group of parents; 73% are single parents, 27% have less than a high school degree (and only 8% have a college degree), and 61% are racial minorities.

1.4.2 Identification Strategy

To investigate the effects of the Child Tax Credit on labor supply, I exploit the age cutoff for the credit. A convenient feature of the CTC for research purposes is that it provides a credit for each child in the tax unit aged 16 or below; thus, a child who turns 17 on January 1st of a year will entitle his or her parents to a credit for the previous year, but an otherwise identical child born one day earlier on December 31st would not provide a credit for that year. Thus, performing a regression discontinuity using dates of birth for families with children near age 17 in the previous year can potentially provide internally valid estimates of the effects of the CTC on parental employment for the population of families with older children.[16]

Using the age 17 cutoff is valuable for several reasons. The cutoff is unrelated to all the major tax and transfer benefits available to families with children, which sunset when a child turns 18 or 19. In particular, the dependent exemption and EITC end at age 19 (or age 24 if the child is a full-time student); the child care credit and child care subsidies end at age 13; and children receive special treatment under SSI and SNAP until age 18. TANF rules change at age 16, 18 or 19 depending on the state and child's student status, Medicaid and CHIP end at age 19, and WIC benefits end at age 5. (All parameters taken from Urban Institute (2019)). Additionally, other major changes in children's living situations (such as leaving home or graduating high school) tend to change at ages other than 17. This implies that changes at age 17 are likely to reflect the effect of the CTC rather than the loss of other programs or lifecycle factors.

Caveats to Identification: Birth Timing and Seasonality

A natural concern about the use of an age cutoff is the possibility of parents manipulating birth timing to gain eligibility for the CTC. Prior research has established that some parents do

[16] As noted above, this source of variation has been used previously by Feldman, Katuščák, and Kawano (2016).

Table 1.1: Descriptive Statistics for All Households and Primary Sample

	All Households	Primary Sample
Share of tax units with earnings	0.91	0.63
Share of tax units in labor force	0.93	0.72
Either parent is non-white	0.43	0.61
Parents are married	0.66	0.27
Highest Parent Education		
< High School	0.11	0.27
HS Grad	0.19	0.29
Some College	0.36	0.36
College Grad	0.20	0.06
Advanced Degree	0.14	0.02
Post-tax income (2016$)	58,286.64	19,629.09
	(50,780.30)	(17,573.99)
	[-193,947.39—797,144.44]	[-77,207.22—258,318.23]
Amount of CTC received (2016$)	1,401.55	652.44
	(1,321.77)	(964.07)
	[0.00—11,426.12]	[0.00—9,114.30]
Child Age as of end of tax year	15.47	15.46
	(1.16)	(1.15)
	[13.50—17.42]	[13.50—17.42]
Parent Age (max)	45.65	44.27
	(7.67)	(9.52)
	[16.00—88.00]	[16.00—88.00]
Tax unit number of dependents	2.45	2.55
	(1.24)	(1.35)
	[1.00—14.00]	[1.00—11.00]
Observations	41619	9443

Notes: Values are means, with standard deviations in parentheses and minimum/maximum in brackets. Statistics are presented for all children in the 2001 to 2014 SIPP panels who are age 13.5 to 17.5 at the end of the year, live with their parents, whose parents have valid (non-imputed) ages and dependents for federal taxes, have non-imputed birth months, and have at least one parent observed for 8+ months in the current and prior year. Primary sample is further restricted to children whose parents have AGI in the prior year below $20,000 in 2016 dollars. Due to the need for full-year and lagged data, excluded years are 2001, 2004, and 2008.

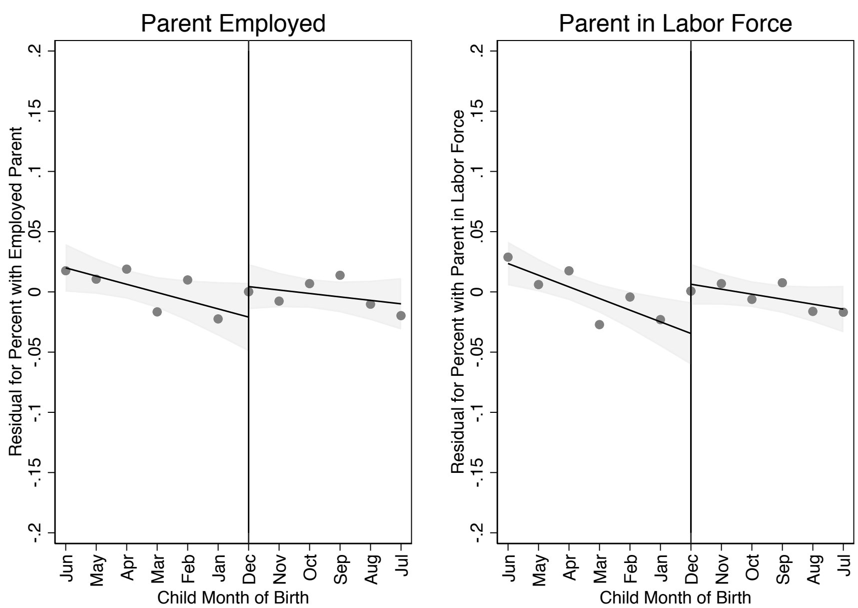
alter the timing of their births around end of year or other date cutoffs to gain earlier eligibility for tax credits (Dickert-Conlin and Chandra 1999; Gans and Leigh 2009; Schulkind and Shapiro 2014). If parents are able to change when their children are born using elective Cesarean sections or induced labor, this would imply that parents can manipulate their eligibility for the CTC, meaning that births would nonrandomly fall in December or January and thus potentially bias estimates of the CTC's effect.

These concerns may be well founded. A recent study by LaLumia, Sallee, and Turner (2015) using all income tax returns in the US suggests that birth timing effects are present (although small in magnitude); they estimate that $1,000 of child tax benefits (including the CTC) are associated with a 1 percentage point shift of births from early January to late December. Similarly, Schulkind and Shapiro (2014, table 1) find that mothers giving birth in December and January differ in several demographic characteristics (although by less than 1 percentage point). While the bias from birth timing appears relatively small, it will be addressed explicitly in this analysis, as discussed below.

In addition to the concern of manipulation of birth timing, the RD results could be influenced by seasonal trends in birth outcomes. A long literature has noted that children born in the winter tend to have worse later economic outcomes than children born in the summer (Buckles and Hungerman 2013; Schulkind and Shapiro 2014; Bound and Jaeger 1996). In particular, Buckles and Hungerman (2013) find that while children born to parents who were not planning to have children display no seasonal patterns in parental characteristics, parents who wanted to have children disproportionally give birth in the spring and summer. To the extent that this makes parents not demographically similar across the December / January cutoff, the RD results will be biased.

These seasonal effects could be compounded in my data because the SIPP only includes date of birth at the monthly level, hiding daily variation that could show more smoothness across the cutoff. Furthermore, for most of my sample period, December children receive one fewer years of exposure to the CTC than January children (since the credit was introduced after their birth, and the December children age out sooner). This means the two groups differ in terms of lifetime wealth.

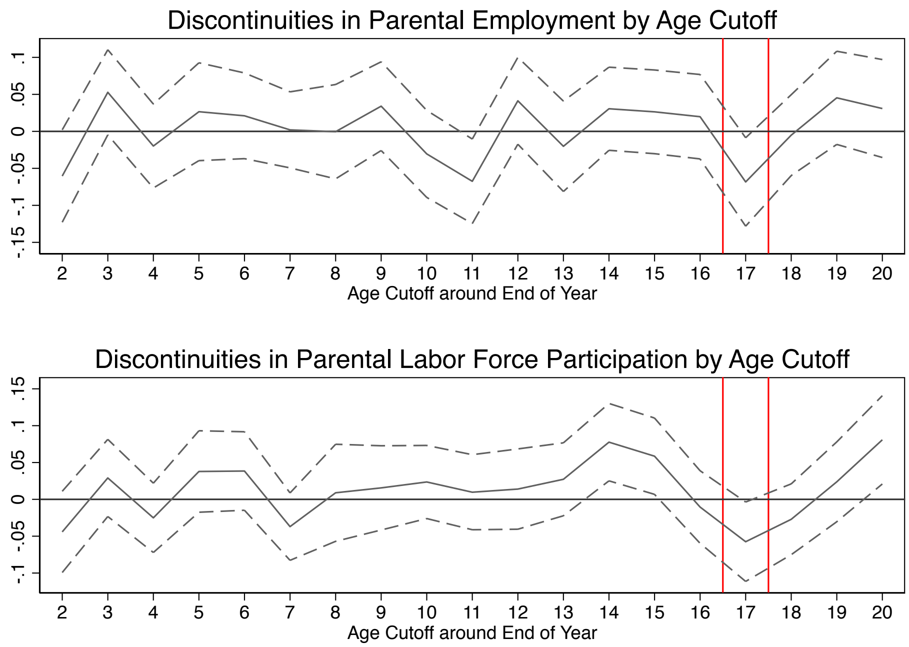
Some evidence for these biases is apparent in Figure 1.6, which displays an RD design for the jump at December for the dependent variables of interest (parental extensive margin labor supply and labor force participation) among children in the primary sample for the age ranges *before* the CTC cutoff occurs, around the age cutoffs from 14 to 16. In general, low-income parents of children born in December are more likely to be employed or in the labor force than parents of children born in January, even when both are eligible for the CTC. (The estimate for employment is not statistically significant, but the point estimate is positive).



Notes: This figure presents regression discontinuity estimates among children aged 13.5 to 16.5 (i.e, around the age 14 to age 16 end-of-year cutoffs). Discontinuities are estimated in primary sample with local linear regressions, uniform kernels, in 6-month bandwidths centered around each December age cutoff. Shaded areas are 90 percent confidence intervals. Results are estimated on residuals from a regression including lagged outcomes, year and state fixed effects, and other controls (as discussed in text).

Figure 1.6: Discontinuities Prior to Loss of CTC Eligibility, Ages 14-16

This seasonal pattern is fairly consistent over time. Figure 1.7 illustrates how this seasonality influences estimated discontinuities around various potential age cutoffs (e.g., an age cutoff at age 16 compares children turning 16 in December to those turning 16 in January) within a window of 6 months on either side of the cutoff. While the results are imprecise due to low sample sizes for each individual age, and any given discontinuity could reflect either seasonal factors or responses to policies at other age cutoffs, the point estimates of discontinuities are more often positive for children aged 2 to 16 (especially for labor force participation). This suggests the seasonal variation in outcomes is persistent. The drop in the discontinuities at age 17, where parents lose CTC eligibility, represents a break from the trend.



Notes: Discontinuities are estimated in primary sample with local linear regressions, uniform kernels, in 6-month bandwidths centered around each December age cutoff. Dashed lines are 90 percent confidence intervals. Age 1 is excluded due to imprecision. Results are estimated on residuals from a regression including lagged outcomes, year and state fixed effects, and other controls (as discussed in text). Results without controls are presented in Figure A.8.

Figure 1.7: Discontinuities by Age Cutoff

To address this consistent difference in characteristics, I make use of a difference-in-regression discontinuities (DiRD) design (Grembi, Nannicini, and Troiano 2016). The motivation for this design is that while there may be seasonal differences in demographics (observed and unobserved) between parents of children born in different months, those seasonal difference should be constant over time. The estimated discontinuity for children at ages before 17 can thus identify the seasonal and birth timing effects, which are then differenced out from the CTC's effect at age 17. Under the assumptions that the seasonal differences are constant and that the effect of the CTC does not itself depend on seasonality (that is, parents with children born in different months respond similarly to the incentives, as discussed by Grembi, Nannicini, and Troiano (2016)), the DiRD design will identify the causal effect of the loss of the CTC at age 17 on labor supply.

One potential concern about the age 17 cutoff is that some states set their minimum school-leaving ages at 17, meaning factors besides the CTC could be changing at that age. Such a response would require parents to change their labor supply in advance of their children actually leaving school (as their child would age out of school in December of the tax year, but labor supply is measured from the previous January to December), which seems implausible, but I investigate this factor explicitly in robustness checks.

1.4.3 Empirical Model

My preferred reduced form specification is given in equation (1.4), which represents a DiRD design.

$$
\begin{aligned}
y_{it} = \alpha + \delta D_{it} + \phi_1 A_{it} D_{it} + \phi_2 A_{it}\left(1 - D_{it}\right) \\
+ \left(\gamma + \beta D_{it} + \phi_3 A_{it} D_{it} + \phi_4 A_{it}\left(1 - D_{it}\right)\right) T_{it} + \theta X_{it} + \varepsilon_{it}
\end{aligned}
\tag{1.4}
$$

I define y_{it} as the outcome variable of interest (an indicator for employment or labor force participation) for the tax unit of child i for year t; the model is thus a linear probability model. The running variable A_{it}, capturing age, is the birth month of each child relative to December of year t (normalized so that the December cutoff is zero, November is +1, January is -1, and so forth, until June is -6 and July is +5).

As is standard for a regression discontinuity design, I define D_{it} as an indicator for the discontinuity (equal to one if a child is born in December through July), and include both D_{it} and interactions of D_{it} with A_{it} to estimate the slopes of the running variable on both sides of the cutoff. This corresponds to a local linear regression with a uniform kernel within my estimation window. The baseline coefficient δ captures the discontinuity at the December-January birth month cutoff for ages prior to the loss of the CTC (estimated by pooling among all the ages before 16.5 included within the regression).

I then incorporate the "difference" aspect of the design by interacting the initial RD variables with T_{it}, an indicator for the child being age 16.5 or above. Thus, the coefficient of interest β indicates the jump in outcomes when the running variable crosses the threshold (i.e., the child turns 17 in December of year t) relative to the jump in outcomes for December births at earlier ages. The interactions with functions of A_{it} allow changes in slopes of the running variable around age 17 as well.

Equation (1.4) includes additional variables X_{it} beyond a standard RD framework to increase precision. I include the lagged dependent variable $y_{i(t-1)}$ and other covariates X_{it} in some specifications, namely year and state fixed effects, parental race (non-white), education (5 categories), age (of the oldest parent, quadratic), number of dependents, marital status (an indicator for married parents), and indicators for residence in a metropolitan area and the current and lagged number of months observed. I use the SIPP monthly weights for December in my analysis, and follow Nielsen et al. (2009) and Seay and Nielsen (2012) in clustering standard errors for my results by SIPP panel, variance stratum code, and half-sample code to account for the clustered survey design.

A key element of an RD design is the choice of the estimation window. In the case of this design, there are two windows to consider: the bandwidth for the RD design around each cutoff, and the number of age cutoffs prior to 16.5 to pool when estimating the baseline seasonal discontinuity D_{it}. Because of the cyclical nature of the data, where an age A_{it} value that is far to the right of one December cutoff is inherently located to the left of the next December cutoff, my bandwidth cannot exceed 6 months without having overlapping observations (preventing separate estimation of linear trends). Thus, I choose a 6 month bandwidth as the default to maximize power (Figure 1.15 shows the sensitivity of results to other bandwidths). Similarly, I consider multiple windows of ages to include as "pre-treatment" ages, and adopt 13.5 - 16.5 as the preferred period based on the fact that such children are all around high school age and past the age 13 discontinuity in eligibility for the Dependent Care Tax Credit (Figure 1.13 shows the estimates are quite stable regardless of the age chosen).

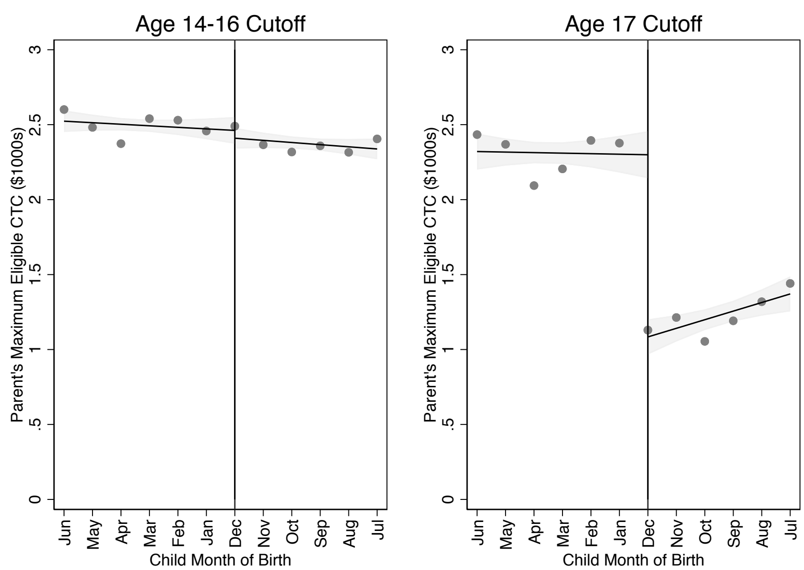
To directly link the effects of the CTC to the change in outcomes at age 17, I also estimate an intent-to-treat (ITT) estimate modeled after a fuzzy RD design (Lee and Lemieux 2010), comparing the jump in the maximum value of CTC received by parents at the cutoff to the jump in outcomes. To do so, I estimate equation (1.4), but estimate a first stage coefficient β_c by replacing the dependent variable with C_{it}, the maximum potential value of the CTC received by the parents of child i as of December of year t, with the CTC value in 2016 dollars inflated by the Consumer Price Index. This CTC maximum value depends only on the number of CTC-eligible children in the family, and will thus mechanically fall at the cutoff due to the loss of a CTC eligible child. I then simultaneously estimate the reduced form and first stage using seemingly unrelated regression. The resulting ratio of the two discontinuities, β/β_c, represents an ITT estimate of the change in probability of employment per dollar of CTC eligibility lost at the cutoff. When rescaled by the mean levels of employment and the gain in post-tax income from employment in the sample, this estimate provides an elasticity measure comparable to other studies.

My hypothesis is that the CTC increases parental labor supply; thus, I expect extensive margin labor supply and labor force participation to fall after the loss of the credit at age 17. The effects of gaining the CTC could differ from the effects of losing the CTC; in particular, individuals who are induced to enter the labor force by the credit could find it easier to remain employed even after the credit expires. However, the counterfactual of gaining the CTC cannot be cleanly identified, as only the age 17 cutoff differs between the CTC and other tax and transfer benefits. My results reflect a temporary (one year) anticipated shock to labor supply due to the CTC, as the parents whose children are located to the left of the cutoff will automatically lose CTC eligibility for their children in the following year.

1.5 Results

1.5.1 Graphical Results

I first present reduced form graphical results, illustrating the presence of discontinuities at the age 17 and earlier cutoffs in my estimation window for the time period of interest (2001 and later, when the CTC was refundable). Each point in these graphs represents outcomes for children in a given month of birth (the lowest level of birthdate aggregation available in the SIPP).[17]



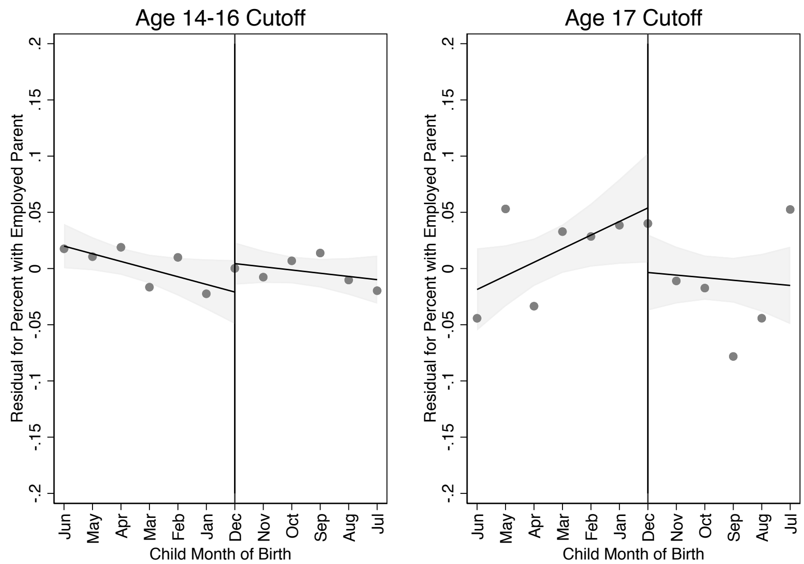
Notes: Discontinuities are estimated in primary sample with local linear regressions, uniform kernels, in 6-month bandwidths centered around each December age cutoff. Shaded areas are 90 percent confidence intervals.

Figure 1.8: First Stage RD Results for CTC Eligibility of Child's Parents

I first show that the maximum CTC does drop at the cutoff, as expected, in Figure 1.8.

[17] Additional figures are provided in Appendix A.3, Figures A.4 to A.7.

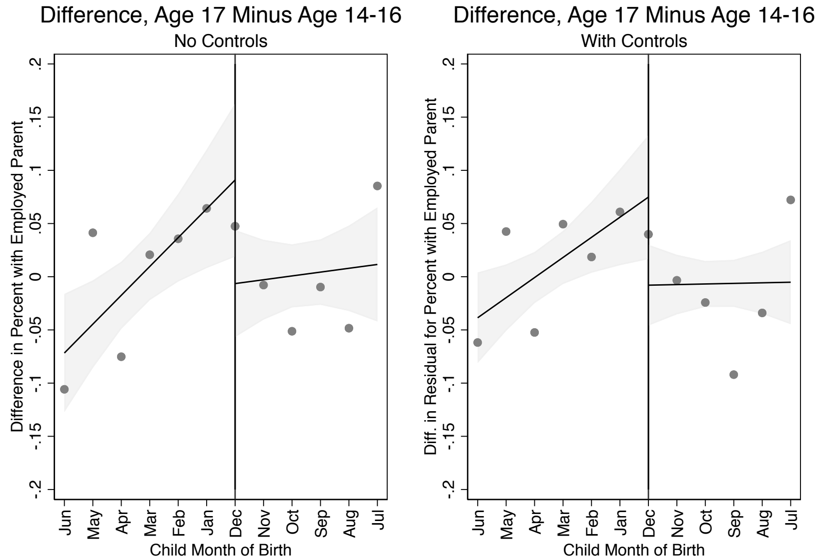
Because Taxsim automatically computes the value of the CTC using the number of under-17 children in each tax unit, the large discontinuity seen here is a mechanical result of the tax model. However, it is reassuring that Taxsim generates a large drop in the amount of CTC eligibility at the age 17 cutoff, with no discernible discontinuity in the credit received at ages 14 - 16. (Note the value of the credit does not drop to zero at age 17, because many parents with a child turning 17 have other younger children who remain eligible for the credit). Thus, the first stage (indicating the credit is reduced by the cutoff) appears valid.



Notes: Discontinuities are estimated in primary sample with local linear regressions, uniform kernels, in 6-month bandwidths centered around each December age cutoff. Shaded areas are 90 percent confidence intervals. Results are estimated on residuals from a regression including lagged outcomes, year and state fixed effects, and other controls (as discussed in text).

Figure 1.9: RD Results for Parental Employment

Turning to the primary reduced form results, Figure 1.9 shows a standard linear RD design. The data suggest a discontinuity in the expected direction, with a drop in the proportion of children in tax units with working parents at the age 17 cutoff (although the results are imprecise). However, the discontinuity at ages 14 -16 has the opposite sign, suggesting that seasonal factors (leading parents with December children to have higher levels of employment than parents of January children) are partially offsetting the drop in outcomes at age 17.

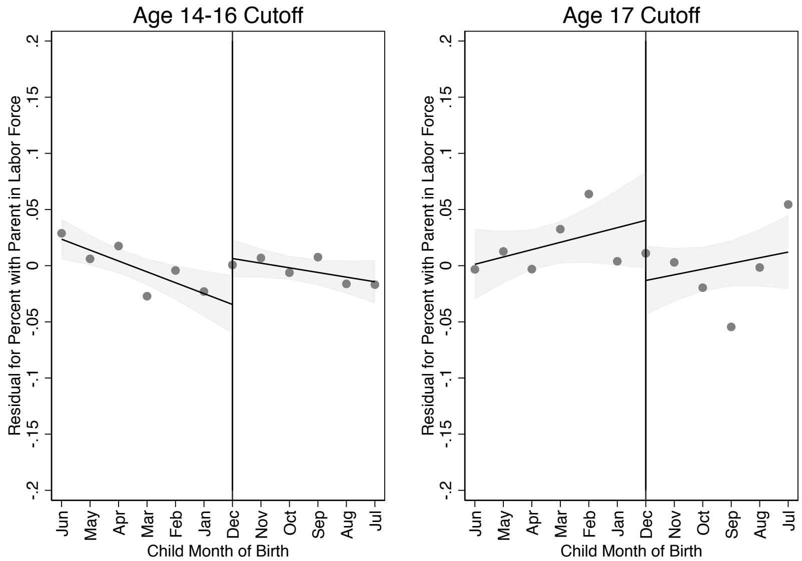


Notes: Each point in the figures represents the difference in mean outcomes by month of birth between children around the age 17 cutoff (ages 16.5 to 17.5) and children at the age 14 -16 cutoffs (ages 13.5 to 16.5). Discontinuities are estimated in primary sample with local linear regressions, uniform kernels, in 6-month bandwidths centered around each December age cutoff. Shaded areas are 90 percent confidence intervals. Results with controls are estimated on residuals from a regression including lagged outcomes, year and state fixed effects, and other controls (as discussed in text).

Figure 1.10: DiRD Results for Parental Employment

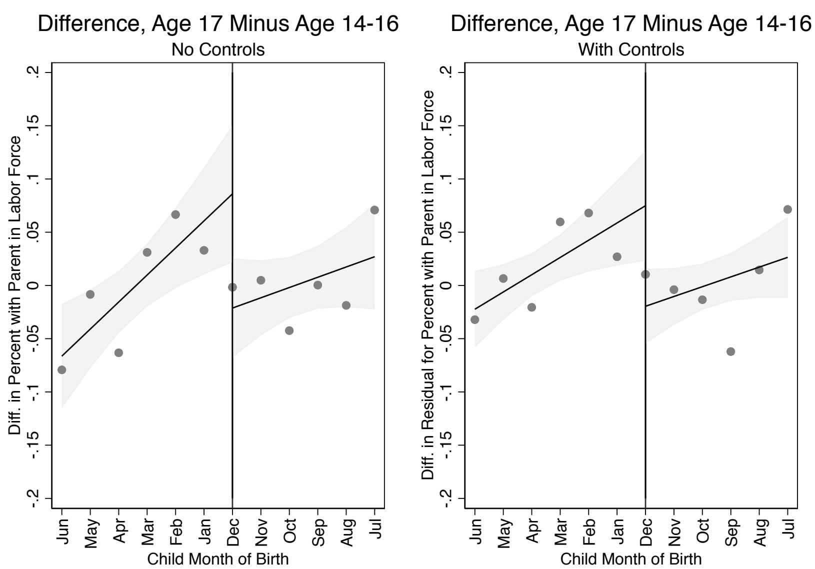
Next, Figure 1.10 indicates the effect of accounting for seasonality via the DiRD design. Each point in the figure represents the difference between the results estimated around the age 17 cutoff and the age 14 - 16 pooled cutoff. As the figure shows, the difference in discontinuities at age 17 is higher in magnitude than the age 17 discontinuity alone, and estimating the results with controls reduces the variance with little change to the point estimate. The results thus indicate a significant and negative discontinuity at age 17.

I show similar results for parental labor force participation in Figures 1.11 and 1.12. The results are similar in direction and magnitude to the results for parental employment. Here, the preexisting discontinuity at ages 14 - 16 is much larger in magnitude, underscoring the need to use a difference in discontinuities to correct for persistent seasonality.



Notes: Discontinuities are estimated in primary sample with local linear regressions, uniform kernels, in 6-month bandwidths centered around each December age cutoff. Shaded areas are 90 percent confidence intervals. Results are estimated on residuals from a regression including lagged outcomes, year and state fixed effects, and other controls (as discussed in text).

Figure 1.11: RD Results for Parental Labor Force Participation



Notes: Each point in the figures represents the difference in mean outcomes by month of birth between children around the age 17 cutoff (ages 16.5 to 17.5) and children at the age 14 -16 cutoffs (ages 13.5 to 16.5). Discontinuities are estimated in primary sample with local linear regressions, uniform kernels, in 6-month bandwidths centered around each December age cutoff. Shaded areas are 90 percent confidence intervals. Results with controls are estimated on residuals from a regression including lagged outcomes, year and state fixed effects, and other controls (as discussed in text).

Figure 1.12: DiRD Results for Parental Labor Force Participation

1.5.2 Regression Results

Building on the results illustrated by the figures above, I next present my main DiRD models. Table 1.2 shows reduced form regression results for a basic DiRD model for parental employment with no controls in column 1, adding the lagged outcome in column 2, and including controls for parental characteristics in column 3 (with a similar structure for labor force results in columns 4-6). The table shows the difference in discontinuities estimator as well as the change in outcomes for children aged 16.5 or older (the "post-policy" period in this context for the DiRD model), and the discontinuity for December in the "pre-policy" ages of 14 - 16. In the simplest specification, the results are large and significant; including lagged outcomes reduces the effect size slightly (reflecting heterogeneity in responses by prior year employment status, as discussed below), but the coefficients remain nearly identical when adding further controls. Column 3, my preferred specification, suggests an 8.4 percentage point fall in the proportion of parents working at the cutoff, relative to the 2.6 percentage point seasonal difference between December and January at earlier ages. This result is significant at the 5 percent level. The labor force results follow the same pattern, with a preferred estimate of a 9.6 percentage point fall in labor force participation at the cutoff (significant at the 5 percent level).

These results are consistent across different age windows used for estimation. Figure 1.13 shows the specifications with controls from Table 1.2, with the pre-treatment December discontinuity estimated in windows from various minimum ages up to age 16.5. Thus, the rightmost value in the figure (15.5) represents the minimum window length, while points further to the left reflect longer windows. As the figure shows, the DiRD estimates are quite stable over windows of various widths. (The estimate does shrink for a very short one year window for labor force participation, but this result is also imprecise).

I also examine the implications of these findings for labor supply in the full population, not limiting to the primary sample of parents with incomes below $20,000. Table 1.3 displays the results for the entire sample, as well as by lagged household income categories. I find no effect

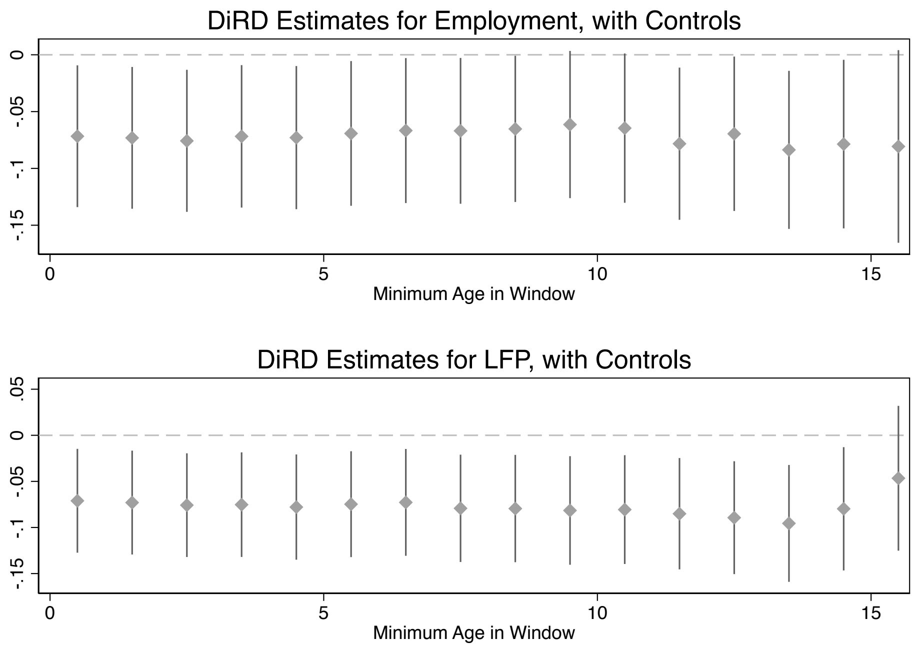
Table 1.2: DiRD Results for Primary Sample

	Parent Employed			Parent in Labor Force		
	(1)	(2)	(3)	(4)	(5)	(6)
Diff in Disc.	-0.097*	-0.087*	-0.084**	-0.107**	-0.091**	-0.096**
	(0.054)	(0.044)	(0.042)	(0.048)	(0.040)	(0.039)
Age 16.5+ (Post)	0.091**	0.067*	0.076**	0.086**	0.067**	0.076**
	(0.044)	(0.038)	(0.035)	(0.039)	(0.034)	(0.032)
December Disc.	-0.007	0.021	0.026	0.025	0.034*	0.042**
	(0.036)	(0.021)	(0.021)	(0.034)	(0.018)	(0.019)
Lagged DV		Yes	Yes		Yes	Yes
Controls			Yes			Yes
N	9,443	9,443	9,443	9,443	9,443	9,443
Clusters	1,034	1,034	1,034	1,034	1,034	1,034

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors (clustered by variance strata) in parentheses. Discontinuities are estimated with local linear regressions, uniform kernels, in 6-month windows centered around the December age cutoff. Estimated in window of 13.5 to 17.5 year old children in tax units with prior year AGI below $20,000. Controls are year and state fixed effects and parental characteristics (race, education, max age [quadratic], marriage, metro residence, number of dependents, and indicators for current and lagged months observed).

on employment or labor force participation among households with AGI above $20,000. It is reassuring that the effect of the CTC on labor supply appears limited to households where the credit should provide the largest change in average taxes (and thus the largest subsidy encouraging employment).

Next, I test whether results vary by whether households are entering or exiting employment; while theoretically both types of households face the same potential tax schedule, if there are state-dependent costs to finding employment, households with different levels of labor force attachment could respond differently to the CTC. Table 1.4 thus shows results by the subgroups of households that were not employed or participating in the prior year (the potential entrants), as compared to those that were already employed or participating (the potential exiters). While results are not as precise for these subgroups, the estimates are 50-70% smaller for exiters. This implies that the loss of the CTC operates more strongly by reducing the incentive for non-workers



Notes: Discontinuities are estimated in primary sample with local linear regressions, uniform kernels, in 6-month bandwidths centered around the December age cutoff. Each age window extends from the minimum age indicated up to age 16.5. Spikes indicate 90 percent confidence intervals. Results with controls are estimated on residuals from a regression including lagged outcomes, year and state fixed effects, and other controls (as discussed in text).

Figure 1.13: DiRD Results by Age Window Used to Estimate Pre-Treatment Discontinuity

Table 1.3: DiRD Results for All Households by Prior Year Income

	Parent Employed				Parent in Labor Force			
	(1) All Hhlds	(2) <\$20k	(3) \$20k-<\$30k	(4) \$30k+	(5) All Hhlds	(6) <\$20k	(7) \$20k-<\$30k	(8) \$30k+
Diff in Disc.	-0.014	-0.084**	0.049	0.001	-0.017*	-0.096**	0.016	0.003
	(0.011)	(0.042)	(0.032)	(0.007)	(0.010)	(0.039)	(0.023)	(0.005)
Age 16.5+	0.013	0.076**	-0.022	-0.003	0.016**	0.076**	-0.012	-0.001
	(0.009)	(0.035)	(0.026)	(0.005)	(0.008)	(0.032)	(0.018)	(0.004)
Dec. Disc.	0.004	0.026	-0.002	-0.001	0.008*	0.042**	0.011	-0.002
	(0.005)	(0.021)	(0.017)	(0.003)	(0.005)	(0.019)	(0.012)	(0.003)
Lagged DV	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	41,619	9,443	3,988	28,188	41,619	9,443	3,988	28,188
Clusters	1,141	1,034	897	1,130	1,141	1,034	897	1,130

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors (clustered by variance strata) in parentheses. Discontinuities are estimated with local linear regressions, uniform kernels, in 6-month windows centered around the December age cutoff. Estimated in window of 13.5 to 17.5 year old children in tax units, classified by prior year AGI. Controls are year and state fixed effects and parental characteristics (race, education, max age [quadratic], marriage, metro residence, number of dependents, and indicators for current and lagged months observed).

Table 1.4: DiRD Results for Households by Entry or Exit Status

	Employed		In Labor Force	
	(1) Entry	(2) Exit	(3) Entry	(4) Exit
Diff in Disc.	-0.108	-0.046	-0.182**	-0.060*
	(0.080)	(0.041)	(0.090)	(0.034)
Age 16.5+ (Post)	0.117*	0.037	0.164**	0.037
	(0.066)	(0.033)	(0.070)	(0.028)
December Disc.	0.026	0.005	0.060	0.022
	(0.042)	(0.022)	(0.045)	(0.019)
Controls	Yes	Yes	Yes	Yes
N	3,695	5,748	2,609	6,834
Clusters	810	931	696	971

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors (clustered by variance strata) in parentheses. Discontinuities are estimated with local linear regressions, uniform kernels, in 6-month windows centered around the December age cutoff. Estimated in window of 13.5 to 17.5 year old children in tax units with prior year AGI below $20,000. Controls are year and state fixed effects and parental characteristics (race, education, max age [quadratic], marriage, metro residence, number of dependents, and indicators for current and lagged months observed).

Table 1.5: DiRD Results for Households by Marital Status and Number of Children

	Parent Employed				Parent in Labor Force			
	(1) Single	(2) Married	(3) 1 Kid	(4) 2+ Kids	(5) Single	(6) Married	(7) 1 Kid	(8) 2+ Kids
Diff in Disc.	-0.095*	-0.064	-0.041	-0.101**	-0.089*	-0.129***	-0.064	-0.098**
	(0.052)	(0.062)	(0.072)	(0.051)	(0.049)	(0.049)	(0.066)	(0.046)
Age 16.5+	0.101**	0.012	0.070	0.075*	0.073*	0.092**	0.079*	0.061
	(0.044)	(0.050)	(0.050)	(0.044)	(0.040)	(0.039)	(0.046)	(0.039)
Dec. Disc.	0.015	0.032	-0.000	0.031	0.033	0.048	-0.020	0.054**
	(0.025)	(0.041)	(0.044)	(0.024)	(0.024)	(0.032)	(0.038)	(0.022)
Lagged DV	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	6,862	2,581	2,168	7,275	6,862	2,581	2,168	7,275
Clusters	951	689	716	959	951	689	716	959

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors (clustered by variance strata) in parentheses. Discontinuities are estimated with local linear regressions, uniform kernels, in 6-month windows centered around the December age cutoff. Estimated in window of 13.5 to 17.5 year old children in tax units with prior year AGI below \$20,000. Controls are year and state fixed effects and parental characteristics (race, education, max age [quadratic], marriage, metro residence, number of dependents, and indicators for current and lagged months observed).

to begin work, rather than leading current workers to stop working (although the response for current workers could be attenuated, as they would need to stop working at the very start of the year to have an employment change in my model). Thus, an important channel of the CTC's effects is to encourage households to move from nonparticipation into job search and employment.

Examining heterogeneity, Table 1.5 finds similar results across martial status and number of children in a tax unit. This implies the effects of the CTC are fairly consistent across household compositions (conditional on having low prior year income). The point estimates are slightly lower (but noisier) for households with one child as compared to two or more children, which could reflect higher fixed costs in families with more children that leave them closer to the margin of employment.

I run other specifications of my main results in Table 1.6. As an alternative to my prior year income cutoff, I run results limiting to single parents with education levels of high school or

Table 1.6: Further Robustness Checks for DiRD Results

	Parent Employed				Parent in Labor Force		
	(1) Base	(2) Single Low Educ.	(3) Leave School $\neq 17$	(4) LFP Measure	(5) Base	(6) Single Low Educ.	(7) Leave School $\neq 17$
Diff in Disc.	-0.084**	-0.055	-0.091*	-0.075*	-0.096**	-0.041	-0.107**
	(0.042)	(0.040)	(0.047)	(0.042)	(0.039)	(0.035)	(0.044)
Age 16.5+	0.076**	0.072**	0.069*	0.057	0.076**	0.047*	0.062*
	(0.035)	(0.031)	(0.038)	(0.036)	(0.032)	(0.027)	(0.036)
Dec. Disc.	0.026	0.008	0.034	0.025	0.042**	0.008	0.050**
	(0.021)	(0.020)	(0.023)	(0.020)	(0.019)	(0.019)	(0.021)
Lagged DV	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	9,443	6,420	7,659	9,443	9,443	6,420	7,659
Clusters	1,034	943	963	1,034	1,034	943	963

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors (clustered by variance strata) in parentheses. Discontinuities are estimated with local linear regressions, uniform kernels, in 6-month windows centered around the December age cutoff. Estimated in window of 13.5 to 17.5 year old children in tax units with prior year AGI below \$20,000. Controls are year and state fixed effects and parental characteristics (race, education, max age [quadratic], marriage, metro residence, number of dependents, and indicators for current and lagged months observed).

less. The results in columns 2 and 6 are similar to my main results, but are slightly smaller in magnitude and not precisely estimated. I include results in columns 3 and 7 that drop states where children can leave school at age 17 (as this provides a potential alternative factor that changes at the age 17 cutoff).[18] These results are substantially similar. I also test whether measuring earnings using survey measures reporting employment status by month (as opposed to the presence of positive earned income) changes the results for employment, and find very similar results in column 4. These results thus support my main specification.

Table 1.7: DiRD Results for Parental Characteristics

	(1) Educ (Coll+)	(2) Race (Non-White)	(3) Married	(4) Age (max)	(5) Num Dep.	(6) Lag Emp.	(7) Lag LFP	(8) Index
Diff in Disc.	-0.041	-0.087	0.042	-0.723	-0.153	-0.016	-0.023	-0.071
	(0.048)	(0.055)	(0.050)	(0.970)	(0.152)	(0.056)	(0.053)	(0.132)
Age 16.5+	0.042	0.054	-0.077*	2.062**	-0.174	0.035	0.028	0.064
	(0.044)	(0.046)	(0.042)	(0.803)	(0.130)	(0.045)	(0.043)	(0.123)
Dec. Disc.	-0.016	0.039	-0.024	0.834	-0.034	-0.042	-0.013	-0.137*
	(0.023)	(0.038)	(0.034)	(0.692)	(0.109)	(0.036)	(0.033)	(0.072)
N	9,443	9,443	9,443	9,443	9,443	9,443	9,443	9,443
Clusters	1,034	1,034	1,034	1,034	1,034	1,034	1,034	1,034
Mean DV	0.08	0.61	0.27	44.27	2.55	0.61	0.72	0.41
χ^2 p-value								0.32

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors (clustered by variance strata) in parentheses. Discontinuities are estimated with local linear regressions, uniform kernels, in 6-month windows centered around the December age cutoff. Estimated in window of 13.5 to 17.5 year old children in tax units with prior year AGI below \$20,000. Index refers to standardized index of all other columns. χ^2 p-value is for test of first 7 columns being jointly different from zero in seemingly unrelated regression.

1.5.3 Robustness Checks

For any RD design, it is critical to check the robustness of the results, as there are typically many researcher degrees of freedom (Lee and Lemieux 2010). I first provide a test that the discontinuity in earnings at age 17 is not spurious by examining whether other demographic variables show jumps at the cutoff. As Table 1.7 shows, there is no significant jump at the cutoff in terms of the education of children's parents (defined as the proportion with a college degree), in racial composition (defined as the percentage with non-white parents), in the proportion with married parents, age of the oldest parent, the number of dependents claimed by parents, or in the value of parental outcomes in the previous year (which provides a check that the discontinuity does not derive from pre-existing differences). To maximize the power of these tests, I also test for differences in a standardized index of all these variables in column 8 (which remains

[18]School leaving ages are drawn from the Digest of Education Statistics (various years), https://nces.ed.gov/programs/digest/

Table 1.8: DiRD Results for Children's Characteristics

	(1) Enrolled in School	(2) Highest Grade Completed	(3) Attrition	(4) Future Months Obs.
Diff in Disc.	0.026	-0.015	-0.004	-0.210
	(0.024)	(0.110)	(0.050)	(0.589)
Age 16.5+ (Post)	-0.041**	0.805***	0.015	0.220
	(0.018)	(0.091)	(0.038)	(0.456)
December Disc.	-0.022	-0.086	-0.008	-0.155
	(0.017)	(0.089)	(0.025)	(0.307)
N	4,691	4,686	7,167	7,167
Clusters	938	937	997	997
Mean DV	0.96	9.56	0.13	8.80

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors (clustered by variance strata) in parentheses. Discontinuities are estimated with local linear regressions, uniform kernels, in 6-month windows centered around the December age cutoff. Estimated in window of 13.5 to 17.5 year old children in tax units with prior year AGI below \$20,000. (Except columns 1 and 2 are estimated in window of 15.5 to 17.5 year old children).

insignificant)[19] and perform a χ^2 test on joint significant of each coefficient in a seemingly unrelated regression ($p = 0.32$). This implies covariates are smooth at the cutoff; the main results are also robust to controlling for all these factors, as shown in columns 3 and 6 of Table 1.2.

I also test for discontinuities in children's characteristics. To address the concern of minimum school-leaving ages occurring at 17, I check if children's school enrollment changes at the cutoff. The SIPP data only include educational information for respondents at the age of 15 or above, meaning the sample for these tests is restricted to ages 15.5 to 17.5. However, children just above and below the discontinuity are no more likely to be enrolled in school or in a higher grade (see Table 1.8), suggesting that the discontinuity in parental earnings does not stem from their children's educational attributes. (I also test for school leaving age effects directly in Table 1.6). Columns 3 and 4 show children are no more likely to drop out of the SIPP sample in the next year (or be observed for fewer months) when they are just above the cutoff.

[19] The index is constructed by standardizing each control variable from columns 1-7, then taking the average of the resulting variables.

Table 1.9: DiRD Results for Receipt of Other Income Sources

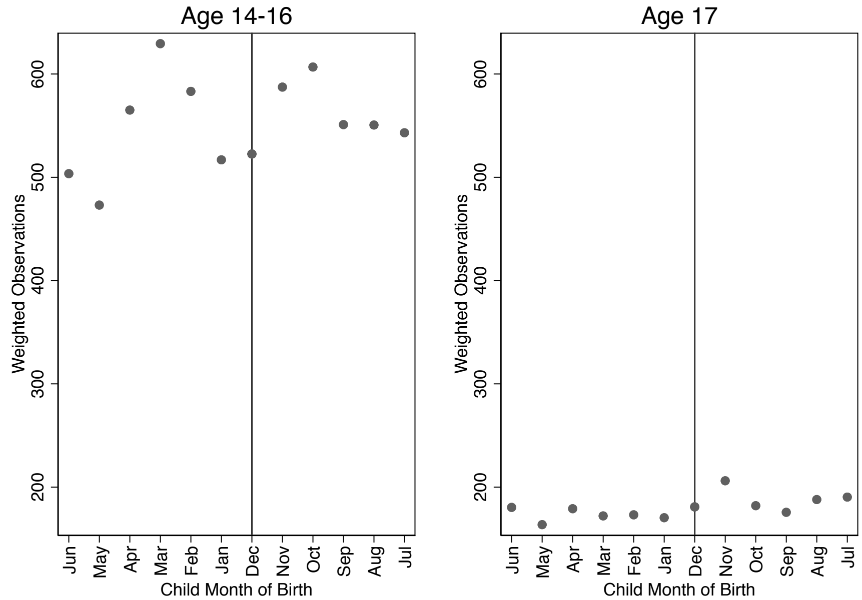
	(1) Dividends	(2) Property	(3) Pensions	(4) Soc. Sec.	(5) Transfers	(6) UI
Diff in Disc.	-0.001	0.005	-0.006	-0.031	0.015	-0.001
	(0.011)	(0.010)	(0.017)	(0.029)	(0.032)	(0.022)
Age 16.5+	-0.000	-0.004	0.001	0.005	0.011	-0.012
	(0.010)	(0.009)	(0.014)	(0.023)	(0.023)	(0.016)
Dec. Disc.	-0.010	-0.005	-0.000	0.033**	-0.018	0.006
	(0.007)	(0.005)	(0.008)	(0.014)	(0.017)	(0.011)
Lagged DV	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	9,443	9,443	9,443	9,443	9,443	9,443
Clusters	1,034	1,034	1,034	1,034	1,034	1,034

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors (clustered by variance strata) in parentheses. Discontinuities are estimated with local linear regressions, uniform kernels, in 6-month windows centered around the December age cutoff. Estimated in window of 13.5 to 17.5 year old children in tax units with prior year AGI below $20,000. Controls are year and state fixed effects and parental characteristics (race, education, max age [quadratic], marriage, metro residence, number of dependents, and indicators for current and lagged months observed).

I also test whether other sources of income change at the cutoff. As Table 1.9 shows, households at the cutoff are no more likely to have income from any source aside from earnings; they also do not have significantly more income in dollars from any source (results not shown). This implies that households do not offset their changes in labor supply by increasing other income sources or relying on transfers, but simply lower their income overall after losing the CTC's subsidy.

Another consideration for an RD design is whether individuals are able to manipulate the value of the running variable to fall on a given side of the cutoff. I present evidence on this test (McCrary 2008) in Figure 1.14, which displays the monthly weighted distribution of the running variable in my sample. The distribution appears smooth, implying that manipulation of birth timing is not driving the results.

Table 1.10 displays the sensitivity of the results to different RD estimation methods, including a triangular kernel, a quadratic polynomial, and a simple average across quarters of



Notes: Each point in the figures represents the weighted sum of observations by month of birth between children around the age 17 cutoff (ages 16.5 to 17.5) and children at the age 14 -16 cutoffs (ages 13.5 to 16.5).

Figure 1.14: Distribution of Children in Tax Units by Age

the year (effectively a difference-in-differences model at a quarterly level). The results are fairly noisy, but all estimates have the same sign and are close in magnitude to the baseline case (except the quadratic specification, which has the largest confidence interval).

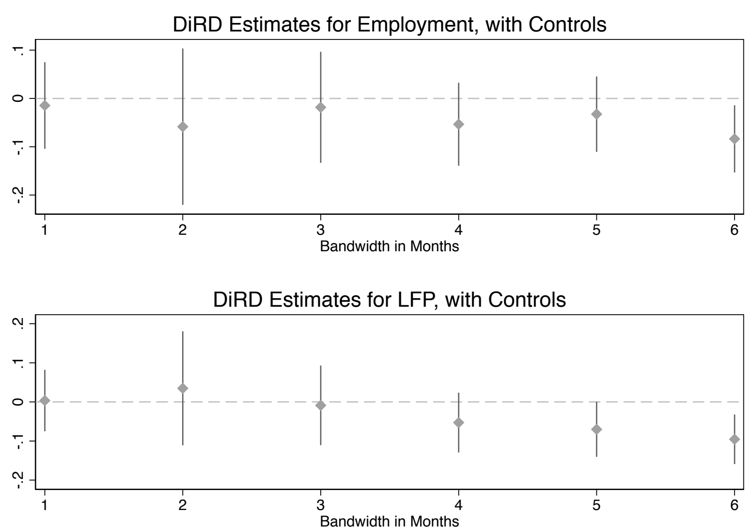
The main results are also consistent when estimated across a range of bandwidths. Figure 1.15 shows the main DiRD estimates for both outcomes in an age 13.5 to 17.5 window when changing the bandwidth of the local linear regression to different values. The first value (for a bandwidth of 1) is identical to a difference-in-differences estimator, treating December as the treated group and January as the control group. The results are quite noisy, but generally have the same sign (except for bandwidths of less than 2 months for labor force participation). Thus, the use of a 6 month bandwidth to maximize precision does not appear to bias the results.

I also perform a placebo test by examining results from years before the CTC was refundable and provided a subsidy to labor force participation. Using the 1984 to 1996 panels of the SIPP, I extend the analysis in my primary sample to the 1980s and 1990s period, when

Table 1.10: DiRD Results by RD Estimation Method

	Employment				LFP			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Diff in Disc.	-0.084**	-0.049	-0.007	-0.037	-0.096**	-0.062	-0.009	-0.050**
	(0.042)	(0.048)	(0.075)	(0.026)	(0.039)	(0.043)	(0.066)	(0.025)
Age 16.5+ (Post)	0.076**	0.073*	0.064	0.043**	0.076**	0.063*	0.032	0.052***
	(0.035)	(0.041)	(0.071)	(0.020)	(0.032)	(0.037)	(0.062)	(0.018)
December Disc.	0.026	0.022	0.023	0.009	0.042**	0.035*	0.024	0.018
	(0.021)	(0.023)	(0.034)	(0.014)	(0.019)	(0.021)	(0.031)	(0.012)
Lagged DV	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	9,443	9,443	9,443	4,840	9,443	9,443	9,443	4,840
Clusters	1,034	1,034	1,034	899	1,034	1,034	1,034	899
Degree	1	1	2	0	1	1	2	0
Kernel	Uni	Tri	Uni	Uni	Uni	Tri	Uni	Uni
Bandwidth	6	6	6	3	6	6	6	3

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors (clustered by variance strata) in parentheses. Estimated in window of 13.5 to 17.5 year old children in tax units with prior year AGI below $20,000. Results are estimated for varying degree local linear regresions with uniform or triangular kernels. Controls are year and state fixed effects and parental characteristics (race, education, max age [quadratic], marriage, metro residence, number of dependents, and indicators for current and lagged months observed).



Notes: Discontinuities are estimated in primary sample with local linear regressions, uniform kernels, in varying bandwidths centered around the December age cutoff. Each regression is estimated among children in the primary sample, ages 13.5 to 17.5. Spikes indicate 90 percent confidence intervals. Results are estimated on residuals from a regression including lagged outcomes, year and state fixed effects, and other controls (as discussed in text).

Figure 1.15: DiRD Estimates by Bandwidth

Table 1.11: Placebo DiRD Results for Period Before CTC was Refundable

	1984-1999				1990-1999			
	(1) Emp.	(2) Emp.	(3) LFP	(4) LFP	(5) Emp.	(6) Emp.	(7) LFP	(8) LFP
Diff in Disc.	0.074	0.044	-0.011	0.010	0.075	0.044	-0.011	0.010
	(0.086)	(0.065)	(0.078)	(0.063)	(0.086)	(0.065)	(0.079)	(0.063)
Age 16.5+	-0.069	-0.092*	-0.001	-0.043	-0.069	-0.092*	-0.001	-0.043
	(0.066)	(0.053)	(0.064)	(0.053)	(0.066)	(0.053)	(0.064)	(0.053)
Dec. Disc.	-0.053	-0.043	-0.005	-0.012	-0.053	-0.043	-0.005	-0.012
	(0.061)	(0.031)	(0.058)	(0.030)	(0.061)	(0.031)	(0.058)	(0.030)
Lagged DV		Yes		Yes		Yes		Yes
Controls		Yes		Yes		Yes		Yes
N	7,467	7,466	7,467	7,466	5,830	5,830	5,830	5,830
Clusters	1,396	1,395	1,396	1,395	826	826	826	826

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors (clustered by variance strata) in parentheses. Discontinuities are estimated with local linear regressions, uniform kernels, in 6-month windows centered around the December age cutoff. Estimated in window of 13.5 to 17.5 year old children in tax units with prior year AGI below $20,000. Controls are year and state fixed effects and parental characteristics (race, education, max age [quadratic], marriage, metro residence, number of dependents, and indicators for current and lagged months observed). Note that full-year data for 2000 is not included in SIPP data. "Emp." is employment, "LFP" is labor force participation.

the CTC did not exist or was effectively not refundable.[20] The results, in Table 1.11 (and Figure A.9), show that estimated discontinuities in this period are insignificant, with some opposite signs (and differing signs of the pre-existing seasonal pattern). This implies that some factor changed between the 1990s and 2000s that encouraged parents with age 17 children to leave the labor force; the CTC provides a plausible explanation for this factor.

1.5.4 Elasticity Estimates

Building on the results in my primary specification, I estimate labor supply elasticities in Table 1.12. The results are presented in four panels. Panel A provides the previously computed reduced form estimates for the drop in outcomes at the age 17 cutoff, while Panel B shows the

[20] As previously noted, while the CTC was refundable from 1998 to 2000 for families with three or more children and payroll taxes exceeding their EITC, these conditions provide a very minimal subsidy in practice.

corresponding first stage drop in the maximum eligible CTC. The ratio of these two estimates gives an intent-to-treat (ITT) estimate of an 8.0 percentage point increase in employment and 9.1 percentage point increase in labor force participation for every increase of $1,000 in CTC eligibility for the primary sample.

Table 1.12: ITT and Elasticity Estimates

	(1) Employed	(2) In Labor Force
(A) Parent Employed / In LF		
Diff in Disc.	-0.084**	-0.096**
	(0.042)	(0.038)
(B) Maximum Eligible CTC ($1,000s)		
Diff in Disc.	-1.049***	-1.048***
	(0.065)	(0.065)
(C) Percent Working / LFP (lag)		
Mean	0.608***	0.723***
	(0.009)	(0.009)
(D) Return to Work / LFP (lag, $1,000s)		
Mean	7.928***	4.683***
	(0.396)	(0.437)
ITT Estimate	0.080*	0.091**
$(= A/B)$	(0.041)	(0.038)
Elasticity at Average	1.040*	0.591**
$(= (A/C)/(B/D))$	(0.539)	(0.251)
N	9,443	9,443
Clusters	1,034	1,034

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors (clustered by variance strata) in parentheses. Discontinuities are estimated with local linear regressions, uniform kernels, in 6-month windows centered around the December age cutoff. Estimated in window of 13.5 to 17.5 year old children in tax units with prior year AGI below $20,000. Return to work is computed as difference in post-tax income between working and non-working parents (and likewise for LFP). Fuzzy RD and elasticity standard errors are computed using the delta method. Controls are year and state fixed effects and parental characteristics (race, education, max age [quadratic], marriage, metro residence, number of dependents, and indicators for current and lagged months observed).

Next, I rescale the ITT estimates to compute overall elasticities with respect to the change in return to employment caused by the CTC.[21] To compute the return to work (panel D), I calculate

[21]This measure of income is the relevant one for assessing extensive margin labor supply, as shown in Section

the total post-tax income accruing to workers and non-workers (or labor force participants and non-participants) in the primary sample, taking the difference in means for lagged income as representative of the expected income growth for a given member of this population when beginning to work (or enter the labor force). Combined with figures on the mean proportion of parents working (or in the labor force) in each group (panel C), I can generate an elasticity estimate at the average according to the standard formula:

$$\begin{aligned} \varepsilon &= \frac{\Delta y_{it}}{\Delta R_{it}} \cdot \frac{E\left(R_{it}\right)}{E\left(y_{it}\right)} \\ &= \frac{\beta}{\beta_c} \cdot \frac{E\left(R_{it}\right)}{E\left(y_{it}\right)} \end{aligned}$$

where R_{it} is the return to work (or participation) for a given child's parents at time t, and the change in the CTC $(\Delta C_{it} = \beta_c)$ is the only change in returns to work at the cutoff.

The resulting elasticity estimate in Table 1.12 is 1.04 for employment and 0.59 for labor force participation in the primary sample of low-income parents. This reflects an intertemporal substitution response, as the loss of the credit is temporary (relative to the control group of children born in January, who will lose the credit next year) and can be anticipated. Theory thus predicts stronger labor supply responses in this context due to a lack of wealth effects.

The implied employment elasticity for the population of interest is higher than that found in other studies of the intertemporal effects of changes in CTC eligible children or dependents on the intensive margin (Feldman, Katuščák, and Kawano 2016; Looney and Singhal 2006). This confirms the theoretical prediction that the CTC would have stronger extensive than intensive margin effects.

To derive a steady-state measure comparable to studies on the EITC, I perform a calibration exercise. Using formula (1.3), we have that

1.3.2 and Chetty et al. (2013, p. 33).

$$\varepsilon_I \approx \left(\frac{1 - \frac{\gamma W_t}{1-s_t}\left(1 - \frac{2\alpha}{1+r_t} + \frac{(2+r_t)\alpha^2}{(1+r_t)^2}\right)}{1 - \frac{\gamma W_t}{1-s_t}} \right) \varepsilon_S$$

where the relationship depends on the coefficient of relative risk aversion γ, the marginal propensity to save α, the interest rate on assets r_t, the savings rate s_t, and the percent change in post-tax income when working W_t. To calibrate the model, I set:

- $\gamma = 1$ (following Chetty (2006))
- $\alpha = 0.75$ (following Johnson, Parker, and Souleles (2009), who find $\mu = 0.25$ in a study based on a response to the Child Tax Credit)
- $r_t = 0.073$, $s_t = -0.02$ (using Saez and Zucman (2016), Appendix Tables B30 and B33, with values for the bottom 90% of households averaged for the 2000-09 and 2010-12 periods)
- $W_t = 0.80$ for employment and $W_t = 0.41$ for labor force participation (computed based on the mean changes in post-tax income when working in my sample, computed for the prior year).

We thus have for employment

$$\begin{aligned}
\varepsilon_I^{emp} &= \frac{\left(1 - \frac{(1)(0.8)}{1-(-0.02)}\left(1 - \frac{2(0.75)}{1+.05} + \frac{(2+.05)(0.75)^2}{(1+.05)^2}\right)\right)}{\left(1 - \frac{(1)(0.8)}{1-(-0.02)}\right)} \varepsilon_S^{emp} \\
&= 2.42 * \varepsilon_S^{emp} \\
\Rightarrow \varepsilon_S^{emp} &= \frac{\varepsilon_I^{emp}}{2.42} = \frac{1.04}{2.42} = 0.43
\end{aligned}$$

In an analogous calculation for labor force participation, I find

$$\varepsilon_I^{lfp} = 1.26 * \varepsilon_S^{lfp}$$
$$\Rightarrow \varepsilon_S^{lfp} = \frac{\varepsilon_I^{lfp}}{1.26} = \frac{0.59}{1.26} = 0.47$$

Thus, my intertemporal substitution elasticities are consistent with steady-state elasticities of 0.43 for employment and 0.47 for labor force participation. (Figure A.2 shows the sensitivity of this calculation to different parameter values). These values are in the range of results found for the EITC from the earlier difference-in-differences studies (Eissa and Liebman 1996; Meyer and Rosenbaum 2001, as corrected by Chetty et al. 2013) as well as studies using other identification strategies (Chetty, Friedman, and Saez 2013).

1.6 Conclusion

The results presented here suggest that the Child Tax Credit increases labor supply by 8.4 percentage points among low-income parents of older children, and increases labor force participation by 9.6 percentage points. This implies that the original proponents of the refundable CTC were correct that the credit would increase incentives to work (Sawhill and Thomas 2001). These results also imply that although some studies have found labor supply elasticities that are lower in the 2000s than in the 1990s, or cast doubt on the EITC's extensive margin effects, the Child Tax Credit has labor supply effects comparable to the EITC–with an estimated steady-state employment elasticity of 0.43, very similar to EITC studies (Chetty et al. 2013). Thus, my results confirm that tax credits play an important role in encouraging individuals to enter the labor force.

One limitation of these results is that the power of the SIPP sample is not large, despite using 16 years of data. This limits the range of hypotheses that can be tested in my sample. Thus, a primary topic for future research is to examine whether the effects shown here replicate in other datasets, such as population level tax data. Such cross-validation would help confirm the evidence reported here that the Child Tax Credit encourages labor supply.

1.7 Acknowledgements

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Chapter 1 is currently being prepared for submission for publication of the material. Kye Lippold was the primary investigator and sole author of this material.

Chapter 2

The Effects of Transfer Programs on Childless Adults: Evidence from Food Stamps

2.1 Introduction

Recent popular and policy interest has focused on Universal Basic Income proposals that would provide an income floor to all members of a society. Such policies differ from traditional transfer programs by providing a larger share of benefits to adults who are not "tagged" with characteristics indicating greater standards of need, such as caring for children, being elderly, or having a disability (Hoynes and Rothstein 2019). However, as most traditional transfers do not benefit such adults, little is known about the effects of a basic income on the population of able-bodied adults without dependents. I exploit a novel source of variation in one of the few near-universal transfer programs in the United States, the Supplemental Nutrition Assistance Program (SNAP, also known as Food Stamps), to examine the effects of an income supplement for unemployed childless adults. This work helps inform the debate about basic income and related policies.

The SNAP program provides benefits of just under $200 per month to households with a single able-bodied adult without dependents (known as an ABAWD); such individuals make up about 8% of program recipients. This paper exploits the fact that SNAP benefits are available to ABAWDs differentially based on their local area unemployment rate. Normally, ABAWDs must work 20 hours per week to receive SNAP benefits for more than 3 months every 3 years; however, in areas with high unemployment, states can apply for waivers so that ABAWDs who do not work can still receive benefits from the program. The rules for an area to be waived use several complicated formulas that compare state and county unemployment rates to cutoffs based on national unemployment. For example, in a year when the national cutoff is 6%, an ABAWD living in a county with a 6% unemployment rate will be eligible to receive benefits without working, but a comparable ABAWD living in a county with a 5.9% unemployment rate will not be eligible. This policy variation allows me to investigate the effects of SNAP on ABAWDs using a regression discontinuity (RD) design, comparing counties just above and below the unemployment cutoffs.

This source of variation has not been previously used in the literature, and provides a rare source of local variation in the SNAP program (where most policies are set at the federal level). As selection issues are endemic to research on SNAP (Hoynes, Miller, and Simon 2015), this plausibly exogenous variation helps provide more convincing identification of the effects of the program.

In addition to informing the debate on basic income, waiver rules in the SNAP program have been of recent policy interest; the US Department of Agriculture (USDA) recently released new regulations restricting ABAWD waivers, in response to criticism that current policies are too generous (Sykes 2017; Adolphsen et al. 2018). These restrictions were scheduled to go into effect on April 1, 2020, but were temporarily suspended by the CARES Act in response to the 2020 coronavirus pandemic. This research can thus help inform policymakers of the anticipated effects of changes to work requirement policies.

I find that the work requirements significantly reduce SNAP enrollment, with more ABAWDs participating in the program in counties just above the unemployment cutoffs. The program has negative effects on labor supply (in line with the prior literature), but positive spillover effects on reducing homelessness and property crime. Health and food spending outcomes are mixed (as of this writing). Taken together, the results suggest that the quasi-experimental variation from ABAWD waiver policies can provide valuable information about SNAP's effects.

The paper precedes as follows. Section 2.2 discusses the SNAP program, related literature, and the policy context. Section 2.3 describes the data and regression discontinuity method used. Section 2.4 presents results, and Section 2.5 concludes.

2.2 Background

2.2.1 The SNAP Program

SNAP provides benefits for low-income people to spend on food at approved retailers, typically grocery or convenience stores. Transfer values are equal to the estimated cost of a thrifty diet for a given household size, with the benefit taxed away as income rises. Since SNAP benefits are below typical household spending on food, the program is commonly viewed as close to a cash transfer (Currie 2003; but Hastings and Shapiro 2018, find evidence that SNAP benefits disproportionally increase food spending). SNAP participation is widespread, with the program providing benefits to more than 1 in 8 Americans.

The SNAP program is funded by the federal government, which sets program regulations through the USDA's Food and Nutrition Service (FNS), and is administered by states; thus, program policies are typically homogenous across the country. This has limited opportunities to use research designs based on policy changes to the program (Currie 2003). Some of the most credible research on program effects comes from work exploiting the roll-out of the program across counties in the 1960s (Hoynes and Schanzenbach 2009, 2012; Hoynes and Rothstein 2016). However, the program operated very differently in this early era (for example, benefits were distributed as physical "stamps" rather than through electronic benefit transfer cards). This study complements earlier work by providing causal evidence of the effects of SNAP using identification from a more recent period.

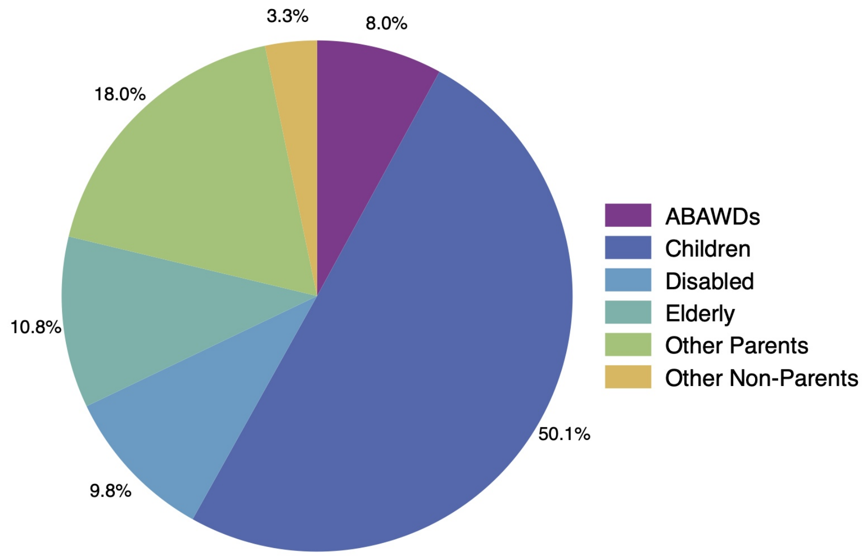
Most SNAP recipients are children, parents of children, elderly (for SNAP purposes, over age 60), or disabled; however, this study focuses on the SNAP population who are Able-Bodied Adults (aged 18-49) Without Dependents (referred to as ABAWDs). ABAWDs are a relatively small share of the SNAP caseload, as shown in Figure 2.1; ABAWDs represented 8.0% of SNAP recipients and 15.9% of SNAP households over the entire study period (although this share grew substantially during the Great Recession–see Appendix Figure B.2). However, SNAP plays an especially important role for this population; most other transfer programs in the United States are limited to people who are elderly, disabled, or have children, so SNAP provides one of the few sources of public income support for childless adults.

In part because non-elderly childless adults are typically viewed as having lower opportunity costs of work, ABAWDs have been subject to stricter SNAP policies related to work requirements ever since welfare reform in 1996. In particular, ABAWDs who wish to receive SNAP are required to work 80 hours per month (roughly 20 hours per week); those who do not are limited to receiving 3 months of benefits every 3 years. Because states typically do not provide "workfare" positions giving ABAWDs opportunities to meet the work requirement, in practice unemployed ABAWDs are barred from receiving SNAP. However, when this 3-month time limit was established in the welfare reform bill of 1996, policymakers acknowledged that unemployment among ABAWDs may be due to circumstances rather than choice; the bill thus allowed states to apply for waivers from FNS exempting any state area that "does not have a sufficient number of jobs" from the ABAWD work requirements.[1] These "ABAWD waivers" are the focus of this paper.

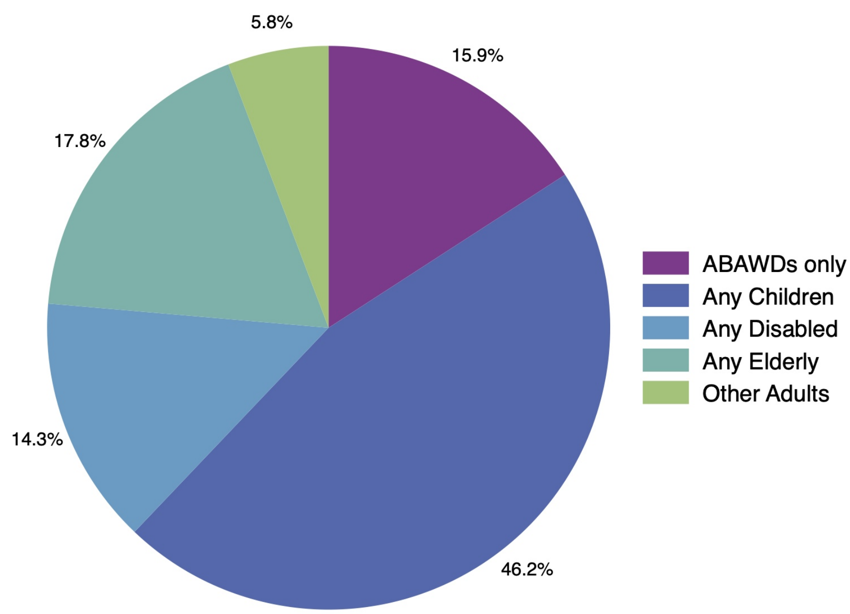
The SNAP benefit structure in the presence of the work requirement is summarized in Figure 2.2. Under normal circumstances (with no waiver, solid line), ABAWDs are barred from receiving SNAP after their first three months unless they work 20 hours per week; thus, the benefits received in the figure spike upward at 20 hours, at which point SNAP benefits begin to phase out with more hours of work and decline to a notch.[2] However, with a waiver (the dashed line), ABAWDs who are not working or at low levels of labor supply can receive the maximum SNAP benefit, close to $200 per month. The presence of a waiver thus represents a substantial income transfer to ABAWDs who are not working.

[1] Personal Responsibility and Work Opportunity Reconciliation Act, 7 U.S.C. § 2015 (1996).

[2] This notch is caused by the gross income test in the SNAP program, and its size varies based on household size.



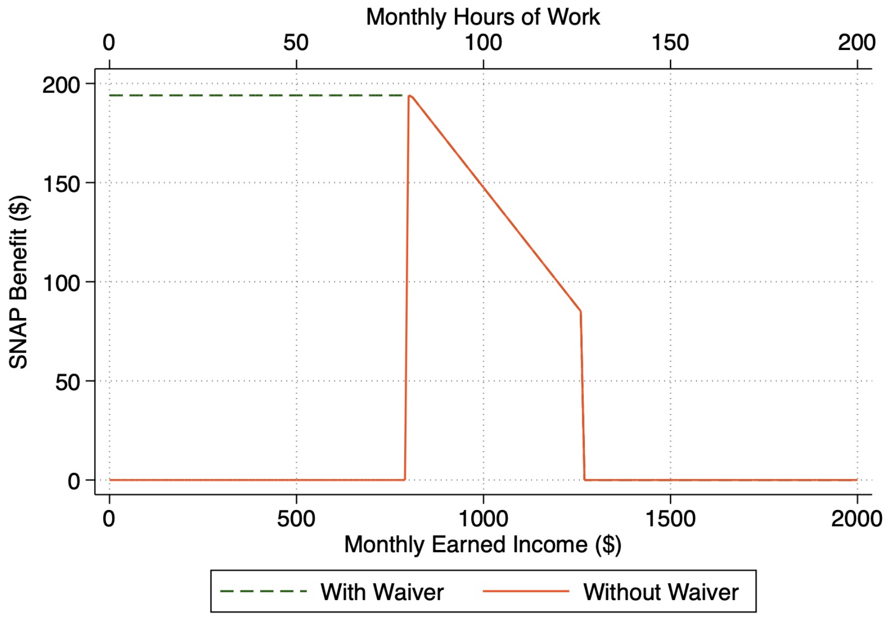
(a) Recipients



(b) Households

Source: Fiscal Year (FY) 2004-2018 SNAP Quality Control data. Categories are mutually exclusive (for households, each category excludes those in categories previously listed). "Other Adults" category are households that have only adults aged 50-59 (above the ABAWD age range). "Parents" are defined as adults living in households with children.

Figure 2.1: Composition of SNAP Households and Recipients



Note: Results are for FY 2015 policies, computed for a SNAP household of one ABAWD in California who has exhausted the 3-month time limit, with a potential wage of $10 per hour, rent of $650 per month, and no unearned income. Source: Author's calculations (based on Vigil et al. 2016).

Figure 2.2: Sample SNAP Benefit Schedule for One ABAWD Recipient

2.2.2 Policy Details

The full details of the ABAWD wavier policy are quite complicated, and are summarized below (with full details in Appendix B.1). States typically prepare requests of areas to be waived every year, which are submitted to regional FNS offices and approved or denied by the agency. Waivers usually last for one year, although lengths can vary.

States have strong incentives to apply for waivers to cover as much of their SNAP population as possible, as benefits are federally funded, and the process of tracking months of SNAP participation for non-waived ABAWDs is viewed as an "administrative nightmare" (USDA Office of Inspector General 2016). However, some states have recently voluntarily chosen not to apply for waivers (typically due to political desires to avoid providing benefits to non-workers, encouraged by advocacy groups such as in Ingram and Horton 2015). Additionally, states are allowed to exempt 15% of the case-months of non-waived ABAWDs from the work requirement at their discretion, meaning the waivers are not perfectly binding. Thus, the application of eligibility rules in this project reflects an intent-to-treat analysis.

A state can apply for an ABAWD waiver for a local area if the area meets conditions related to recent unemployment rates; for my analysis, I assume each waived area is a county.[3] The local unemployment data used for the waivers is required by FNS to come from the Bureau of Labor Statistics (BLS) Local Area Unemployment Statistics (LAUS) program (FNS 2016), meaning states cannot easily manipulate the unemployment rates used to apply for waivers. The waiver criteria have been generally consistent since the year 2004.

As detailed in Appendix B.1, a county's eligibility for waivers depends on several cutoffs related to unemployment rates:

1. If a county has a recent 12-month unemployment rate above 10%, it qualifies for a waiver.

2. If an entire state is eligible for extended benefits (EB) in the unemployment insurance (UI) program in the last 12 months, the entire state qualifies for a waiver. This eligibility is based on a 3-month moving average of state unemployment relative to a cutoff value.

3. If a county is designated as a Labor Surplus Area (LSA) by the Department of Labor, based on having a 24-month unemployment rate that is 20% above the national unemployment rate for the same period, the county qualifies for a waiver.

4. States have flexibility to combine contiguous counties into groups to meet the LSA criteria. Thus, if the 24-month unemployment rate for a group of counties is 20% above the national rate, the entire group can be waived (even if some counties would not qualify on their own).

I apply these rules in Section 2.3 to implement a RD design. Note that in practice, states and counties have discretion in whether to apply for a waiver, and can attempt to game the cutoff

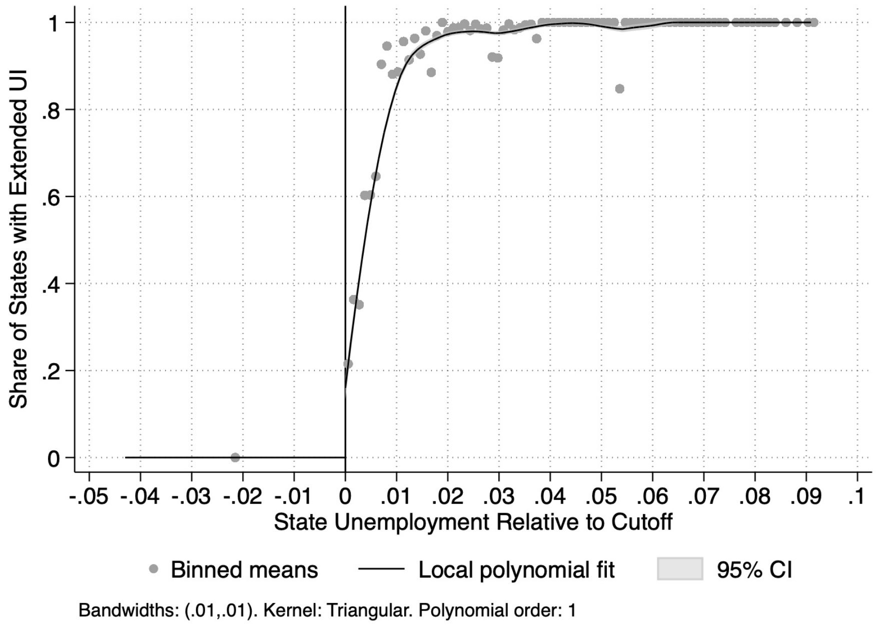
[3] States can apply using cities, towns, Indian reservations, or other census recognized areas as the areas to be waived; however, such classifications are rare except for reservations and towns in New England, so are not discussed here.

by combining labor market areas together in order to achieve an aggregate unemployment rate above the cutoff. I explore these issues further below; however, to the extent that manipulation is not perfect (due to states' lack of control over the underlying unemployment data), RD inference is still valid.

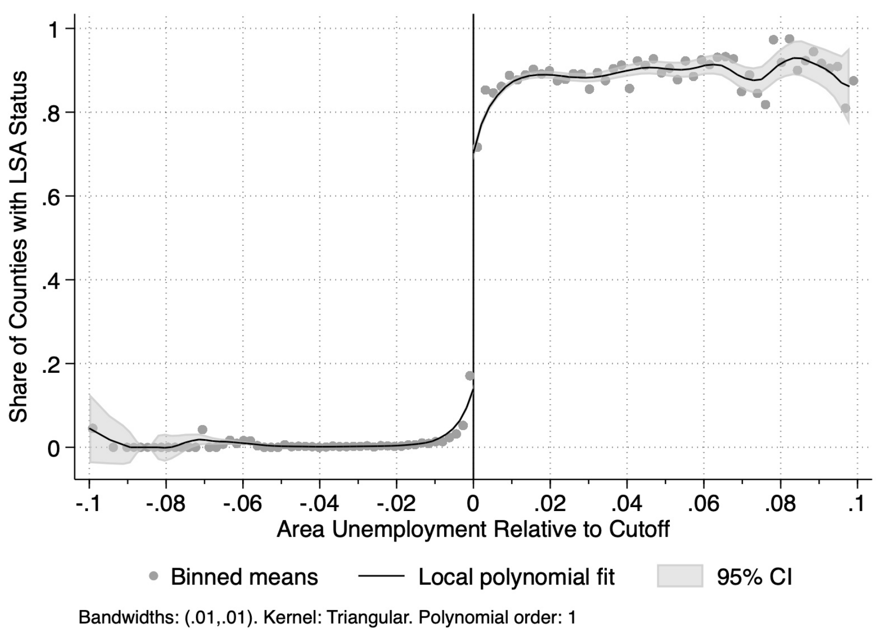
Figures 2.3a and 2.3b display how eligibility for two of the qualifying policies (extended UI and LSA status) are discontinuous in state and county unemployment, respectively. Although the state discontinuity is not as sharp (with several states right at the cutoff choosing not to take up extended UI benefits), waiver status is based only on *eligibility* for extended benefits, meaning all states on the right side of the graph are waiver-eligible. It is this discontinuous variation in eligibility for policies related to insufficient jobs, which in turn qualify counties for ABAWD waivers, that generates the variation I study.

2.2.3 Literature Review

Some prior papers have examined the effects of ABAWD waivers, but since waivers directly depend on unemployment, it is difficult to avoid selection issues in ABAWD research. For example, Ziliak, Gundersen, and Figlio (2003) find that the proportion of a state's population covered by a waiver increases growth of SNAP caseloads, but this effect becomes insignificant when controlling for traditional welfare caseloads (implying that waivers are correlated with a common variable, such as state generosity of transfer policies). Cuffey, Mykerezi, and Beatty (2015) also use ABAWD waivers in a difference-in-difference framework, examining the effect of waivers in a given area on employment among ABAWDs; as their paper notes, the presence of waivers seems to reject parallel trends, with areas that are always and never waived experiencing different employment and SNAP participation growth. As Appendix Figure B.3 shows, counties that are covered by an ABAWD waiver in my data have lower SNAP participation than comparison counties in years prior to waiver status, suggesting that parallel trends do not hold for this policy. While the presence of ABAWD waivers for an area is clearly endogenous (as states choose to



(a) Qualification for Extended UI by State Unemployment



(b) Qualification for LSA Status by County Unemployment

Note: Panel (a) displays results aggregated by state, while Panel (b) is aggregated by county.

Figure 2.3: Discontinuities in Policy Eligibility Qualifying Counties for ABAWD Waivers

apply for the waivers, and waivers can only affect areas with high unemployment, which itself influences SNAP receipt), my identification strategy relies on the more plausibly exogenous variation in unemployment rates between counties that are just eligible and just ineligible for a waiver.

Some related work has used regression discontinuity and DiD approaches to examine ABAWD waivers using age variation, comparing outcomes for childless adults just above and below the age 50 cutoff at which they are no longer subject to the ABAWD work requirements (Gray et al. 2019; Han 2019; Harris 2018; Stacy, Scherpf, and Jo 2018). These studies find reduced SNAP participation from the work requirement, and ambiguous effects on work. My results complement this literature by studying the entire ABAWD population rather than just older adults, which allows me to examine a broader set of outcomes.

This study also contributes to a broader literature on the effects of SNAP. Prior research has examined the effects of the program on food spending (Hoynes and Schanzenbach 2009; Ratcliffe, McKernan, and Zhang 2011; Hastings and Washington 2010), dynamics of consumption (Goldin, Homonoff, and Meckel 2019; Hastings and Shapiro 2018; Shapiro 2005), labor supply (Hoynes and Schanzenbach 2012), program participation (Ganong and Liebman 2018), health outcomes (Hoynes and Rothstein 2016), and crime (Tuttle 2019). By using a previously unexamined source of exogenous variation in SNAP eligibility, I am able to assess the effects of SNAP work reqruirements on all of these outcomes for childless adults.

2.3 Method

2.3.1 Characteristics of ABAWDs

I first use SNAP Quality Control data, drawn from a random sample of SNAP participants each month, to examine ABAWD characteristics. Tables 2.1 and 2.2 show that ABAWDs typically live in small households (1 person on average), and have very low income (55% have no income from any source reported to SNAP). ABAWDs are much more likely to be male than the typical SNAP recipient; as Appendix Figure B.4 shows, this is driven by a sharp decline in ABAWD status for women in their 30s (as women are more likely to be custodial parents of children, thus making them no longer childless adults). ABAWDs are more often US citizens, less likely to be Hispanic, and are somewhat more educated than other adults receiving SNAP. The vast majority of ABAWD recipients of SNAP are not working (with 34% out of the labor force, and 42% unemployed), meaning the work requirements are an important constraint for this group.

2.3.2 Data Sources

I construct a novel dataset of historical real-time unemployment information linked to ABAWD waiver status for each county in the US. Unemployment estimates are drawn from the the BLS LAUS program, which is the same dataset that states are required to use when preparing their ABAWD waivers based on local unemployment rates. Two factors have contributed to the difficulty of compiling such data in the past:

1. The BLS does not keep records of historical unemployment estimates from the LAUS program for areas such as counties. The LAUS statistics are primarily model-based, and when the models are updated (typically each year, as well as major revisions in 2005 and 2015), historical data are adjusted to match. While this means more recent vintages of unemployment information are likely more accurate in capturing true labor market conditions, they do not match the data that was used to qualify counties for waivers in real time.

   I overcome this challenge by compiling historical data from a variety of sources. State unemployment data are drawn from Chodorow-Reich, Coglianese, and Karabarbounis (2018), who performed a similar analysis of real-time unemployment rates at the state level to examine availability of extended UI benefits (one means of qualifying for ABAWD

Table 2.1: Descriptive Statistics for Abawds

	Non-ABAWD	ABAWD	Total
Household Size	2.2	1.1	2.1
	(1.5)	(0.4)	(1.4)
Age	35.53	33.31	35.20
	(24.69)	(9.92)	(23.12)
Earned Income ($/mon)	349.36	185.58	325.33
	(627.10)	(408.00)	(602.78)
Gross Income ($/mon)	819.184	282.441	740.439
	(608.360)	(459.894)	(618.788)
Any Income	0.876	0.450	0.814
	(0.329)	(0.498)	(0.389)
Male	0.406	0.559	0.429
	(0.491)	(0.496)	(0.495)
U.S. Citizen	0.886	0.968	0.898
	(0.318)	(0.175)	(0.303)

Source: SNAP QC Data for 1996-2016. Notes: Standard deviations in parentheses. ABAWDs are adults are 18-49 without children in the household or (imputed) disability status.

Table 2.2: Additional Descriptive Statistics for Abawds

	Non-ABAWD	ABAWD	Total
Race			
White	0.4	0.4	0.4
Black	0.22	0.30	0.24
Hispanic	0.18	0.10	0.17
Other	0.062	0.047	0.059
Missing	0.178	0.149	0.174
Work Status (age 18+)			
NiLF	0.660	0.339	0.595
Unemp	0.117	0.422	0.179
Employed	0.192	0.239	0.201
Missing	0.031	0.000	0.025
Hours Worked (age 18+)			
Not Employed	0.777	0.761	0.774
1-19	0.050	0.088	0.058
20-29	0.042	0.074	0.049
30-39	0.061	0.058	0.060
40+	0.039	0.018	0.035
Missing	0.031	0.000	0.025
Education (age 18+)			
<HS	0.328	0.258	0.313
HS	0.428	0.513	0.445
Some Coll	0.087	0.102	0.090
Coll+	0.036	0.038	0.037
Missing	0.121	0.089	0.114

Source: SNAP QC Data for 1996-2016. Notes: Standard deviations in parentheses. ABAWDs are adults are 18-49 without children in the household or (imputed) disability status. NiLF is Not in Labor Force.

waivers). Recent county and city data are drawn from LAUS records in the Internet Archive. County data for early years are digitized from the City and County Data Book series (Gaquin 2005), which publishes prior year unemployment rates by county from LAUS data. I also gathered internal BLS LAUS data providing county-level unemployment prior to the 2005 change in methods. Finally, I fill in data for some periods with the St. Louis Fed's ALFRED database of archival time series records.

Data on county LSA status (another input in determining waiver eligibility) is taken from the Department of Labor (preserved at the Internet Archive).[4] County populations are from the Census Bureau's Population Estimates program.[5]

2. Another difficulty is that records on the ABAWD waiver status of counties are not publicly available. I rely on an internal database of waivers from FY 2005-2015 created by FNS, as well as information estimated from waiver status maps created by the Center on Budget and Policy Priorities (2019).[6] I improve these measurements of waiver sources by using information on the dates that states applied for waivers (drawn from FNS documents obtained via the Internet Archive) to align waivers with the correct time periods. The end result is a database of waiver status for each county from 2004-2018.

My primary sample consists of counties in the United States, observed in July of each year from 2004-2018.[7] I exclude April 2009 to September 2010 data, as all counties were eligible for ABAWD waivers during that period due to the American Recovery and Reinvestment Act. I also exclude Texas, Delaware, and South Dakota from the analysis, as these states never took

[4]Available at https://www.dol.gov/agencies/eta/lsa.

[5]https://www2.census.gov/programs-surveys/popest/. These values are estimated between Census years, and available for July of each year. I linearly interpolate values across months.

[6]Because the raw data from these maps is not publicly available, I estimate waiver status by treating the maps as raster images, merging the maps to GIS shapefiles, and assigning areas that were more than 50% colored with waiver status as waived. (This method is similar to the one used by Bauer, Parsons, and Shambaugh 2019, one of the few papers to examine different eligibility methods for ABAWD waiver status).

[7]July is chosen as the midpoint of each year to enable better comparisons for data available only on an annual basis, and the study period begins in 2004 because FNS's waiver procedures have been consistent since that date.

up a waiver in a large proportion of the state (even when one was available in every area in FY 2010), and are thus arguably “never-takers” for the waiver treatment.

Figure 2.4 shows how ABAWD waiver status has evolved in the continental United States since 2004. Waivers were relatively uncommon in 2004, increased substantially during the Great Recession, and then slowly declined over time. A major change in waiver status occurred in 2016, when many states lost eligibility due to a policy change in the extended UI program.

I draw outcome variables by county from a wide variety of sources. To reduce the influence of outliers, all these variables are winsorized at the 1% and 99% levels. These variables can be classified into a few primary categories:

SNAP Enrollment and Labor Supply

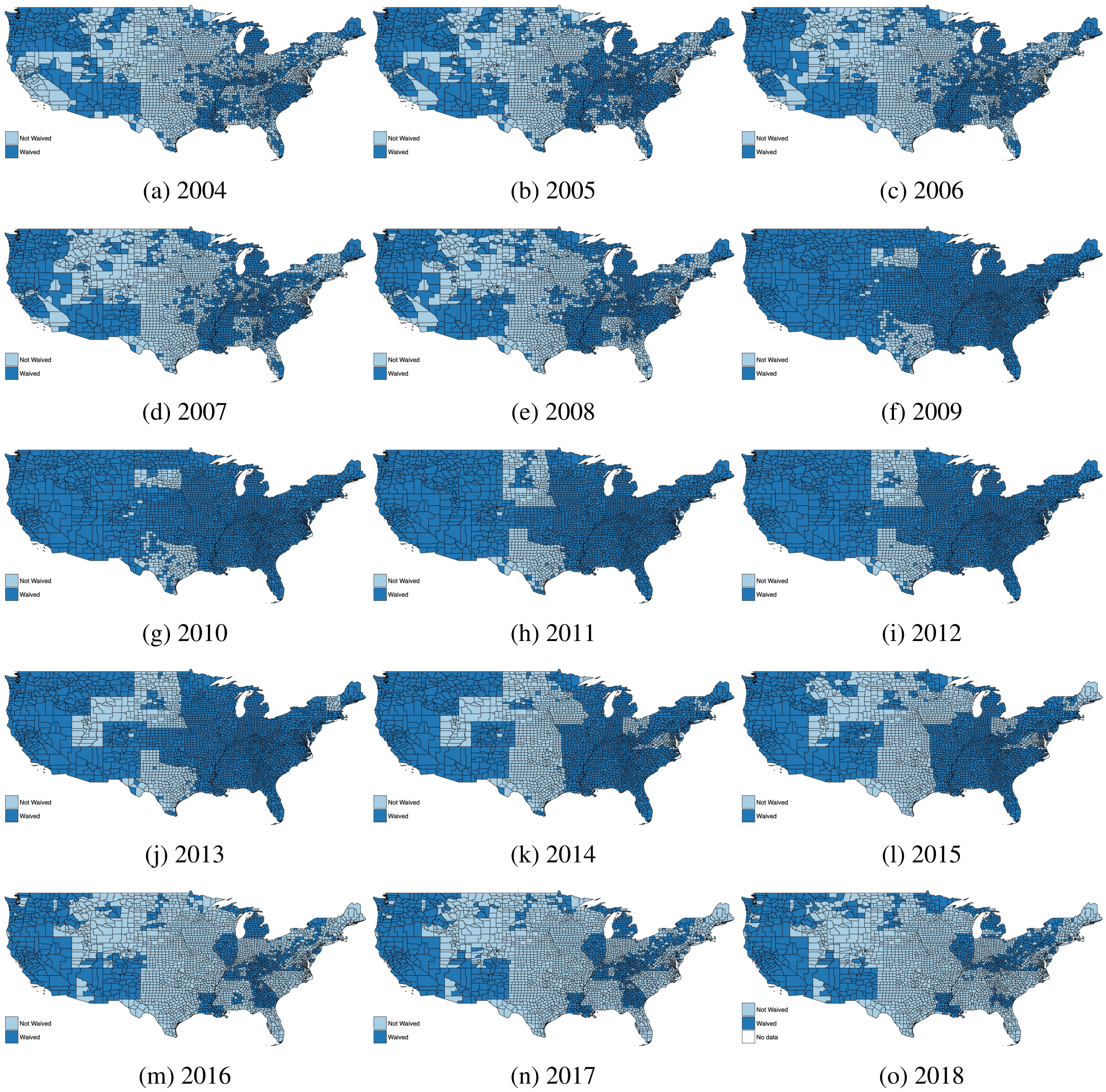
- Characteristics for ABAWDs are taken from public use American Community Survey (ACS) data in 2005-2018 (Ruggles et al. 2020). The ACS public use file only identifies counties with large populations, meaning these outcomes can only be examined for a subset of the data. However, this information is valuable because it identifies characteristics of adults aged 18-49 without children or a disability, whether or not they are receiving SNAP.
- SNAP county-level enrollment from the USDA.[8]

Other Outcomes for ABAWDS

- Mortality data by county from the CDC Wonder database.[9] This data is only available in years when there are 10 or more deaths reported in a county; thus, these results are more often available for counties with larger populations. I focus on mortality rates for people age 20-44 (particularly men), as this age range covers most of the ABAWD population.
- Hospital discharges by county and Diagnosis Related Group code from the Agency for

[8]https://www.fns.usda.gov/pd/supplemental-nutrition-assistance-program-snap

[9]Compressed Mortality File (CMF) 1999-2016, Series 20, No. 2V, 2017. https://wonder.cdc.gov/



(a) 2004 (b) 2005 (c) 2006

(d) 2007 (e) 2008 (f) 2009

(g) 2010 (h) 2011 (i) 2012

(j) 2013 (k) 2014 (l) 2015

(m) 2016 (n) 2017 (o) 2018

Source: CBPP (2019), unpublished data from FNS, and waiver request information from the Internet Archive. Notes: Darker color indicates area is waived. Waivers are measured as of July of each year.

Figure 2.4: Maps of ABAWD Waiver Status

Healthcare Research and Quality's Healthcare Cost and Utilization Project.[10] This data is available only from 2011-2015, and many smaller counties have information suppressed. However, it allows me to examine if health outcomes related to diet quality (such as hospital admissions for diabetes) change with access to SNAP.

- Counts of homeless people by area from the Department of Housing and Urban Development, cross-walked from "Communities of Care" to counties (using methods developed by Almquist, Helwig, and You 2020). This data is available since 2007. As ABAWDs make up a large share of homeless individuals, it is plausible that SNAP could affect the likelihood of childless adults becoming homeless.

Spillover Effects

- County-level rates of reported crime from the FBI's Uniform Crime Reports (compiled by Kaplan 2019). If SNAP access reduces crime (as in Tuttle (2019)), I would expect rates of crime to fall when acess for ABAWDs is expanded.

- To see how SNAP access affects food spending, I use information on food spending at grocery stores from the Nielsen Retail Scanner datasets available through the Kilts Center for Marketing Data Center at the University of Chicago Booth School of Business. These files contain weekly UPC-level sales of products from roughly 50% of US grocery stores. I use data on food sales at grocery stores to examine the effects of SNAP on food spending. For confidentiality reasons, I exclude all counties with fewer than 3 grocery stores in the dataset, meaning many counties have missing data.

- Information on employment, firms, and wages from the Quarterly Census of Employment and Wages.[11] I focus here on grocery stores and specialty food stores (NAICS codes 4451

[10] https://hcupnet.ahrq.gov/
[11] https://www.bls.gov/cew/

Table 2.3: Summary Statistics of Policy Variables for Counties in Sample

	count	mean	sd	min	max
Resident population (1000s)	36,501	100.1	312.1	0.4	10120.5
County has ABAWD Waiver	36,510	0.606	0.489	0.0	1.0
County unemp. rate, last 24 months	36,473	0.0667	0.0280	0.0	0.3
Max neighbor unemp. rate, last 24 months	36,509	0.0644	0.0230	0.0	0.2
National unemp. rate, last 24 months	36,519	0.0652	0.0167	0.0	0.1
County is Labor Surplus Area	36,519	0.241	0.428	0.0	1.0
State unemp. rate for UI, last 3 months	36,520	0.0547	0.0175	0.0	0.1
Cutoff for Extended UI	36,520	0.0659	0.00657	0.1	0.1
State has Extended UI	36,520	0.187	0.390	0.0	1.0
State Weeks of Extended UI	36,520	11.11	20.23	0.0	73.0

Note: Unit of observation is county-year. Estimated for all counties in 2004-2018 (less ARRA waived period), excluding DE, TX, and SD.

and 4452, jointly referred to as the "grocery" industry for this paper), as these retailers should be most directly affected by changes in SNAP spending. The logic for examining these outcomes involves general equilibrium effects; if ABAWD waivers increase total SNAP benefits distributed to a county, there is a potential to increase economic activity (depending on the size of the spending increase).

Information from this compiled data is summarized in Table 2.3 and Table 2.4. During my sample period, about 61% of counties are waived, reflecting the high unemployment during the Great Recession. A smaller share of counties qualify under the LSA (24%) and Extended UI (19%) criteria, meaning the ability of states to group counties is important in providing eligibility.

Table 2.4 shows that 13.6% of the average county population receives SNAP, with a lower share of 8.5% among ABAWDs, reflecting the high level of disadvantage needed for ABAWDs to qualify.[12] Most ABAWDs in the population (82.5%) do meet the work requirement of 20 hours per week. Other outcome variables are presented to illustrate typical county-level averages.

Table 2.4: Summary Statistics of Counties in Sample

	count	mean	sd	min	max
Share Population Receiving SNAP (FNS)	28,372	0.136	0.0765	0.0	0.4
Share ABAWDs Receiving SNAP (ACS)	4,459	0.0848	0.0555	0.0	0.3
Share ABAWDs Working 20+ Hours (ACS)	4,459	0.825	0.0680	0.6	0.9
ABAWD Usual Work Hours/Wk (ACS)	4,459	33.91	3.329	24.4	40.7
Share ABAWDs Working Any Hours (ACS)	4,459	0.877	0.0563	0.7	1.0
Grocery Stores per 10k Pop. (QCEW)	33,409	3.881	2.156	0.0	12.9
Grocery Employment per 10k (QCEW)	20,840	83.59	34.23	17.3	187.1
Average Grocery Wage ($/wk) (QCEW)	20,849	361.3	82.86	172.0	574.3
Age 20-44 Mortality per 100k (CDC)	19,293	179.8	75.85	64.5	445.8
Male 20-44 Mortality per 100k (CDC)	15,410	234.2	103.7	81.5	615.2
Hospital Discharges per 100k (AHRQ)	5,856	11441.3	2107.3	7403.2	18515.7
Diabetes Discharges per 100k (AHRQ)	5,230	171.6	71.12	62.3	426.5
Subst./Mental Disch. per 100k (AHRQ)	4,349	981.0	396.9	320.0	2398.2
Alcohol/Drug Discharges per 100k (AHRQ)	4,346	126.5	81.29	25.2	479.7
Homeless Population per 10k (HUD)	22,528	15.74	22.57	1.2	172.3
Property Crimes per 100k (UCR)	33,583	2277.0	1291.4	0.0	7598.6
Violent Crimes per 100k (UCR)	33,583	279.5	228.9	0.0	1356.0
Monthly Food Sales per Capita (Nielsen)	15,360	40.21	30.27	1.4	142.7

Note: Unit of observation is county-year. Estimated for all counties in 2004-2018 (less ARRA waived period), excluding DE, TX, and SD. All variables are winsorized at 1 and 99% levels.

2.3.3 Model

I combine the indicators described in Section 2.2.2 into two running variables, one for state unemployment (the distance of state unemployment from the extended UI cutoff in method 2), and one for county unemployment (taking the maximum of the distance of county unemployment from the cutoffs in methods 1, 3, and 4). For these measures, values of zero or above imply an area is eligible for an ABAWD wavier, and the units are percentage points of unemployment in each area type (county or state) relative to the relevant cutoff. For example, if national unemployment is 5.0% for the last two years, the 20% cutoff for LSA status (method 3) would be 6.0%. Thus, an area with 6.0% unemployment will qualify for a waiver (with a running variable value of 0%),

[12]Note that the 8.5% figure is an underestimate, as SNAP recipients are underreported in the ACS (see Meyer, Mittag, and Goerge 2018; Mittag 2019; Meyer and Mittag 2019; Wheaton 2008).

while an area with 5.9% unemployment will not qualify (running variable of -0.1%).

One challenge to this method involves how to aggregate results across the two running variables; when either variable crosses the threshold, the treatment is the same, but the counties at the cutoffs either themselves have high unemployment, or are in *states* with high unemployment. I resolve this by alternately considering counties in low-unemployment states (where only the county measure binds, and the state measure does not) and low-unemployment counties (where the state measure binds, and the county measure does not). Thus, in each case only the one measure of interest (state or county unemployment) changes across the cutoff.

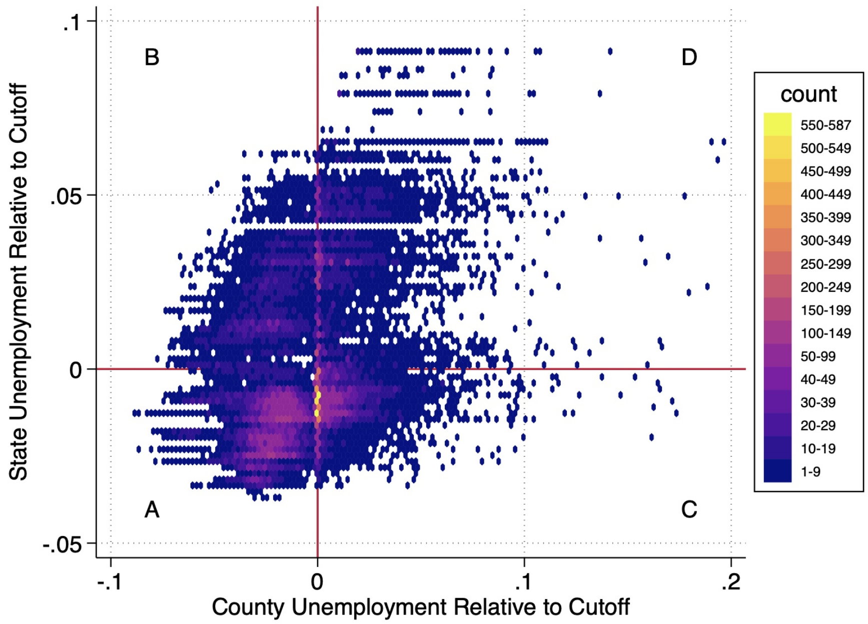
Figure 2.5 illustrates the method through a density plot. As the figure shows, counties are fairly evenly spread across the distribution of the state and county unemployment measures (with a ridge at the county cutoff, due to the ability of states to group counties to bring them just over the threshold). My method involves comparing quadrant A to quadrant B (for state unemployment) and quadrant A to quadrant C (for county unemployment). These measures thus summarize the effects of the waivers in a frontier regression discontinuity design (Reardon and Robinson 2012).

Given this setup, I investigate the effects of ABAWD waivers on SNAP participation using a standard local linear regression discontinuity design, but split across two the A/B and A/C quadrants:

$$y_{ct} = \alpha + \beta \cdot I(z_{ct} \geq 0) + \gamma \cdot z_{ct} + \delta \cdot z_{ct} \cdot I(z_{ct} \geq 0) + \varepsilon_{ct} \text{ iff } z_{st} < 0 \tag{2.1}$$

$$y_{ct} = \alpha + \beta \cdot I(z_{st} \geq 0) + \gamma \cdot z_{st} + \delta \cdot z_{st} \cdot I(z_{st} \geq 0) + \varepsilon_{ct} \text{ iff } z_{ct} < 0 \tag{2.2}$$

where y_{ct} is an outcome variable for county c (in state s) in month t, and z_{ct} (z_{st}) is the county (state) unemployment rate for that period relative to the ABAWD waiver cutoff. In practice, values are only consistently available for many variables in July of each year, meaning the time periods index annual values (and I pool observations across years, normalizing the running variable by



Note: Labels A through D designate quadrants. Values are plotted for main estimation sample.

Figure 2.5: Distribution of County vs. State Unemployment Rates Relative to Cutoff

the cutoff for each year). The coefficient of interest, β, gives the discontinuous change in y_{ct} among counties that have an unemployment rate just above the relevant ABAWD waiver cutoff. I estimate the main models using a triangular kernel in a bandwidth of one percentage point of unemployment away from the cutoffs.

2.4 Results

2.4.1 Policy Variables and SNAP Participation

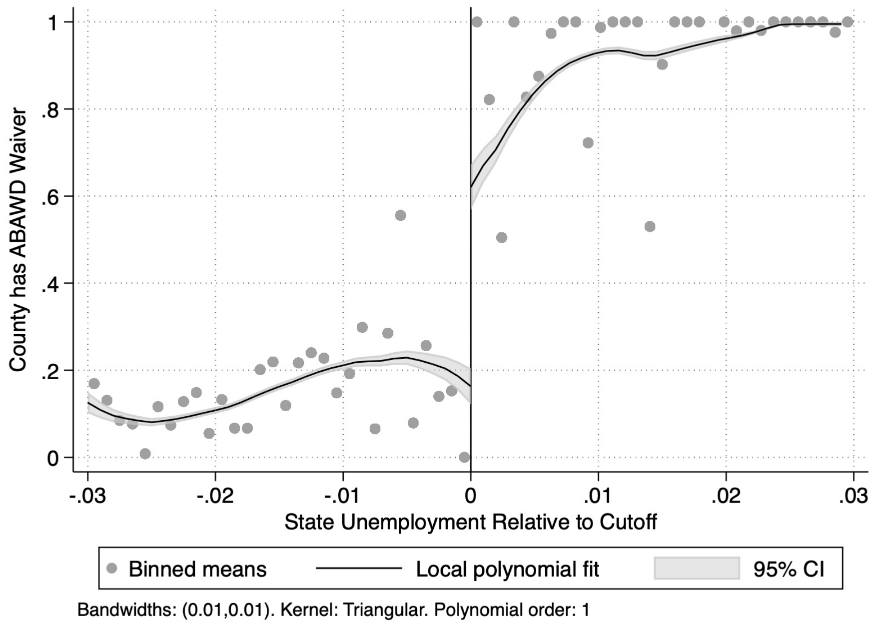
Figure 2.6 shows that my measures of unemployment correctly identify a discontinuity in waiver status of counties. Some measurement error remains in the data (particularly for county unemployment right at the cutoff value of 0, leading to a significant but small discontinuity), which will attenuate my other results, but the overall conclusion is that unemployment rates can serve as a good instrument for waiver status.

The results from Figure 2.6 are presented in regression form in Table 2.5. As expected, there is also an increase in counties qualifying for LSA status at the county cutoff. However, there is a decline in actual receipt of extended UI benefits (which can be explained by the fact that waivers are based on lagged eligibility for UI, rather than current eligibility). Counties on the state cutoff do have discontinuously lower population levels, which I intend to explore in future work.

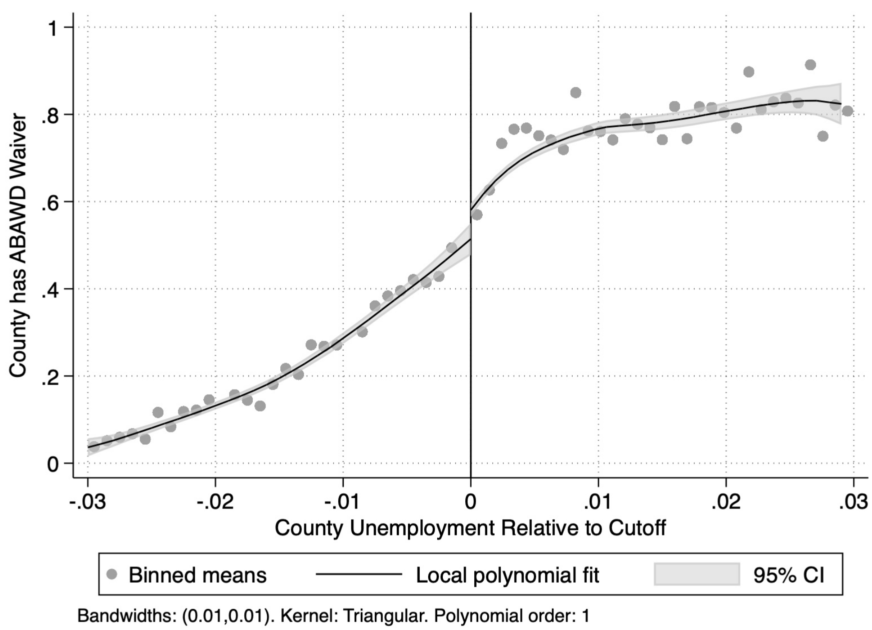
The last row of Table 2.5 and Figure 2.7 shows that the discontinuity in waiver status is accompanied by a discontinuity in SNAP receipt in each county. This confirms that the work requirements are indeed a binding constraint on participation. The increase from ABAWD waivers is small, at 2.1% for states and 0.7% for counties; however, as ABAWDs on SNAP are about 1% of the entire population in this period,[13] this represents a substantial rise in SNAP enrollment for this group.

Further evidence on the increase in SNAP at the cutoff can be seen using ACS data in Figure 2.8 and Table 2.6. Although the data is less precise (since it includes only large publicly identified counties), it suggests that 1.8% more ABAWDs participate in SNAP in counties just above the state cutoff. (The effect for counties is not significant, although the standard errors cannot rule out a wide range of effects).

[13] As a back-of-the-envelope calculation, since 8% of the SNAP population are ABAWDs, and 13.6% of the population receives SNAP in an average county, ABAWDS on SNAP are about 1% of the entire population.

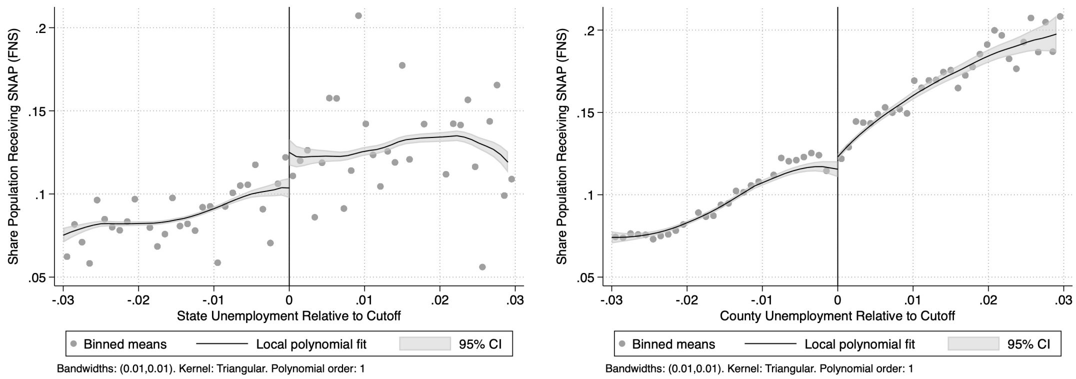


(a) State Unemployment



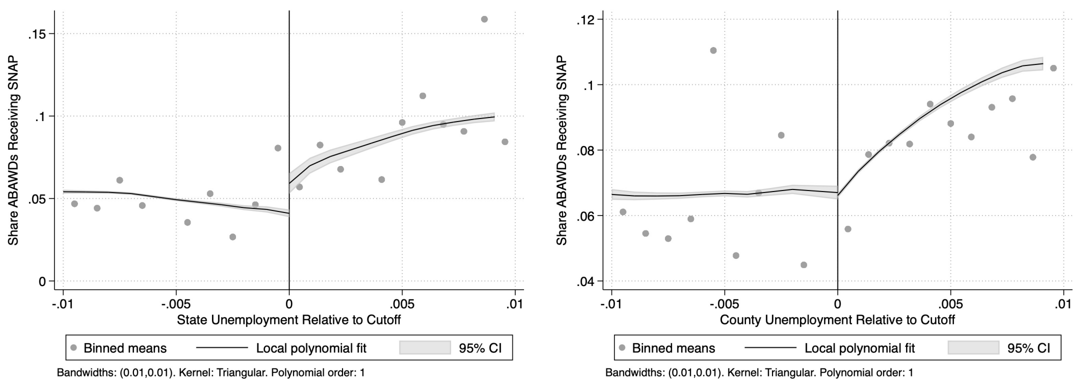
(b) County Unemployment

Figure 2.6: Discontinuities in ABAWD Waiver Status at State and County Cutoffs



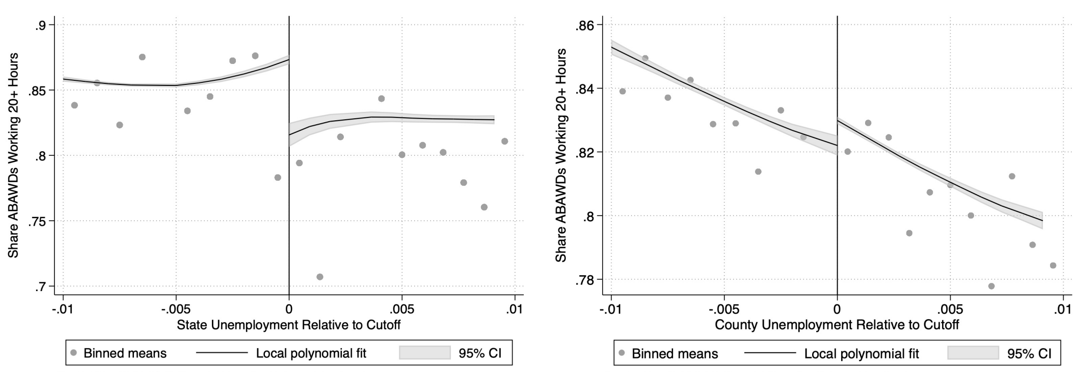
(a) State (b) County

Figure 2.7: Share of Population Receiving SNAP (FNS Data)



(a) State (b) County

Figure 2.8: SNAP Participation by ABAWDs, ACS Data



(a) State (b) County

Figure 2.9: Proportion of ABAWDs Working 20+ Hours per Week, ACS Data

Table 2.5: Summary of RD Results, Policy Variables

	State	County
County has ABAWD Waiver	0.457***	0.070***
	(0.036)	(0.021)
	[4,776]	[8,412]
County is Labor Surplus Area	-0.001	0.044***
	(0.004)	(0.007)
	[4,776]	[8,413]
State has Extended UI	-0.123***	0.001
	(0.004)	(0.001)
	[4,776]	[8,413]
Resident population (1000s)	-93.882***	6.533
	(19.978)	(11.706)
	[4,775]	[8,413]
Share Population Receiving SNAP (FNS)	0.021***	0.007***
	(0.005)	(0.003)
	[3,781]	[6,847]

* p<0.10, ** p<0.05, *** p<0.01. Standard errors (clustered by county) in parentheses, N of each model in brackets. Results are RD estimates from local linear regressions, triangle kernels, bandwidths of .01 for state and .01 for county.

Table 2.6: Summary of ACS RD Results

	State	County
Share ABAWDs Receiving SNAP	0.018**	0.001
	(0.009)	(0.009)
	[421,135]	[926,621]
Share ABAWDs Working 20+ Hours	-0.070**	-0.000
	(0.034)	(0.008)
	[421,135]	[926,621]
ABAWD Usual Work Hours/Wk	-2.880**	-0.217
	(1.441)	(0.407)
	[421,135]	[926,621]
Share ABAWDs Working Any Hours	-0.039**	0.004
	(0.020)	(0.007)
	[421,135]	[926,621]
ABAWD Usual Hours/Wk If Pos.	-1.557*	-0.445
	(0.893)	(0.279)
	[376,539]	[809,727]
Share ABAWDs Worked Last Week	-0.020	0.002
	(0.030)	(0.007)
	[388,213]	[847,398]

* p<0.10, ** p<0.05, *** p<0.01. Estimated on ACS data, 2005-2018. Standard errors (clustered by county) in parentheses, N of each model in brackets. Results are RD estimates from local linear regressions, triangle kernels, bandwidths of .01 for state and .01 for county.

2.4.2 Labor Supply

A natural first outcome to examine is how the change in SNAP affects labor supply. Hoynes and Schanzenbach (2012) examine the rollout of SNAP across counties, and find results consistent with a 4-10 percentage point reduction in extensive margin labor supply among female-headed families due to SNAP. The RD in Figure 2.9 estimates a 3.9 percentage point reduction at the (state) cutoff, which is on the lower end of this range. Examining the share of ABAWDs working 20 or more hours per week (the level of hours that the work requirement should incentivize), I find a 7 percentage point drop in the the proportion of ABAWDs working this level of hours at the cutoff, implying strong labor supply effects. Table 2.6 presents estimates for other labor supply variables in the ACS; the decline in work is observed at both the extensive and intensive margin, although no significant effects appear at the county cutoff.

2.4.3 Consumption Outcomes

Another outcome of interest when studying SNAP involves the degree to which the transfer promotes food consumption, or has general equilibrium effects of supporting employment in grocery stores. Table 2.7 examines these outcomes, and actually finds discontinuously *lower* numbers of grocery stores and grocery workers per capita in waived areas, and higher grocery wages (for the county cutoff). Similarly, food sales in the Nielsen data are lower at the state cutoff (RD graphs appear in Appendix Figure B.5). These results are somewhat difficult to explain, as SNAP would normally be expected to *increase* food consumption, and I plan to investigate this area further.

2.4.4 Health Outcomes

SNAP has the potential to improve health outcomes, particularly diseases linked to food insecurity (such as diabetes and hypertension) and mental health outcomes. Although health data

Table 2.7: Summary of RD Results, Grocery Variables

	State	County
Grocery Stores per 10k Pop. (QCEW)	-0.377**	-0.184**
	(0.147)	(0.084)
	[4,706]	[7,086]
Grocery Employment per 10k (QCEW)	-14.359***	-3.143*
	(4.417)	(1.878)
	[2,854]	[4,664]
Average Grocery Wage ($/wk) (QCEW)	-8.276	32.144***
	(7.305)	(4.455)
	[2,854]	[4,664]
Monthly Food Sales per Capita (Nielsen)	-25.085***	1.149
	(4.169)	(2.257)
	[1,714]	[3,221]

* p<0.10, ** p<0.05, *** p<0.01. Standard errors (clustered by county) in parentheses, N of each model in brackets. Results are RD estimates from local linear regressions, triangle kernels, bandwidths of .01 for state and .01 for county.

Table 2.8: Summary of RD Results, Health Variables

	State	County
Age 20-44 Mortality per 100k (CDC)	44.551***	-13.478***
	(8.418)	(3.860)
	[2,689]	[4,232]
Male 20-44 Mortality per 100k (CDC)	59.020***	-16.475***
	(13.650)	(5.817)
	[2,235]	[3,367]
Hospital Discharges per 100k (AHRQ)	1071.848***	2229.364*
	(202.806)	(1164.389)
	[835]	[57]
Diabetes Discharges per 100k (AHRQ)	60.640***	70.812**
	(8.323)	(33.258)
	[689]	[49]
Subst./Mental Disch. per 100k (AHRQ)	-368.970***	31.899
	(52.245)	(435.970)
	[669]	[56]
Alcohol/Drug Discharges per 100k (AHRQ)	-5.643	21.070
	(13.565)	(67.622)
	[518]	[38]

* p<0.10, ** p<0.05, *** p<0.01. Standard errors (clustered by county) in parentheses, N of each model in brackets. Results are RD estimates from local linear regressions, triangle kernels, bandwidths of .01 for state and .01 for county.

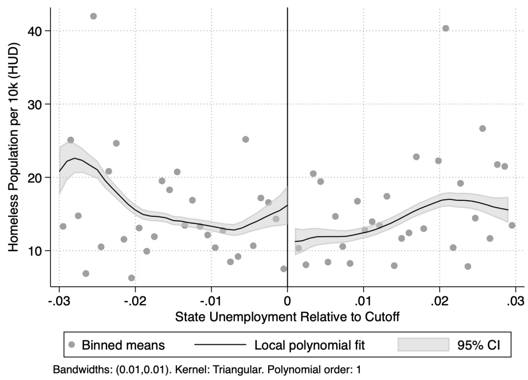
at the county level are limited, I present RD results for health outcomes in Table 2.8 (with graphs in Appendix Figures B.6 and B.7). Mortality for people in the ABAWD age range is (surprisingly) higher at the state cutoff, and lower at the county cutoff; hospital and diabetes discharges are higher across both cutoffs, while substance abuse and mental health discharges are lower (in the state case). The implications of higher hospital discharges for welfare are unclear; SNAP could worsen health, or relaxed budget constraints from SNAP eligibility could allow more people to seek medical attention for untreated health conditions. Future work will attempt to tease out the difference between the state and county results, and investigate which sources of mortality could be driving the increase at the cutoff.

2.4.5 Homelessness and Crime

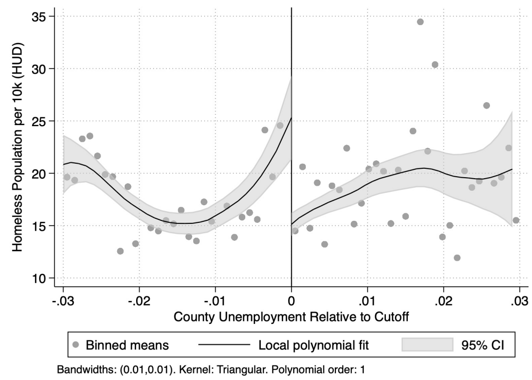
Table 2.9 shows the effects of access to SNAP on homelessness and crime. These outcomes are particularly interesting because many ABAWD recipients of SNAP are homeless, and prior work has suggested SNAP access can reduce criminal recidivism (Tuttle 2019). I find that the share of the homeless population drops slightly at the cutoff (by 0.05-0.1%), implying resources from SNAP could help ABAWDs provide for basic needs (Figure 2.10a, 2.10b). I also find a 0.1-0.3% reduction in property crime, and (for the county cutoff) an even smaller reduction in violent crime (Figure 2.10). These results imply SNAP access for unemployed ABAWDs can generate positive social externalities.

2.4.6 Robustness Checks and Placebo Tests

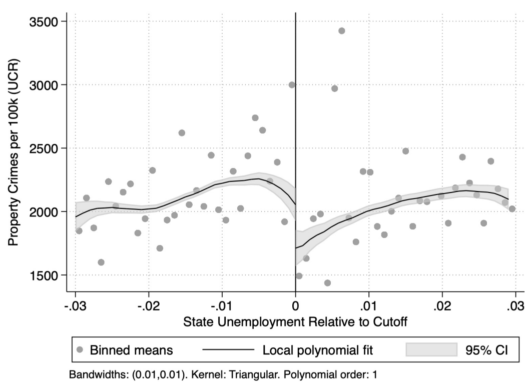
My methodology suggests a natural placebo test; counties that have high values for both the state and county running variables (quadrant D in Figure 2.5) do not contribute to the main results, so I can use them to examine the effects of crossing the state or county cutoff when ABAWD waiver eligibility does not change. Comparing quadrants B and D gives the effect of



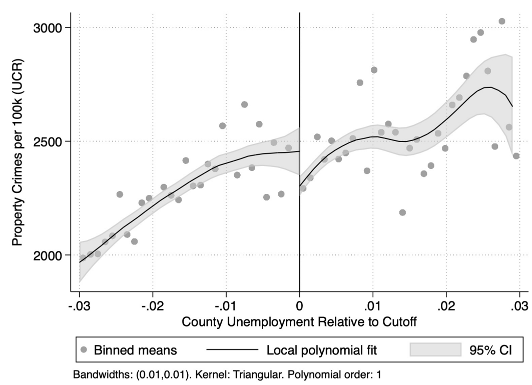
(a) Homelessness, State



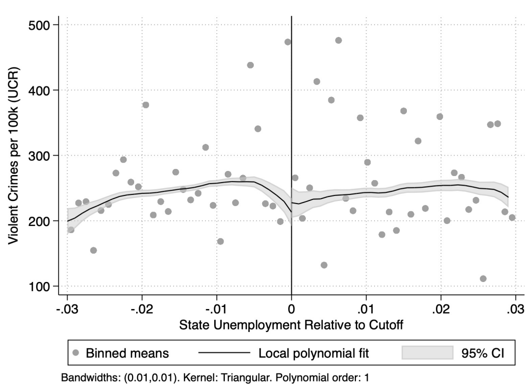
(b) Homelessness, County



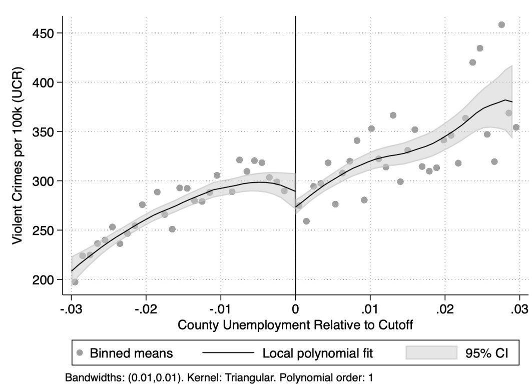
(c) Property Crimes, State



(d) Property Crimes, County



(e) Violent Crimes, State



(f) Violent Crimes, County

Figure 2.10: RD Plots for Homelessness and Crime Indices

Table 2.9: Summary of RD Results, Crime Variables

	State	County
Homeless Population per 10k (HUD)	-5.156***	-10.203***
	(1.916)	(2.455)
	[2,898]	[3,563]
Property Crimes per 100k (UCR)	-342.560***	-152.789***
	(95.331)	(56.109)
	[4,731]	[7,107]
Violent Crimes per 100k (UCR)	14.500	-15.991*
	(15.147)	(9.169)
	[4,731]	[7,107]

* p<0.10, ** p<0.05, *** p<0.01. Standard errors (clustered by county) in parentheses, N of each model in brackets. Results are RD estimates from local linear regressions, triangle kernels, bandwidths of .01 for state and .01 for county.

county eligibility (including correlated variables such as LSA status) in states that are already eligible for waivers (the "county placebo"), and quadrants C and D indicate how (lagged) state-level eligibility for extended UI affects outcomes in counties that are already waiver eligible (the "state placebo"). Appendix Tables B.1 to B.4 compare outcomes for the two cases for selected variables, and also plot the Difference-in-RD (DiRD) results that compute the difference between the baseline and placebo columns. The results from the standard models and the DiRD models are qualitatively similar (although the state DiRD shows a new reduction in violent crime).

I also examine results controlling for county fixed effects, where all variation in waiver status comes from *changes* in which side of the cutoff the county falls. As Appendix Table B.5 shows, most of my main results are robust in this specification. Appendix Table B.6 indicates similar results when including lagged dependent variables in the regressions.

Finally, Appendix Tables B.7 and B.8 shows results varying the local linear regression bandwidth to 0.5 or 2 percentage points of unemployment. Results are qualitatively similar (except for the hospital discharge data, which has low power).

2.5 Conclusion

The results of this research suggest that SNAP benefits for childless adults provide important externalities. When work requirements are just waived at an unemployment cutoff, SNAP participation rises by about 1%, and property crime and homelessness fall by about 0.1%. The program does have negative impacts on labor supply, so the relative weight of these factors should be considered when evaluating social welfare gains from the program.

The ABAWD waiver unemployment cutoff provides a new instrument for SNAP, opening avenues for new research in the policy literature. Future work is needed to improve the remaining measurement issues (such as tracking ABAWD waiver status and evaluating areas where my assignment methodology does not match program reports). I have an active FOIA request to FNS to access primary source waiver documents, which I hope to use to iron out the remaining problem cases.

Using non-public or administratively linked ACS data (as in Stacy, Scherpf, and Jo 2018) could help with measurement issues when identifying outcomes for individual ABAWDs. Another line of inquiry would be to examine how employment outcomes change in waived counties using IRS tax data, as the fine geographic detail of this data could improve the precision of estimating effects.

Overall, the effects of ABAWD waivers are potentially substantial for their target population, but identifying these effects with certainty requires more research. Further study in this area will help clarify how universal transfer programs impact the lives of childless adults.

2.6 Acknowledgements

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Researcher(s) own analyses calculated (or derived) based in part on data from The Nielsen Company (US), LLC and marketing databases provided through the Nielsen Datasets at the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business. The conclusions drawn from the Nielsen data are those of the researcher(s) and do not reflect the views of Nielsen. Nielsen is not responsible for, had no role in, and was not involved in analyzing and preparing the results reported herein.

Chapter 2 is currently being prepared for submission for publication of the material. Kye Lippold was the primary investigator and sole author of this material.

Chapter 3

The Distributional Impacts of Taxes on Health Products: Evidence from Diaper Sales Tax Exemptions

This chapter is joint work with Chelsea Swete.

3.1 Introduction

Many economists argue that commodity taxes should be uniform across products, as in the classic results of Atkinson and Stiglitz (1976). There are two main ways that policies often alter these recommendations: excise or "sin" taxes on goods such as cigarettes or gas, and sales tax exemptions on goods such as groceries or health products. In this paper, we analyze sales taxes on diapers, a product that has direct health implications but is not always considered a health product in tax codes.

Policies determining sales taxes are subject to a trade-off between efficiency and equity. Since lower income households spend a greater share of their budget on consumption goods, sales taxes are regressive, which decreases equity. However, it is more efficient to tax goods for which demand is relatively inelastic, as this minimizes distortions to behavior. This has led some commentators to argue that health products should be subject to tax without exemptions (Kaeding 2017).

There is evidence from the cigarette tax literature that while all consumers respond to the more salient posted excise tax, only poorer consumers respond to the sales tax applied at the register (Goldin and Homonoff 2013). This opens the possibility that poorer families could be disproportionately responsive to sales taxes on diapers as well. Adda and Cornaglia (2006) find that smokers respond to cigarette taxes by purchasing fewer in number but using them more intensely, therefore inhaling higher concentrations of nicotine and carcinogens. A related concern for diapers is the risk that parents may purchase too few diapers for their children, requiring a longer duration for each diaper use and leading to potential health consequences.

There is reason to believe that the elasticity of demand for diapers is particularly variable in income. For high-income households, diaper purchases may already be at their optimum level regardless of price, as there are only so many diaper changes necessary per day. However, many American families struggle to afford enough diapers for their children. A survey of low-income mothers in New Haven found that 27.5% reported diaper need (Smith et al. 2013), and an Obama White House blog post concurred that one in three American families face this difficulty (Muñoz 2016). If low-income households are liquidity constrained, their demand for diapers could be especially elastic (as diapers represent a greater share of their budget), which would imply a higher excess burden of sales taxes on diapers for this group. Thus, setting sales tax policy based on inelastic responses by high-income households could mean low-income households may be under-spending on diapers relative to the socially optimal amount.

Both survey and medical evidence suggest that diaper need is associated with negative health consequences. When poor families are not able to purchase enough diapers, they are forced to decrease the frequency of changing their children's diapers. Infrequent diaper changes lead to increased risks of urinary tract infections, diaper dermatitis, and secondary diaper dermatitis infections such as staph (Sugimura et al. 2009; Adalat, Wall, and Goodyear 2007; Fernandes, Machado, and Oliveira 2009). These increased infections lead to more doctors' visits and emergency room visits. Smith et al. (2013)'s survey of low socioeconomic status women showed a correlation between diaper need and increased stress and depression for mothers. The proposed mechanism for this relationship is that the financial inability to change a child's diaper when necessary increases their discomfort and crying, which can reinforce feelings of inadequacy and helplessness for their mothers.

To assess these issues, we study the response of diaper purchasing behavior to changes in taxes on diapers between 2006 and 2013. In addition to state-level changes in sales tax rates, there were four larger changes to diaper sales tax exemptions in this period. Connecticut exempted diapers from sales taxes under a clothing exemption until it was repealed in 2011. Meanwhile, New York eliminated their state sales taxes on diapers in 2006 as part of an overall sales tax exemption for clothing, and temporarily reinstated these taxes between 2010 and 2011 (creating three changes in policy). New York counties were allowed to individually decide whether to apply this exemption to local taxes, which created county-level variation in the changes in tax rates over this period. We use this variation to examine how diaper purchasing patterns respond after sales tax changes, and whether these responses are higher in low-income households and communities.

Our data sources are the Nielsen consumer and retail panel datasets, which are uniquely positioned to study the effects of sales taxes given their detailed product-level information (as previously used by Harding, Leibtag, and Lovenheim (2012) and Kroft et al. (2019)). We first use the Nielsen Homescan Consumer Panel data to investigate diaper purchasing patterns at the household level. We demonstrate that low-income households buy fewer diapers and spend less on diapers than high-income households.

We then analyze the effects of taxes on diapers by examining Nielsen Retail Scanner data, which provides information on the universe of point-of-sale transactions at a large panel of retailers. Overall, we find that stores in lower (below median) income counties show a high (1.6) elasticity of diaper purchases with respect to sales tax rates, with minimal responses in high income areas. Focusing on the four main changes to diaper tax exemptions in New York and Connecticut, we find the tax changes are almost completely passed through to consumers by retailers, leading to a 5.4% increase in the quantity of diapers sold at stores in low-income areas when diapers are tax-exempt. There is only a small (-1%) change in sales for high income areas, implying that low-income consumers are indeed more responsive to sales taxes on diapers.

Finally, we observe changes in spending on health-related products that are consistent with taxes on diapers inducing negative health outcomes; in particular, sales of children's pain medication fall by 6.2% in low-income exempt areas. These results imply that recent efforts to expand sales tax exemptions for diapers could have positive spillover effects.

The structure of this paper is as follows: Section 2 provides more detailed policy context about the tax changes we analyze and describe current tax changes; Section 3 describes our methods, including the data and models; and Section 4 summarizes the results of the descriptive Consumer Panel analysis, price changes, and main findings from the Retail Panel diaper sales analyses. Section 5 describes future directions for the project and Section 6 concludes.

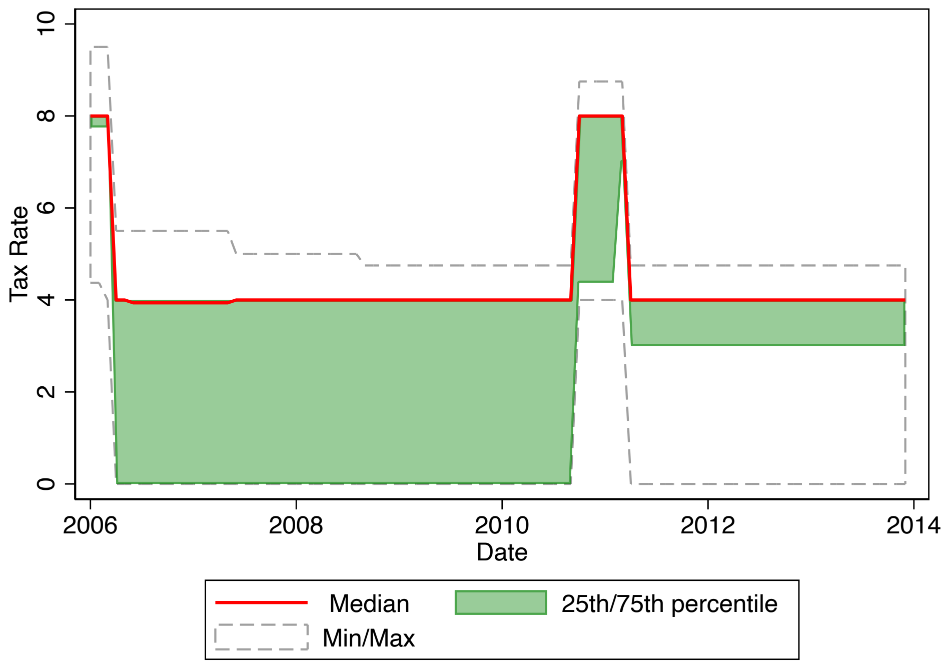
3.2 Policy Context

Federal transfer programs such as the Supplemental Nutrition Assistance Program (SNAP) and Women, Infants, and Children (WIC) do not cover diapers with their benefits. Thus, low-income households potentially face liquidity constraints when buying diapers that are not addressed by current programs.

This paper focuses on changes in diaper taxes between 2006 and 2013. Six states exempted diapers from sales taxes prior to 2006: Massachusetts, Minnesota, New Jersey, Pennsylvania, Rhode Island, and Vermont (Weir et al. 2014). Also, several states (Alaska, Delaware, Montana, New Hampshire, and Oregon) do not have sales taxes. These states did not change their tax treatment of diapers during the study period. Our first source of variation involves the remaining states, which experienced small changes in taxes on diapers due to variations in their regular sales tax rates.

Additionally, there were several larger changes to sales taxes on diapers in New York. First, diapers were included in an overall state sales tax exemption for clothing under $110 starting April 1, 2006. New York took the unusual step of allowing counties to decide whether local sales taxes would also exempt clothing; some did and some did not. The state sales tax rate in New York at the time was 4% and counties levied local sales taxes ranging from 0 to 5.5%. Additionally, New York repealed this exemption temporarily between October 1, 2010 and March 31, 2011 (with the phased ending of the exemption suspension announced at the time of implementation). The variation in county-level taxes this created is graphed in Figure 3.1, and shown as a map in Figure 3.2; there was considerable geographic heterogeneity in diaper taxes.

As of 2006, Connecticut exempted clothing purchases under $50 from sales tax, and diapers are categorized as clothing in their tax code. The clothing exemption was repealed effective July 1, 2011. This meant that diapers went from not being taxed to being taxed at the state rate of 6.35%. Connecticut would later pass a diaper-specific sales tax exemption in 2016



Source: New York Department of Taxation and Finance, Publication 718-C, various years.

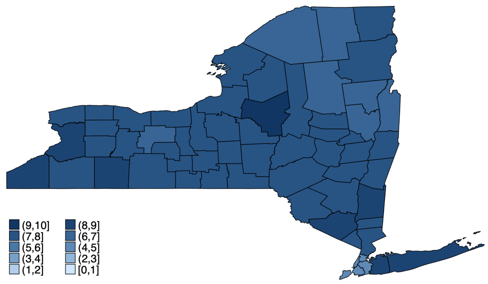
Figure 3.1: Distribution of County Tax Rates on Diapers in New York State over Time

that went into effect July 1, 2018, but this occurred after the time period covered in this paper.

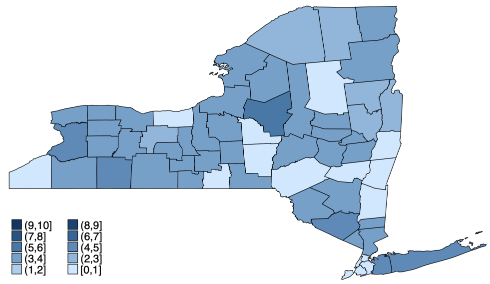
The New York and Connecticut tax changes applied to all clothing purchases under certain amounts, so the effects of the tax changes cannot be attributed to diapers alone. Annual spending by families in the lowest quintile of incomes on apparel averaged $845 in 2006 and $774 in 2010, meaning the income change from this channel should be relatively modest at $50-$60.[1] However, to the degree that the tax reduction generates income effects, our results will overstate the effects of diaper taxes alone on behavior. We address this issue through model specifications comparing stores in high and low income regions, which should help control for economic trends such as changing incomes.

Connecticut's diaper-specific exemption is part of a recent trend in states considering specific policies to exempt diapers from sales taxes, often in conjunction with exempting feminine

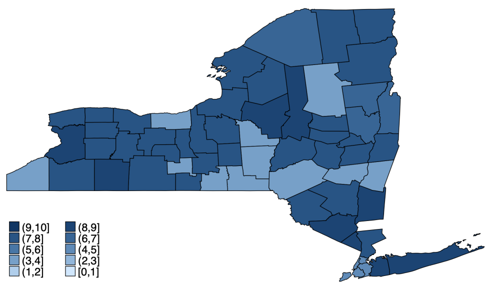
[1]Figures from "Quintiles of income before taxes: Average annual expenditures and characteristics, Consumer Expenditure Survey" (https://www.bls.gov/cex/csxstnd.htm.)



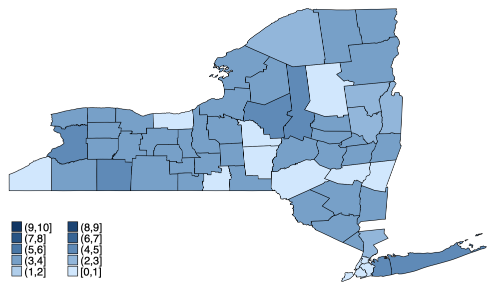
(a) Jan. 2006



(b) Apr. 2006



(c) Oct. 2010



(d) Apr. 2011

Source: New York Department of Taxation and Finance, Publication 718-C, various years.

Figure 3.2: Tax Rates by County in New York State over Time

hygiene products such as tampons. In 2017 alone, 18 states introduced bills that would eliminate or decrease sales taxes on diapers (Loughead 2018). A potential law that would add diapers to sales tax exemptions failed in California in 2016 after it was vetoed by the governor, but a later effort passed an exemption for 2020-2021. In 2020, Virginia lowered taxes on diapers and other hygiene products to the lower (2.5%) rate at which food is taxed. Washington D.C. eliminated sales taxes on diapers in October 2019, and Louisiana will no longer tax diapers starting in 2021. Findings on earlier changes to diaper taxes could help inform debates about these current laws.

3.3 Methods

3.3.1 Data

Our first data source is the Nielsen Consumer Panel (Homescan) data, a panel that tracks the purchases of 40,000-60,000 households. Participants scan in all purchases, amounts, and trips for both food and non-food spending intended for in-home use. Demographic information about the panelists includes years of birth for children, race, geography, and lagged income categories. We use this purchasing data from 2004-2013 to illustrate diaper purchasing patterns by income at the household level, restricting the sample to household-years with a child born within the previous 3 years and at least one diaper purchase.[2]

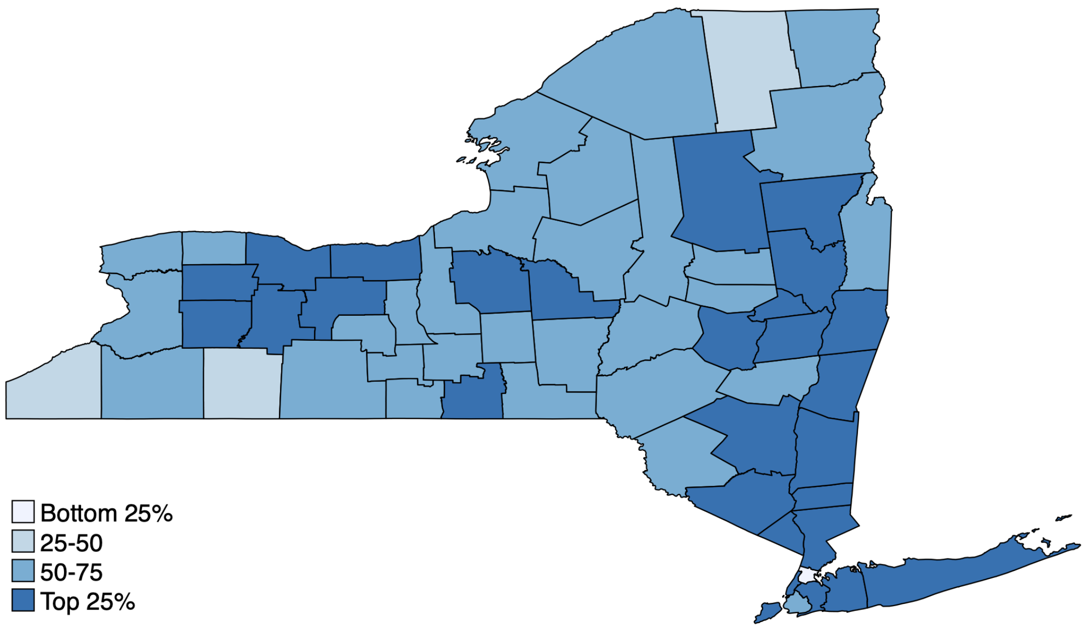
As the sample size of the Homescan data is limited for small geographic areas, we also use the Nielsen Retail Scanner Data to investigate diaper purchases. This data includes the universe of scanned point-of-sale retail transactions for a large sample of businesses, including about 50% of U.S. grocery and drug stores. Information is recorded on sales and average prices at the store-UPC-week level, which we aggregate into information about product categories at the store-month level (from 2006-2013).

While the size of the scanner data provides considerable precision, one limitation is that the data contains information only on stores, without information on individual households purchasing at those stores. To focus on low income households, we use the location of stores in low income areas as a proxy for customer characteristics. Low income areas are defined as those in the bottom 50% of median household income, as measured by American Community Survey data (from 2005-2010) at the county level. This corresponds to a median household income of $42,224. Our sample includes all stores with at least one sale of diapers in the study period.

Figure 3.3 displays the distribution of these low income areas across New York State (the main area where treatment varies in the sample); as the figure shows, the low income areas are

[2]Since children with the birth year in that panel year are not reported until the following year, the sample is also restricted to households that are in the data the following year.

geographically diverse, including both the Bronx and more rural counties upstate.



Source: American Community Survey, 2005-2010.

Figure 3.3: Counties in New York State by Quartile of Median Income

To gather information on diaper sales tax exemptions, we consulted a report by the National Diaper Bank Network (Weir et al. 2014), supplemented by tax rate notices in the state of New York. We also gathered information on sales tax rates by county over time from the Avalara AvaTax API (a commercial software product used by retailers to compute sales taxes due).

3.3.2 Models

When analyzing the consumer response to a sales tax change, it is important to consider how much of these tax changes are passed through to consumers. Diapers are a very concentrated market with two main producers (Procter & Gamble and Kimberly-Clark), but they are also often sold as a loss leader at stores (Neff 2006). Dellavigna and Gentzkow (2019) demonstrate that there are largely uniform prices across grocery store chains, so we might not expect large local price responses to local tax changes. In order to test whether prices changed at stores following the changes in tax exemption status, we run the following model in the retail panel data for each change to statewide tax exemptions of diapers:

$$\ln(p_{iwu}) = \alpha_i + \theta_w + \gamma_u + \beta \text{ TaxChange}_{sw} + \varepsilon_{iwu} \tag{3.1}$$

where *TaxChange* indicates the first full week of the tax change through the rest of the calendar year, p is price in store i in week w for UPC code u (UPC codes identify a specific brand, type, and count of a package of diapers). By having store, week, and UPC fixed effects, the coefficient of interest on *TaxChange* shows how prices at the UPC level changed after the change to the state diaper exemption policy.

To investigate the effects of sales taxes on diaper purchases in the retail panel data, we use a treatment intensity model exploiting spatial and temporal variation in the tax rates applied to diapers. This method has the advantage of using the most variation in treatment status (as sales tax changes other than a full repeal contribute to identification). We use the following model:

$$\begin{aligned} \ln(y_{it}) &= \alpha_i + \theta_t + \beta \ln(1 + \tau_{ct}) + \varepsilon_{it} && (3.2) \\ &\iff \text{low}_{ct} = 1 \end{aligned}$$

where i indexes stores located in state s and county c, t indexes (monthly) time periods, y_{it} is an outcome, and τ_{ct} is the *ad valorem* tax rate on diapers in county c at time t. Outcomes include the count of diapers sold, units of diapers sold (packages or boxes), sales revenue from diapers, price per diaper, and price per unit. The coefficient of interest β thus captures the elasticity of the measure of interest with respect to the change in the diaper sales tax rate. This specification limits the sample to stores located in low-income counties (indicated by low_{ct}).

We also examine the relative response in low and high income areas with a triple-difference model,

$$\ln(y_{it}) = \alpha_i + \theta_t \cdot \text{low}_{at} + \gamma \ln(1+\text{Tax}_{ct}) + \beta \ln(1+\text{Tax}_{ct}) \cdot \text{low}_{at} + \varepsilon_{it} \tag{3.3}$$

where stores in all areas are included, and the tax variable and time fixed effects θ_t are interacted with the indicator low_{ct} for a store being located in a low-income county. The coefficient of interest β thus captures the change in elasticity for low-income stores relative to higher income stores in counties with a tax change.

To examine effects of a discrete change in exemption status (rather than marginal changes), we also use a difference-in-differences model,

$$\ln(y_{it}) = \alpha_i + \theta_t + \beta \text{ Notax}_{st} + \varepsilon_{it} \tag{3.4}$$

$$\iff \text{low}_{ct} = 1$$

where Notax_{st} is an indicator for whether state s has no sales tax on diapers at time t. As discussed above, only New York and Connecticut vary the exemption status of diapers during the study period, so identification of the coefficient of interest β comes from within-store changes in outcomes before and after the policy changes in those states. The sample is still limited to stores located in low income areas.[3]

We use a triple-difference model analogous to Equation 3.3 to investigate distributional effects:

$$\ln(y_{it}) = \alpha_i + \theta_t + \theta_t \cdot \text{low}_{ct} + \gamma \text{ Notax}_{st} + \beta \text{ Notax}_{st} \cdot \text{low}_{ct} + \varepsilon_{it} \tag{3.5}$$

and to see how treatment effects vary over time, we use an event study model building on Equation

[3] Connecticut has no counties in the bottom half of median incomes, so all variation in this regression comes from New York.

3.4,

$$\ln(y_{it}) = \alpha_i + \theta_t + \sum_{d=-q, d\neq -1}^{p} \beta_d \cdot I(t = e_s + d) + \varepsilon_{it} \tag{3.6}$$

$$\iff -q \leq t \leq p \text{ and } \text{low}_{ct} = 1$$

where the model includes indicators $I(\cdot)$ of time to the treatment event e_s, ranging from q periods before the event to p periods after, giving time varying coefficients β_d. There are two possible treatments in the policy context during our sample period: going from no sales tax exemption to having an exemption (New York in 2006 and 2011), or losing an existing exemption (New York in 2010 and Connecticut in 2011). We run separate models for the two cases.

3.4 Results

3.4.1 Differences By Household Income

First, we examine the Nielsen Consumer Panel (Homescan) data for diaper purchasing patterns at the household level. Although there is insufficient power to analyze changes across the two tax changes of interest, the advantage of this data is the ability to look at diaper purchases by household income.

Descriptive statistics on demographics by income level are in Table 3.1. The average number of children 3 and under is relatively similar at 1.25 for households with income over \$35,000 and 1.27 for households with income under \$35,000.[4] This number is over 1 in both cases, so to facilitate interpretation, further results are divided by the number of children 3 and under in the household. The lower income group has more members of racial minorities and lower levels of education for female heads of household.

[4]Child age is inferred from year of birth; this variable includes children with the concurrent year of birth and previous 3 calendar years.

Table 3.1: Descriptive Statistics By Income Level, Consumer Panel

	Mean Over 35k	**Mean Under 35k**
Num Children 3 & under	1.25	1.27
Race & Ethnicity		
White Non-Hispanic	0.76	0.72
Black	0.07	0.11
Asian	0.07	0.04
Other	0.06	0.09
Hispanic	0.09	0.11
Female Head Education		
Less Than HS	0.01	0.05
HS	0.10	0.25
Some Coll	0.22	0.32
College	0.48	0.34
Post College	0.20	0.06

Source: Nielsen Consumer Panel Dataset.
Sample: Households 2004-2013 with at least 1 child born within past 3 years, in the data the following year, that made at least 1 diaper purchase that year.

The Consumer Panel data shows that low-income families buy fewer diapers per child than high-income families. Table 3.2 reports a t-test on the mean of yearly diaper quantity purchased. Higher income families purchase nearly 1000 diapers per child each year while lower income families purchase about 880; this difference is statistically significant. A further breakdown by more granular income levels is shown in Figure 3.4.[5] Diaper quantity monotonically increases with household income category. Yearly diaper spending is also lower for low-income households; Table 3.3 shows that households with yearly income over \$35,000 spend \$32.05 more per year than households with yearly income under \$35,000. We would expect our observed levels of yearly spending to be an underestimate for a year of a child's life because our sample restriction of a child born within the past 3 years depends on calendar year. A child could be born part way through the year, so diapers would not be needed until then, or a child could stop using diapers part way through the year, so diapers would not be needed after. Additionally, spending incorporates both price and quantity, so differences in spending could reflect a combination of

[5]These levels map to the stratified sampling of the Nielsen Consumer Panel.

Table 3.2: Yearly Diaper Quantity Per Child Under 3, by Income Level

	Mean	Std Err	Obs
Over 35k	994.57	8.22	6071
Under 35k	879.89	21.13	1228
Difference	114.68	7.72	
Pr 2-sided T-test	0.000		

Source: Nielsen Consumer Panel Dataset.
Sample: Households 2004-2013 with at least 1 child born within past 3 years, in the data the following year, that made at least 1 diaper purchase that year.

Table 3.3: Yearly Diaper Spending Per Child Under 3, by Income Level

	Mean	Std Err	Obs
Over 35k	227.65	2.00	6071
Under 35k	195.60	4.67	1228
Difference	32.05	1.85	
Pr 2-sided T-test	0.000		

Source: Nielsen Consumer Panel Dataset.
Sample: Households 2004-2013 with at least 1 child born within past 3 years, in the data the following year, that made at least 1 diaper purchase that year.

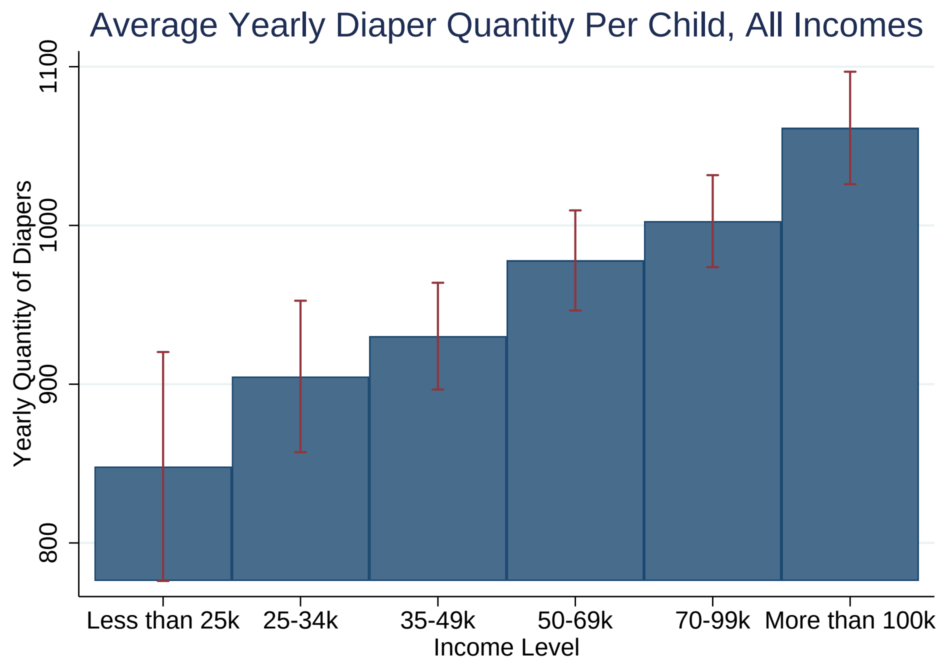
lower quantities and cheaper options.

There is some seasonality in diaper purchases. Figure 3.5 shows monthly purchases by income level.[6] Although there are some differences by month, there are not drastically different monthly patterns by income.

3.4.2 Pricing and Passthrough

Looking at the effects of sales taxes on prices at the product level, results from the model of price passthrough (from Equation 3.1) appear in Table 3.4. Two of the tax changes, NY in 2006 and NY in 2010, show a small but statistically significant change in price. The signs on the coefficients are consistent with less than complete passthrough to consumers; following a sales tax decrease, prices slightly rose, and following a sales tax increase, prices slightly decreased.

[6]The Nielsen panel years do not map perfectly to calendar years because of a cutoff in reporting the last Saturday of December, so only household-months with full coverage of a month are used. Household-months are counted from the first diaper purchase to the last diaper purchase, with any intervening months without diaper purchases assumed to be 0.



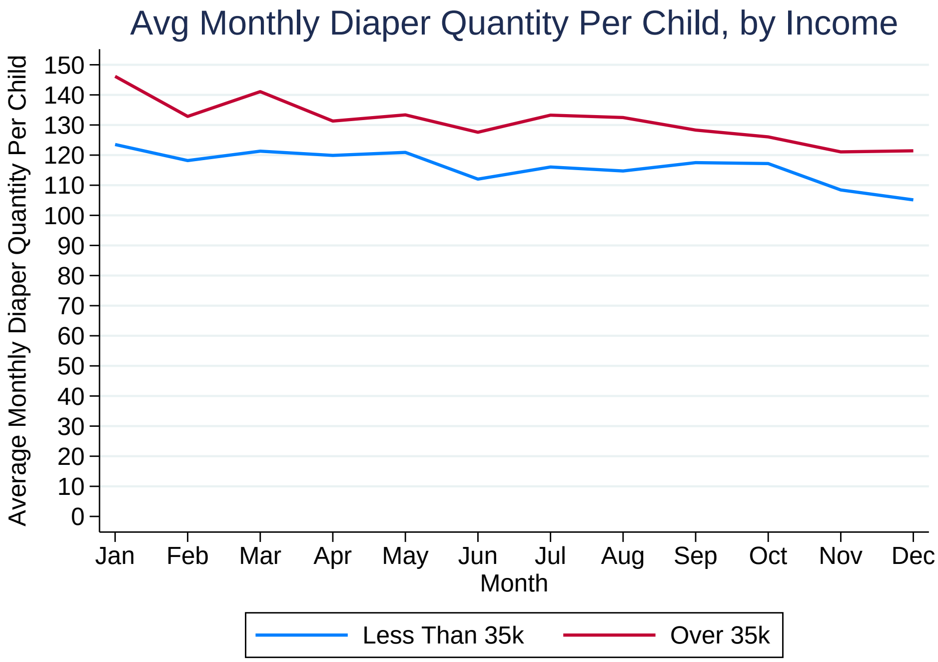
Source: Nielsen Consumer Panel Dataset.
Sample: Households 2004-2013 with at least 1 child born within past 3 years, in the data the following year, that made at least 1 diaper purchase that year.

Figure 3.4: Consumer Panel Yearly Quantity of Diapers Per Child, By Income Levels

However, these changes are very small. The tax changes were 4% at minimum (because of the state tax changes, with additional 0-5.5% changes in county tax rates) and the responses in prices were both less than 0.6%. Neither tax change in 2011 was associated with a statistically significant change in prices. Because there was slightly less than perfect passthrough, our estimates for responses to the stated tax rates and changes could be considered underestimates of responses to overall prices.

3.4.3 Primary Regression Results

Turning now to the primary regression specification in the retail data, we measure elasticities of diaper sales outcomes with a treatment intensity model (as given in Equation 3.2). This method has the advantage of using all variation in tax rates that do not represent a complete



Source: Nielsen Consumer Panel Dataset.
Sample: Households 2004-2013 with at least 1 child born within past 3 years, in the data the following year and for whole month, that made at least 1 diaper purchase that year.

Figure 3.5: Consumer Panel Monthly Quantity of Diapers Per Child, By Income

Table 3.4: Prices After Tax Changes, UPC Level

	NY 2006 4-9.5% Tax Dec. Ln Unit Price	NY 2010 4-9.5% Tax Inc. Ln Unit Price	NY 2011 4-9.5% Tax Dec. Ln Unit Price	CT 2011 6.35% Tax Inc. Ln Unit Price
Tax Change	0.00587***	-0.00371***	0.0000361	-0.00188
	(0.000768)	(0.000948)	(0.00106)	(0.00270)
Constant	2.418***	2.436***	2.478***	2.476***
	(0.0000297)	(0.0000106)	(0.0000387)	(0.0000131)
N	59891011	64149230	64176220	61612013
Clusters	30947	33639	33428	31805

Source: Nielsen Retail Scanner Data.
Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors (clustered by store) in parentheses. All models include store, week, and UPC fixed effects. Treated indicates area has no sales tax on diapers in effect. The sample for each regression contains the store-week-UPC sales for the whole year. The NY 2011 sample does not contain CT and the CT 2011 sample does not contain NY.

Table 3.5: Effects on Diaper Purchases, Treatment Intensity Model

	(1) Ln Diapers Sold	(2) Ln Units	(3) Ln Sales	(4) Ln Diaper Price	(5) Ln Unit Price	(6) Ln Pkg Size
Tax Rate	-1.644***	-1.651***	-1.686***	-0.0406	-0.0300	0.0221
	(0.346)	(0.351)	(0.354)	(0.0555)	(0.0583)	(0.0634)
N	533919	533919	533919	533919	533919	533919
Clusters	6621	6621	6621	6621	6621	6621
R^2	0.929	0.920	0.932	0.811	0.875	0.792

Source: Nielsen Retail Scanner Data.
Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors (clustered by store) in parentheses. All models include store and (monthly) date fixed effects. Tax rate is measured as $\ln(1+\tau_{ct})$. Sample is limited to stores in counties in bottom 50% of median incomes (measured in 2005-2010 ACS data).

Table 3.6: Effects on Diaper Purchases, Treatment Intensity Model, Triple Difference

	(1) Ln Diapers Sold	(2) Ln Units	(3) Ln Sales	(4) Ln Diaper Price	(5) Ln Unit Price	(6) Ln Pkg Size
Tax Rate	0.0178	-0.195*	-0.0204	-0.0505**	0.164***	0.219***
	(0.116)	(0.112)	(0.117)	(0.0217)	(0.0274)	(0.0265)
Tax x Low Inc	-1.642***	-1.437***	-1.643***	0.00680	-0.192***	-0.197***
	(0.363)	(0.366)	(0.371)	(0.0592)	(0.0642)	(0.0684)
N	3092075	3092075	3092075	3092075	3092075	3092075
Clusters	38657	38657	38657	38657	38657	38657
R^2	0.957	0.947	0.957	0.843	0.881	0.890

Source: Nielsen Retail Scanner Data.
Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors (clustered by store) in parentheses. All models include store and (monthly) date fixed effects interacted with low income indicator. "Low Income" refers to stores in counties in bottom 50% of median incomes (measured in 2005-2010 ACS data). Tax rate is measured as a proportion.

repeal, including county variation in New York and changes to overall sales tax rates in other states. Table 3.5 shows results consistent with our hypothesis that low income households will have relatively elastic demand; the elasticity of diapers sales with respect to the tax rate is 1.6, indicating these sales are relatively elastic. This stands in sharp contrast to the effects on stores in high income areas in the triple difference (Table 3.6), which show little or no response to the tax change; there is no effect on diapers sold or total sales, and only a small change in the number of packages sold.

To examine discrete effects of tax changes, we consider the difference-in-differences model of Equation 3.4. This method reduces the amount of variation used (as it considers only the changes in state-level exemptions in New York and Connecticut), but focuses on areas where the tax change may be more salient (as diapers are completely exempt, rather than changes occurring through overall changes to sales tax rates). Table 3.7 shows a 5.4% increase in diapers sold in stores in low-income areas after diaper taxes are repealed.

We obtain similar results when measuring the number of package units (unique UPC codes) sold or total dollar sales. Prices per diaper sold fall very slightly (0.7%), consistent with the results in Table 3.4 on nearly complete passthrough. Since we have ruled out large retailer responses on UPC-level prices, price per diaper could vary with changes in bulk purchasing (with higher count packages having lower prices per diaper), or quality (with changes in purchasing patterns toward brands or product lines having higher prices per diaper). The price of the average package sold rises by 0.7%, which is consistent with an increase in bulk purchasing (average package size rises by 1.2%).

3.4.4 Effects on Health-Related Products

Table 3.8 examines how sales of diapers may affect health outcomes by examining sales of health products related to diaper need. We find an 6.2% drop in purchases of children's liquid pain medications in stores in low-income counties with a diaper sales tax exemption, suggesting

Table 3.7: Effects on Diaper Purchases, Difference-in-Differences

	(1) Ln Diapers Sold	(2) Ln Units	(3) Ln Sales	(4) Ln Diaper Price	(5) Ln Unit Price	(6) Ln Pkg Size
Treated	0.0535***	0.0433***	0.0473***	-0.00764*	0.00732**	0.0122**
	(0.0146)	(0.0126)	(0.0137)	(0.00442)	(0.00367)	(0.00552)
N	533919	533919	533919	533919	533919	533919
Clusters	6621	6621	6621	6621	6621	6621
R^2	0.929	0.920	0.932	0.811	0.875	0.792

Source: Nielsen Retail Scanner Data.
Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors (clustered by store) in parentheses. All models include store and (monthly) date fixed effects. Treated indicates area has no sales tax on diapers in effect. Sample is limited to stores in counties in bottom 50% of median incomes (measured in 2005-2010 ACS data).

Table 3.8: Effects on Log Sales of Health Products, Difference-in-Differences

	(1) Baby Powder	(2) Baby Oil	(3) Baby Ointments	(4) UTI Pain Meds	(5) Child Pain Meds
Treated	0.0316*	-0.0290	-0.0186	0.00124	-0.0623***
	(0.0178)	(0.0220)	(0.0187)	(0.0259)	(0.0177)
N	530435	522658	523103	357289	544537
Clusters	6591	6511	6491	6190	6824
R^2	0.765	0.715	0.849	0.810	0.874

Source: Nielsen Retail Scanner Data.
Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors (clustered by store) in parentheses. Outcomes are log sales of each product. All models include store and (monthly) date fixed effects. Treated indicates area has no sales tax on diapers in effect. Sample is limited to stores in counties in bottom 50% of median incomes (measured in 2005-2010 ACS data).

that children are less likely to experience painful medical conditions as diaper sales increase. This result is particularly striking if children's pain medication is a normal good; the sales tax exemption was part of an overall clothing exemption and that income effect, although small, would increase sales of normal goods. We also see a 3.2% increase in sales of baby powder, a complimentary product to diapers. Other products such as ointments do not show a price response.

Table 3.9: Effects on Diaper Purchases, Triple Difference

	(1) Ln Diapers Sold	(2) Ln Units	(3) Ln Sales	(4) Ln Diaper Price	(5) Ln Unit Price	(6) Ln Pkg Size
Treated	-0.0108**	-0.00694	-0.0135**	-0.00370***	-0.00528***	-0.00424***
	(0.00546)	(0.00526)	(0.00552)	(0.00118)	(0.00134)	(0.00132)
Treated x Low Inc	0.0581***	0.0437***	0.0547***	-0.00188	0.0132***	0.0166***
	(0.0157)	(0.0145)	(0.0153)	(0.00455)	(0.00407)	(0.00560)
N	3097203	3097203	3097203	3097203	3097203	3097203
Clusters	38671	38671	38671	38671	38671	38671
R^2	0.957	0.947	0.957	0.843	0.881	0.890

Source: Nielsen Retail Scanner Data.
Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors (clustered by store) in parentheses. All models include store and (monthly) date fixed effects interacted with low income indicator. Treated indicates area has no sales tax on diapers in effect. "Low Income" refers to stores in counties in bottom 50% of median incomes (measured in 2005-2010 ACS data).

3.4.5 Triple Difference Results

One potential concern is that because the difference-in-difference models compare stores in low-income areas across states, there may be underlying changes in New York or Connecticut over time that contaminate the results. Table 3.9 presents results of a triple-difference specification (Equation 3.5), and finds that in the states that changed their diaper tax exemptions, there is a small (1.1%) decrease in sales of diapers, along with a small (0.4%) fall in price per diaper. It is only for stores in low income areas that sales increase (a relative 5.8% difference). This supports the hypothesis that low income consumers are more responsive to the diaper tax exemptions.

We also apply this methodology to child-related health products. Table 3.10 shows that while low and high income areas generally had precise and small (1-3%) changes in spending on health products after diaper taxes were removed, low income areas see a relative decrease of 8.9% in spending on liquid children's pain medication. This implies that the increase in diaper purchases induced by the tax reduction could have positive health effects in low income areas.

Table 3.10: Effects on Log Sales of Health Products, Triple Difference

	(1) Baby Powder	(2) Baby Oil	(3) Baby Ointments	(4) UTI Pain Meds	(5) Child Pain Meds
Treated	0.0137**	-0.00883	-0.0228***	0.0255***	0.0317***
	(0.00646)	(0.00538)	(0.00537)	(0.00583)	(0.00703)
Treated x Low Inc	0.0160	-0.0271	0.000220	-0.0256	-0.0889***
	(0.0187)	(0.0232)	(0.0192)	(0.0257)	(0.0196)
N	3082971	3054452	3051801	2343532	3139240
Clusters	38174	37494	37425	35738	39151
R^2	0.850	0.811	0.902	0.793	0.913

Source: Nielsen Retail Scanner Data.
Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors (clustered by store) in parentheses. Outcomes are log sales of each product. All models include store and (monthly) date fixed effects interacted with low income indicator. Treated indicates area has no sales tax on diapers in effect. "Low Income" refers to stores in counties in bottom 50% of median incomes (measured in 2005-2010 ACS data).

3.4.6 Event Study

To examine how the sales tax removal affects outcomes over time, Figure 3.6 shows an event study (as in Equation 3.6) of changes in diaper sales before and after statewide exemptions were applied in New York (in April 2006 and again in April 2011). Because the Retail Scanner data only begins in January 2006, we are only able to examine 3 months of pre-period observations. Diaper sales are slightly higher in New York in the months prior to the exemption, which does raise some concerns about parallel trends; however, the increase in sales after the tax exemption is higher in magnitude and appears to be sustained.

Figure 3.7 displays similar information for the times exemptions were ended (New York in 2010 and Connecticut in 2011).[7] In these cases, we see roughly parallel trends before the change (with treated areas having slightly lower diaper sales), and lower levels after the change (although results are noisy). Because the October 2010 change in New York was reversed 6 months later, the time period to observe effects is limited.

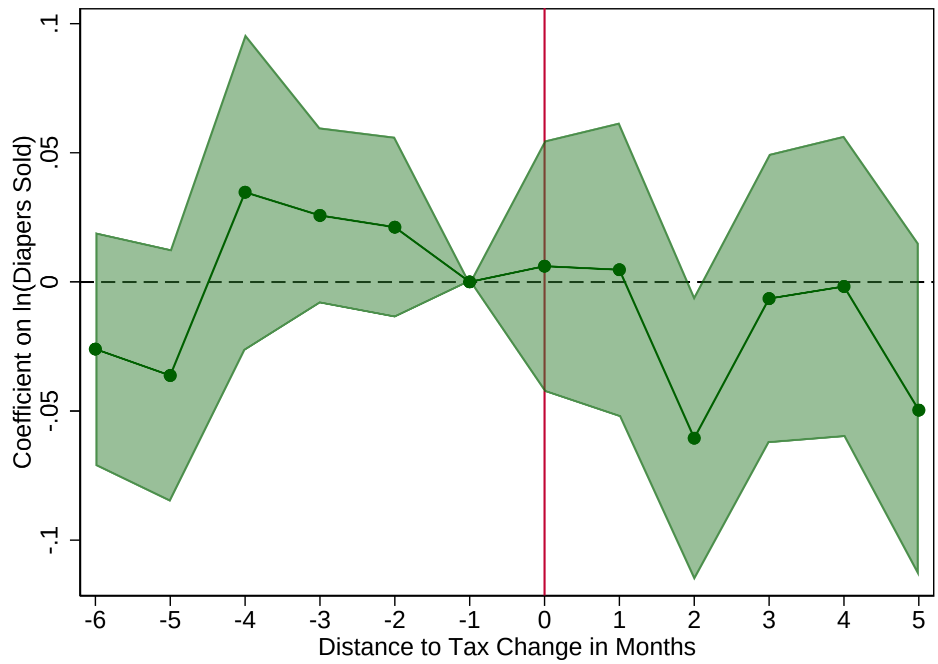
[7] Connecticut has no counties in the bottom half of median incomes, so all variation in the current specification comes from New York.

Source: Nielsen Retail Scanner Data.
Notes: Graph depicts coefficients from a regression of diaper sales on treatment status (NY in April 2006 or April 2011). Shaded ares are 95% confidence intervals.

Figure 3.6: Event Study at Beginning of Exemption for Diapers

3.5 Directions for Future Work

- Analyze further breakdowns of responses by income level.
- Investigate potential sources for health outcomes data.
- Consider a synthetic control model.
- Include further robustness checks using alternate definitions of low-income (including high poverty rate) and controlling for local unemployment rates.



Source: Nielsen Retail Scanner Data.
Notes: Graph depicts coefficients from a regression of diaper sales on end of treatment status (NY in October 2010 or CT in July 2011). Shaded ares are 95% confidence intervals.

Figure 3.7: Event Study at End of Exemption for Diapers

3.6 Conclusion

Since diapers are a product for which low-income areas have a higher elasticity of demand, are not covered under current federal aid programs, and have potential health consequences for children, the policies surrounding regressive sales taxes on diapers are important to study. We find that consumers in low income areas are quite responsive to diaper taxes, and removing taxes on diapers can have potential positive spillovers in reducing use of children's pain medication. Our findings have policy relevance for states that are currently considering adding diapers to their lists of tax-exempt products.

3.7 Acknowledgements

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Researcher(s) own analyses calculated (or derived) based in part on data from The Nielsen Company (US), LLC and marketing databases provided through the Nielsen Datasets at the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business. The conclusions drawn from the Nielsen data are those of the researcher(s) and do not reflect the views of Nielsen. Nielsen is not responsible for, had no role in, and was not involved in analyzing and preparing the results reported herein.

Chapter 3 is coauthored with Chelsea Swete, and is currently being prepared for submission for publication of the material. Kye Lippold and Chelsea Swete shared writing and analysis responsibilities equally for this chapter.

Appendix A

Appendices to Chapter 1

A.1 Details on Taxsim Calculations

To prepare the SIPP data for use with Taxsim (using the Taxsim27 program, http://users.nber.org/~taxsim/taxsim27/stata.html), I made the following assumptions:

1. I assumed all tax units filed as single, head of household, married filing jointly, or as dependent filers (following Taxsim procedures, which assume no units file as married filing separately or qualifying widow[er]s). To construct tax units, I grouped individuals with their spouses (with the person listed first in the SIPP household roster treated as the "primary" filer), treated records with no spouse but with dependents as heads of household, and counted other records as single filers.

2. While the SIPP topical modules include some information on claiming of dependents, I found this data to be poor quality in recent years (with many non-responses). Therefore, I constructed counts of dependents for each household using information available in all years of the panel. Following tax rules, I counted potential dependent children as those who are under age 19 (or under age 24 if a full-time student), unmarried, and living in the same household as their parents or guardians. I also included unmarried parents who have income less than that of their children (but only if their AGI is below the filing threshold for dependents), to account for the fact that many non-child dependents are elderly parents. I only allowed children and parents to be claimed as dependents; other individuals in the household (i.e., "qualifying relatives" for tax purposes) were not captured, as well as children who lived with their parents during the year but not in the last observed month (usually December).[1]

3. The SIPP data on itemized deductions in topical modules is also limited, so I attempted to use as much information as was available to capture these deductions. My method broadly

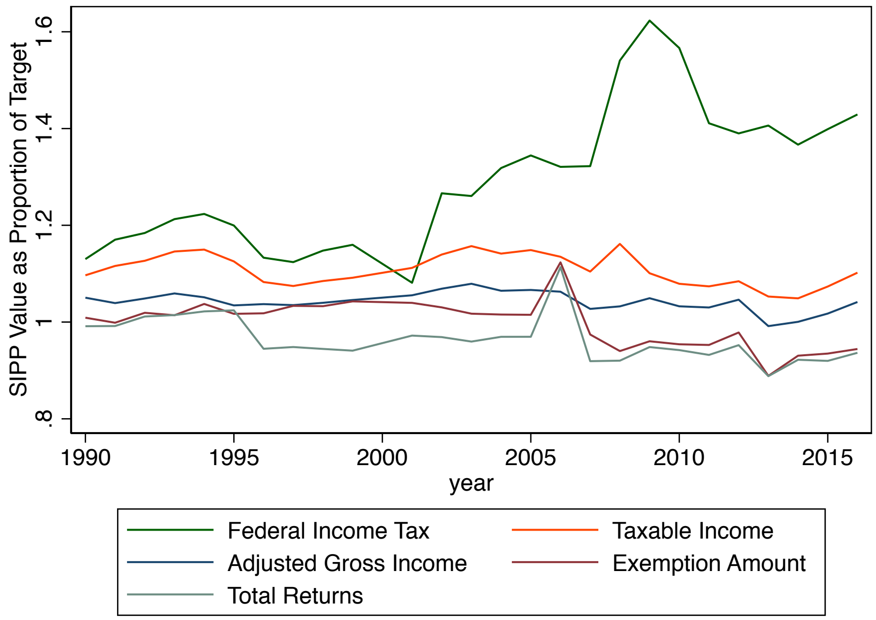
[1] For consistency over time, I excluded the rule in 2005 and later that allows disabled children of any age to be claimed as dependents.

follows Coey (2010); I computed mortgage interest as the reported amount of the mortgage on a primary residence times the reported interest rate, computed real estate taxes as the midpoint of the available bins for property taxes, included state and local income taxes as part of Taxsim's standard methods, and counted medical expenses as a deduction when they exceeded a certain percentage of AGI. Charitable donations and other miscellaneous deductions were not captured.

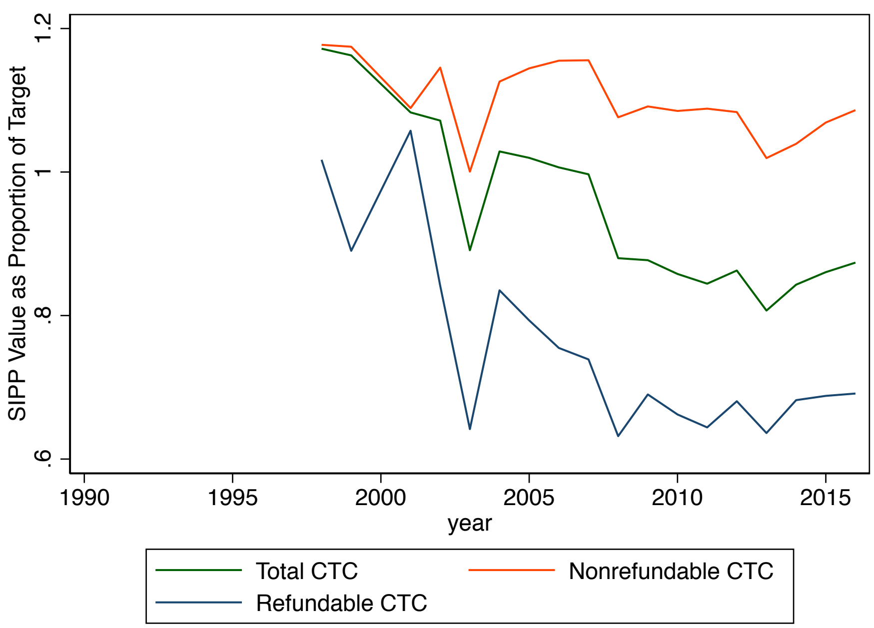
4. The information in the SIPP topical modules (on deductions, capital gains, medical expenses, rent paid, and child care expenses) are only asked in some years of each panel. To impute the missing years, I carried forward values from prior topical modules within each panel, and inflated items using the CPI-U.[2]

Figure A.1 compares the totals for several variables between the SIPP and the official aggregates from the IRS Statistics of Income for this period, focusing on households with AGI less than $100,000 in nominal terms (to avoid issues with topcoding of income in the SIPP) who are likely to file returns (having positive earnings, or non-zero calculated taxes). While there are some discrepancies (for example, the number of dependents tends to be too low in the SIPP, and estimated federal income tax tends to be too high), most SIPP totals are within 10% of the administrative value (Figure A.2a). Figure A.2b shows how estimates of the CTC compare to IRS totals; the SIPP consistently finds too large an amount of nonrefundable CTC and too low an amount of refundable CTC, consistent with other datasets such as the Current Population Survey. However, the total amount of CTC captured is about 90% of target.

[2] I did not inflate the value of mortgage interest paid, as this is usually fixed in nominal terms, or of capital gains or property taxes, which are taken as the mean of intervals and thus imprecise.



(a) Tax Totals



(b) CTC Totals

Notes: Targets taken from IRS Statistics of Income (various years). Values are for non-dependent tax filers with Adjusted Gross Income under $100,000. Only households observed in December in the SIPP are included.

Figure A.1: Calibration of SIPP Total Tax Values to Administrative Targets

A.2 Mathematical Appendix: Dynamic Model of Discrete Labor Force Participation with Taxes

In this section, I develop a dynamic model to illustrate the differences between steady-state and intertemporal substitution elasticities of labor force participation. This clarifies how the CTC affects intertemporal incentives when it is lost at the age 17 cutoff. The model draws heavily on the fixed work costs framework of Eissa, Kleven, and Kreiner (2006; 2008), but provides a new extension of their results to a dynamic setting (in the vein of Macurdy (1981)).

Consider a continuum of households who live for τ periods indexed by $t = 1, \ldots, \tau$ and maximize a concave, continuous utility function $U[C_t]$ over consumption C_t to solve

$$V[A_t, \theta_t] = \max_{p_t, A_{t+1}} U[C_t] - F p_t + \beta E_t [V[A_{t+1}, \theta_{t+1}]]$$

$$\text{s.t. } A_{t+1} = (1 + r_t)(A_t + N_t + (1 - p_t) G_t + w_t p_t - T[w_t, \nu_t] p_t - C_t)$$

where p_t is an indicator for labor force participation, A_t are assets, and β is a discount rate. Wages w_t, non-labor income N_t, and interest rates r_t are given exogenously in each period. Households are identical except for a varying disutility of participating in work F distributed according to a generic CDF $\phi[F]$, drawn once and then held fixed for each household. The parameter $\theta_t = \{w_t, G_t, N_t, \nu_t, r_t\}$ is a vector of the exogenous state variables. Note this model assumes additive intertemporal separability of utility, additive separability of fixed costs, complete capital markets (to borrow and save), rational expectations, and unitary household decision-making.[3] I use square brackets to denote arguments of a function, and abstract from intensive margin decisions by assuming indivisible labor, so households in each period can either earn the wage w_t

[3]The problem could equivalently be considered as the work decision of the primary earner in each household, which is the margin examined by my empirical analysis.

or earn 0.[4] Households then choose whether to participate in work and how much they will save for the next period.

Households face a tax-transfer system $T[w_t, N_t, \nu_t]$ if they have earned income w_t, where ν_t indexes parameters of the tax system. T can be negative if households receive net transfers, and I define $G = -T[0, N_t, \nu_t]$ as the value of transfers when not working. I then define

$$a_t = a_t[w_t, G_t, N_t, \nu_t] = \frac{T[w_t, N_t, \nu_t] + G_t}{w_t} \tag{A.1}$$

as the average tax rate faced when going to work (due to taxes paid and transfers forgone). We can thus simplify the household's problem to

$$\begin{aligned} V[A_t, \theta_t] &= \max_{p_t, A_{t+1}} U[C_t] - F p_t + \beta E_t [V[A_{t+1}, \theta_{t+1}]] \\ \text{s.t. } A_{t+1} &= (1 + r_t)(A_t + Y_t - C_t) \\ Y_t &= N_t + G_t + p_t w_t (1 - a_t) \end{aligned}$$

for current period (non-asset) income Y_t.

Finally, to simplify notation, define variables with a superscript as those evaluated at the superscript level of p_t. For example, U^1 (or U^0) is utility evaluated at the working (nonworking) state in that period. We then define

$$\Delta x = x^1 - x^0$$

as the difference between variables in the work and non-work states.

[4] As is standard for dynamic models (Blundell and Macurdy 1999), the intensive margin choice of hours worked conditional on participation involves intratemporal decisions only, and is not affected by the separable fixed cost. Thus, if labor is divisible by hours h_t, we can define an optimal h_t^* that will be chosen conditional on work, which would then carry through all the equations and generate the same result relating the two elasticities of interest.

In each period, available consumption levels with or without work are:

$$
\begin{aligned}
C_t^1 &= A_t + N_t + G_t - \frac{A_{t+1}^1}{1+r_t} + w_t\left(1-a_t\right) \\
C_t^0 &= A_t + N_t + G_t - \frac{A_{t+1}^0}{1+r_t}
\end{aligned}
$$

(where the desired savings level A_{t+1} may also vary with work status, as work affects available income).

We can first optimize over A_{t+1}, and then over p_t. In each of the two working states, the first order condition for optimization of savings is the standard Euler equation

$$
\begin{aligned}
U_c\left[C_t^1\right] &= \left(1+r_t\right)\beta E_t\left[\frac{\partial V\left[A_{t+1}^1, \theta_{t+1}\right]}{\partial A_{t+1}}\right] = \lambda^1 \\
U_c\left[C_t^0\right] &= \left(1+r_t\right)\beta E_t\left[\frac{\partial V\left[A_{t+1}^0, \theta_{t+1}\right]}{\partial A_{t+1}}\right] = \lambda^0
\end{aligned} \tag{A.2}
$$

which pins down the optimal asset values in work and non-work, A_{t+1}^{1*} and A_{t+1}^{0*}.

We can approximate optimal assets with a first order Taylor series as follows:

$$
\begin{aligned}
A_{t+1}^{1*}\left[A_t, \theta_t\right] &\approx A_{t+1}^{0*}\left[A_t, \theta_t\right] + \alpha \Delta Y \\
&= A_{t+1}^{0*} + \alpha w_t\left(1-a_t\right) \\
\Rightarrow \Delta A_{t+1}^{*} &= \alpha w_t\left(1-a_t\right)
\end{aligned} \tag{A.3}
$$

where $\alpha = \frac{\partial A_{t+1}^{0*}}{\partial Y_t^0}$ is the marginal propensity to save out of current period income when not working.

With this expression, we have

$$\begin{aligned}
\Delta C &= C_t^1 - C_t^0 \\
&= w_t\left(1-a_t\right) - \frac{\Delta A_{t+1}^{*}}{1+r_t} \\
&\approx w_t\left(1-a_t\right) - \frac{\alpha w_t\left(1-a_t\right)}{1+r_t} \\
&= w_t\left(1-a_t\right)\left(1-\frac{\alpha}{1+r_t}\right) \qquad \text{(A.4)}
\end{aligned}$$

Each person then works according to a cutoff condition that arises once assets are chosen optimally by (A.2):

$$\begin{aligned}
V^1\left[A_t,\theta_t\right] &\geq V^0\left[A_t,\theta_t\right] \\
\Rightarrow U^1 - F + \beta E_t\left[V\left[A_{t+1}^{1*},\theta_{t+1}\right]\right] &\geq U^0 + \beta E_t\left[V\left[A_{t+1}^{0*},\theta_{t+1}\right]\right] \\
\Rightarrow \Delta U + \beta E_t\left[\Delta V_{t+1}\right] &\geq F \qquad \text{(A.5)}
\end{aligned}$$

To clarify equation (A.5), I note that by taking a second order Taylor expansion, we have

$$\begin{aligned}
U\left[C_t^1\right] &= U\left[C_t^0\right] + U_c^0\cdot(\Delta C) + \frac{1}{2}U_{cc}^0\cdot(\Delta C)^2 + R_2 \\
V\left[A_{t+1}^{1*},\theta_{t+1}\right] &= V\left[A_{t+1}^{0*},\theta_{t+1}\right] + V_A^0\cdot\left(\Delta A_{t+1}^{*}\right) + \frac{1}{2}V_{AA}^0\cdot\left(\Delta A_{t+1}^{*}\right)^2 + R_2
\end{aligned}$$

(where R_2 represents the third and higher order Taylor series terms). Note that a second order expansion is needed (rather than first order) to account for consumption smoothing in the dynamic model. Call the marginal utility of consumption in the non-work state λ^0, where its derivative is

$\lambda_c^0 = U_{cc}^0$. Then we have (by substituting from (A.2), (A.3), and (A.4)):

$$\Delta U \approx U_c^0 \cdot \left(C_t^1 - C_t^0\right) + \frac{1}{2} U_{cc}^0 \cdot \left(C_t^1 - C_t^0\right)^2$$
$$= \lambda^0 w_t \left(1 - a_t\right) \left(1 - \frac{\alpha}{1 + r_t}\right) + \frac{1}{2} \lambda_c^0 \left(w_t \left(1 - a_t\right) \left(1 - \frac{\alpha}{1 + r_t}\right)\right)^2 \tag{A.6}$$

and also

$$\Delta V_{t+1} \approx V_A^0 \cdot \left(\Delta A_{t+1}^*\right) + \frac{1}{2} V_{AA}^0 \cdot \left(\Delta A_{t+1}^*\right)^2$$
$$\Rightarrow E_t \left[\Delta V_{t+1}\right] = \frac{\lambda^0}{\beta \left(1 + r_t\right)} \left(\Delta A_{t+1}^*\right) + \frac{\lambda_c^0}{2\beta \left(1 + r_t\right)} \left(\Delta A_{t+1}^*\right)^2 \tag{A.7}$$
$$= \frac{\lambda^0 \alpha w_t \left(1 - a_t\right)}{\beta \left(1 + r_t\right)} + \frac{\lambda_c^0 \left(\alpha w_t \left(1 - a_t\right)\right)^2}{2\beta \left(1 + r_t\right)}$$

Putting the expressions (A.5), (A.6), and (A.7) together gives

$$F \leq \lambda^0 w_t \left(1 - a_t\right) \left(1 - \frac{\alpha}{1 + r_t}\right) + \frac{1}{2} \lambda_c^0 \left(w_t \left(1 - a_t\right) \left(1 - \frac{\alpha}{1 + r_t}\right)\right)^2$$
$$\cdots + \beta \left(\frac{\lambda^0 \alpha w_t \left(1 - a_t\right)}{\beta \left(1 + r_t\right)} + \frac{\lambda_c^0 \left(\alpha w_t \left(1 - a_t\right)\right)^2}{2\beta \left(1 + r_t\right)}\right)$$
$$= \lambda^0 w_t \left(1 - a_t\right) \left(1 - \frac{\alpha}{1 + r_t} + \frac{\alpha}{\left(1 + r_t\right)}\right) + \frac{1}{2} \lambda_c^0 w_t^2 \left(1 - a_t\right)^2 \left(\left(1 - \frac{\alpha}{1 + r_t}\right)^2 + \frac{\alpha^2}{1 + r_t}\right)$$
$$= \lambda^0 w_t \left(1 - a_t\right) + \frac{1}{2} \lambda_c^0 w_t^2 \left(1 - a_t\right)^2 \left(1 - \frac{2\alpha}{1 + r_t} + \frac{\alpha^2}{\left(1 + r_t\right)^2} + \frac{\alpha^2}{1 + r_t}\right)$$
$$= \lambda^0 w_t \left(1 - a_t\right) + \frac{1}{2} \lambda_c^0 w_t^2 \left(1 - a_t\right)^2 \left(1 - \frac{2\alpha}{1 + r_t} + \frac{\left(2 + r_t\right) \alpha^2}{\left(1 + r_t\right)^2}\right)$$
$$= \Delta V^*$$

Considering this equation intuitively, the term depending on α reflects the household's desire to smooth consumption in the dynamic model. To the extent that marginal utility is decreasing, the household is better off moving assets from a current higher wage period (where the household works) to a future period.

In each period, the probability of participation is thus

$$P_t = E\left[p_t\right] = \phi\left[\Delta V^*\right]$$

We can define

$$\frac{\partial P_t}{\partial\left(1-a_t\right)} = \phi'\left[\Delta V^*\right]\left(\lambda^0 w_t + \lambda_c^0 w_t^2\left(1-a_t\right)\left(1-\frac{2\alpha}{1+r_t}+\frac{\left(2+r_t\right)\alpha^2}{\left(1+r_t\right)^2}\right)\right)$$

and the intertemporal elasticity of substitution with respect to the next-of-tax rate (for an anticipated temporary change) is thus

$$\begin{aligned}
\varepsilon_I &= \frac{\partial P_t}{\partial\left(1-a_t\right)}\cdot\frac{1-a_t}{P_t} \qquad \text{(A.8)}\\
&= \frac{\phi'\left[\Delta V^*\right]}{\phi\left[\Delta V^*\right]}\left(\lambda^0 w_t\left(1-a_t\right)+\lambda_c^0 w_t^2\left(1-a_t\right)^2\left(1-\frac{2\alpha}{1+r_t}+\frac{\left(2+r_t\right)\alpha^2}{\left(1+r_t\right)^2}\right)\right)\\
&= \frac{\phi'\left[\Delta V^*\right]}{\phi\left[\Delta V^*\right]}\lambda^0 w_t\left(1-a_t\right)\left(1+\frac{\lambda_c^0}{\lambda^0}w_t\left(1-a_t\right)\left(1-\frac{2\alpha}{1+r_t}+\frac{\left(2+r_t\right)\alpha^2}{\left(1+r_t\right)^2}\right)\right)
\end{aligned}$$

Now consider the labor force participation elasticity in the steady-state (i.e. in response to an anticipated permanent change in taxes). In this case, a_{t+1} and future values change in the same way as a_t, so marginal utility will increase by the same amount in all periods. This implies there is no need to smooth consumption due to the wage change, or equivalently, $\alpha = 0$. Thus, we have the steady-state elasticity of participation with respect to the average tax rate as

$$\varepsilon_S = \frac{\phi'[\Delta V^*]}{\phi[\Delta V^*]}\lambda^0 w_t\left(1-a_t\right)\left(1+\frac{\lambda_c^0}{\lambda^0}w_t\left(1-a_t\right)\right) \qquad \text{(A.9)}$$

Comparing equations (A.9) and (A.8) we have

$$\frac{\varepsilon_I}{\varepsilon_S} = \frac{\frac{\phi'[\Delta V^*]}{\phi[\Delta V^*]}\lambda^0 w_t(1-a_t)\left(1+\frac{\lambda_c^0}{\lambda^0}w_t(1-a_t)\left(1-\frac{2\alpha}{1+r_t}+\frac{(2+r_t)\alpha^2}{(1+r_t)^2}\right)\right)}{\frac{\phi'[\Delta V^*]}{\phi[\Delta V^*]}\lambda^0 w_t(1-a_t)\left(1+\frac{\lambda_c^0}{\lambda^0}w_t(1-a_t)\right)}$$

We can cancel most terms, and get

$$\begin{aligned}
\frac{\varepsilon_I}{\varepsilon_S} &= \frac{\left(1+\frac{\lambda_c^0}{\lambda^0}w_t(1-a_t)\left(1-\frac{2\alpha}{1+r_t}+\frac{(2+r_t)\alpha^2}{(1+r_t)^2}\right)\right)}{\left(1+\frac{\lambda_c^0}{\lambda^0}w_t(1-a_t)\right)} \\
\Rightarrow \varepsilon_I &= \frac{\left(1+\frac{\lambda_c^0}{\lambda^0}w_t(1-a_t)\left(1-\frac{2\alpha}{1+r_t}+\frac{(2+r_t)\alpha^2}{(1+r_t)^2}\right)\right)}{\left(1+\frac{\lambda_c^0}{\lambda^0}w_t(1-a_t)\right)}\varepsilon_S
\end{aligned}$$

We can now rewrite several of these quantities in terms of empirically measurable parameters (which can be taken as constant over a small tax change). Note that $\frac{\lambda_c^0}{\lambda^0}$ is related to the coefficient of relative risk aversion γ and the savings rate $s_t = \frac{A_{t+1}^{0*}-A_t}{Y_t^0}$ as follows:

$$\begin{aligned}
\gamma = \frac{-C_t^0 U_{cc}^0}{U_c^0} &= -\left(A_t + N_t + G_t - A_{t+1}^{0*}\right)\frac{\lambda_c^0}{\lambda^0} \\
&= -\left(Y_t^0 + \left(A_t - A_{t+1}^{0*}\right)\right)\frac{\lambda_c^0}{\lambda^0} \\
&= -\left(Y_t^0 - s_t Y_t^0\right)\frac{\lambda_c^0}{\lambda^0} \\
\Rightarrow \frac{\lambda_c^0}{\lambda^0} &= \frac{-\gamma}{Y_t^0(1-s_t)}
\end{aligned}$$

We can then define the percentage return to work (i.e. the percent change in post-tax income when entering work)

$$W_t = w_t(1-a_t)/Y_t^0$$

So we have

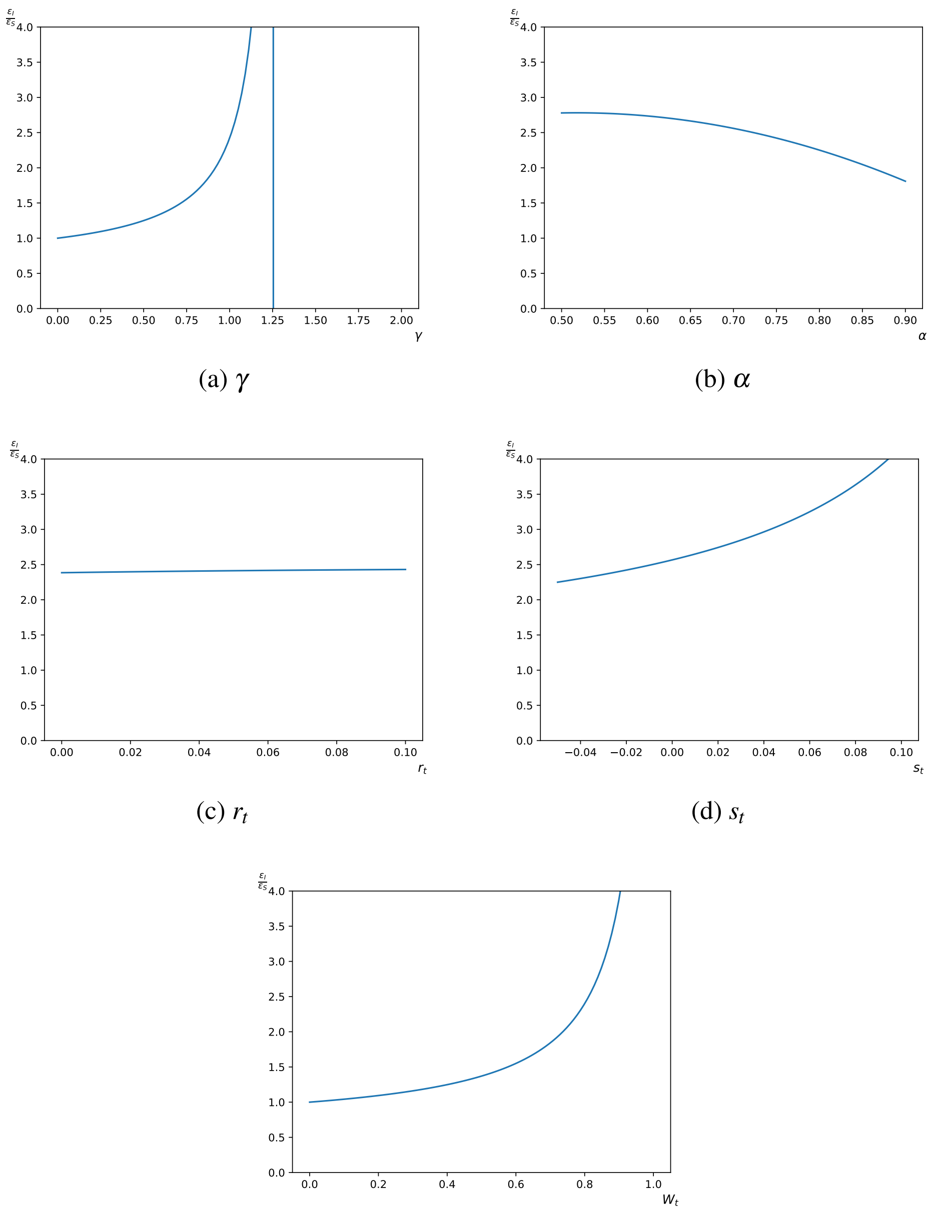
$$\varepsilon_I \approx \left(\frac{1 - \frac{\gamma W_t}{1-s_t}\left(1 - \frac{2\alpha}{1+r_t} + \frac{(2+r_t)\alpha^2}{(1+r_t)^2}\right)}{1 - \frac{\gamma W_t}{1-s_t}} \right) \varepsilon_S \tag{A.10}$$

This gives an expression for the ratio of the two elasticities that depends on several parameters:

- The coefficient of relative risk aversion γ
- The marginal propensity to save α (equal to $1-\mu$, where μ is the marginal propensity to consume)
- The interest rate on assets r_t
- The savings rate s_t
- The percent change in post-tax income when working W_t

I then use formula (A.10) to compare the two elasticities in the context of my study, using the calibration discussed in Section 1.5.4.

Figure A.2 displays how the ratio between the two elasticities depends on the values of the parameters (holding other parameters constant at their calibrated value). The relationship is quite stable across reasonable values of r_t and α, varies more based on s_t and W_t, and is quite sensitive to γ (with a discontinuity around a value of 1.25).



(a) γ

(b) α

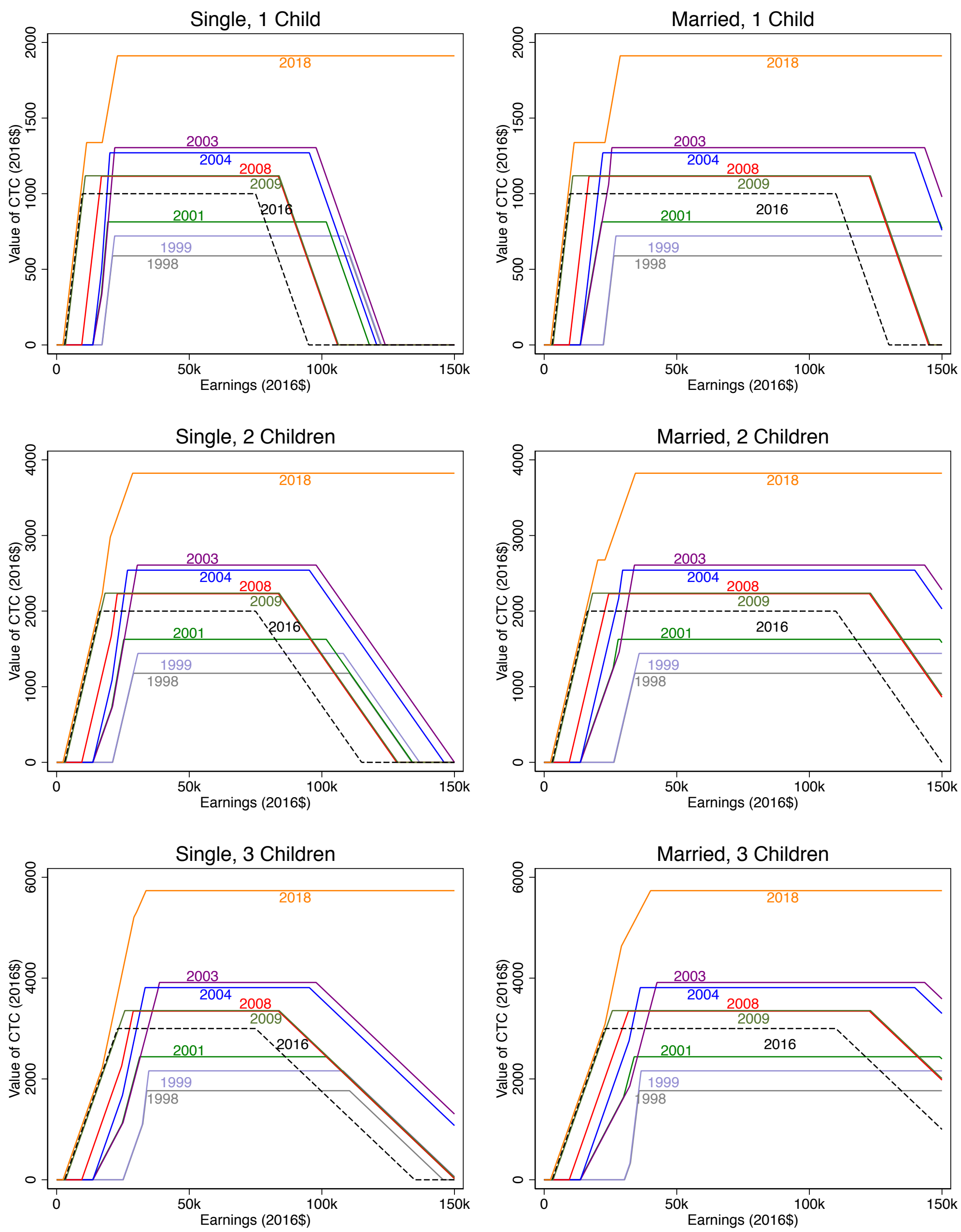
(c) r_t

(d) s_t

(e) W_t

Figure A.2: Sensitivity of Elasticity Ratio Calculation to Parameter Values

A.3 Additional Figures and Tables



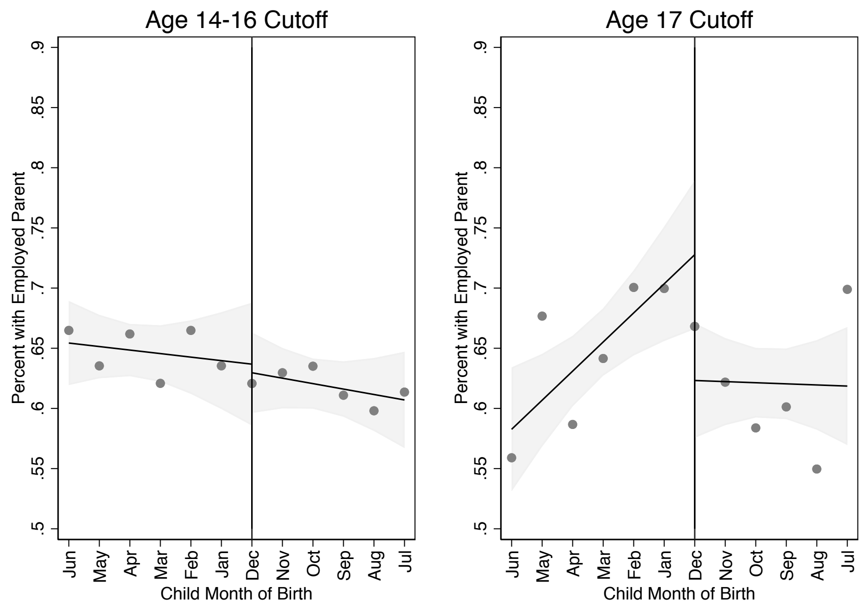
Source: Author's calculations using Taxsim. Calculations assume no unearned income or itemized deductions. Years listed are those where the main CTC parameters were changed by law, as well as 2016.

Figure A.3: Amount of CTC by Family Type, Tax Laws for Various Years

Table A.1: Parameters of the CTC Over Time

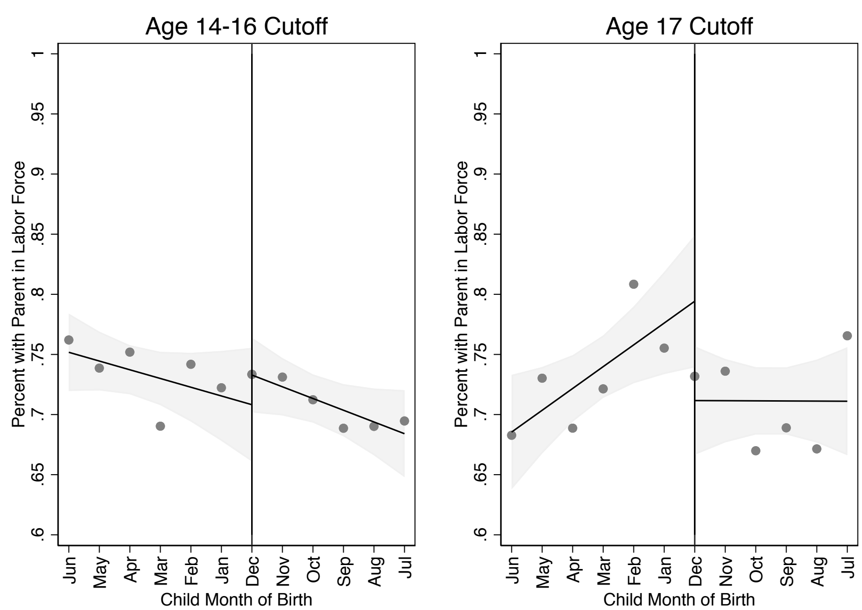
Year	Refundable Phase-In Start	Refundable Phase-in Rate	Maximum Credit per Child	Start of Phase-Out (AGI, for single / joint filers)	Phase-Out Rate
1998	N/A	N/A	$400	$75,000 / $110,000	5%
1999	N/A	N/A	$500	$75,000 / $110,000	5%
2000	N/A	N/A	$500	$75,000 / $110,000	5%
2001	$10,000	10%	$600	$75,000 / $110,000	5%
2002	$10,350	10%	$600	$75,000 / $110,000	5%
2003	$10,500	10%	$1000	$75,000 / $110,000	5%
2004	$10,750	15%	$1000	$75,000 / $110,000	5%
2005	$11,000	15%	$1000	$75,000 / $110,000	5%
2006	$11,300	15%	$1000	$75,000 / $110,000	5%
2007	$11,750	15%	$1000	$75,000 / $110,000	5%
2008	$8,500	15%	$1000	$75,000 / $110,000	5%
2009	$3,000	15%	$1000	$75,000 / $110,000	5%
2010	$3,000	15%	$1000	$75,000 / $110,000	5%
2011	$3,000	15%	$1000	$75,000 / $110,000	5%
2012	$3,000	15%	$1000	$75,000 / $110,000	5%
2013	$3,000	15%	$1000	$75,000 / $110,000	5%
2014	$3,000	15%	$1000	$75,000 / $110,000	5%
2015	$3,000	15%	$1000	$75,000 / $110,000	5%
2016	$3,000	15%	$1000	$75,000 / $110,000	5%
2017	$3,000	15%	$1000	$75,000 / $110,000	5%
2018	$2,500	15%	$2,000*	$200,000 / $400,000	5%

Notes: Refundable credit excludes special calculation of credit for 3+ kids. Refundable phase-in start refers to minimum level of earned income (from wages or self-employment).
* In 2018, only $1,400 of the credit per child is refundable.
Source: Crandall-Hollick (2018), Urban Institute (2019), and Tax Policy Center (2019b)



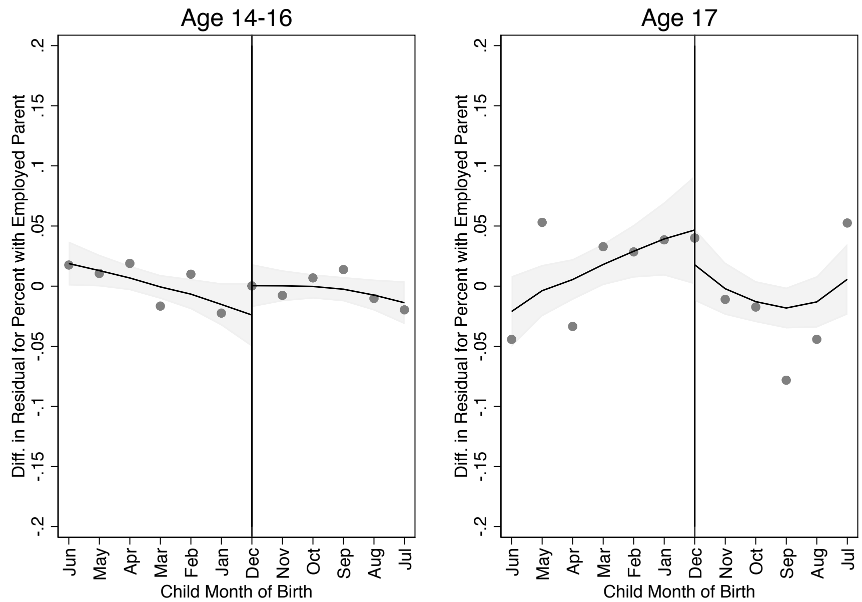
Notes: Discontinuities are estimated in primary sample with local linear regressions, triangular kernels, in 6-month bandwidths centered around each December age cutoff. Shaded areas are 90 percent confidence intervals.

Figure A.4: RD Results for Parental Employment, No Controls



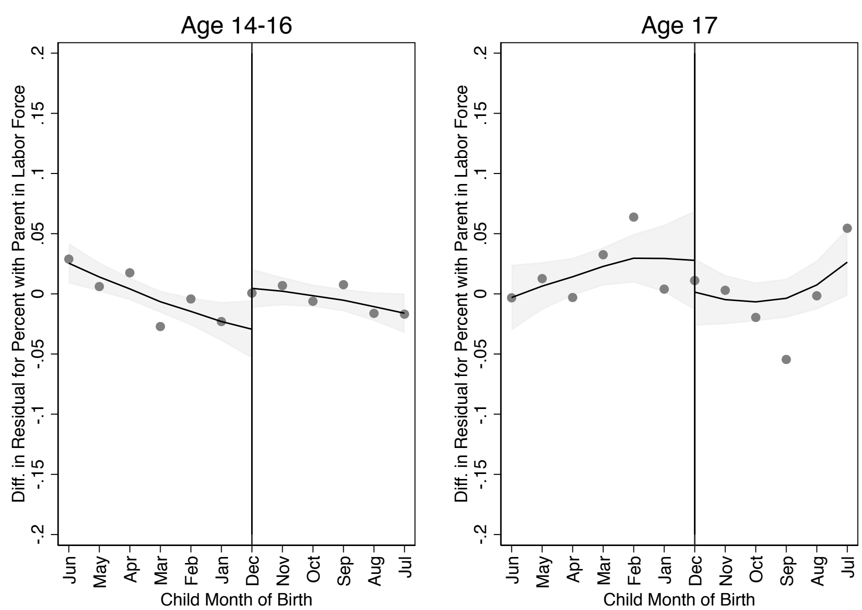
Notes: Discontinuities are estimated in primary sample with local linear regressions, triangular kernels, in 6-month bandwidths centered around each December age cutoff. Shaded areas are 90 percent confidence intervals.

Figure A.5: RD Results for Parental Labor Force Participation, No Controls



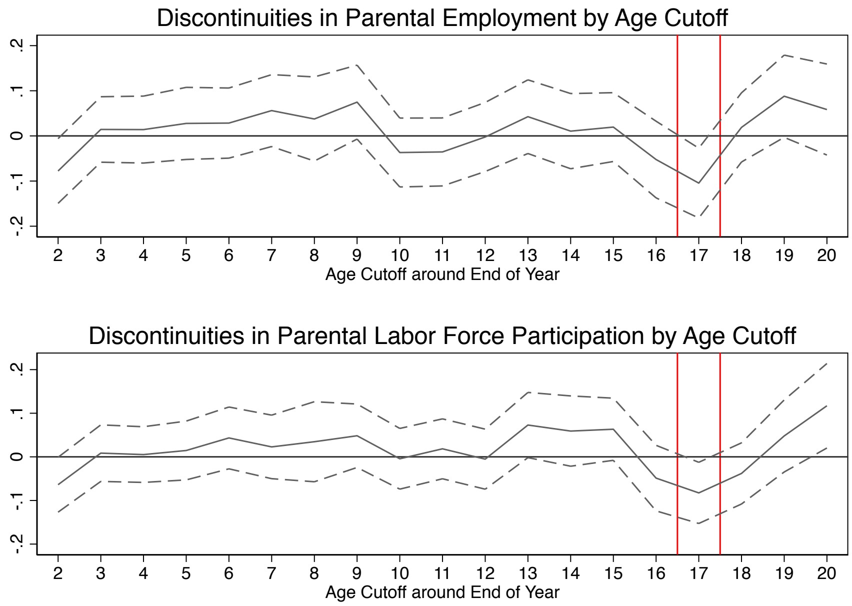
Notes: Discontinuities are estimated in primary sample with local linear regressions, triangular kernels, in 6-month bandwidths centered around each December age cutoff. Shaded areas are 90 percent confidence intervals. Results are estimated on residuals from a regression including lagged outcomes, year and state fixed effects, and other controls (as discussed in text).

Figure A.6: RD Results for Parental Employment, Local Polynomial Fit



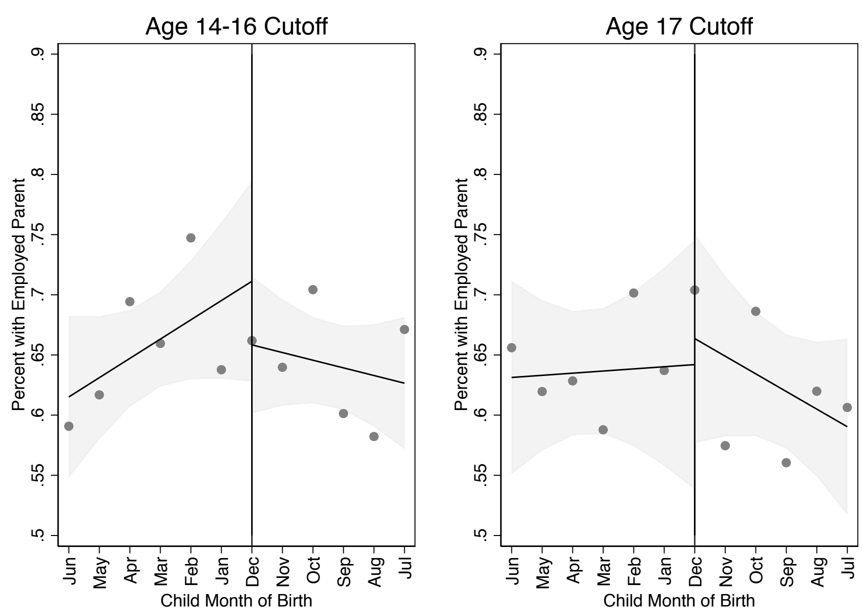
Notes: Discontinuities are estimated in primary sample with local linear regressions, triangular kernels, in 6-month bandwidths centered around each December age cutoff. Shaded areas are 90 percent confidence intervals. Results are estimated on residuals from a regression including lagged outcomes, year and state fixed effects, and other controls (as discussed in text).

Figure A.7: RD Results for Parental Labor Force Participation, Local Polynomial Fit



Notes: Discontinuities are estimated in primary sample with local linear regressions, uniform kernels, in 6-month bandwidths centered around each December age cutoff. Dashed lines are 90 percent confidence intervals. Age 1 is excluded due to imprecision.

Figure A.8: Discontinuities by Age Cutoff, No Controls



Notes: Results are for the 1984-2000 period, before CTC was a refundable credit. Discontinuities are estimated in primary sample with local linear regressions, uniform kernels, in 6-month bandwidths centered around each December age cutoff. Shaded areas are 90 percent confidence intervals.

Figure A.9: Placebo: RD Results for Parental Employment, Period Before CTC was Refundable

Appendix B

Appendices to Chapter 2

B.1 Details of Waiver Eligibility Calculations

In this section, I discuss the technical details of how ABAWD waiver eligibility is calculated, and how I model these rules in the empirical section of the paper. In all cases, states have some flexibility to choose when during a year to apply for waivers to maximize waiver coverage. Typically, states begin waivers in a given month (often near the start of the year), then renew those waivers every year thereafter. I thus treat the dates of application as exogenously determined by administrative considerations, and use those dates when computing waiver eligibility.

Define the variable $W(c,t)$ as a binary indicator for the waiver eligibility of county c for a waiver application at time t.[1] Waiver status is determined based on a binary indicator function $I(\cdot)$ and some measure of unemployment relative to cutoff $z(c,t)$, as follows:

$$W(c,t) = I\left(z(c,t) \geq 0\right)$$

The cutoff variable $z(c,t)$ is in turn based on functions using the unemployment rate $u_c(t_1,t_2)$ for county c (in state s) computed in a look-back period between months t_1 and t_2, defined as follows:

$$u_c(t_1,t_2) = \frac{\sum_{\tau=t_1}^{t_2} U(c,\tau)}{\sum_{\tau=t_1}^{t_2} L(c,\tau)}$$

where $U(c,\tau)$ is the number of unemployed individuals in county c at time τ, and $L(c,\tau)$ is the equivalent number of individuals in the labor force. This county unemployment rate is compared to some threshold value, which often depends on the national unemployment rate $u_n(t_1,t_2)$ over all counties c and states s:

$$u_n(t_1,t_2) = \frac{\sum_{\tau=t_1}^{t_2} \sum_s \sum_{c\in s} U(c,\tau)}{\sum_{\tau=t_1}^{t_2} \sum_s \sum_{c\in s} L(c,\tau)}$$

[1]Most waivers cover 12 months, so the waiver status of a county persists from time t until time $t+12$.

The policy rules that qualify an area (such as a county) to be waived are as follows:

1. The area unemployment rate for the last 12 months is 10% or higher.[2] Thus, the waiver formula under this method can be expressed as

$$W(c,t) = I\left(u_c\left(t-12, t-1\right) \geq 0.1\right)$$

2. All areas in a state that is eligible for unemployment compensation Extended Benefits (EB) or emergency benefits at any point in the prior 12 months are waived. Historically, a state is eligible for these benefits if the state has a 3-month unemployment rate that is above 6.5% (or above 6% during the 2008 recession, when standards were expanded). In particular, the extended benefit formula $EB_s\left(u_s^m(t)\right)$ in state s for time t is a complicated function of a 3-month moving average of the state "Total Unemployment Rate" (TUR),[3]

$$u_s^m(t) = \frac{\sum_{\tau=t-3}^{t-1} \sum_{c \in s} u_c(\tau, \tau)}{3}$$

which then is used to determine the extended benefit formula as follows (see Chodorow-Reich, Coglianese, and Karabarbounis 2018, for more details):[4]

$$EB_s\left(u_s^m(t)\right) = I\left(u_s^m(t) \geq \max\left(6.5, 1.1 \cdot \min\left(u_s^m(t-12), u_s^m(t-24)\right)\right)\right)$$

Thus, the extended benefit formula is indirectly a function of county unemployment rates,

[2] Areas can also qualify with a 10% average unemployment rate in a recent 3-month period, and the 12-month look-back period can technically include any period where even a single month is within one year of time t. I currently omit these complications in my formula.

[3] While there are additional criteria for unemployment insurance based on the "Insured Unemployment Rate" (IUR) using administrative statistics from the unemployment insurance system, these criteria rarely bind in practice, as discussed by Chodorow-Reich, Coglianese, and Karabarbounis (2018). Technically, the BLS estimates the state unemployment rates first and controls county unemployment rates to this total (rather than building up the state rate as the sum of counties), but I adopt this notation to clarify how county unemployment is related to extended benefits.

[4] This formula was modified during the Great Recession to have a cutoff value of 6 rather than 6.5 and suspend or modify the second term of the maximum at various points; I incorporate these details into the paper's calculations, but omit them here for simplicity.

as it relies on the state unemployment rate $u_s^m(t)$. The EB formula in turn determines waiver status based on the presence of EB status in the prior 12 months:

$$W(c,t) = I\left(\max_{\tau \in (t-12,t)} \left[EB_s\left(u_s^m(\tau)\right)\right] = 1\right)$$

3. The area is designated as a Labor Surplus Area (LSA) by the Department of Labor. The criteria in turn for being designated an LSA are that the area must have a 24-month unemployment rate that is 20% above the national average unemployment rate for the same period. The look-back period consists of the two calendar years ending prior to the current fiscal year. Thus, assuming a waiver application in January (so the fiscal year starts at $t-3$, the previous October), the waiver formula is

$$W(c,t) = I\left(u_c(t-36, t-13) \geq 1.2 \cdot u_n(t-36, t-13)\right)$$

4. In addition to the explicit LSA formula, states can also qualify areas for waivers based on a similar formula applying to broader areas and more recent time periods. In particular, states can waive any area or set of contiguous areas with an overall 24-month unemployment rate 20% above the national unemployment rate over a recent time period.[5]

    a. The 24 month period can be any period between the beginning of the LSA look-back period ($t-36$ for a waiver based on January data) and the date of the waiver request ($t-1$). In practice, many states use the most recent two calendar year period ($t-24$ to $t-1$), so I adopt this convention for this paper. Thus, an individual county can be

[5] One quirk of this method is that the LSA calculation for national unemployment includes unemployment in Puerto Rico, and the national cutoff is bounded in the range $(6\%, 10\%)$. However, the national unemployment calculation under this method does not include these features.

waived as follows:

$$W(c,t) = I\left(u_c\left(t-24,t-1\right) \geq 1.2 \cdot u_n\left(t-24,t-1\right)\right)$$

b. Areas that are contiguous and in the same state may be combined for the purposes of calculating the unemployment rates under this method.[6] Thus, a high unemployment area can be grouped with lower unemployment areas to bring the overall unemployment rate for the areas above the 20% cutoff (in some cases, an entire state can be exempt under the 20% criterion if the overall state unemployment rate is high). This leads to an alternate unemployment calculation $u_G\left(t_1,t_2\right)$, where the set G is the group of contiguous counties that are combined for purposes of applying for a waiver:

$$u_G\left(t_1,t_2\right) = \frac{\sum_{\tau=t_1}^{t_2} \sum_{c \in G} U(c,\tau)}{\sum_{\tau=t_1}^{t_2} \sum_{c \in G} L(c,\tau)}$$

Given these details, and defining the set B_c as all possible permutations of bordering counties, the overall formula to capture the 20% unemployment rule becomes:

$$W(c,t) = I\left(\max_{G \in B_c}\left[u_G\left(t-24,t-1\right)\right] \geq 1.2 \cdot u_n\left(t-24,t-1\right)\right)$$

c. States have control over which counties are grouped, which is difficult to mathematically model due to tradeoffs in states' objective functions (for example, a state might choose to group areas so as to keep the same areas waived over time for administrative simplicity, or might instead group to maximize the total number of people living in waived areas). I therefore approximate these groupings by using an algorithm that attempts to maximize the total population living in waived areas.

[6] Areas may also be noncontiguous if they are considered part of the same "economic area"; however, I do not consider this detail in my method.

i. I first compute the combined unemployment of each county group across all possible pairings with adjacent neighbors.

ii. If the unemployment of a pair is above the waiver cutoff, I group the two counties together. If there are multiple possible pairings, I group only the counties with the highest combined population.

iii. I define the grouped areas as a new county, and determine the new set of adjacent counties for the group.

iv. I then repeat this process iteratively until all counties are either grouped together in a region that is above the cutoff unemployment rate, or are ungrouped (because they cannot achieve higher unemployment than the cutoff by combining with their neighbors).

Once groups are established, I take the maximum of the group unemployment rate and the county's own unemployment rate, and use this value to compare to the national cutoff.

d. The FNS method of rounding unemployment rates under this method, as described in FNS (2016), is somewhat unusual; unemployment rates are to be first truncated to 4 decimal places, then rounded. The net effect is that areas that are -0.05 percentage points below the unemployment cutoff can be waived by rounding up (a detail criticized by Adolphsen et al. 2018). I incorporate this detail into my calculations for Methods 1 and 4.

e. County unemployment rates for a given month are typically not published by the BLS LAUS program until 3 months after the month in question. Thus, I apply a 3-month lag in all county calculations.

5. In 2006, FNS began allowing states to be waived for a two-year period if their prior 36 month unemployment rate exceeded the cutoff of 120% of national unemployment. This

method is similar to method 4, but extends the lookback period to 36 months. However, I find this rule is not typically pivotal when qualifying counties, so its' use mostly reflected an administrative simplification for states.

6. All areas of the country were waived as a stimulus measure from April 2009 to September 2010. Thus, I exclude these time periods from the study.

7. States can also use other criteria to apply for waivers, such as an area having a low or declining employment-to-population ratio or being cited in an academic study as an area where there is a lack of jobs. However, these criteria are rarely used by states or approved by FNS, so are not considered for this study.

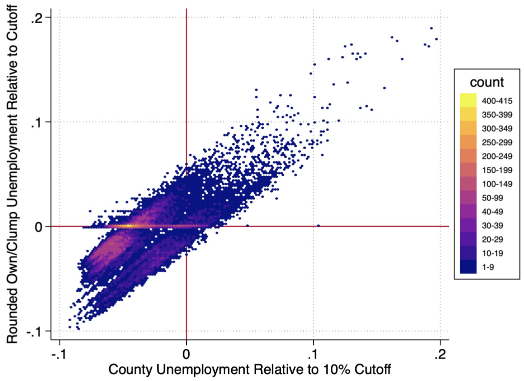
These formulas may seem complicated, but they are all tightly linked, as each involves a relationship between county (or state) unemployment rates and waiver status. In particular, I find that empirically only methods 2 and 4 are typically pivotal in qualifying a county for a waiver. As Figure B.1 shows, very few observations in the data are above the cutoff for methods 1, 3 or 5 and not above for method 4 (as there is little density in the lower right quadrant of the graphs). Only method 2 (the state unemployment calculation) shows substantial discrepancies in waiver eligibility compared to method 4.

Thus, I use two unemployment measures in the study:

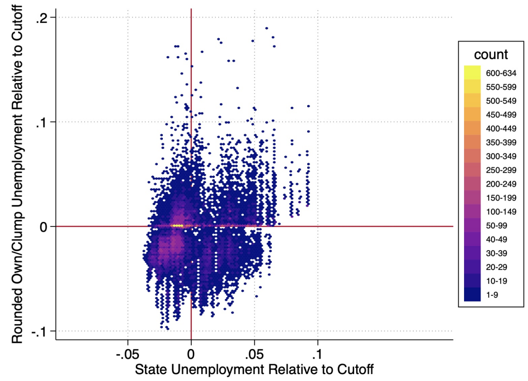
1. The state unemployment rate distance from the cutoff computed with Method 2.

2. A summary county unemployment rate, computed as the maximum distance from the cutoff for methods 1, 3, 4, and 5.

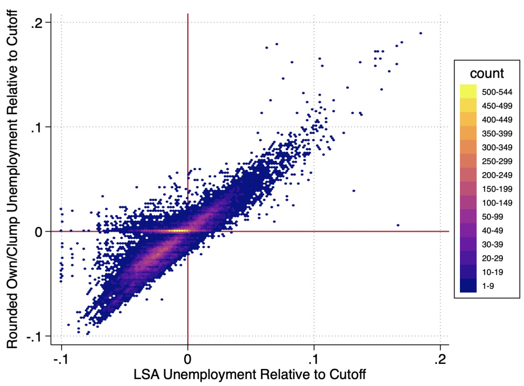
For these measures, values of zero or above imply an area is eligible for an ABAWD wavier, and the units are percentage points of unemployment in each area type (county, county group, or state) relative to the cutoff (120% of national unemployment, 10%, or the EB cutoff).



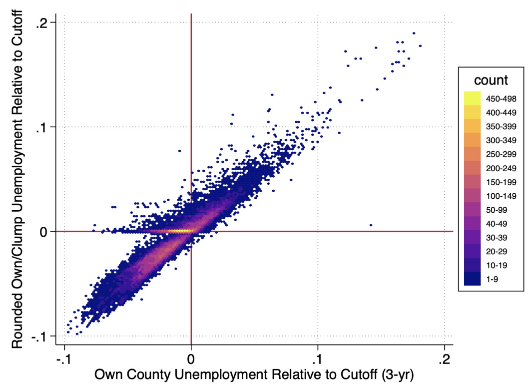
(a) 12-Month Unemployment (Method 1)



(b) State Unemployment (Method 2)



(c) LSA Unemployment (Method 3)



(d) 3-Year Unemployment (Method 5)

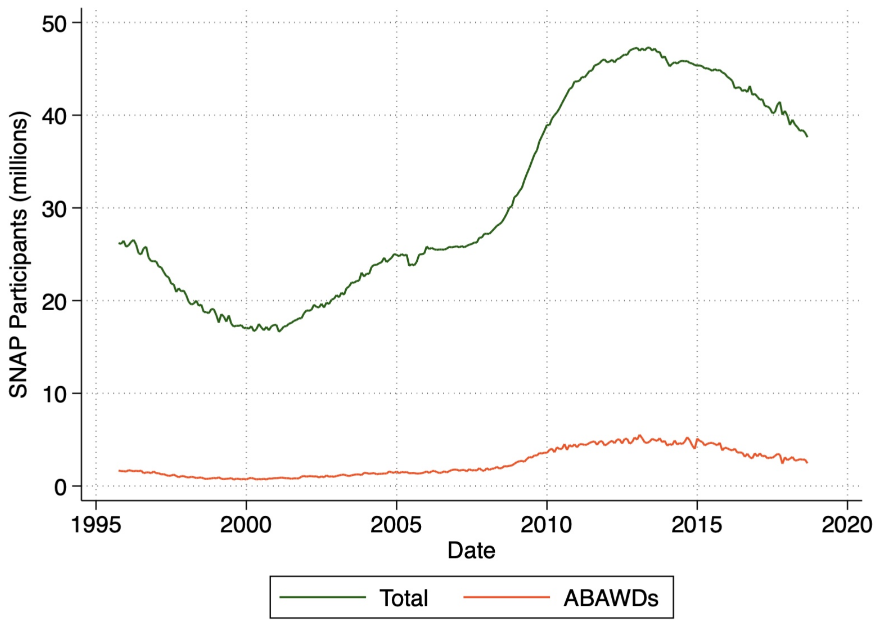
Note: The y-axis of each figure displays the unemployment rate relative to cutoff for a county group, computed with Method 4. The methods on the x-axis are as listed.

Figure B.1: Distribution of Counties in Sample by Various Unemployment Rate Calculations

It is worth noting that the USDA regulations adopted in 2019 make several changes to the waiver policies listed above.[7] Under the new policies, waivers through method 1 would be limited to 12 month periods only, methods 2, 3, and 5 would be eliminated, and method 4 would require states to use Labor Market Area (LMA) groupings and a minimum 6% unemployment cutoff when qualifying areas for waivers. These changes have been temporarily suspended by the CARES Act is response to the 2020 coronavirus pandemic.

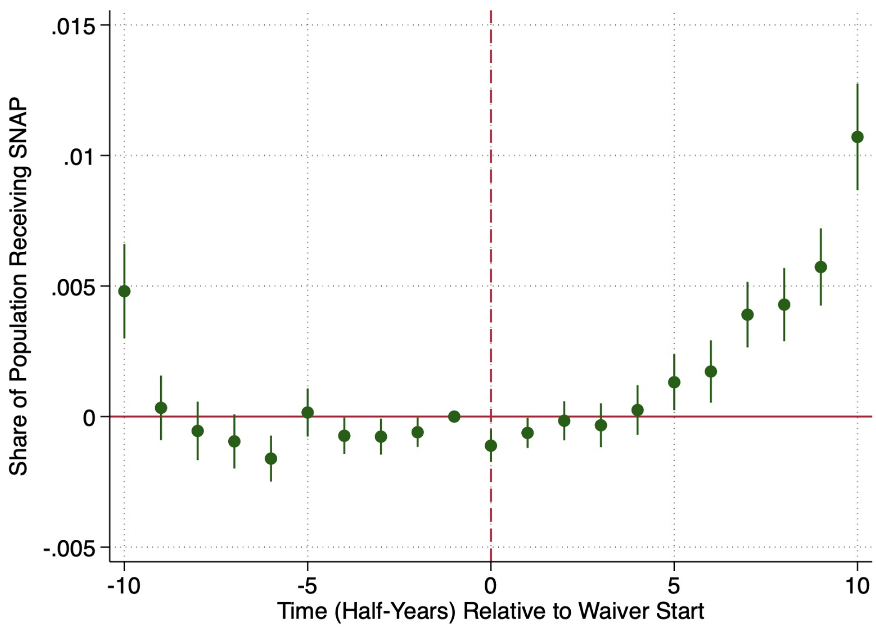
[7]These changes are detailed in 84 FR 66782, https://www.federalregister.gov/d/2019-26044.

B.2 Additional Figures and Tables



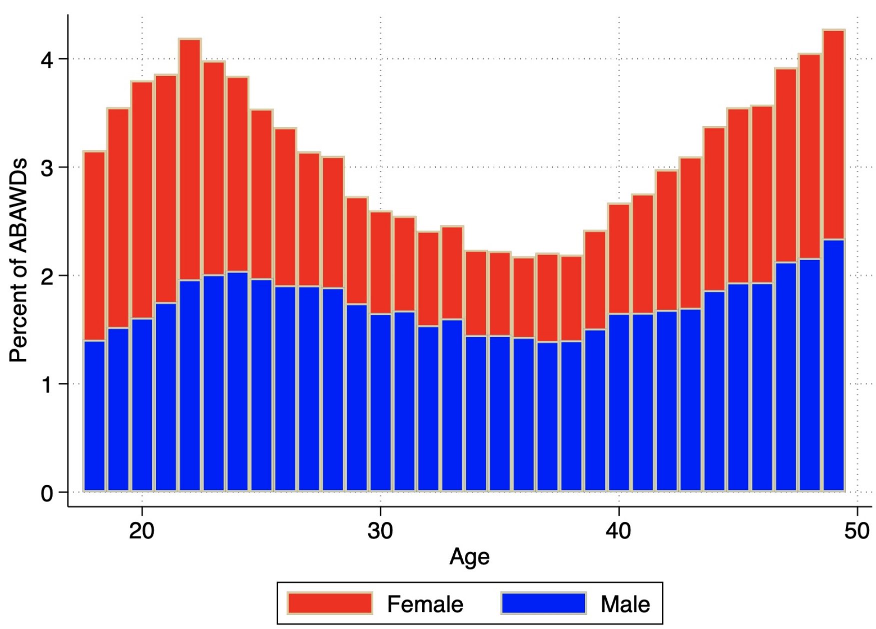
Source: SNAP QC data. Note: The drop in 2005 is due to sampling disruptions caused by Hurricane Katrina. ABAWD status is slightly underestimated in 2003-2006 due to limitations in measures of disability status in those years.

Figure B.2: SNAP Participation by Month and ABAWD Status, FY 1996-2018



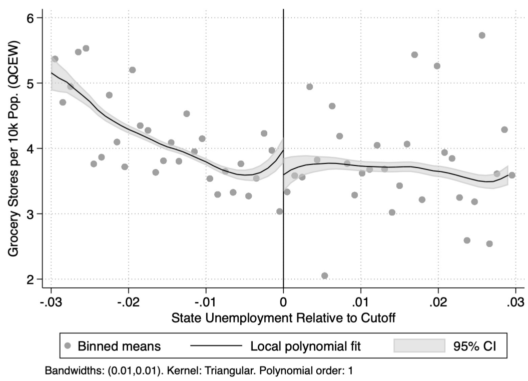
Note: Data is primary sample from 2004-2018. Results are coefficients of wavier status × time from a regression with county and time fixed effects. Bars are 95% CI (clustered at county level). Relative times are capped at an absolute value of 5 years.

Figure B.3: Event Study Estimates of ABAWD Waivers on SNAP Participation

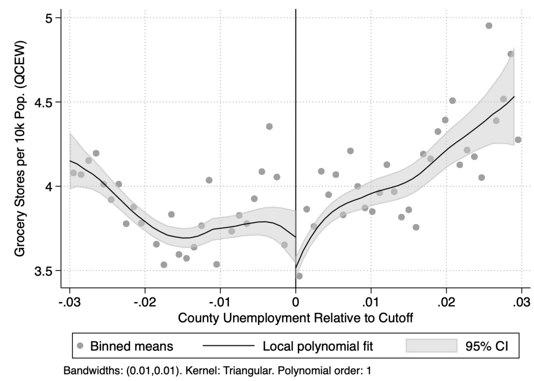


Source: SNAP QC data, FY 2004-2018.

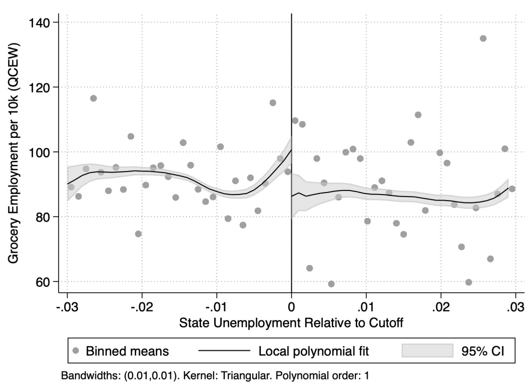
Figure B.4: Distribution of ABAWDs by Age



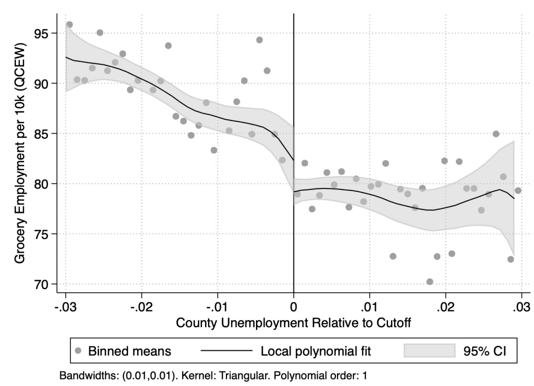
(a) Grocery Stores per 10k, State



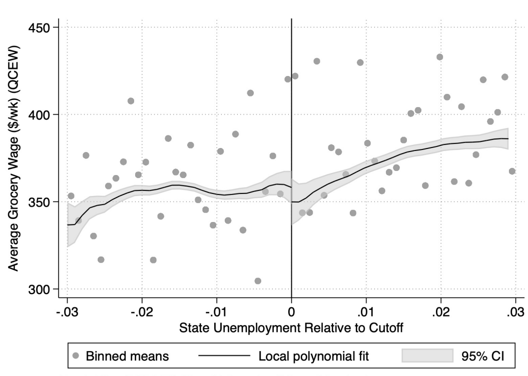
(b) Grocery Stores per 10k, County



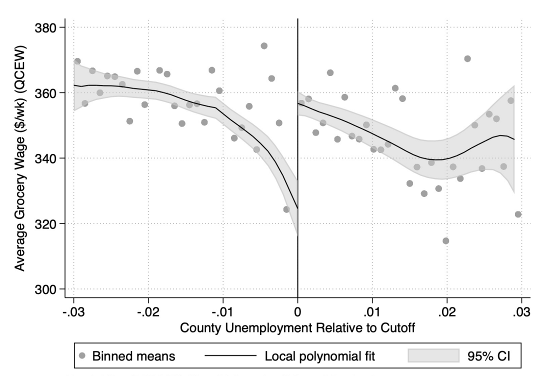
(c) Grocery Employment per 10k, State



(d) Grocery Employment per 10k, County



(e) Average Weekly Wage in Grocery Stores, State



(f) Average Weekly Wage in Grocery Stores, County

Source: QCEW.

Figure B.5: RD Plots for Grocery Store Employment

Table B.1: Comparison of Select RD Results, State Cutoff

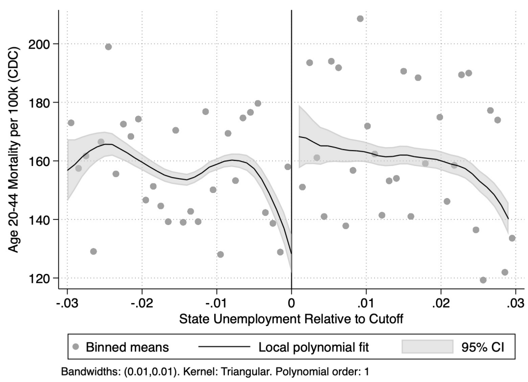
	Base	Placebo	DiRD
County has ABAWD Waiver	0.457***	0.195***	0.262***
	(0.036)	(0.027)	(0.045)
Share Population Receiving SNAP (FNS)	0.021***	-0.017***	0.038***
	(0.005)	(0.006)	(0.008)
Share ABAWDs Working 20+ Hours (ACS)	-0.084***	-0.020*	-0.064**
	(0.028)	(0.011)	(0.029)
Share ABAWDs Working Any Hours (ACS)	-0.049***	-0.010	-0.039*
	(0.019)	(0.009)	(0.021)
ABAWD Usual Hours/Wk If Work (ACS)	-2.064**	-0.697*	-1.366
	(0.810)	(0.362)	(0.875)
Grocery Stores per 10k Pop. (QCEW)	-0.377**	-0.347***	-0.030
	(0.147)	(0.116)	(0.186)
Grocery Employment per 10k (QCEW)	-14.359***	-3.015	-11.345**
	(4.417)	(2.369)	(4.983)
Average Grocery Wage ($/wk) (QCEW)	-8.276	19.932***	-28.208***
	(7.305)	(6.173)	(9.677)
Age 20-44 Mortality per 100k (CDC)	44.551***	-9.127	53.677***
	(8.418)	(7.440)	(11.116)
Hospital Discharges per 100k (AHRQ)	1071.848***	295.595	776.253
	(202.806)	(1698.894)	(1682.234)
Diabetes Discharges per 100k (AHRQ)	60.640***	-144.707**	205.347***
	(8.323)	(61.006)	(60.369)
Subst./Mental Disch. per 100k (AHRQ)	-368.970***	-701.907*	332.937
	(52.245)	(425.447)	(421.731)
Homeless Population per 10k (HUD)	-5.156***	2.674	-7.830*
	(1.916)	(3.704)	(4.218)
Property Crimes per 100k (UCR)	-342.560***	482.275***	-824.835***
	(95.331)	(100.796)	(136.605)
Violent Crimes per 100k (UCR)	14.500	163.990***	-149.490***
	(15.147)	(17.886)	(23.314)
Monthly Food Sales per Capita (Nielsen)	-25.085***	-5.564**	-19.521***
	(4.169)	(2.418)	(4.806)

* p<0.10, ** p<0.05, *** p<0.01. Standard errors (clustered by county) in parentheses. Results are RD estimates from local linear regressions, triangle kernels, bandwidth of .01.

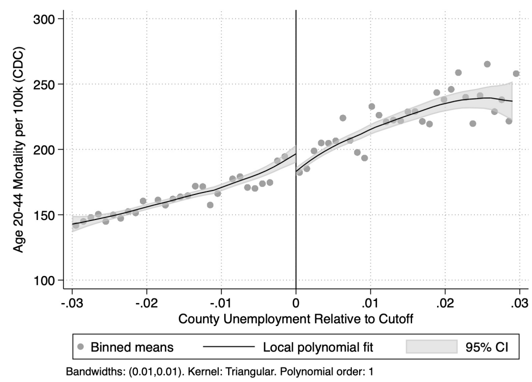
Table B.2: Comparison of Select RD Results, County Cutoff

	Base	Placebo	DiRD
County has ABAWD Waiver	0.070***	-0.063***	0.132***
	(0.021)	(0.010)	(0.023)
Share Population Receiving SNAP (FNS)	0.007***	0.015**	-0.008
	(0.003)	(0.007)	(0.008)
Share ABAWDs Working 20+ Hours (ACS)	-0.002	0.022*	-0.024
	(0.009)	(0.012)	(0.015)
Share ABAWDs Working Any Hours (ACS)	0.001	0.018	-0.017
	(0.006)	(0.011)	(0.013)
ABAWD Usual Hours/Wk If Work (ACS)	-0.013	0.546	-0.558
	(0.295)	(0.458)	(0.515)
Grocery Stores per 10k Pop. (QCEW)	-0.184**	-0.046	-0.149
	(0.084)	(0.138)	(0.162)
Grocery Employment per 10k (QCEW)	-3.143*	-4.637	1.486
	(1.878)	(3.699)	(4.136)
Average Grocery Wage ($/wk) (QCEW)	32.144***	7.666	24.435***
	(4.455)	(7.152)	(8.032)
Age 20-44 Mortality per 100k (CDC)	-13.478***	9.329	-22.807***
	(3.860)	(7.674)	(8.234)
Hospital Discharges per 100k (AHRQ)	2229.364*	-422.675*	2652.039**
	(1164.389)	(241.519)	(1154.310)
Diabetes Discharges per 100k (AHRQ)	70.812**	1.693	69.119**
	(33.258)	(8.879)	(34.269)
Subst./Mental Disch. per 100k (AHRQ)	31.899	25.875	6.024
	(435.970)	(73.308)	(428.273)
Homeless Population per 10k (HUD)	-10.203***	-1.198	-9.068***
	(2.455)	(2.011)	(3.240)
Property Crimes per 100k (UCR)	-152.789***	74.386	-229.267**
	(56.109)	(94.805)	(107.829)
Violent Crimes per 100k (UCR)	-15.991*	31.180*	-47.529**
	(9.169)	(16.438)	(18.766)
Monthly Food Sales per Capita (Nielsen)	1.149	7.598**	-6.449*
	(2.257)	(3.328)	(3.829)

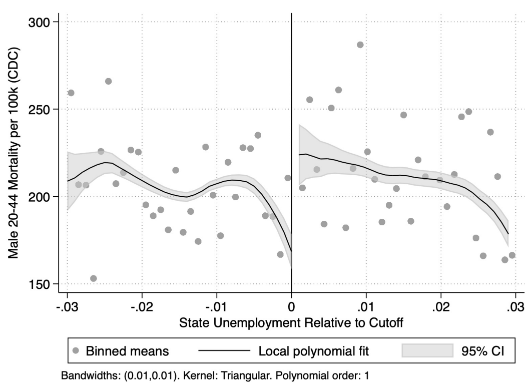
* p<0.10, ** p<0.05, *** p<0.01. Standard errors (clustered by county) in parentheses. Results are RD estimates from local linear regressions, triangle kernels, bandwidth of .01.



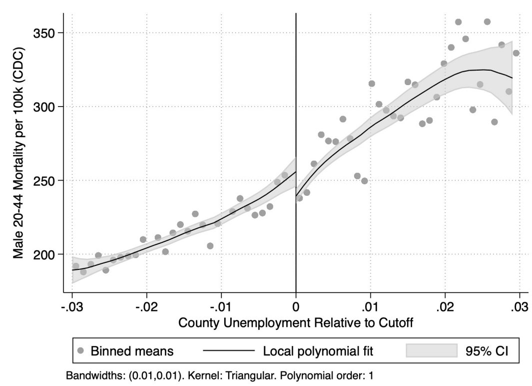
(a) All, State



(b) All, County

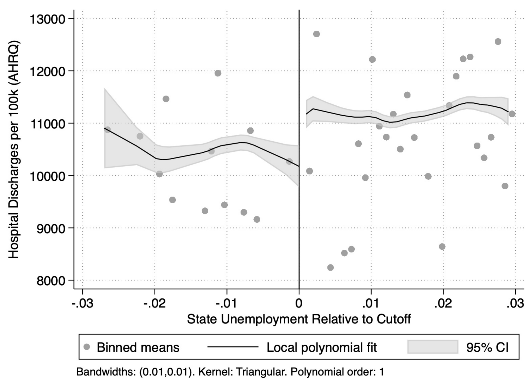


(c) Men, State

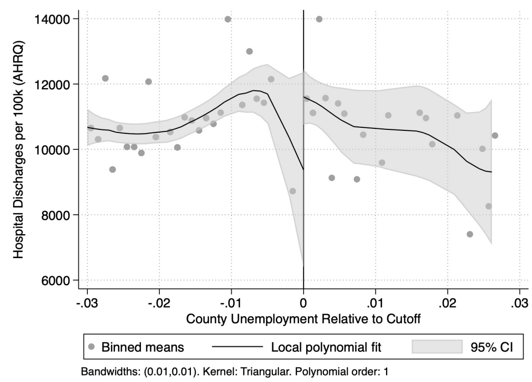


(d) Men, County

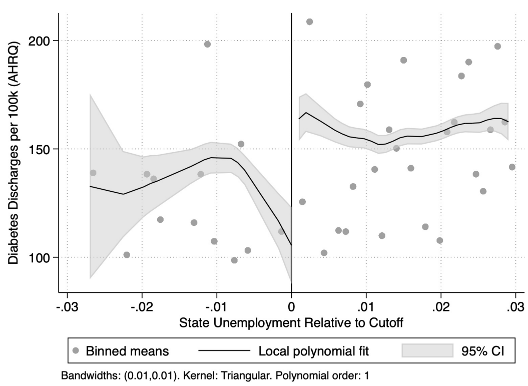
Figure B.6: RD Plots for Crude Mortality, Ages 20-44



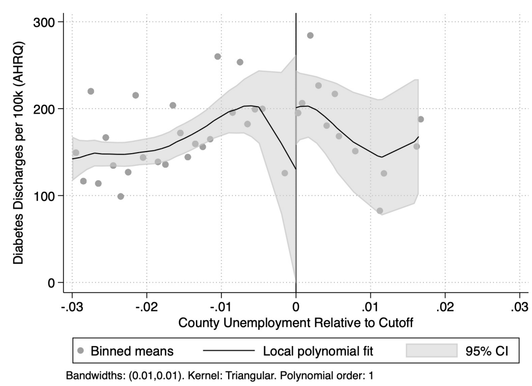
(a) All Diagnoses, State



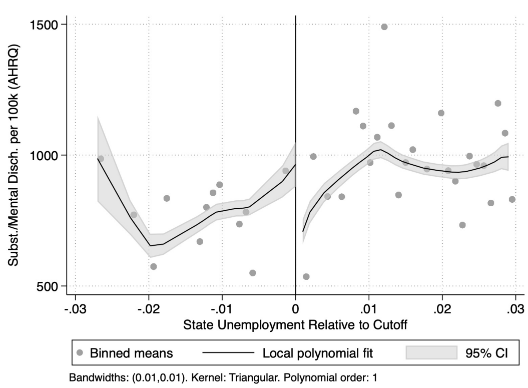
(b) All Diagnoses, County



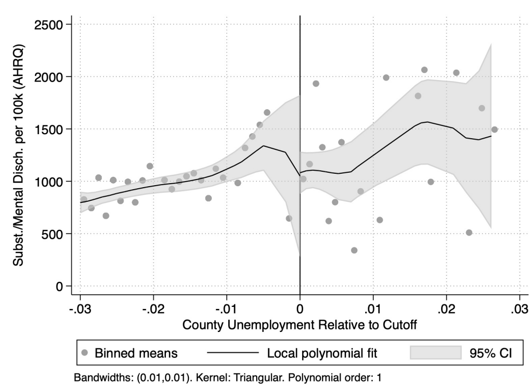
(c) Diabetes Complications, State



(d) Diabetes Complications, County



(e) Substance Use and Mental Health, State



(f) Substance Use and Mental Health, County

Source: AHRQ. Data is limited to 2011-2015 period.

Figure B.7: RD Plots for Hospital Discharges

Table B.3: Comparison of ACS RD Results, State Cutoff

	State	(Placebo)	(DiRD)
Share ABAWDs Receiving SNAP	0.018**	0.047***	-0.029**
	(0.009)	(0.008)	(0.011)
	[421,135]	[404,160]	[825,295]
Share ABAWDs Working 20+ Hours	-0.070**	-0.030***	-0.040
	(0.034)	(0.007)	(0.034)
	[421,135]	[404,160]	[825,295]
ABAWD Usual Work Hours/Wk	-2.880**	-1.532***	-1.347
	(1.441)	(0.485)	(1.498)
	[421,135]	[404,160]	[825,295]
Share ABAWDs Working Any Hours	-0.039**	-0.012*	-0.027
	(0.020)	(0.007)	(0.020)
	[421,135]	[404,160]	[825,295]
ABAWD Usual Hours/Wk If Pos.	-1.557*	-1.203***	-0.353
	(0.893)	(0.393)	(0.959)
	[376,539]	[353,217]	[729,756]
Share ABAWDs Worked Last Week	-0.020	0.031**	-0.051
	(0.030)	(0.013)	(0.033)
	[388,213]	[371,682]	[759,895]

* p<0.10, ** p<0.05, *** p<0.01. Estimated on ACS data, 2005-2018. Standard errors (clustered by county) in parentheses, N of each model in brackets. Results estimated with local linear regressions, triangle kernels, bandwidth of .01 for state.

Table B.4: Comparison of ACS RD Results, County Cutoff

	State	(Placebo)	(DiRD)
Share ABAWDs Receiving SNAP	0.001	-0.019*	0.020
	(0.009)	(0.011)	(0.016)
	[926,621]	[657,696]	[1584317]
Share ABAWDs Working 20+ Hours	-0.000	0.019**	-0.019
	(0.008)	(0.010)	(0.014)
	[926,621]	[657,696]	[1584317]
ABAWD Usual Work Hours/Wk	-0.217	0.716*	-0.933
	(0.407)	(0.424)	(0.658)
	[926,621]	[657,696]	[1584317]
Share ABAWDs Working Any Hours	0.004	0.013	-0.008
	(0.007)	(0.011)	(0.015)
	[926,621]	[657,696]	[1584317]
ABAWD Usual Hours/Wk If Pos.	-0.445	0.268	-0.713**
	(0.279)	(0.326)	(0.355)
	[809,727]	[555,254]	[1364981]
Share ABAWDs Worked Last Week	0.002	0.012	-0.010
	(0.007)	(0.008)	(0.010)
	[847,398]	[592,508]	[1439906]

* p<0.10, ** p<0.05, *** p<0.01. Estimated on ACS data, 2005-2018. Standard errors (clustered by county) in parentheses, N of each model in brackets. Results estimated with local linear regressions, triangle kernels, bandwidth of .01 for county.

Table B.5: Summary of Select RD Results with County Fixed Effects

	State	County
County has ABAWD Waiver	0.650***	0.040*
	(0.038)	(0.024)
Share Population Receiving SNAP (FNS)	0.011***	0.015***
	(0.002)	(0.001)
Share ABAWDs Working 20+ Hours (ACS)	-0.049**	-0.008
	(0.023)	(0.006)
Share ABAWDs Working Any Hours (ACS)	-0.037*	-0.005
	(0.020)	(0.005)
ABAWD Usual Hours/Wk If Work (ACS)	-0.834	-0.221
	(0.562)	(0.198)
Grocery Stores per 10k Pop. (QCEW)	-0.165**	-0.163***
	(0.069)	(0.048)
Grocery Employment per 10k (QCEW)	0.116	-2.946***
	(1.688)	(1.020)
Average Grocery Wage ($/wk) (QCEW)	-1.012	21.162***
	(4.735)	(2.909)
Monthly Food Sales per Capita (Nielsen)	-3.811***	0.707
	(1.458)	(1.357)
Age 20-44 Mortality per 100k (CDC)	-4.750	1.083
	(5.362)	(3.317)
Hospital Discharges per 100k (AHRQ)	-105.631	3602.442*
	(135.891)	(2017.441)
Diabetes Discharges per 100k (AHRQ)	5.901	111.974
	(10.078)	(123.203)
Subst./Mental Disch. per 100k (AHRQ)	34.155	248.614
	(42.507)	(433.246)
Homeless Population per 10k (HUD)	0.436**	-5.184***
	(0.203)	(1.536)
Property Crimes per 100k (UCR)	1.519	-104.930***
	(52.522)	(38.985)
Violent Crimes per 100k (UCR)	8.560	13.387**
	(9.770)	(6.650)

* p<0.10, ** p<0.05, *** p<0.01. Standard errors in parentheses, N of each model in brackets. Results are RD estimates from local linear regressions, triangle kernels, bandwidths of .01 for state and .01 for county.

Table B.6: Summary of Select RD Results with Lagged Dependent Variables

	State	County
County has ABAWD Waiver	0.444***	-0.029
	(0.038)	(0.022)
Share Population Receiving SNAP (FNS)	0.004***	-0.002**
	(0.001)	(0.001)
Share ABAWDs Working 20+ Hours (ACS)	-0.042***	0.009*
	(0.014)	(0.005)
Share ABAWDs Working Any Hours (ACS)	-0.031***	0.008**
	(0.012)	(0.004)
ABAWD Usual Hours/Wk If Work (ACS)	-0.724*	0.276*
	(0.414)	(0.155)
Grocery Stores per 10k Pop. (QCEW)	-0.108**	0.020
	(0.053)	(0.031)
Grocery Employment per 10k (QCEW)	2.502	-0.338
	(2.304)	(0.893)
Average Grocery Wage ($/wk) (QCEW)	-5.343	0.576
	(4.112)	(1.750)
Age 20-44 Mortality per 100k (CDC)	3.731	-1.027
	(5.462)	(3.591)
Hospital Discharges per 100k (AHRQ)	-495.702***	1327.877***
	(115.097)	(490.822)
Diabetes Discharges per 100k (AHRQ)	17.809***	68.077***
	(6.509)	(19.572)
Subst./Mental Disch. per 100k (AHRQ)	10.897	-7.186
	(41.804)	(60.602)
Homeless Population per 10k (HUD)	-1.609***	-2.179**
	(0.322)	(0.917)
Property Crimes per 100k (UCR)	-25.354	17.021
	(34.826)	(25.862)
Violent Crimes per 100k (UCR)	11.242*	8.407
	(6.296)	(5.634)
Monthly Food Sales per Capita (Nielsen)	-1.581*	-4.515***
	(0.852)	(0.951)

* p<0.10, ** p<0.05, *** p<0.01. Standard errors in parentheses, N of each model in brackets. Results are RD estimates from local linear regressions, triangle kernels, bandwidths of .01 for state and .01 for county.

Table B.7: Comparison of RD Results by Bandwidth, State Results

	0.5%	1%	2%
County has ABAWD Waiver	0.599***	0.457***	0.509***
	(0.051)	(0.036)	(0.025)
Resident population (1000s)	-108.459***	-93.882***	-91.868***
	(28.825)	(19.978)	(14.225)
Share Population Receiving SNAP (FNS)	0.010	0.021***	0.012***
	(0.008)	(0.005)	(0.004)
Grocery Stores per 10k Pop. (QCEW)	-0.783***	-0.377**	0.255**
	(0.234)	(0.147)	(0.119)
Grocery Employment per 10k (QCEW)	-0.530	-14.359***	-0.465
	(6.876)	(4.417)	(3.123)
Average Grocery Wage ($/wk) (QCEW)	-17.378	-8.276	-4.397
	(12.442)	(7.305)	(5.741)
Monthly Food Sales per Capita (Nielsen)	-15.449***	-25.085***	-11.994***
	(5.775)	(4.169)	(2.613)
Age 20-44 Mortality per 100k (CDC)	60.796***	44.551***	16.208***
	(13.586)	(8.418)	(5.464)
Male 20-44 Mortality per 100k (CDC)	90.551***	59.020***	28.357***
	(22.853)	(13.650)	(8.780)
Hospital Discharges per 100k (AHRQ)	-4249.996***	1071.848***	850.215***
	(678.576)	(202.806)	(171.381)
Diabetes Discharges per 100k (AHRQ)	-113.687***	60.640***	46.602***
	(23.974)	(8.323)	(7.273)
Subst./Mental Disch. per 100k (AHRQ)	-1192.155***	-368.970***	-158.714***
	(111.235)	(52.245)	(44.266)
Homeless Population per 10k (HUD)	-7.932***	-5.156***	-2.506**
	(2.729)	(1.916)	(1.232)
Property Crimes per 100k (UCR)	-37.739	-342.560***	-501.525***
	(153.453)	(95.331)	(77.773)
Violent Crimes per 100k (UCR)	51.209**	14.500	-22.187*
	(24.487)	(15.147)	(11.945)

* p<0.10, ** p<0.05, *** p<0.01. Standard errors (clustered by county) in parentheses. Results are RD estimates from local linear regressions, triangle kernels, varying bandwidths.

Table B.8: Comparison of RD Results by Bandwidth, County Results

	0.5%	1%	2%
County has ABAWD Waiver	0.053*	0.070***	0.093***
	(0.031)	(0.021)	(0.018)
Resident population (1000s)	88.927**	6.533	-25.422*
	(41.710)	(11.706)	(14.599)
Share Population Receiving SNAP (FNS)	0.011***	0.007***	0.002
	(0.004)	(0.003)	(0.002)
Grocery Stores per 10k Pop. (QCEW)	0.058	-0.184**	-0.219***
	(0.124)	(0.084)	(0.073)
Grocery Employment per 10k (QCEW)	0.772	-3.143*	-4.494***
	(2.657)	(1.878)	(1.586)
Average Grocery Wage ($/wk) (QCEW)	50.676***	32.144***	24.780***
	(6.212)	(4.455)	(3.946)
Monthly Food Sales per Capita (Nielsen)	-2.054	1.149	6.078***
	(3.141)	(2.257)	(1.733)
Age 20-44 Mortality per 100k (CDC)	-19.806***	-13.478***	-8.999***
	(5.628)	(3.860)	(3.239)
Male 20-44 Mortality per 100k (CDC)	-25.045***	-16.475***	-11.156**
	(8.151)	(5.817)	(4.871)
Hospital Discharges per 100k (AHRQ)	3946.668***	2229.364*	-310.216
	(482.520)	(1164.389)	(1015.136)
Diabetes Discharges per 100k (AHRQ)	94.620***	70.812**	-6.948
	(24.527)	(33.258)	(36.018)
Subst./Mental Disch. per 100k (AHRQ)	724.239***	31.899	-300.100
	(90.325)	(435.970)	(306.343)
Homeless Population per 10k (HUD)	-11.764***	-10.203***	-6.984***
	(3.673)	(2.455)	(1.752)
Property Crimes per 100k (UCR)	-230.470***	-152.789***	-159.936***
	(81.194)	(56.109)	(47.790)
Violent Crimes per 100k (UCR)	-8.040	-15.991*	-25.368***
	(13.826)	(9.169)	(8.503)

* p<0.10, ** p<0.05, *** p<0.01. Standard errors (clustered by county) in parentheses. Results are RD estimates from local linear regressions, triangle kernels, varying bandwidths.

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