The Effects of Child Care Subsidies on Paid Child Care Participation and Labor Market Outcomes: Evidence from the Child and Dependent Care Credit

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Abstract

Child care is expensive in the United States. The Child and Dependent Care Credit (CDCC), a tax credit based on taxpayers' income and child care expenses, reduces families' child care costs. The nonrefundable federal CDCC is available to working families with children younger than 13 years in all states, and nearly half of states supplement the federal credit with their own refundable or nonrefundable state child care credits. The Bush Tax Cuts expanded the federal CDCC in 2003, and this led to differential increases in CDCC generosity across states and family sizes. I use policy-induced increases in CDCC generosity to create measures akin to simulated instruments for CDCC benefits. Using data from the March Current Population Survey, I find that a 10 percent increase in CDCC benefits increases annual paid child care participation by three to five percent among households with children younger than 13 years. I also find that a 10 percent increase in CDCC benefits leads to a one percent increase in employment, a one percent increase in hours worked per week, and a six percent increase in annual earnings among married women with children younger than 13 years. Evidence suggests that making the federal CDCC refundable could lead to substantial increases in labor supply as less than three percent of single mothers with children younger than 13 years, who tend to have lower household incomes, qualify for CDCC benefits when accounting for nonrefundability.

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1 Introduction

Child care is expensive within the US. According to a 2018 Care.com survey of around 1,300 US parents, 33 percent of families spent at least 20 percent of their income on child care (Care.com 2018).[1] High child care costs may induce parents to leave the labor market or to place their children in inexpensive and, perhaps, low-quality child care arrangements (Kuziemko et al. 2018). In existing work, authors show that substitution into high-quality child care arrangements accelerates children's human capital development (Cornelissen et al. 2018; Cunha and Heckman 2007; Havnes and Mogstad 2011) and that parents who remain in the labor market may experience long-run earnings gains (Angelov, Johansson, and Lindahl 2016; Kleven et al. 2019; Kleven, Landais, and Søgaard 2019). In light of this, many policymakers have advocated for measures to decrease child care costs within the US. In fact, President Donald Trump's proposed 2020 budget includes a one-time one billion dollar investment in child care for parents at work or in school (Office of Management and Budget 2019).

Currently, the Child and Dependent Care Credit (CDCC) reduces child care costs for working families. The CDCC, a tax credit based on taxpayers' income and child care expenses, is "designed to help families pay employment-related expenses for care of a child" (Gitterman and Howard 2003). The nonrefundable federal CDCC is available to working families with children younger than 13 years in all states, and nearly half of states supplement the federal credit with their own refundable or nonrefundable state child care credits. In this paper, I use variation in CDCC generosity over time and across states and family sizes to estimate the effects of child care subsidies on family outcomes. Figure 1 shows that the Bush Tax Cuts, which expanded the federal CDCC in 2003, led to large increases in federal CDCC expenditures. Annual federal CDCC expenditures increased from $2.7 billion in 2002 to $3.2 billion in 2003 and continued to increase to over $3.6 billion by 2016.[2] The 2003 federal CDCC expansion increased CDCC benefits differentially across states and family sizes because states calculate their CDCC benefits as a percent of federal CDCC benefits.

[1] Survey respondents were recruited via the Care.com website.

[2] Figure 1 displays federal CDCC expenditures in nominal terms. Because the CDCC is not tied to inflation, federal CDCC expenditures decreased in real value between 2003 and 2016.

For example, taxpayers with two or more children in New York could receive up to $2,663 in federal and state CDCC benefits in 2002 and up to $3,738 in 2003. Similar taxpayers in Kentucky could receive up to $1,367 in CDCC benefits in 2002 and $2,058 in benefits in 2003. Additionally, taxpayers with two or more children were eligible for larger CDCC benefit amounts than taxpayers with one child.

Because the CDCC functions as a child care subsidy, increased CDCC generosity should distort taxpayers' spending toward child care. In addition, increased CDCC benefits are effectively increases in wages net of child care costs. This, combined with the fact that each parent in the household must work to receive benefits,[3] implies that increased CDCC generosity incentivizes increases in extensive margin labor supply. The nonrefundability of the federal and some state credits, however, reduces CDCC generosity and therefore, extensive margin work incentives among low-income taxpayers. On the intensive margin, labor supply incentives vary across the income distribution. Taxpayers in high-income households face negative intensive margin labor supply incentives as CDCC benefits increase their net income; nonetheless, CDCC benefits constitute only a small proportion of income among high-income households. Taxpayers in low-income households experience negative substitution effects as CDCC benefits increase with income due to the federal credit's nonrefundability. These negative substitution effects may outweigh positive income effects and lead to increases in labor supply along the intensive margin among low-income households.

In this paper, I document CDCC eligibility, participation, and expenditures over time and across income and demographic groups and test whether theoretical effects of increased CDCC generosity hold empirically using data from the March CPS. When accounting for the nonrefundability of the federal and some state CDCCs, I find that while over 20 percent of fathers and married mothers qualify for CDCC benefits, only 2.6 percent of single mothers, who tend to have lower household incomes, qualify for benefits. Conditional on eligibility, about 30 percent of taxpayers claim the federal CDCC at all income levels before the expansion, with the exception of very-low-income taxpayers, who are relatively unlikely to participate. Participation rates, conditional on eligibility, increase to around 0.35 across

[3]Taxpayers with positive annual earnings who currently are looking for work and full-time students are eligible for CDCC benefits. Taxpayers who are looking for work with $0 in annual earnings, however, are ineligible for the credit, making the CDCC a poor income stabilizer during recessions.

all but very-low-income taxpayers after the expansion. I also find that, among eligible CDCC recipients, the majority of federal expenditures are allocated toward low- and middle-income taxpayers, who qualify for the largest CDCC benefit amounts, both before and after the federal expansion. I then turn to estimating the effects of CDCC benefits on paid child care participation and labor market outcomes. Because actual CDCC benefits likely are correlated with unobservable characteristics of taxpayers that affect both CDCC benefits and outcomes as well as behavioral responses to changes in the tax code, I use increases in CDCC generosity from the Bush Tax Cuts to create an instrument for actual CDCC benefits. Specifically, I start with a collection of taxpayers from before the federal CDCC expansion. I then replicate this sample of taxpayers for each tax year in the analysis. After converting income and expenditure values into current year dollars, I use the National Bureau of Economic Research's TAXSIM program (Feenberg 2017) to calculate the CDCC benefits that each of the replicated taxpayers would have received if they had existed in the current tax year. I then use TAXSIM's estimates of taxpayers' CDCC benefits to calculate average CDCC benefits across states, years, and demographic groups. These "simulated" CDCC measures summarize changes in tax policy but not individual-level omitted variables that are correlated with both CDCC benefits and outcomes or behavioral responses to changes in the tax code. As a result, identification comes from the federal CDCC expansion and the differential increases in CDCC generosity that it generated across states and family sizes.

I find that a 10 percent increase in CDCC benefits increases annual paid child care participation by three to five percent among households with children younger than 13 years. I also find that a 10 percent increase in CDCC benefits leads to a one percent increase in employment, a one percent increase in hours worked per week, and a six percent increase in annual earnings among married women with children younger than 13 years. Evidence suggests that making the federal CDCC refundable could lead to substantial increases in labor supply as less than three percent of single mothers with children younger than 13 years qualify for benefits, especially since low-income households face the highest child care costs as a proportion of income on average (Herbst 2018).

My work contributes to the literature on the CDCC and US policies to increase parents'

access to child care and labor supply more broadly.[4] Miller and Mumford (2015) estimate the effect of the federal CDCC expansion on child care expenditures. The authors find large elasticities of child care expenditures but fail to account for cross-state changes in CDCC generosity or for changes in households' tax-filing behavior over time. Rodgers (2018) estimates the tax incidence of the CDCC and finds that over half of every CDCC dollar is passed through in the form of higher child care prices and child care provider wages. Rodgers' estimates may be inconsistent, however, as he controls for individuals' income bins, which may be a function of the child care prices that they face. Finally, Guner, Kaygusuz, and Ventura (Conditionally Accepted) estimate the effects of transfer programs for families, including the CDCC, using life-cycle models of fertility, labor supply, and child care. The authors find that expanding the CDCC would increase labor force participation among married women.

Studies of universal kindergarten and pre-kindergarten provide some of the most credible evidence on mothers' labor supply responses to effective decreases in the cost of child care within the US.[5] In existing work, researchers find both null and large positive effects of universal schooling on mothers' labor supply (Cascio 2009; Fitzpatrick 2010, 2012; Gelbach 2002). Effects of universal schooling may differ from those of child care subsidies, however, as these two policies differ in several ways. First, universal schooling is fully subsidized by the government and, in some cases, mandatory. Second, taxpayers' income and expenditures do not affect schooling availability whereas their CDCCs are functions of income and child care expenditures. Third, taxpayers face a much larger set of choices over child care quality than over school quality. Finally, taxpayers must work to receive CDCC benefits.

My results also have implications for children's human capital formation and short- and long-run child outcomes. Cunha and Heckman (2007) develop and estimate a model of human capital formation to show that early life investments in children have large effects on their long-run outcomes. Empirical evidence supports this notion and shows that increases

[4]See Baker, Gruber, and Milligan (2008), Bettendorf, Jongen, and Muller (2015), Cornelissen et al. (2018), Givord and Marbot (2015), Havnes and Mogstad (2011), Lefebvre and Merrigan (2008), Lefebvre, Merrigan, and Verstraete (2009), and Lundin, Mörk, and Öckert (2008) for evidence on the effects of child care subsidies within Europe and Canada.

[5]Tekin (2005, 2007) estimates the effects of child care subsidies for very-low-income parents through the Aid to Families with Dependent Children (AFDC) and Temporary Assistance for Needy Families (TANF) programs.

in family income during childhood lead to increases in educational attainment and cognitive achievement (Akee et al. 2010; Dahl and Lochner 2012; Milligan and Stabile 2011), decreases in criminal behavior (Akee et al. 2010), and improvements in health (Milligan and Stabile 2011). Hence, increases in earnings due to increased CDCC generosity may lead to better outcomes for children. Nonetheless, authors find mixed results on the effects of child care incentive programs (Baker, Gruber, and Milligan 2008; Cornelissen et al. 2018; Havnes and Mogstad 2011). Hence, it is unclear whether increased CDCC generosity improves or worsens children's outcomes. All else equal, if increases in CDCC benefits lead to increases in child care quality, then outcomes may improve. If not, positive effects of increased family income may or may not counteract negative effects of lower-quality child care arrangements.

In the following section, I provide institutional details about the CDCC. In Section 3, I present a conceptual framework of CDCC incentives. I describe the data in Section 4. I study CDCC benefits and paid child care participation in Section 5 and labor supply in Section 6. I discuss results in Section 7 and conclude in Section 8.

2 Institutional Details and CDCC Participation and Expenditures

Congress implemented the CDCC in 1976. At that time, as shown in Table 1, taxpayers could receive up to $400 in CDCC benefits per child younger than age 13 for up to two children. More specifically, taxpayers could claim up to $2,000 of child care expenditures per child and would receive a nonrefundable credit worth 20 percent of those expenses. CDCC claimants had to be working to qualify for benefits, and while benefits did not depend on filing status,[6] both spouses had to work among taxpayers married filing jointly. Eligible child care expenditures included spending on child care provided by anyone but the taxpayer's spouse or dependent or the child's parent.[7] Taxpayers had to list their earnings, child care expenditures, and child care providers' tax identification or Social Security numbers on

[6]Taxpayers married filing separately are ineligible for CDCC benefits.

[7]In particular, taxpayers may receive CDCC benefits for child care provided by the child's grandparents or other relatives.

federal Form 2441 to claim the credit.[8]

In 1981, Congress expanded the CDCC and increased its statutory progressivity, the progressivity of the benefit rate without accounting for the CDCC's nonrefundability. As noted in Table 1, the limit on eligible child care expenses increased to $2,400 in 1982. Taxpayers with AGI under $10,000 could receive a tax credit worth up to 30 percent of these expenses, and the benefit rate gradually decreased to 20 percent for taxpayers with AGI above $28,000.

Because the CDCC was not indexed to inflation and there were no legislative changes to the credit between 1981 and 2001, it decreased substantially in real value over this time. Then, in 2001, as part of the Bush Tax Cuts, a broad set of initiatives to lower taxes on the middle class, Congress increased the CDCC expense limit and rate schedule. Table 1 shows that these changes took effect in 2003, at which time taxpayers could claim up to $3,000 in child care expenses per child for up to two children and receive a nonrefundable tax credit worth up to 35 percent of those expenses. The benefit rate decreased by one percentage point for each additional $2,000 in AGI above $15,000 until it remained at 20 percent for those with $43,000 or more in AGI.

Both before and after the federal CDCC expansion, the CDCC interacted with other elements of the tax code in a complicated way. For instance, federal CDCC benefits directly offset the nonrefundable portion of the Child Tax Credit (CTC), a tax credit for families with children. Before 2001, the CTC was a nonrefundable tax credit worth up to $500 per child.[9] The Bush Tax Cuts increased the CTC to $600 per child in 2001 and $1,000 per child in 2003.[10] The Additional Child Tax Credit (ACTC), a refundable portion of the CTC, also took effect in 2001. Because of this, taxpayers without taxable income after deductions could receive ACTC benefits worth up to 10 percent of their AGI above $10,000 beginning in 2001 and up to 15 percent of their AGI over $10,000 beginning in 2004. Hence, while low-income families can benefit from both the CDCC and ACTC, high-income families' federal CDCC

[8] If either spouse's earnings are less than child care expenditures, then the CDCC is calculated as a percent of the lesser of the two taxpayers' earnings. For example, as of 1976, a tax unit that was married filing jointly with one spouse earning $50,000 and the other earning $1,000 that spent $2,000 on child care would receive CDCC benefits worth 20 percent of $1,000, or $200.

[9] The CTC was only worth up to $400 in 1998.

[10] As a result of the Bush Tax Cuts, federal tax brackets and marginal tax rates also changed concurrently with federal CDCC benefits between 2002 and 2003.

benefits directly offset their CTC benefits.

Additionally, 40 percent of workers have access to dependent care flexible spending accounts (FSA) that their employers offer (United States Department of Labor, Bureau of Labor Statistics 2018). These accounts interact with federal CDCC benefits. When an employer offers an FSA, employees may set aside up to $5,000 of income before taxes for dependent care expenses. The employer deducts this income from employees' paychecks, but employees are reimbursed for qualified child care expenses, which, similarly to the CDCC, include expenditures on care inside and outside of the home. Unlike the CDCC, however, the decision to set aside funds for an FSA occurs before the employee's child care expenditures are realized.

While taxpayers may receive benefits from both FSAs and the federal CDCC, they may not double count expenses across the two child care subsidy programs. FSAs generally provide larger tax benefits than the federal CDCC, given the CDCC's nonrefundability and high marginal tax rates among high-income taxpayers. Nevertheless, lower-income families are relatively unlikely to have access to FSAs (United States Department of Labor and United States Bureau of Labor Statistics 2018); high-income families may spend over $5,000 per year in child care; and taxpayers may be unable to minimize expected taxes well, causing them to underinvest in FSAs. For all of these reasons, taxpayers may benefit from CDCCs, despite the existence of dependent care FSAs.

Furthermore, the CDCC may interact indirectly with the Earned Income Tax Credit (EITC), an earnings subsidy targeted at low- and moderate-income families with children. While EITC benefits are not a function of the CDCC, both the CDCC and EITC provide work incentives for low- to middle-income taxpayers. In particular, EITC benefits increase with household earnings until they reach a maximum benefit level. Benefits then remain constant until household earnings reach another level at which benefits begin to phase out toward zero. And as with the CDCC, several states have their own EITC programs. Unlike the federal CDCC, however, the federal EITC is fully refundable and therefore, provides additional benefits to low-income taxpayers. In existing work, researchers show that EITC benefits increase extensive margin labor supply among single mothers (Eissa and Liebman 1996; Hoynes and Patel 2018; Keane and Moffitt 1998; Meyer and Rosenbaum 2001) and

decrease extensive margin labor supply among married mothers (Eissa and Hoynes 1998). While intensive margin effects tend to be smaller, Chetty, Friedman, and Saez (2013) use data on all US taxpayers to find intensive-margin earnings elasticities of 0.31 in the phase-in region and 0.14 in the phase-out region of the credit. These EITC labor supply elasticities may interact with those of the CDCC in a complicated way.[11]

Interactions with other elements of the tax code and differences in benefit rates suggest that there may be heterogeneity in CDCC participation and benefits across the income distribution. In light of this, I use data from the IRS Statistics of Income (SOI) (Internal Revenue Service 2019) and the March CPS (Roth 2019) to document federal CDCC participation and expenditures across the income distribution. The IRS SOI tables document the proportion of federal tax returns with CDCC claims and the amount of federal CDCC expenditures across adjusted gross income (AGI) categories, such as $0 to $15,000 in AGI, $15,000 to $25,000 in AGI, and so forth. I use the March CPS data to scale the CDCC participation rates and proportions of federal CDCC expenditures from the IRS SOI tables by the number of taxpayers eligible for positive CDCC benefits in each AGI category. Because I do not observe CDCC eligibility directly in the data, I use March CPS respondents' income and demographic information to calculate their CDCC benefits. Because I observe annual paid child care participation but not child care expenditures in the March CPS data, I impute child care expenditures using data from the Survey of Income and Program Participation (SIPP) Child Care Topical Module that was administered in 2002 (United States Census Bureau 2019). Specifically, I impute $0 in child care expenditures for households that did not pay for child care during the tax year. Among households that did pay for child care, I impute the average child care expenditure amount among SIPP respondents in the same demographic group by marital status, number of children, child age, and education level using sample weights.[12] Table 2, which displays average child care expenditures across

[11]The Child Care and Development Fund (CCDF) also provides funding to states to administer child care subsidy programs for very-low-income families. These state child care subsidy programs generally target families participating in or transitioning out of the Temporary Assistance for Needy Families (TANF) program, serving about 800,000 families as of 2017 (Office of Child Care, Administration for Children and Families, United States Department of Health and Human Services 2019). Families that receive child care subsidies through CCDF generally do not benefit from the federal CDCC as their incomes are too low to have positive AGI after other deductions.

[12]SIPP respondents report child care expenditures during the past month. I multiply monthly expendi-

demographic groups, conditional on paid child care participation, shows that child care expenditures vary considerably across demographic groups. Annual child care expenditures range from around $2,000 among mothers without college degrees, only one child younger than 13 years, and no children younger than six years to over $6,500 among married college-educated mothers with children younger than six years. Child care expenditures tend to increase with educational attainment and number of children. This evidence suggests that CDCC benefits may vary across demographic groups, even conditional on paying for child care.

After imputing expenditure amounts, I the use income and demographic data from the March CPS and the National Bureau of Economic Research's (NBER) TAXSIM program (Feenberg 2017) to calculate households' CDCC benefits, assuming tax-minimization behavior. The left panel of Figure 2 displays federal CDCC take-up rates, or the proportion of eligible tax units that claimed the credit, by federal AGI categories. Black bars represent take-up rates between 2000 and 2002, and white bars represent take-up rates between 2003 and 2005. The left panel of Figure 2 shows that while the conditional take-up rate was low among very-low-income taxpayers, about 30 percent of eligible taxpayers at all other income levels participated in the CDCC before the expansion, and about 35 percent participated after the expansion.

Given that CDCC take-up rates are fairly similar across the income distribution, the left panel of Figure 2 suggests that low-income households (but not very-low-income households) may receive the majority of CDCC expenditures, conditional on eligibility, as statutory benefits decrease as income increases. To study CDCC expenditures across the income distribution, I calculate CDCC eligibility using the aforementioned procedure and graph the proportion of total federal CDCC expenditures by federal AGI bins, conditional on eligibility, in the right panel of Figure 3. The right panel of Figure 2 confirms that low- and middle-income taxpayers received the majority of federal CDCC benefits, conditional on eligibility; taxpayers with federal AGI between $25,000 and $50,000 received over 30 percent of all federal CDCC expenditures both before and after the federal expansion. Very-low- and very-high-income taxpayers combined received less than four percent of federal CDCC

tures by 12 to obtain annual expenditures.

benefits.

Low CDCC participation rates and low levels of CDCC expenditures among very-low-income taxpayers suggest that the nonrefundability of the federal credit may limit CDCC benefits among taxpayers with little tax liability, creating a wedge between statutory and effective, or actual, CDCC benefits received. I therefore use tax filing thresholds, AGI levels at which taxpayers begin to have positive tax liability, to graph effective federal CDCC benefit schedules for taxpayers with the maximum child care expenditures before and after the federal CDCC expansion in Figure 3.[13] The solid orange line graphs the effective tax schedule for taxpayers with one child younger than 13 years as of 2002, the dashed purple line the schedule for taxpayers with two or more children as of 2002, the dotted red line the schedule for taxpayers with one child as of 2003, and the dash-dotted blue line the schedule for taxpayers with two or more children as of 2003. As expected, very-low-income taxpayers do not receive CDCC benefits. Taxpayers with one child and two or more children must have income of around $13,000 and $16,000 or more, respectively, to receive CDCC benefits both before and after the federal expansion. In 2002, CDCC benefits increase for taxpayers with one child before reaching a peak of about $600 at approximately $19,000 in income. Benefits then decrease until they reach $480 at $30,000 in income. After the federal expansion, the CDCC phase-in region extends so that benefits reach a peak of about $940 at $22,500 in income. CDCC benefits also decrease over a larger range of incomes before remaining constant at $600 beginning at $45,000 in income. Hence, among taxpayers with one child younger than age 13, the federal CDCC expansion creates longer effective CDCC phase-in and phase-out regions while (weakly) increasing CDCC generosity at all income levels.[14]

Taxpayers with two or more children younger than age 13 face more generous but otherwise similar effective CDCC schedules. In 2002, CDCC benefits for these taxpayers peak at about $1,050 at $26,300 in income and remain at $960 for those with $30,000 or more income. Similarly to the pattern for households with one child, the CDCC phase-in and phase-out ranges extend in 2003 so that benefits peak at around $1,640 at $30,400 in income

[13]Effective federal CDCC benefit schedules for taxpayers with lower child care expenditure levels are less generous but otherwise similar.

[14]For some income levels, the federal CDCC expansion decreases maximum CDCC benefits by up to $25.

and remain a constant $1,200 at $45,000 in income.

Additionally, as of 2002, taxpayers in 21 states and the District of Columbia (DC) could receive additional CDCC benefits through state supplements to the federal credit. Column "Statutory Max" of Table 2 lists statutory maximum state CDCC benefits for taxpayers with two or more children as of 2002. Statutory maximum benefits vary considerably across states, ranging from $288 in Arkansas and Kentucky to $1,920 in Oregon. In the nine states (and DC) that offer refundable CDCCs, statutory maximum state CDCC benefits equal effective maximum benefits, which are listed in Column "Effective Max" of Table 2. In states without refundable CDCCs, statutory maximum CDCC benefits exceed effective maximum benefits, as with the federal credit. In addition to refundability, some states make their CDCCs progressive by limiting benefits to taxpayers with AGI below a certain threshold.

The federal CDCC expansion increased CDCC generosity differentially across states because states calculate their CDCC benefits as a percent of the federal credit or the child care expenses used to calculate it.[15] Column "Provision" of Table 2 lists the calculation used to determine each state's statutory CDCC benefits. As a result of the federal CDCC expansion, benefits increased the most in states with CDCCs that were large proportions of the federal credit. For example, New York's maximum CDCC increased from $1,584 in 2002 to $2,100 in 2003. Meanwhile, maximum state CDCC benefits increased from $288 to $420 in Kentucky.[16] While changes in state CDCC generosity between 2002 and 2003 generally arose due to differences in preexisting credit formulas, California and Maine decreased and Vermont increased its CDCC generosity relative to that of the federal credit in 2003. Louisiana also implemented a CDCC program for the first time in 2003. I address the possibility of state policy responses to the federal CDCC expansion in Sections 5 and 6.

[15] New Mexico's CDCC was not a function of the federal credit or the expenses used to calculate it.

[16] As of 2002, Hawaii, Minnesota, and New Mexico had maximum CDCC levels, which limited the amounts by which their maximum CDCCs could increase between 2002 and 2003. Additionally, some states offered larger CDCC benefit rates to lower-income taxpayers, which led to larger increases in CDCC benefits for such households.

3 Conceptual Framework of CDCC Incentives

In this subsection, I discuss CDCC incentives for child care and labor supply and their implications. First, because the CDCC functions as a child care subsidy, CDCC benefits incentivize eligible taxpayers to increase their child care expenditures.[17] This may occur via increases in time spent in nonparental care or substitution into more expensive child care arrangements. Moreover, because decreases in the cost of child care effectively function as increases in wages net of child care costs and work is required for CDCC eligibility, CDCC benefits also generate labor supply incentives.

Suppose that in each period, households face a tradeoff between consumption, C, and leisure, L. Each parent in a household can supply up to 40 hours of labor, and I assume that the primary earner always supplies 40 hours of labor in dual-earner households.[18] I also assume that households pay for child care only while both parents are working. For single parents, the budget constraint is then illustrated by line segment $\overline{ab}$ in Panel (a) of Figure 4.[19] The slope of this line represents wages less hourly child care costs.

Now, suppose that the CDCC becomes available. Then, the budget constraint for single parents becomes $\overline{acde}$.[20] Individuals who supply $L > 0$ can (weakly) increase consumption; therefore, the shift in the budget constraint incentivizes increases in extensive margin labor supply. Intensive margin labor supply incentives, however, are less clear and differ across the income distribution. Individuals at all income levels experience positive income effects that imply decreases in labor supply. Individuals whose original labor supplied fell between points d and e also experience positive substitution effects that reinforce the income effect.

[17] Child care providers may respond to increases in child care subsidies by increasing child care prices and therefore, capture a portion of CDCC benefits. CDCC incentives for paid child care will be abated to the extent that this occurs.

[18] Data from the March CPS indicate that 91 percent of married fathers with children younger than 13 years work full-time, so this assumption seems reasonable.

[19] The model generalizes to increasing consumption at 0 hours of labor for households with initial endowments.

[20] I use data from the March CPS to document the budget constraints that single and married mothers generally face based on their income levels. While 92 percent of single mothers earned less than $45,000 per year between 2000 and 2002, 70 percent of married households earned over $45,000 during this period. Hence, while some single parents may face budget constraints more akin to those in Panels (c) and (d) of Figure 4 and some married second-earners may face budget constraints more akin to those in Panels (a) and (b) of Figure 4, I focus on the budget constraints that the majority of single and married mothers face throughout the analysis.

Individuals who originally supplied labor between points c and d, however, experience negative substitution effects that may outweigh income effects and cause them to increase their labor supply along the intensive margin.

Next, suppose that the government expands the existing CDCC program so that CDCC benefits (weakly) increase at all income levels. The single parent's budget constraint then becomes line segment $\overline{acdfg}$ of Panel (b) of Figure 4. Again, the increase in CDCC generosity generates an increased incentive for labor supply along the extensive margin while intensive margin labor supply incentives differ across the income distribution. While all individuals experience positive income effects due to the CDCC expansion, individuals who previously supplied labor between points d and f experience negative substitution effects that may offset the income effect and lead to increases in intensive margin labor supply.

In Panel (c) of Figure 4, I study budget constraints among dual-earner households. Given the primary earner's labor supplied, dual earner households are guaranteed a consumption floor at a. In the absence of CDCC benefits, the household faces the budget constraint illustrated by line segment $\overline{ab}$. When CDCC benefits become available, the household continues to receive the consumption bundle represented by point a if the second earner does not work as both spouses must work to receive CDCC benefits. Household consumption increases, however, for all choices of $L > 0$ for the second earner. The introduction of the CDCC incentivizes extensive margin labor supply; the positive income effect of CDCC benefits, however, creates a disincentive for intensive margin labor supply. Nonetheless, CDCC benefits constitute only a small proportion of income among high-income households, so decreased incentives for intensive margin labor supply likely are small.

Panel (d) of Figure 4 illustrates how dual earners' budget constraints shift when CDCC generosity increases. After the CDCC expansion, households in which the secondary earner does not work continue to locate at consumption bundle a, but consumption increases for all $L > 0$ for the second earner. As in Panel (c), increased CDCC generosity creates an increased incentive for extensive margin labor supply and a decreased incentive for labor supply along the intensive margin.

In practice, many households' budget constraints may be more complicated than those in Panels (b) and (d) of Figure 4. For example, some states provide refundable CDCCs that

provide child care subsidies to low-income households. In the online appendix, I show that in this case, single earners with very low income levels experience increased consumption due to the CDCC and that the expansion of the CDCC generates incentives for single parents similar to those illustrated in Panel (b) of Figure 4.

It also is possible that individuals may not be able to perfectly adjust their labor supply to maximize utility. For simplicity, suppose that some individuals must choose among supplying 0, 20, or 40 hours of work per week. First, consider the decision of a single parent. Before the CDCC expansion, suppose that this individual must choose among points i (0 hours), ii (20 hours), and iii (40 hours) in Panel (e) of Figure 4. Each of these points falls along line segment $\overline{acde}$ of Panel (b), but the individual's inability to perfectly adjust hours limits the choice set. After the CDCC expansion, the individual chooses among points i, ii, and iv in Panel (d), points along $\overline{acdfg}$ of Panel (b). As illustrated by the vertical difference between points iii and iv, the CDCC expansion increases consumption at 40 hours of labor, generating an incentive for the single earner to work full time.

Now, consider dual-earner households in which the second earner cannot perfectly adjust their labor supply. As in Panel (e), suppose that the second earner faces a choice among supplying 0, 20, or 40 hours of work per week. These labor supply decisions correspond to points i (0 hours), ii (20 hours), and iii (40 hours) in Panel (f) of Figure 4. After the CDCC expansion, the household must choose among consumption bundles i, iv (20 hours), and v (40 hours). In this case, the household experiences an increased labor supply incentive along the extensive margin as consumption increases at both 20 and 40 hours of work due to the federal CDCC expansion. The intensive margin labor supply effects that this household experiences, however, are theoretically unclear and depend on the secondary earner's labor supplied before the CDCC expansion as well as the household's preferences over consumption and leisure.

Child care expenditures and child care quality offer another channel through which households' budget constraints may deviate from those pictured in Figure 4. In the current setup, households with different numbers or ages of children and with different preferences over child care quality face different budget constraints. The CDCC expansion may allow some households to consume higher-quality, more expensive child care while maintaining their

current levels of consumption and leisure. When households substitute into more expensive child care arrangements, their net-of-child care wages decrease, which reduces the magnitude of the extensive margin labor supply incentive that the CDCC expansion generates.

Finally, interactions between the CDCC and other aspects of the tax code that I discuss in Section 2 affect individuals' budget constraints (Keane and Moffitt 1998; Moffitt 2014). In the online appendix, I use TAXSIM to graph taxpayers' effective total federal CDCC, CTC, and EITC benefits both before and after the federal CDCC expansion. Incorporating the CTC and EITC into the budget constraint results in figures similar to those in Figure 4.[21]

4 Data

I study paid child care participation and labor market outcomes using the March CPS 2001 through 2009 surveys (Roth 2019). The CPS is a monthly state-representative survey of over 75,000 households. The CPS data document individuals' demographics and labor market outcomes; the March Supplement also documents individuals' income from various sources and whether households paid for child care during the previous calendar year.

To isolate the population affected by child care subsidies, I limit my sample to parents ages 26 through 54 in households with children younger than 13 years in the main analyses.[22] There are over 300,000 parents in this sample. Table 4 displays summary statistics from the 2001 through 2003 surveys, in which respondents reported paid child care participation and earnings from before the federal CDCC expansion, by sex and marital status. At baseline, single parents are 36 years old, and married parents are 38 years old on average. Across all demographic groups, parents have about two children on average, and around 25 percent of them pay for child care. Education levels and labor market outcomes, however, differ considerably by sex and marital status. While 34 percent of married parents have college degrees, only 14 percent of single mothers and 11 percent of single fathers are college-educated. And

[21] Labor supply incentives of CDCC benefits also remain similar if households face fixed child care costs, conditional on each parent working. In this case, initial consumption decreases at L slightly greater than 0 for the single or secondary earner, but the budget line is steeper without hourly child care costs.

[22] I drop parents who report receipt of child care subsidies through the TANF program. This decreases the sample size by one percent. Results are robust to including these parents in the sample.

despite relatively high education levels, married mothers exhibit low levels of labor force attachment. Sixty-six percent of married mothers worked for pay during the past week,[23] and they work 21.59 hours per week and earn $20,353 (2000 dollars) per year on average. Married fathers are the most attached to the labor force, with a 91 percent employment rate and average annual earnings of $56,264 per year. Single mothers have slightly lower labor force attachment than single fathers; 74 percent of single mothers and 79 percent of single fathers are employed. Average annual earnings are $20,839 among single mothers and $29,486 among single fathers.

While CDCC policies probably affect some parents within the sample, they may not affect others. To consider the characteristics of those most affected by CDCC policies, I compare characteristics of parents in households that are eligible for the CDCC to those in households that are ineligible for the credit using data from the March CPS from 2001 through 2003 in the online appendix. I determine eligibility using the procedure documented in Section 2 and find that while over 20 percent of fathers and married mothers qualify for CDCC benefits, only 2.6 percent of single mothers qualify for benefits when accounting for nonrefundability.

Households that are eligible for the CDCC tend to be younger and have more young children than parents in households that are ineligible for the credit. Parents in CDCC-eligible households are also much more likely to be college-educated and tend to work more hours than parents in CDCC-ineligible households. Differences in labor force attachment across CDCC-eligible and CDCC-ineligible household are especially large among single mothers, single fathers, and married mothers, which suggests that these groups may be the most responsive to increases in CDCC generosity.

[23] While work during the past week may be an imperfect measure of employment, data from the Current Population Survey (CPS) suggest that nearly all full-time employees are at work during a given week.

5 Paid Child Care Participation

5.1 Empirical Strategy

Evidence documented in Sections 2, 3, and 4 suggests that effects of increased CDCC generosity may differ by sex and marital status. I therefore study paid child care participation separately across single mothers, single fathers, and married households.

I begin by estimating the following naive model of child care subsidies and paid child care participation:

$$Y_{igst} = \beta \log(CDCC_{igst}) + \alpha_g + \alpha_s + \alpha_t + \rho_{st}\Omega + X_{igst}\Gamma + \epsilon_{igst}, \tag{1}$$

where Y_{igst} is an indicator for annual paid child care participation for household i in demographic group g in state s during year t. $\log(CDCC_{igst})$ is the log of inflation-adjusted CDCC benefits for taxpayer i in demographic group g in state s during year t, which I calculate using the procedure documented in Section 2. α_g, α_s, and α_t are demographic group, state, and year fixed effects, respectively. ρ_{st} is a vector of state characteristics, including the unemployment rate (Bureau of Labor Statistics, United States Department of Labor 2019) and the log of the inflation-adjusted minimum wage (Office of Communication, Wage and Hour Division, United States Department of Labor 2018) and indicators for the refundability of the state's CDCC and the availability of universal preschool (Jordan and Grossmann 2017). X_{igst} is a vector of individual characteristics, and ϵ_{igst} is the error term.[24] I cluster standard errors at the state level.

The ordinary least-squares (OLS) estimates from Equation (1) are likely inconsistent, however. For example, one can expect a spurious correlation between paid child care participation and CDCC benefits as income likely affects both child care choices and CDCC benefits. Additionally, changes in tax policy may lead to changes in taxpayers' behavior that are correlated with CDCC benefits. For instance, increased CDCC generosity may increase both paid child care participation and CDCC benefits. Hence, in addition to OLS estimates,

[24] I control for the number of children younger than age 17 to control for potential CTC benefits. I also control for the maximum total federal and state EITC benefit by number of children.

I present estimates from instrumental variables (IV) models. The IV models abstract away from characteristics of taxpayers that may be correlated with both CDCC benefits and paid child care participation as well as behavioral responses to changes in the tax code (Currie and Gruber 1996; Hoynes and Patel 2018).

To construct an instrument for CDCC benefits, I start with a collection of taxpayers in the sample who reported income and paid child care participation for tax year 2002, before the federal CDCC expansion. I then replicate this subsample of taxpayers for each tax year from 2001 through 2009. After converting income and expenditure values into current year dollars, I use TAXSIM to calculate the log of total federal and state CDCC benefits that each of the replicated taxpayers would have received if they had existed in the current tax year using the procedure documented in Section 2.

Next, I use TAXSIM's estimates of taxpayers' log federal and state CDCC benefits to calculate averages of log CDCC benefits across states, years, and demographic groups, defined by marital status, number of children, child age, and education, using sample weights. These "simulated" CDCC measures summarize changes in tax policy but not individual-level omitted variables that are correlated with both CDCC benefits and outcomes or behavioral responses to changes in the tax code. As a result, identification comes from the federal CDCC expansion and the differential changes in CDCC generosity that it generated across states and family sizes.

Figure 5 graphs the simulated CDCC benefit measures between 2002 and 2003. In particular, in Figure 6, a unique shape and color represents each demographic group, and each node represents a given state. Figure 5 shows that there is a considerable amount of variation in average simulated CDCCs across both states and demographic groups between 2002 and 2003. Simulated benefits range from $0 to over $1,000 per year and tend to increase with the federal CDCC expansion. In particular, single mothers with two or more children and college degrees have some of the largest simulated CDCC benefits. Moreover, given large differences in simulated benefits, one may expect different effects of increased CDCC generosity across demographic groups.

The first-stage equation of the IV model is as follows:

$$\text{(2)} \qquad \log(CDCC_{igst}) = \lambda\overline{\log(SimCDCC_{gst})} + \delta_g + \delta_s + \delta_t + \rho_{st}\xi + X_{igst}\Omega + \nu_{igst},$$

where $\log(CDCC_{igst})$ is the log of actual CDCC benefits for individual i in demographic group g in state s during year t. $\overline{\log(SimCDCC_{gst})}$ is the log of simulated CDCC benefits for taxpayers in demographic group g in state s during year t. δ_g, δ_s, and δ_t are demographic group, state, and year fixed effects, ρ_{st} is a vector of state characteristics, X_{igst} is a vector of individual characteristics, and ν_{igst} is the error term.

The second-stage equation is as follows:

$$\text{(3)} \qquad Y_{igst} = \beta\overline{\widehat{\log(SimCDCC_{gst})}} + \alpha_g + \alpha_s + \alpha_t + \rho_{st}\phi + X_{igst}\Gamma + \epsilon_{igst},$$

where Y_{igst} is an indicator for paid child care use for household i in demographic group g in state s during year t. $\overline{\widehat{\log(SimCDCC_{gst})}}$ are the fitted values from the first-stage equation, and the remaining parameters are similar to those listed earlier. I cluster standard errors at the state level. β is the coefficient of interest and represents the causal effect of an increase in CDCC benefits.

5.2 Results

Table 5 presents estimates of the effects of CDCC benefits on annual paid child care participation among single mothers. Row "OLS" lists naive estimates from Equation (1), and rows "1st Stage" and "2nd Stage" list IV estimates from Equations (2) and (3), respectively. The statistically significant OLS estimate in Table 5 suggests that a 10 percent increase in CDCC benefits is associated with a five percent increase in annual paid child care participation. Turning to the IV estimates, the first stage is strong with an F-statistic of 19. The statistically significant second-stage IV estimate, 0.114, is slightly smaller than the OLS estimate and implies that a 10 percent increase in CDCC benefits causes a four percent increase in annual paid child care use among single mothers.

Next, Table 6 presents paid child care estimates among single fathers. The statistically

significant OLS estimate, 0.128, suggests that a 10 percent increase in child care subsidies is associated with a five percent increase in paid child care use among single fathers. Turning to the IV estimates, the first stage is not particularly strong; the F-statistic is 5, so IV estimates should be treated as suggestive. Nonetheless, the statistically significant second-stage estimate of 0.080 is similar to the estimate among single mothers and suggests that a 10 percent increase in CDCC benefits leads single fathers to increase their paid child care participation by three percent.

Finally, Table 7 displays results among married households. The statistically significant OLS estimate implies that a 10 percent increase in child care subsidies is associated with a five percent increase in paid child care use among married households. The first-stage IV estimate is particularly strong with an F-statistic of 36. The statistically significant second-stage estimate, 0.118, is nearly identical to the estimate among single mothers and suggests that a 10 percent increase in CDCC benefits leads to a five percent increase in paid child care use.

5.3 Robustness

Evidence documented in Section 2 suggests that some states may have responded to the federal CDCC expansion by changing their CDCC generosity relative to that of the federal credit. In this case, states' policy changes may mitigate or exacerbate the increase in CDCC generosity that would have occurred in the absence of a state policy response. If policy effects otherwise would have been relatively small (large) in such states, then failing to control for time-varying state-specific unobservable characteristics leads one to overestimate (underestimate) the effects of CDCC benefits. I therefore estimate IV models in which I instrument for actual CDCC benefits using simulated CDCC benefits within a particular demographic group and state as of 2002, before the federal CDCC expansion. Effects of CDCC benefits as of 2002 should be uncorrelated with any endogenous state policy responses to the federal CDCC expansion.

The first-stage equation for the IV models in which simulated benefits as of 2002 instrument for actual CDCC benefits is as follows:

$$(4) \qquad \log(CDCC_{igst}) = \lambda\overline{\log(SimCDCC_{gs})} + \delta_g + \delta_s + \delta_t + \rho_{st}\xi + X_{igst}\Omega + \nu_{igst},$$

where, as in Equation (2), $\log(SimCDCC_{gs})$ is the log of actual CDCC benefits for household i in demographic group g in state s during year t. $\overline{\log(SimCDCC_{gs})}$ is the simulated log of CDCC benefits for taxpayers in demographic group g in state s as of 2002. The remaining parameters are similar to those in Equation (2).

The second-stage equation is as follows:

$$(5) \qquad Y_{igst} = \beta\overline{\log(\widehat{SimCDCC_{gs}})} + \alpha_g + \alpha_s + \alpha_t + \rho_{st}\phi + X_{igst}\Gamma + \epsilon_{igst},$$

where Y_{igst} is again an indicator for paid child care participation for household i in demographic group g in state s during year t. $\overline{\log(\widehat{SimCDCC_{gs}})}$ are the fitted values from the first-stage equation. I cluster standard errors at the state level.

Estimates from Equations (4) and (5) are quite similar to those from the main specifications listed in Tables 5, 6, and 7. The statistically significant second-stage estimate IV estimate among married households (coefficient of 0.141, SE = 0.0063) is quite similar to that from Equation (3).

I also estimate specifications in which I remove California, Louisiana, Maine, and Vermont, the states that changed their CDCC policies between 2002 and 2003, from the analyses. Results from these specifications are again similar to those in the main analyses. The estimate among married households (coefficient of 0.116, SE = 0.0063) is nearly identical to that in Table 7.

Another potential concern is that increased CDCC generosity may induce mothers to pursue more education or to have more children. Because of this, I estimate IV models in which demographic groups vary only by marital status and number of children, not by educational attainment or the presence of children younger than six years. While standard errors tend to increase under these specifications, coefficients generally are similar, and the second-stage effect on child care use among married mothers (coefficient of 0.071, SE = 0.0430) remains positive and is marginally significant.

Additionally, it is possible that changes in other tax policies for families with children, such as the expansion of the CTC in 2003, drive results. To address this concern, I estimate specifications in which I do not include state- or individual-level controls, some of which are intended to capture effects of the CTC expansions. Estimates from these models suggest that controls play a limited role in determining the coefficients of interest as estimates are very close to those from the main analyses. The statistically significant effect among married households (coefficient of 0.118, SE = 0.0181) is identical to the estimate in Table 7.

Finally, it is possible that in some geographic areas, child care providers may be unable to respond to increased demand for paid child care, which could result in smaller CDCC effects on paid child care use. To address this concern, I estimate Equations (1), (2), and (3) by households' metropolitan status. Child care supply likely is more elastic in metropolitan areas with many child care providers than in nonmetropolitan areas, where paid child care services may be limited. For single fathers and married mothers, IV estimates of paid child care use are actually larger among households in nonmetropolitan areas. Among single mothers, the estimate for mothers in metropolitan areas (coefficient of 0.125, SE = 0.0244) is slightly larger than that for mothers in nonmetropolitan areas (coefficient of 0.088, SE = 0.0225), but I fail to reject the null hypothesis that the estimates are statistically different from one another. Additionally, I use data from the Quarterly Census of Employment and Wages (United States Department of Labor, Bureau of Labor Statistics 2019) to graph changes in average simulated CDCC benefits and numbers of child care centers and workers across states between 2003 through 2008 and 2000 through 2002. I do not find evidence that changes in child care supply, as proxied by changes in the numbers of child care centers and workers, are correlated with changes in simulated CDCC benefits.

6 Labor Market Outcomes

Given the dramatic increases in paid child care participation shown in Section 5, I now investigate possible labor supply responses via effects on work during the past week, usual hours worked per week, and annual earnings. To do so, I estimate Equations (1), (2), and (3), where Y_{igst} is the outcome of interest for individual i in demographic group g in state s

during year t, separately across single mothers, single fathers, married mothers, and married fathers.

6.1 Results

Table 8 presents estimates of the effects of CDCC benefits on work during the past week, usual hours worked per week, and the log of annual earnings among single mothers. Column "Employed" displays employment results. The statistically significant OLS estimate, 0.034, suggests that a 10 percent increase in CDCC benefits is associated with a 0.5 percent increase in employment among single mothers. Turning to the IV estimates, the first stage, which is identical to that in Table 5, is strong, and the second-stage estimate, 0.015, is positive but very small and statistically insignificant.

Next, Column "Hours" presents effects on hours worked per week. The statistically significant OLS estimate of 1.490 implies that a 10 percent increase in child care subsidies is associated with a 0.5 percent increase in weekly hours worked among single mothers. The second-stage IV estimate, 1.213, is insignificant but similar to the OLS estimate. The final column in Table 8, "Log(Earnings)" lists effects on the log of annual earnings. The statistically significant OLS coefficient implies that a 10 percent increase in child care subsidies is associated with a four percent increase in annual earnings; the second-stage IV estimate, 0.262, is positive but statistically insignificant.

Similarly, Table 9 presents estimates of the effects of CDCC benefits on labor market outcomes among single fathers. IV estimates of the effects of child care subsidies on employment, hours worked, and earnings are all positive but statistically insignificant. Employment and hours coefficients are quite small.

Table 10 lists CDCC effects on married mothers' labor market outcomes. The statistically significant OLS employment estimate, 0.048, suggests that a 10 percent increase in CDCC benefits is associated with a one percent increase in work during the past week among married mothers. The first-stage IV estimate is highly statistically significant with an F-statistic of 35. The statistically significant second-stage estimate, 0.060, is slightly larger than the OLS estimate and implies that child care subsidies increase employment among married mothers.

Turning to hours worked per week, the statistically significant OLS estimate suggests

that a 10 percent increase in CDCC benefits is associated with a one percent increase in weekly hours worked. Similarly, the statistically significant second-stage IV estimate, 2.324, implies that a 10 percent increase in child care subsidies causes a one percent increase in hours worked among married mothers. Increases in employment and hours worked translate into increases in annual earnings; the second-stage IV estimate, 0.616, suggests that a 10 percent increase in CDCC benefits leads to a six percent increase in annual earnings among married mothers.

Finally, Table 11 presents estimates for married fathers. Perhaps due to married fathers' high levels of labor force attachment, employment and hours estimates are quite small, and all IV estimates are statistically insignificant.

6.2 Robustness

As in Section 5.3, I test the robustness of results to various sample restrictions and alternative specifications. First, I test whether results are robust to specifications in which I instrument for actual CDCC benefits using simulated CDCC benefits within a particular demographic group and state as of 2002. These estimates from Equations (4) and (5) are quite similar to those in Tables 8, 9, 10, and 11. The statistically significant employment estimate among married mothers (coefficient of 0.069, SE = 0.0200) is slightly larger than the corresponding estimate in Table 10. Results from specifications in which I remove California, Louisiana, Maine, and Vermont from the analyses are also similar to those from the main analyses. The statistically significant effect on married mothers' employment (coefficient of 0.076, SE = 0.0181) is again similar to the estimate from that in Table 10. In specifications in which demographic groups are not functions of education or child age, estimates tend to be smaller and less precise, but the employment estimate among married mothers (coefficient of 0.033, SE = 0.0173) is positive and marginally significant. Finally, estimates change little in specifications without state- and individual-level controls; the statistically significant employment effect among married mothers (coefficient of 0.062, SE = 0.0181) is nearly identical to that in the main analysis.

7 Implications of Results and Evidence Regarding Heterogeneity in Outcomes

Results in Section 5 and 6 indicate that while increases in child care subsidies lead to increases in paid child care participation across all family structures, married mothers exhibit particularly large labor supply responses to increases in CDCC generosity. It remains unclear, however, whether there are changes in married mothers' labor supply along the intensive margin. In Table 12, I estimate the effects of CDCC benefits on part-time and full-time work among married mothers using Equations (1), (2), and (3). While the IV estimate for part-time work is insignificant, the IV estimate for full-time work is a statistically significant 0.043, which implies that a 10 percent increase in child care subsidies increases full-time employment among married women by one percent. In addition, the IV estimates for part-time and full-time work nearly sum to the employment estimate in Table 10, which suggests that married women do not decrease their labor supply along the intensive margin in response to the federal CDCC expansion.

Sizable labor supply responses to increased CDCC generosity among married women suggest that increased CDCC generosity may lead to long-run earnings gains for mothers. In light of this evidence and literature that documents long-run earnings penalties among mothers who leave the labor market around childbirth (Angelov, Johansson, and Lindahl 2016; Kleven et al. 2019; Kleven, Landais, and Søgaard 2019), I estimate models of paid child care participation and labor market outcomes across married mothers with and without children younger than two years. If child care subsidies prevent mothers of very young children from decreasing their labor supplied, such mothers may experience long-run increases in income. Table 13 presents estimates for married mothers by the presence of a child younger than two years in the household. OLS estimates of the effects of CDCC benefits on paid child care use, employment, hours worked per week, and annual earnings are all positive and highly statistically significant among married mothers with children younger than two years. The first-stage IV estimate is weak, however, with an F-statistic of 6. While I treat second-stage IV estimates as suggestive, they are all positive, and the paid child care and employment

estimates are statistically significant at conventional levels. The second-stage estimate of the effect on work during the past week implies that a 10 percent increase in child care subsidies increases employment among married mothers with very young children by three percent. This suggests that increased CDCC generosity may induce mothers to remain the labor market, which could lead to long-run increases in their earnings.

Despite sizable labor supply responses among married mothers, single mothers do not exhibit strong labor supply responses to CDCC benefits. In Section 4, I documented that married women are much more likely to have college degrees than single women. Given this discrepancy, it is plausible that single women increased their labor supply in response to the federal CDCC expansion but that they were unable to find work. To test this hypothesis, I estimate Equations (1), (2), and (3) across single and married mothers, where Y_{igst} is an indicator for self-reported labor force participation. Labor force participation may better measure labor supply than employment because labor force participation theroretically includes unemployment among individuals looking for work. Nevertheless, labor force participation results are similar to employment results in Tables 8 and 10, so differences in labor demand do not seem to drive differences in employment outcomes between married and single women.

Evidence from Sections 2, 3, and 4 suggests that differences in CDCC eligibility and incentives likely explain different labor supply effects across married and single mothers at least to some extent. Specifically, many single mothers have earnings that are too low to benefit from the nonrefundable federal CDCC. Hence, the positive labor supply responses to increased generosity among married women, whose household incomes generally exceed tax filing thresholds, suggests that making the federal CDCC refundable could induce single mothers to increase their labor supply.

8 Conclusion

Taken together, results suggest that child care subsidies lead to increases in child care participation across all family structures and increases in labor supply among married mothers. This empirical evidence, combined with theoretical evidence that low-income households ex-

perience relatively small extensive margin labor supply incentives, suggests that making the federal CDCC refundable could encourage low-income parents to enter the labor force.

Even under very different circumstances, the increases in paid child care use that I find are consistent with those of the existing literature that estimates the effects of expanding highly-subsidized child care programs in Europe (Cornelissen et al. 2018; Givord and Marbot 2015). Further, I find an earnings elasticity with respect to CDCC benefits among women (0.42) that is quite close to the earnings elasticity of 0.39 that Rothstein (2010) estimates for an expansion of EITC benefits, despite the fact that EITC policies tend to target single women (Eissa and Liebman 1996; Hoynes and Patel 2018; Keane and Moffitt 1998; Meyer and Rosenbaum 2001).

The results that I find imply that increased CDCC generosity leads to increases in earnings among married mothers that offset a substantial proportion of increased benefits, which suggests that expanding the CDCC may be a cost-effective way to increase mothers' labor supply. Still, since CDCC benefits are not tied to child care quality, increases in mothers' labor supply could compromise children's quality of child care. Further research may address the effects of increased CDCC generosity on child outcomes and families' tradeoffs between income and child care quality.

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9 Tables and Figures

Table 1: Federal CDCC Parameters over Time

	1976	1982	2003
Max Number of Children	2	2	2
Max Qualifying Expenses per Child	$2,000	$2,400	$3,000
Max Benefit Rate	0.20	0.30	0.35
Max CDCC per Child	$400	$720	$1,050
Start of Phase-Out	N/A	$10,000	$15,000
End of Phase-Out	N/A	$28,000	$43,000
Max CDCC per Child After Phase-Out	$400	$480	$600
Refundable?	No	No	No

Federal CDCC parameters over time. "Max Number of Children" indicates the maximum number of children younger than age 13 for which taxpayers could receive CDCC benefits. "Start of Phase-Out" and "End of Phase-Out" indicate the AGI levels at which benefits begin to decrease and remain constant, respectively. Information retrieved using federal tax forms.

Table 2: Average Annual Child Care Expenditures in 2002 among SIPP Respondents by Demographic Group, Conditional on Paid Child Care Participation

Demographic Group	Child Care Expenditures
Single, 1 Child, No Child <6, No College	$2,327
Single, 1 Child, No Child <6, College	$2,809
Single, 1 Child, Child <6, No College	$4,118
Single, 1 Child, Child <6, College	$5,005
Single, 2+ Children, No Child <6, No College	$3,998
Single, 2+ Children, No Child <6, College	$5,201
Single, 2+ Children, Child <6, No College	$4,690
Single, 2+ Children, Child <6, College	$8,114
Married, 1 Child, No Child <6, No College	$1,844
Married, 1 Child, No Child <6, College	$3,485
Married, 1 Child, Child <6, No College	$4,707
Married, 1 Child, Child <6, College	$6,557
Married, 2+ Children, No Child <6, No College	$3,273
Married, 2+ Children, No Child <6, College	$4,576
Married, 2+ Children, Child <6, No College	$5,556
Married, 2+ Children, Child <6, College	$8,342

Average annual child care expenditures among SIPP Child Care Topical Module respondents from 2002 by marital status, number of children younger than 13 years, the presence of at least one child younger than six years, and an indicator for the mother's (or single father's) education, conditional on paid child care participation. Data retrieved from United States Census Bureau (2019) using household weights.

Table 3: Characteristics of State CDCC Programs as of 2002

State	Statutory Max	Effective Max	Refundable?	AGI Limit	Provision
AR	$288	$237	No	N/A	20% of federal CDCC
CA	$907	$907	Yes	$100,000	63% of federal CDCC
CO	$720	$596	No	$60,000	50% of federal CDCC
DE	$720	$596	No	N/A	50% of federal CDCC
DC	$461	$461	Yes	N/A	32% of federal CDCC
HI	$1,200	$1,200	Yes	N/A	25% of federal CDCC expenses
IA	$1,080	$1,080	Yes	$40,000	75% of federal CDCC
KS	$360	$298	No	N/A	25% of federal CDCC
KY	$288	$288	Yes	N/A	20% of federal CDCC
ME	$360	$262	No	N/A	25% of federal CDCC
MD	$468	$341	No	$50,000	32.5% of federal CDCC
MN	$1,079	$1,079	No	$32,250	Federal CDCC for which eligible
NE	$1,440	$1,193	No	N/A	100% of federal CDCC
NM	$1,200	$1,200	Yes	$21,424	40% of eligible child care expenses
NY	$1,584	$1,584	Yes	N/A	110% of federal CDCC
NC	$432	$432	Yes	N/A	9% of federal CDCC expenses
OH	$1,440	$1,440	Yes	$40,000	100% of federal CDCC
OK	$288	$239	No	N/A	20% of federal CDCC
OR	$1,920	$1,440	No	$45,000	30% of "allowable" federal CDCC expenses
RI	$360	$262	No	N/A	25% of federal CDCC
SC	$336	$336	Yes	N/A	7% of federal CDCC expenses
VT	$346	$252	No	N/A	24% of federal CDCC

Characteristics of state CDCC programs as of 2002. Columns "Statutory Max" and "Effective Max" list statutory and effective maximum state CDCC benefits, respectively. "AGI Limit" lists the AGI at which the household is no longer eligible for CDCC benefits. "Provision" displays the calculation to determine the state's statutory CDCC benefits. Information retrieved using state and federal tax forms (Feenberg 2017).

Table 4: Summary Statistics

	Single Mothers	Single Fathers	Married Mothers	Married Fathers
Age	35.84	36.29	38.29	38.29
	(0.0761)	(0.1496)	(0.0359)	(0.0387)
White	0.64	0.77	0.85	0.85
	(0.0056)	(0.0091)	(0.0023)	(0.0022)
Black	0.31	0.17	0.08	0.08
	(0.0054)	(0.0085)	(0.0017)	(0.0018)
College	0.14	0.11	0.34	0.34
	(0.004)	(0.0069)	(0.0028)	(0.0028)
Kids <6	0.52	0.66	0.74	0.78
	(0.0078)	(0.0159)	(0.0048)	(0.0048)
Kids <17	1.96	1.89	2.08	2.07
	(0.012)	(0.2040)	(0.0059)	(0.0058)
Child Care	0.26	0.25	0.23	0.24
	(0.0050)	(0.0091)	(0.0025)	(0.0025)
Employed	0.74	0.79	0.66	0.91
	(0.0050)	(0.0086)	(0.0028)	(0.0017)
Hours	26.94	31.71	21.59	39.82
	(0.2186)	(0.4352)	(0.1163)	(0.1070)
Earnings	20,839	29,486	20,353	56,264
	(290)	(754)	(172)	(363)
Observations	11,092	3,293	40,966	41,448
Representative of	15,115,011	4,256,430	52,939,088	53,686,920

Summary statistics for parents ages 26 through 54 in households with children younger than 13 from the 2001 through 2003 CPS surveys. "Child Care" indicates whether the household paid for child care during the previous calendar year, "Employed" indicates whether the individual is employed, and "Hours" indicate the number of hours that the individual worked during the past week. Earnings are from the previous calendar year. Standard errors are listed in parentheses. Data retrieved from Roth (2019) using household weights.

Table 5: Effect on Paid Child Care Use among Single Mothers

	Paid for Child Care
OLS	0.126***
	(0.0008)
1st Stage	0.115***
	(0.0263)
2nd Stage	0.114***
	(0.0184)
Mean	0.265
Observations	37,387

Effect of a 1-unit increase in the log of CDCC benefits on annual paid child care use among single mothers using the simulated log of CDCC benefits in a particular state, year, and demographic group as an instrument. Standard errors are clustered at the state level and listed in parentheses.
* Significant at the 10% level
** Significant at the 5% level
*** Significant at the 1% level

Table 6: Effect on Paid Child Care Use among Single Fathers

	Paid for Child Care
OLS	0.128***
	(0.0006)
1st Stage	0.104**
	(0.0463)
2nd Stage	0.080**
	(0.0340)
Mean	0.249
Observations	11,380

Effects of a 1-unit increase in the log of CDCC benefits on annual paid child care use among single fathers using the simulated log of CDCC benefits in a particular state, year, and demographic group as an instrument. Standard errors are clustered at the state level and listed in parentheses.
* Significant at the 10% level
** Significant at the 5% level
*** Significant at the 1% level

Table 7: Effect on Paid Child Care Use among Married Households

	Paid for Child Care
OLS	0.129***
	(0.0005)
1st Stage	0.227***
	(0.0378)
2nd Stage	0.118***
	(0.0186)
Mean	0.236
Observations	138,887

Effects of a 1-unit increase in the log of CDCC benefits on annual paid child care use among married households using the simulated log of CDCC benefits in a particular state, year, and demographic group as an instrument. Standard errors are clustered at the state level and listed in parentheses.
* Significant at the 10% level
** Significant at the 5% level
*** Significant at the 1% level

Table 8: Effects on Labor Market Outcomes among Single Mothers

	Employed	Hours	Log(Earnings)
OLS	0.034***	1.490***	0.417***
	(0.0006)	(0.0390)	(0.0118)
1st Stage	0.115***	0.115***	0.115***
	(0.0263)	(0.0263)	(0.0263)
2nd Stage	0.015	1.213	0.262
	(0.0242)	(1.1743)	(0.2736)
Mean	0.738	29.94	8.56
Observations	37,387	37,387	37,387

Effects of a 1-unit increase in the log of CDCC benefits on labor market outcomes among single mothers using the simulated log of CDCC benefits in a particular state, year, and demographic group as an instrument. "Employed" indicates whether the individual is employed, "Hours" indicates the number of hours that the individual worked during the past week, and "Log(Earnings)" is the log of the individual's annual earnings. Standard errors are clustered at the state level and listed in parentheses.
* Significant at the 10% level
** Significant at the 5% level
*** Significant at the 1% level

Table 9: Effects on Labor Market Outcomes among Single Fathers

	Employed	Hours	Log(Earnings)
OLS	0.019***	0.956***	0.257***
	(0.0012)	(0.0693)	(0.0115)
1st Stage	0.104**	0.104**	0.104**
	(0.0463)	(0.0463)	(0.0463)
2nd Stage	0.027	1.531	0.593
	(0.0424)	(2.5676)	(0.4961)
Mean	0.790	31.71	9.49
Observations	11,380	11,380	11,380

Effects of a 1-unit increase in the log of CDCC benefits on labor market outcomes among single fathers using the simulated log of CDCC benefits in a particular state, year, and demographic group as an instrument. "Employed" indicates whether the individual is employed, "Hours" indicates the number of hours that the individual worked during the past week, and "Log(Earnings)" is the log of the individual's annual earnings. Standard errors are clustered at the state level and listed in parentheses.
* Significant at the 10% level
** Significant at the 5% level
*** Significant at the 1% level

Table 10: Effects on Labor Market Outcomes among Married Mothers

	Employed	Hours	Log(Earnings)
OLS	0.048***	1.939***	0.601***
	(0.0011)	(0.0386)	(0.0157)
1st Stage	0.232***	0.232***	0.232***
	(0.0393)	(0.0393)	(0.0393)
2nd Stage	0.060***	2.324***	0.616***
	(0.0183)	(0.6994)	(0.1792)
Mean	0.660	21.59	7.43
Observations	130,141	130,141	130,141

Effects of a 1-unit increase in the log of CDCC benefits on labor market outcomes among married mothers using the simulated log of CDCC benefits in a particular state, year, and demographic group as an instrument. "Employed" indicates whether the individual is employed, "Hours" indicates the number of hours that the individual worked during the past week, and "Log(Earnings)" is the log of the individual's annual earnings. Standard errors are clustered at the state level and listed in parentheses.
* Significant at the 10% level
** Significant at the 5% level
*** Significant at the 1% level

Table 11: Effects on Labor Market Outcomes among Married Fathers

	Employed	Hours	Log(Earnings)
OLS	0.006***	0.212***	0.082***
	(0.0002)	(0.0226)	(0.0030)
1st Stage	0.232***	0.232***	0.232***
	(0.0386)	(0.0386)	(0.0386)
2nd Stage	-0.009	-0.711	0.040
	(0.0113)	(0.6726)	(0.0938)
Mean	0.910	39.82	10.87
Observations	131,412	131,412	131,412

Effects of a 1-unit increase in the log of CDCC benefits on labor market outcomes among married fathers using the simulated log of CDCC benefits in a particular state, year, and demographic group as an instrument. "Employed" indicates whether the individual is employed, "Hours" indicates the number of hours that the individual worked during the past week, and "Log(Earnings)" is the log of the individual's annual earnings. Standard errors are clustered at the state level and listed in parentheses.
* Significant at the 10% level
** Significant at the 5% level
*** Significant at the 1% level

Table 12: Effects on Part- and Full-Time Work among Married Mothers

	Part-Time	Full-Time
OLS	0.0017**	0.046***
	(0.0007)	(0.0009)
1st Stage	0.232***	0.232***
	(0.0393)	(0.0393)
2nd Stage	0.016	0.043***
	(0.0116)	(0.0167)
Mean	0.206	0.454
Observations	130,141	130,141

Effects of a 1-unit increase in the log of CDCC benefits on part- and full-time work among married mothers using the simulated log of CDCC benefits in a particular state, year, and demographic group as an instrument. Standard errors are clustered at the state level and listed in parentheses.
* Significant at the 10% level
** Significant at the 5% level
*** Significant at the 1% level

Table 13: Effects on Paid Child Care and Labor Market Outcomes among Married Mothers by the Presence of a Child Younger than Two Years

	Child <2 Years				No Child <2 Years			
	Child Care	Employed	Hours	Log(Earnings)	Child Care	Employed	Hours	Log(Earnings)
OLS	0.126***	0.055***	2.119***	0.658***	0.129***	0.046***	1.858***	0.586***
	(0.0005)	(0.0015)	(0.0710)	(0.0188)	(0.0005)	(0.0011)	(0.0342)	(0.0151)
1st Stage	0.102**	0.106**	0.106**	0.106**	0.258***	0.256***	0.256***	0.256***
	(0.0422)	(0.0428)	(0.0428)	(0.0428)	(0.0422)	(0.0431)	(0.0431)	(0.0431)
2nd Stage	0.114***	0.177**	6.788*	1.617*	0.117***	0.047***	2.158***	0.511***
	(0.0415)	(0.0843)	(3.6464)	(0.8449)	(0.0190)	(0.0182)	(0.6744)	(0.1958)
Mean	0.258	0.562	16.79	6.78	0.229	0.686	22.88	7.60
Observations	30,769	27,094	27,094	27,094	108,118	103,047	103,047	103,047

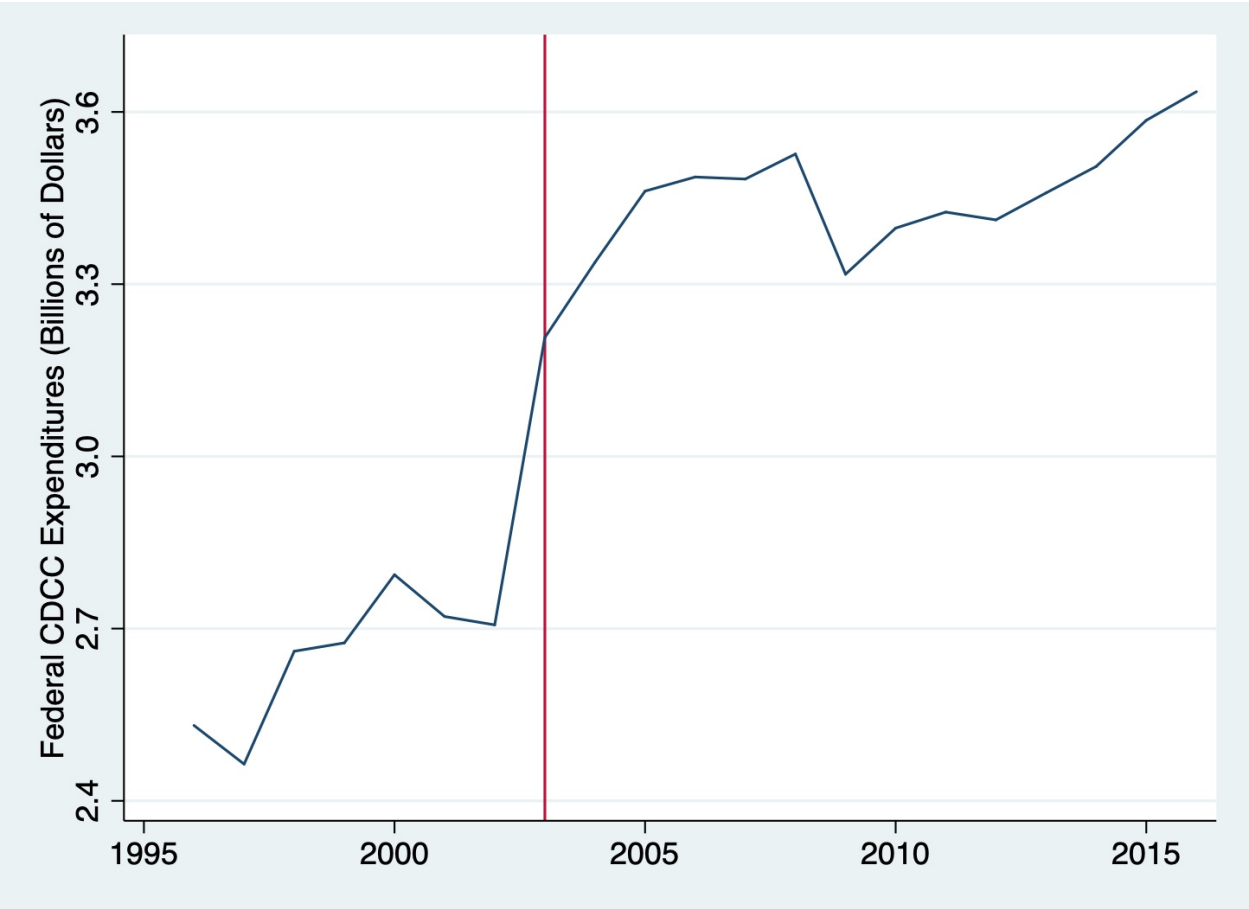
Effects of a 1-unit increase in the log of CDCC benefits on paid child care participation and labor market outcomes among married mothers using the simulated log of CDCC benefits in a particular state, year, and demographic group as an instrument. "Child Care" indicates annual paid child care use, "Employed" indicates whether the individual is employed, "Hours" indicates the number of hours that the individual worked during the past week, and "Log(Earnings)" is the log of the individual's annual earnings. Standard errors are clustered at the state level and listed in parentheses.

* Significant at the 10% level

** Significant at the 5% level

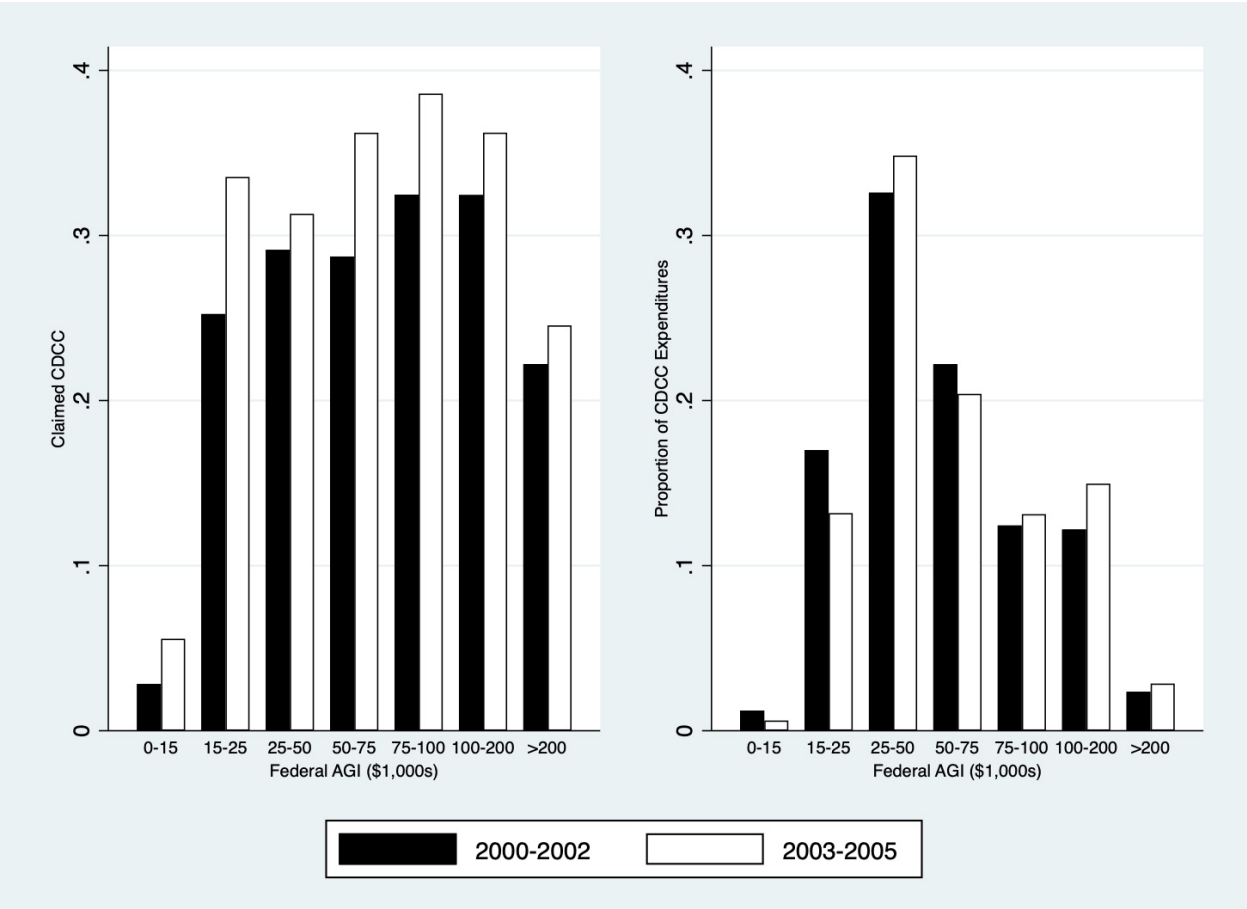
*** Significant at the 1% level

Figure 1: Federal CDCC Expenditures over Time



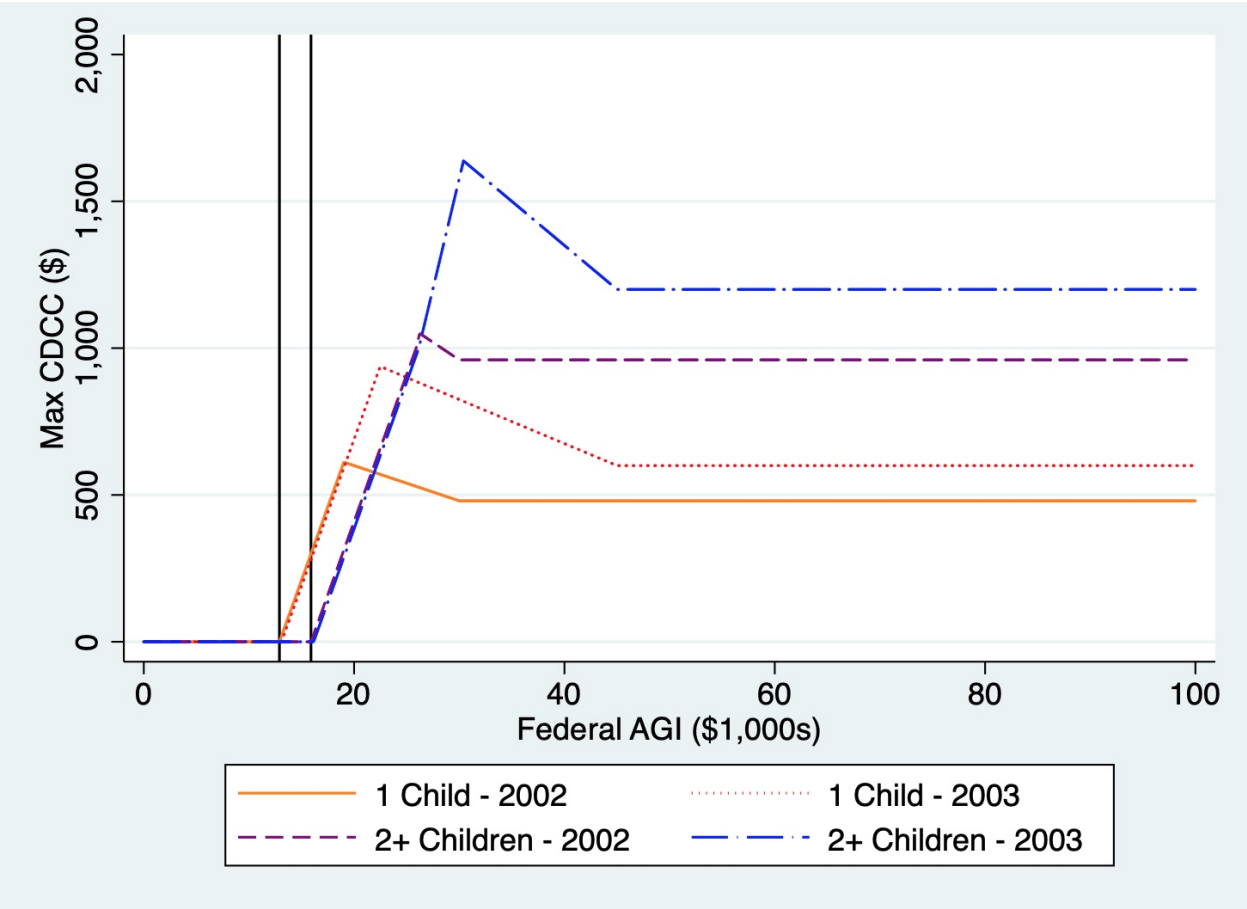
Federal CDCC expenditures over time. Information retrieved from Internal Revenue Service (2018).

Figure 2: Proportion of CDCC Claimants and Expenditures by Federal AGI



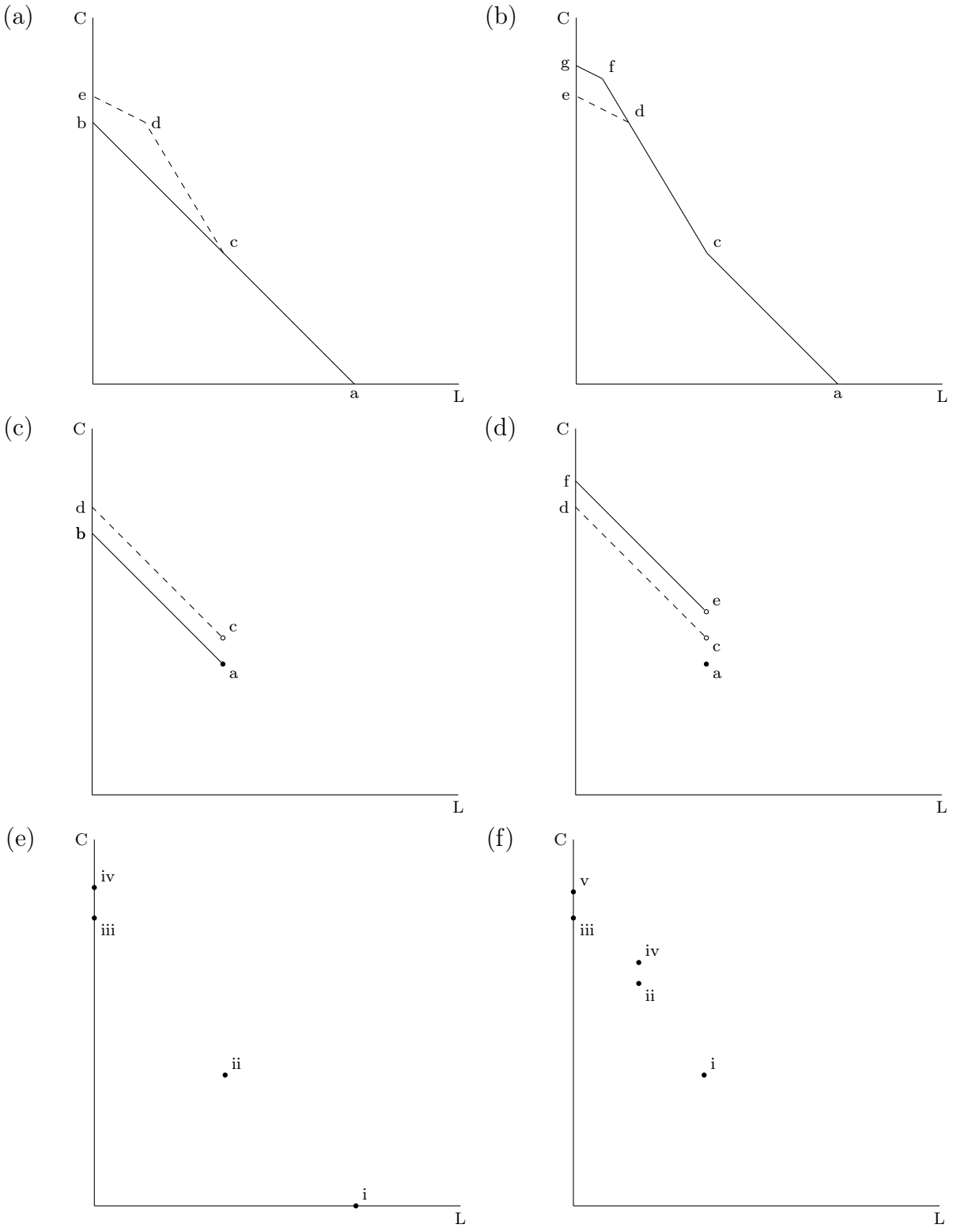
Proportion of federal tax returns in which taxpayers claimed the CDCC and proportion of federal CDCC expenditures from 2000 through 2002 and 2003 through 2005 by federal AGI, conditional on eligibility. Information retrieved using Internal Revenue Service (2018), Roth (2019), and Feenberg (2017).

Figure 3: Maximum Effective Federal CDCC Benefits by Federal AGI



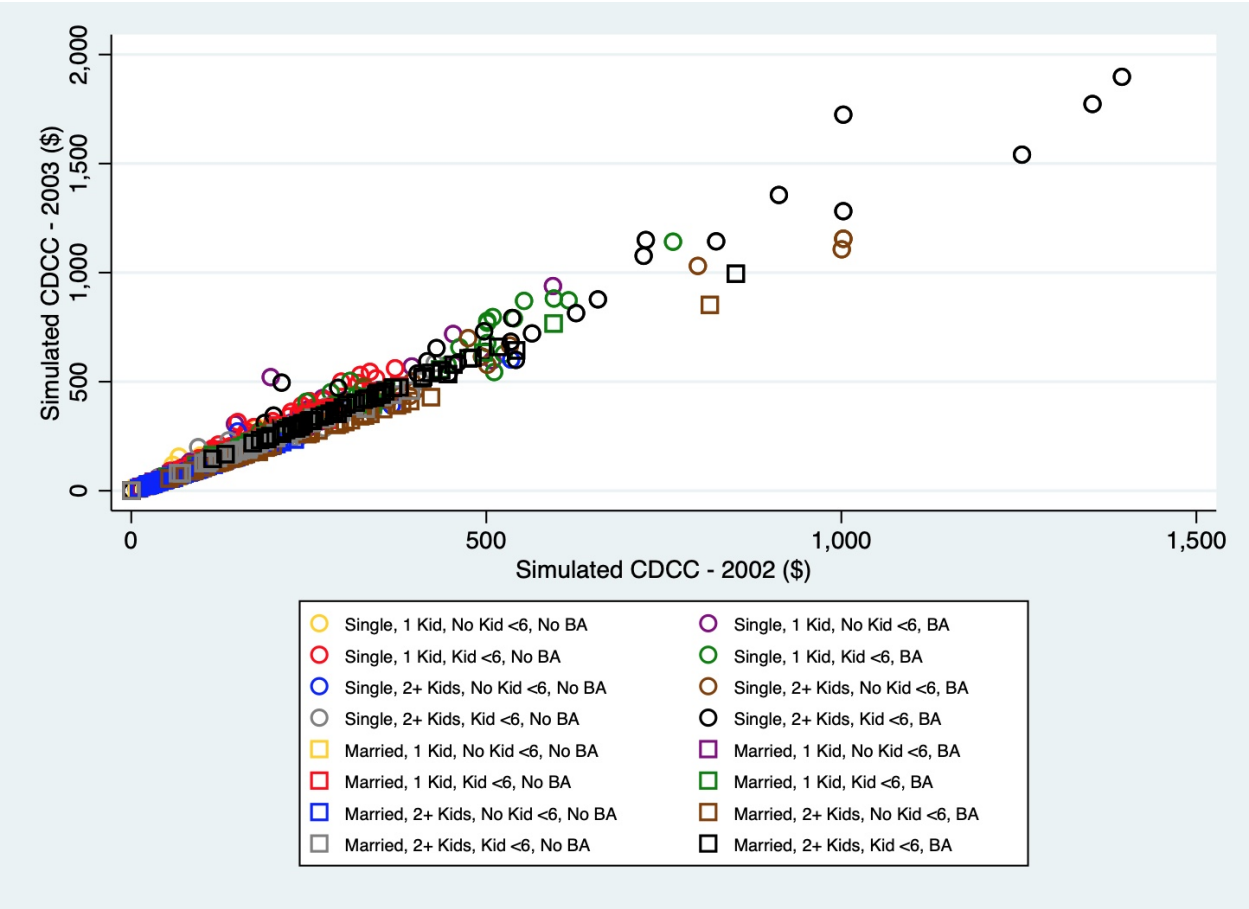
Maximum effective federal CDCC benefits for households with one or two or more eligible dependents before and after the federal CDCC expansion in 2003. Information retrieved using federal tax forms.

Figure 4: Budget Constraints with CDCC Benefits



Budget constraints with CDCC benefits. "C" and "L" denote consumption and leisure, respectively. In Panel (a), the solid line depicts a single earner's budget constraint in the absence of CDCC benefits. The dashed line depicts the single earner's budget constraint with CDCC benefits. In Panel (b), the solid line depicts the single earner's budget constraint if there is an increase in CDCC generosity. In Panel (c), the solid line depicts a secondary earner's budget constraint in the absence of CDCC benefits. The dashed line depicts the secondary earner's budget constraint with CDCC benefits. In Panel (d), the solid line depicts the secondary earner's budget constraint if there is an increase in CDCC generosity. The points in Panel (e) display potential pre- and post-expansion consumption bundles for single earners if hours are fixed. The points in Panel (f) display potential pre-and post-expansion consumption bundles for secondary earners if hours are fixed.

Figure 5: Simulated Real CDCC Benefits between 2002 and 2003



Simulated real CDCC benefits across demographic groups between 2002 and 2003. Demographic groups defined by marital status, number of children, child age, and education. Circles and squares represent states. Information retrieved using Roth (2019) and Feenberg (2017).

A Online Appendix

A.1 CDCC Benefit Imputation Procedure

I calculate households' CDCC benefits using data from the CPS and NBER's TAXSIM program. Given data limitations, I must make some assumptions about individuals' income to calculate their tax credits. Specifically, I make the following assumptions:

1) Households tax-minimize.

2) Married taxpayers file as married, filing jointly, and single taxpayers file as head-of-household.

3) All of the household's dividends are nonqualified.

4) The household does not have capital gains.

5) Effects of rent paid, property taxes, and mortgage interest on CDCC benefits are negligible.

6) I impute child care expenditures using the average child care expenditures among individuals with the same marital status, number of children, age of children, and education level from the 2002 SIPP Child Care Topical Module.

Using these assumptions and the income and demographic data in the CPS, I run TAXSIM to calculate individuals' federal and state CDCC benefits.

A.2 Tables and Figures

Table A1: Summary Statistics by CDCC Eligibility

	Eligible				Ineligible			
	SM	SF	MM	MF	SM	SF	MM	MF
Age	35.33	36.45	35.78	37.37	35.99	36.24	36.85	38.54
	(0.1620)	(0.3239)	(0.0748)	(0.0806)	(0.0861)	(0.1685)	(0.0407)	(0.0439)
White	0.68	0.83	0.86	0.86	0.63	0.76	0.85	0.85
	(0.0120)	(0.0182)	(0.0048)	(0.0047)	(0.0063)	(0.0104)	(0.0028)	(0.0025)
Black	0.27	0.14	0.09	0.09	0.32	0.18	0.08	0.08
	(0.0116)	(0.0177)	(0.0039)	(0.0041)	(0.0062)	(0.0097)	(0.0019)	(0.0020)
College	0.25	0.20	0.45	0.38	0.11	0.09	0.31	0.33
	(0.0106)	(0.0191)	(0.0065)	(0.0062)	(0.0040)	(0.0069)	(0.0031)	(0.0031)
Kids <6	0.54	0.69	0.88	0.90	0.51	0.65	0.71	0.75
	(0.0153)	(0.0315)	(0.0094)	(0.0092)	(0.0090)	(0.0184)	(0.0055)	(0.0055)
Kids <17	1.60	1.66	1.86	1.85	2.06	1.95	2.14	2.13
	(0.0200)	(0.0372)	(0.0112)	(0.0110)	(0.0140)	(0.0236)	(0.0068)	(0.0067)
Hours	35.83	38.22	31.84	41.18	24.38	29.97	18.73	39.45
	(0.3733)	(0.8135)	(0.2116)	(0.2106)	(0.2531)	(0.4983)	(0.1316)	(0.1234)
Earnings	34,816	42,692	35,552	55,362	16,973	25,945	16,333	56,509
	(842)	(2,036)	(463)	(659)	(267)	(757)	(169)	(425)
Observations	2,350	712	8,677	8,973	8,742	2,581	32,289	32,475
Representative of	3,274,580	899,920	11,073,510	11,464,813	11,840,431	3,356,510	41,865,576	42,222,108

Summary statistics for parents ages 26 through 54 in households with children younger than 13 from the 2001 through 2003 CPS surveys by CDCC eligibility. "SM", "SF", "MM", and "MF" denote single mothers, single fathers, married mothers, and married fathers, respectively. "Child Care" indicates whether the household paid for child care during the previous calendar year, and "Hours" indicates the number of hours that the individual worked during the past week. Earnings are from the previous calendar year. Standard errors are listed in parentheses. Data retrieved from Roth (2019) using household weights.

Table A2: Alternative Specifications for Effect on Paid Child Care Use among Single Mothers

	(1)	(2)	(3)	(4)
OLS	0.126***	0.126***	0.126***	0.127***
	(0.0008)	(0.0008)	(0.0008)	(0.0008)
1st Stage	0.115***	0.109***	0.144*	0.113***
	(0.0267)	(0.0261)	(0.0754)	(0.0258)
2nd Stage	0.126***	0.109***	0.110	0.108***
	(0.0162)	(0.0199)	(0.0821)	(0.0192)
Mean	0.265	0.268	0.266	0.265
Observations	37,387	32,651	37,619	37,387

Effect of a 1-unit increase in the log of CDCC benefits on annual paid child care use among single mothers. (1) lists results from specifications in which the simulated log of CDCC benefits in a particular state and demographic group as of 2002 serves as an instrument. (2) lists results from specifications without California, Louisiana, Maine, and Vermont. (3) lists results from specifications in which demographic groups are not functions of education or child age. (4) lists results from specifications without state- and individual-level controls. Standard errors are clustered at the state level and listed in parentheses.
* Significant at the 10% level
** Significant at the 5% level
*** Significant at the 1% level

Table A3: Alternative Specifications for Effect on Paid Child Care Use among Single Fathers

	(1)	(2)	(3)	(4)
OLS	0.128***	0.128***	0.127***	0.128***
	(0.0006)	(0.0008)	(0.0006)	(0.0006)
1st Stage	0.091*	0.116**	0.367***	0.104**
	(0.0475)	(0.0486)	(0.1374)	(0.0477)
2nd Stage	0.094***	0.086***	0.087**	0.079**
	(0.0346)	(0.0300)	(0.0344)	(0.0353)
Mean	0.249	0.257	0.249	0.249
Observations	11,380	9,613	11,427	11,380

Effects of a 1-unit increase in the log of CDCC benefits on annual paid child care use among single fathers. (1) lists results from specifications in which the simulated log of CDCC benefits in a particular state and demographic group as of 2002 serves as an instrument. (2) lists results from specifications without California, Louisiana, Maine, and Vermont. (3) lists results from specifications in which demographic groups are not functions of education or child age. (4) lists results from specifications without state- and individual-level controls. Standard errors are clustered at the state level and listed in parentheses.
* Significant at the 10% level
** Significant at the 5% level
*** Significant at the 1% level

Table A4: Alternative Specifications for Effect on Paid Child Care Use among Married Households

	(1)	(2)	(3)	(4)
OLS	0.129***	0.129***	0.127***	0.129***
	(0.0005)	(0.0004)	(0.0005)	(0.0005)
1st Stage	0.207***	0.224***	0.350***	0.233***
	(0.0310)	(0.0373)	(0.1009)	(0.0379)
2nd Stage	0.141***	0.116***	0.071*	0.118***
	(0.0063)	(0.0197)	(0.0430)	(0.0181)
Mean	0.236	0.239	0.236	0.236
Observations	138,887	121,016	138,887	138,887

Effects of a 1-unit increase in the log of CDCC benefits on annual paid child care use among married households. (1) lists results from specifications in which the simulated log of CDCC benefits in a particular state and demographic group as of 2002 serves as an instrument. (2) lists results from specifications without California, Louisiana, Maine, and Vermont. (3) lists results from specifications in which demographic groups are not functions of education or child age. (4) lists results from specifications without state- and individual-level controls. Standard errors are clustered at the state level and listed in parentheses.
* Significant at the 10% level
** Significant at the 5% level
*** Significant at the 1% level

Table A5: Effects on Paid Child Care Use among Single Mothers by Metropolitan Status

	Metro	Non-Metro
	Paid for Child Care	Paid for Child Care
OLS	0.127***	0.125***
	(0.0008)	(0.0016)
1st Stage	0.092***	0.206***
	(0.0303)	(0.0605)
2nd Stage	0.125***	0.088***
	(0.0244)	(0.0225)
Mean	0.271	0.237
Observations	29,870	7,307

Effect of a 1-unit increase in the log of CDCC benefits on annual paid child care use among single mothers by metropolitan status using the simulated log of CDCC benefits in a particular state, year, and demographic group as an instrument. Standard errors are clustered at the state level and listed in parentheses.
* Significant at the 10% level
** Significant at the 5% level
*** Significant at the 1% level

Table A6: Effects on Paid Child Care Use among Single Fathers by Metropolitan Status

	Metro	Non-Metro
	Paid for Child Care	Paid for Child Care
OLS	0.128***	0.127***
	(0.0006)	(0.0015)
1st Stage	0.085*	0.218**
	(0.0491)	(0.1074)
2nd Stage	0.067	0.111***
	(0.0495)	(0.0286)
Mean	0.257	0.212
Observations	8,878	2,440

Effect of a 1-unit increase in the log of CDCC benefits on annual paid child care use among single fathers by metropolitan status using the simulated log of CDCC benefits in a particular state, year, and demographic group as an instrument. Standard errors are clustered at the state level and listed in parentheses.
* Significant at the 10% level
** Significant at the 5% level
*** Significant at the 1% level

Table A7: Effects on Paid Child Care Use among Married Households by Metropolitan Status

	Metro	Non-Metro
	Paid for Child Care	Paid for Child Care
OLS	0.129***	0.128***
	(0.0005)	(0.0006)
1st Stage	0.195***	0.257***
	(0.0378)	(0.0508)
2nd Stage	0.116***	0.116***
	(0.0203)	(0.0237)
Mean	0.235	0.240
Observations	109,250	28,646

Effects of a 1-unit increase in the log of CDCC benefits on annual paid child care use among married households by metropolitan status using the simulated log of CDCC benefits in a particular state, year, and demographic group as an instrument. Standard errors are clustered at the state level and listed in parentheses.
* Significant at the 10% level
** Significant at the 5% level
*** Significant at the 1% level

Table A8: Alternative Specifications for Effects on Employment among Single Mothers

	(1)	(2)	(3)	(4)
OLS	0.034***	0.033***	0.126***	0.034***
	(0.0006)	(0.0007)	(0.0008)	(0.0007)
1st Stage	0.115***	0.109***	0.144*	0.113***
	(0.0267)	(0.0261)	(0.0754)	(0.0258)
2nd Stage	0.016	0.008	0.110	0.015
	(0.0247)	(0.0273)	(0.0821)	(0.0248)
Mean	0.738	0.745	0.739	0.738
Observations	37,387	32,651	37,619	37,387

Effects of a 1-unit increase in the log of CDCC benefits on employment among single mothers. (1) lists results from specifications in which the simulated log of CDCC benefits in a particular state and demographic group as of 2002 serves as an instrument. (2) lists results from specifications without California, Louisiana, Maine, and Vermont. (3) lists results from specifications in which demographic groups are not functions of education or child age. (4) lists results from specifications without state- and individual-level controls. Standard errors are clustered at the state level and listed in parentheses.

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level

Table A9: Alternative Specifications for Effects on Hours Worked per Week among Single Mothers

	(1)	(2)	(3)	(4)
OLS	1.490***	1.494***	1.581***	1.508***
	(0.0390)	(0.0462)	(0.0368)	(0.0407)
1st Stage	0.115***	0.109***	0.144*	0.113***
	(0.0267)	(0.0261)	(0.0754)	(0.0258)
2nd Stage	1.324	1.154	2.639	1.327
	(1.2055)	(1.3337)	(2.8308)	(1.1934)
Mean	26.94	27.12	26.89	26.94
Observations	37,387	32,651	37,619	37,387

Effects of a 1-unit increase in the log of CDCC benefits on hours worked during the past week among single mothers. (1) lists results from specifications in which the simulated log of CDCC benefits in a particular state and demographic group as of 2002 serves as an instrument. (2) lists results from specifications without California, Louisiana, Maine, and Vermont. (3) lists results from specifications in which demographic groups are not functions of education or child age. (4) lists results from specifications without state- and individual-level controls. Standard errors are clustered at the state level and listed in parentheses.
* Significant at the 10% level
** Significant at the 5% level
*** Significant at the 1% level

Table A10: Alternative Specifications for Effects on Earnings among Single Mothers

	(1)	(2)	(3)	(4)
OLS	0.417***	0.406***	0.439***	0.423***
	(0.0118)	(0.0086)	(0.0116)	(0.0114)
1st Stage	0.115***	0.109***	0.144*	0.113***
	(0.0267)	(0.0261)	(0.0754)	(0.0258)
2nd Stage	0.266	0.171	0.507	0.249
	(0.2884)	(0.3152)	(0.6076)	(0.2838)
Mean	8.56	8.64	8.56	8.56
Observations	37,387	32,651	37,619	37,387

Effects of a 1-unit increase in the log of CDCC benefits on the log of annual earnings among single mothers. (1) lists results from specifications in which the simulated log of CDCC benefits in a particular state and demographic group as of 2002 serves as an instrument. (2) lists results from specifications without California, Louisiana, Maine, and Vermont. (3) lists results from specifications in which demographic groups are not functions of education or child age. (4) lists results from specifications without state- and individual-level controls. Standard errors are clustered at the state level and listed in parentheses.
* Significant at the 10% level
** Significant at the 5% level
*** Significant at the 1% level

Table A11: Alternative Specifications for Effects on Employment among Single Fathers

	(1)	(2)	(3)	(4)
OLS	0.019***	0.018***	0.020***	0.020***
	(0.0012)	(0.0014)	(0.0012)	(0.0012)
1st Stage	0.091*	0.116**	0.367***	0.104**
	(0.0475)	(0.0486)	(0.1374)	(0.0477)
2nd Stage	0.029	0.031	-0.017	0.029
	(0.0521)	(0.0384)	(0.0444)	(0.0413)
Mean	0.790	0.783	0.790	0.790
Observations	11,380	9,613	11,427	11,380

Effects of a 1-unit increase in the log of CDCC benefits on employment among single fathers. (1) lists results from specifications in which the simulated log of CDCC benefits in a particular state and demographic group as of 2002 serves as an instrument. (2) lists results from specifications without California, Louisiana, Maine, and Vermont. (3) lists results from specifications in which demographic groups are not functions of education or child age. (4) lists results from specifications without state- and individual-level controls. Standard errors are clustered at the state level and listed in parentheses.
* Significant at the 10% level
** Significant at the 5% level
*** Significant at the 1% level

Table A12: Alternative Specifications for Effects on Hours Worked per Week among Single Fathers

	(1)	(2)	(3)	(4)
OLS	0.956***	0.986***	1.019***	0.985***
	(0.0693)	(0.0688)	(0.0680)	(0.0720)
1st Stage	0.091*	0.116**	0.367***	0.104**
	(0.0475)	(0.0486)	(0.1374)	(0.0477)
2nd Stage	1.673	1.847	0.585	1.399
	(3.1512)	(2.1379)	(2.3179)	(2.5351)
Mean	31.71	31.48	31.74	31.71
Observations	11,380	9,613	11,427	11,380

Effects of a 1-unit increase in the log of CDCC benefits on hours worked during the past week among single fathers. (1) lists results from specifications in which the simulated log of CDCC benefits in a particular state and demographic group as of 2002 serves as an instrument. (2) lists results from specifications without California, Louisiana, Maine, and Vermont. (3) lists results from specifications in which demographic groups are not functions of education or child age. (4) lists results from specifications without state- and individual-level controls. Standard errors are clustered at the state level and listed in parentheses.
* Significant at the 10% level
** Significant at the 5% level
*** Significant at the 1% level

Table A13: Alternative Specifications for Effects on Earnings among Single Fathers

	(1)	(2)	(3)	(4)
OLS	0.257***	0.250***	0.268***	0.262***
	(0.0115)	(0.0131)	(0.0114)	(0.0104)
1st Stage	0.091*	0.116**	0.367***	0.104**
	(0.0475)	(0.0486)	(0.1374)	(0.0477)
2nd Stage	0.661	0.452	0.149	0.701
	(0.6047)	(0.4419)	(0.3258)	(0.5358)
Mean	9.49	9.52	9.49	9.49
Observations	11,380	9,613	11,427	11,380

Effects of a 1-unit increase in the log of CDCC benefits on the log of annual earnings among single fathers. (1) lists results from specifications in which the simulated log of CDCC benefits in a particular state and demographic group as of 2002 serves as an instrument. (2) lists results from specifications without California, Louisiana, Maine, and Vermont. (3) lists results from specifications in which demographic groups are not functions of education or child age. (4) lists results from specifications without state- and individual-level controls. Standard errors are clustered at the state level and listed in parentheses.
* Significant at the 10% level
** Significant at the 5% level
*** Significant at the 1% level

Table A14: Alternative Specifications for Effects on Employment among Married Mothers

	(1)	(2)	(3)	(4)
OLS	0.048***	0.047***	0.048***	0.048***
	(0.0011)	(0.0012)	(0.0012)	(0.0011)
1st Stage	0.212***	0.228***	0.359***	0.238***
	(0.0337)	(0.0385)	(0.0996)	(0.0394)
2nd Stage	0.069***	0.076***	0.033*	0.062***
	(0.0200)	(0.0181)	(0.0173)	(0.0181)
Mean	0.660	0.670	0.660	0.660
Observations	130,141	113,412	130,141	130,141

Effects of a 1-unit increase in the log of CDCC benefits on employment among married mothers. (1) lists results from specifications in which the simulated log of CDCC benefits in a particular state and demographic group as of 2002 serves as an instrument. (2) lists results from specifications without California, Louisiana, Maine, and Vermont. (3) lists results from specifications in which demographic groups are not functions of education or child age. (4) lists results from specifications without state- and individual-level controls. Standard errors are clustered at the state level and listed in parentheses.
* Significant at the 10% level
** Significant at the 5% level
*** Significant at the 1% level

Table A15: Alternative Specifications for Effects on Hours Worked per Week among Married Mothers

	(1)	(2)	(3)	(4)
OLS	1.939***	1.926***	1.922***	1.967***
	(0.0386)	(0.0415)	(0.0393)	(0.0377)
1st Stage	0.212***	0.228***	0.359***	0.238***
	(0.0337)	(0.0385)	(0.0996)	(0.0394)
2nd Stage	2.686***	2.733***	0.743	2.418***
	(0.7460)	(0.7514)	(0.6456)	(0.7071)
Mean	21.59	21.67	21.48	21.59
Observations	130,141	113,412	130,141	130,141

Effects of a 1-unit increase in the log of CDCC benefits on hours worked during the past week among married mothers. (1) lists results from specifications in which the simulated log of CDCC benefits in a particular state and demographic group as of 2002 serves as an instrument. (2) lists results from specifications without California, Louisiana, Maine, and Vermont. (3) lists results from specifications in which demographic groups are not functions of education or child age. (4) lists results from specifications without state- and individual-level controls. Standard errors are clustered at the state level and listed in parentheses.
* Significant at the 10% level
** Significant at the 5% level
*** Significant at the 1% level

Table A16: Alternative Specifications for Effects on Earnings among Married Mothers

	(1)	(2)	(3)	(4)
OLS	0.601***	0.590***	0.602***	0.610***
	(0.0157)	(0.0148)	(0.0168)	(0.0154)
1st Stage	0.212***	0.228***	0.359***	0.238***
	(0.0337)	(0.0385)	(0.0996)	(0.0394)
2nd Stage	0.705***	0.768***	0.538**	0.654***
	(0.1991)	(0.1770)	(0.2618)	(0.1823)
Mean	7.43	7.52	7.43	7.43
Observations	130,141	113,412	130,141	130,141

Effects of a 1-unit increase in the log of CDCC benefits on the log of annual earnings among married mothers. (1) lists results from specifications in which the simulated log of CDCC benefits in a particular state and demographic group as of 2002 serves as an instrument. (2) lists results from specifications without California, Louisiana, Maine, and Vermont. (3) lists results from specifications in which demographic groups are not functions of education or child age. (4) lists results from specifications without state- and individual-level controls. Standard errors are clustered at the state level and listed in parentheses.
* Significant at the 10% level
** Significant at the 5% level
*** Significant at the 1% level

Table A17: Alternative Specifications for Effects on Employment among Married Fathers

	(1)	(2)	(3)	(4)
OLS	0.006***	0.006***	0.006***	0.006***
	(0.0002)	(0.0003)	(0.0002)	(0.0002)
1st Stage	0.213***	0.227***	0.350***	0.237***
	(0.0321)	(0.0381)	(0.0989)	(0.0389)
2nd Stage	-0.011	-0.012	-0.012	-0.010
	(0.0131)	(0.0115)	(0.0157)	(0.0114)
Mean	0.910	0.912	0.910	0.910
Observations	131,412	114,630	131,412	131,412

Effects of a 1-unit increase in the log of CDCC benefits on employment among married fathers. (1) lists results from specifications in which the simulated log of CDCC benefits in a particular state and demographic group as of 2002 serves as an instrument. (2) lists results from specifications without California, Louisiana, Maine, and Vermont. (3) lists results from specifications in which demographic groups are not functions of education or child age. (4) lists results from specifications without state- and individual-level controls. Standard errors are clustered at the state level and listed in parentheses.
* Significant at the 10% level
** Significant at the 5% level
*** Significant at the 1% level

Table A18: Alternative Specifications for Effects on Hours Worked per Week among Married Fathers

	(1)	(2)	(3)	(4)
OLS	0.212***	0.226***	0.266***	0.199***
	(0.0226)	(0.0222)	(0.0215)	(0.0217)
1st Stage	0.213***	0.227***	0.350***	0.237***
	(0.0321)	(0.0381)	(0.0989)	(0.0389)
2nd Stage	-0.822	-0.933	-1.555	-0.771
	(0.7736)	(0.6964)	(0.9480)	(0.6747)
Mean	39.82	40.02	39.82	39.82
Observations	131,412	114,630	131,412	131,412

Effects of a 1-unit increase in the log of CDCC benefits on hours worked during the past week among married fathers. (1) lists results from specifications in which the simulated log of CDCC benefits in a particular state and demographic group as of 2002 serves as an instrument. (2) lists results from specifications without California, Louisiana, Maine, and Vermont. (3) lists results from specifications in which demographic groups are not functions of education or child age. (4) lists results from specifications without state- and individual-level controls. Standard errors are clustered at the state level and listed in parentheses.
* Significant at the 10% level
** Significant at the 5% level
*** Significant at the 1% level

Table A19: Alternative Specifications for Effects on Earnings among Married Fathers

	(1)	(2)	(3)	(4)
OLS	0.082***	0.081***	0.092***	0.081***
	(0.0030)	(0.0034)	(0.0037)	(0.0031)
1st Stage	0.213***	0.227***	0.350***	0.237***
	(0.0321)	(0.0381)	(0.0989)	(0.0389)
2nd Stage	0.062	0.057	-0.056	0.035
	(0.1081)	(0.0093)	(0.1167)	(0.0918)
Mean	10.87	10.90	10.87	10.87
Observations	131,412	114,630	131,412	131,412

Effects of a 1-unit increase in the log of CDCC benefits on the log of annual earnings among married fathers. (1) lists results from specifications in which the simulated log of CDCC benefits in a particular state and demographic group as of 2002 serves as an instrument. (2) lists results from specifications without California, Louisiana, Maine, and Vermont. (3) lists results from specifications in which demographic groups are not functions of education or child age. (4) lists results from specifications without state- and individual-level controls. Standard errors are clustered at the state level and listed in parentheses.
* Significant at the 10% level
** Significant at the 5% level
*** Significant at the 1% level

Table A20: Effect on Labor Force Participation among Single Mothers

	In the Labor Force
OLS	0.029***
	(0.0009)
1st Stage	0.115***
	(0.0263)
2nd Stage	0.006
	(0.0263)
Mean	0.804
Observations	37,387

Effect of a 1-unit increase in the log of CDCC benefits on labor force participation among single mothers using the simulated log of CDCC benefits in a particular state, year, and demographic group as an instrument. Standard errors are clustered at the state level and listed in parentheses.
* Significant at the 10% level
** Significant at the 5% level
*** Significant at the 1% level

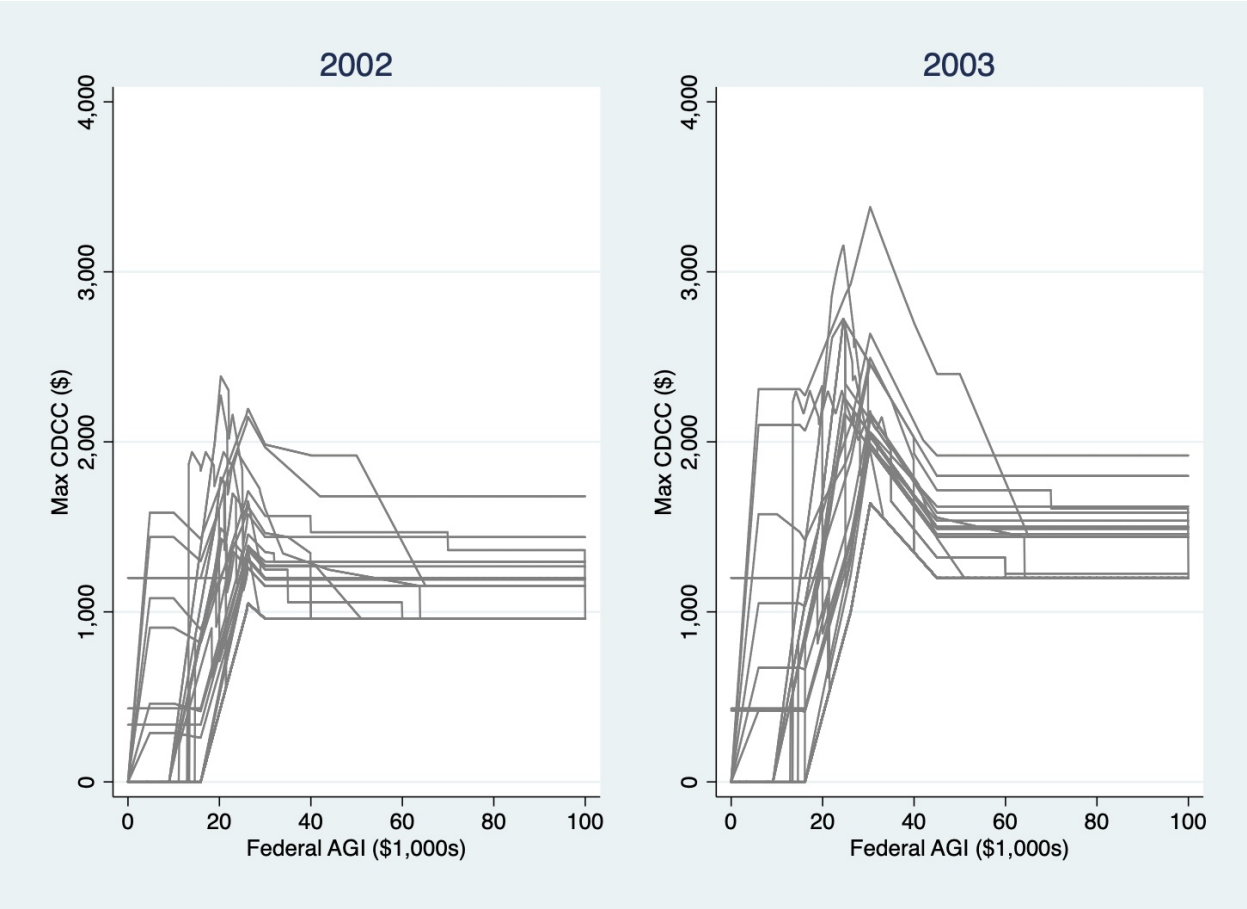
Table A21: Effect on Labor Force Participation among Married Mothers

	In the Labor Force
OLS	0.047***
	(0.0012)
1st Stage	0.232***
	(0.0393)
2nd Stage	0.056***
	(0.0171)
Mean	0.686
Observations	130,141

Effect of a 1-unit increase in the log of CDCC benefits on labor market outcomes among married mothers using the simulated log of CDCC benefits in a particular state, year, and demographic group as an instrument. Standard errors are clustered at the state level and listed in parentheses.
* Significant at the 10% level
** Significant at the 5% level
*** Significant at the 1% level

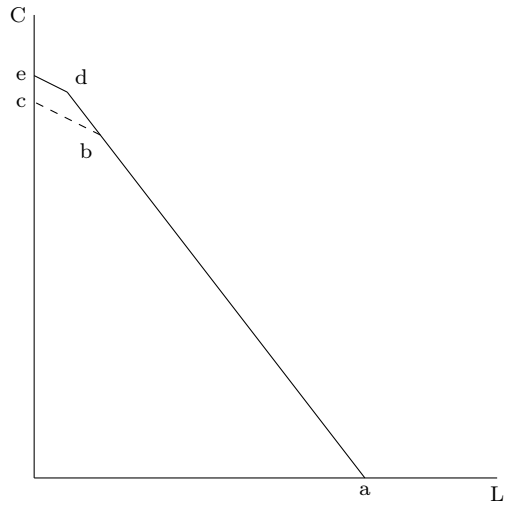
Figure A1: Maximum Effective Federal and State CDCC Benefits by Income

[H]



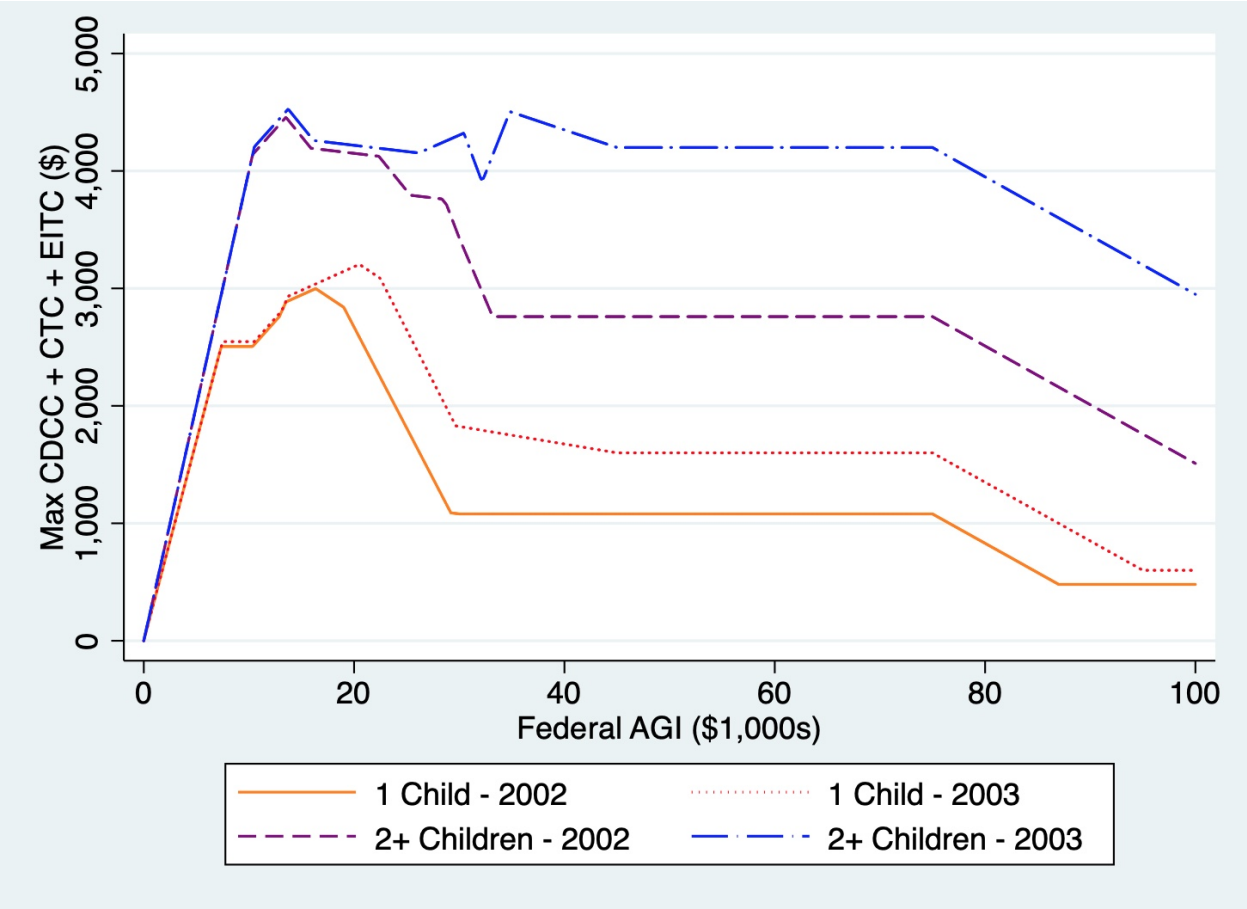
Maximum effective federal and state CDCC benefits among families with two or more eligible dependents by annual income as of 2002. Lines represent states. Information retrieved using state tax forms.

Figure A2: Budget Constraint for Single Earners with Refundable State and Nonrefundable Federal CDCC Benefits



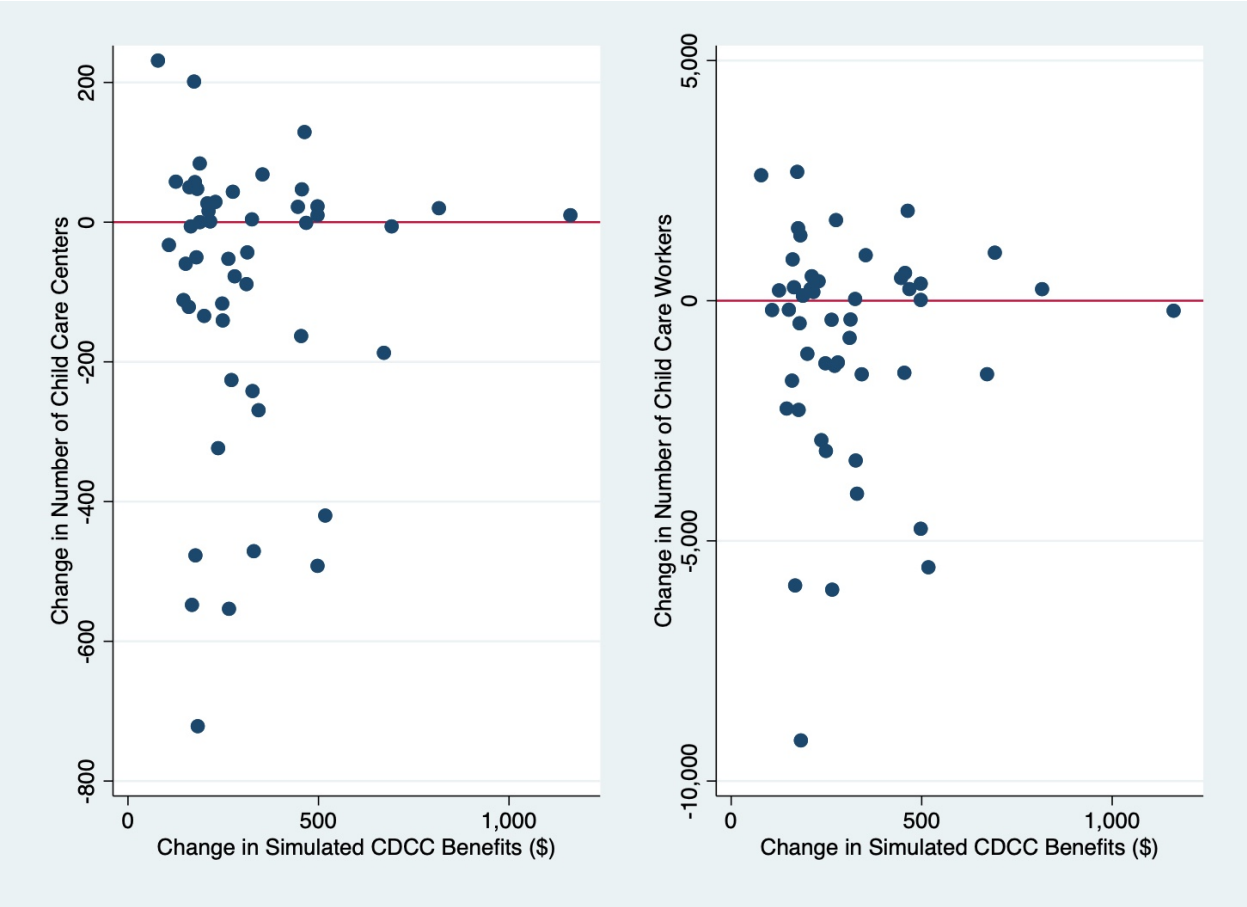
Budget constraint for single earners with refundable state and nonrefundable federal CDCC benefits. "C" and "L" denote consumption and leisure, respectively. The dashed line depicts the budget constraint before the federal CDCC expansion. The solid line depicts the budget constraint after the federal CDCC expansion.

Figure A3: Maximum Effective Total Federal CDCC, CTC, and EITC Benefits by AGI



Maximum effective total federal CDCC, CTC, and EITC benefits among families with one or two or more eligible dependents by annual income as of 2002. Information retrieved using NBER's TAXIM program (Feenberg 2017).

Figure A4: Correlations between Changes in Simulated Real CDCC Benefits and Numbers of Child Care Centers and Workers



Correlations between changes in simulated real CDCC benefits and numbers of child care centers and workers between 2003 through 2008 and 2000 through 2002. Information on child care centers and workers retrieved from United States Department of Labor, Bureau of Labor Statistics (2019).

Technical Efficiency in Michigan Property Tax Assessment

Yunni Deng

Abstract: Most existing literature on property assessment either focus on assessment quality or assessment expenditure. This research combines both assessment quality and expenditure to analyze the efficiency of property assessment of each Michigan individual township and city within the stochastic frontier production framework, and explores its potential determinants and their impacts on the efficiency of different types of assessors. Results indicate that private contractors are most effective assessing larger districts, although they tend to be hired by smaller assessing districts. County assessors are most cost-sensitive. In-house assessors are best when assessing high property value districts, relative to private contractors. The composition of parcels also affects assessment efficiencies: agriculture, commercial, and residential parcels are easier for assessors to assess than industrial property.

1. Introduction

Efficiency in the public administration system is a significant issue in the United States. For real property assessors, the main objective is to maximize property assessment performance given certain assessment expenditure, or to minimize property assessment expenditure while maintaining assessment performance.

Michigan Property tax assessment equalization expenditure reached over $100 million in 2017, as high as 1.2% of total property tax revenue collected.[1] One possible reason for Michigan's high property assessment expenditure is its highly decentralized property tax assessment process. In Michigan, property assessment is conducted annually at the township, city and village levels, with county equalization and state government oversight. Michigan has 83 counties, under which there are 1607 townships and cities. Each township and city is required to maintain its own assessing office and conduct independent property assessment by state statute. Michigan is among the ten states in the U.S. which require local (city, township, or municipal level) assessment, while most of the states only require county assessments and some states even conduct assessment on the state level (See Figure 1 in the Appendix). Moreover, Michigan requires annual reassessment, the most frequent required assessment cycle among U.S. states, and physical inspection of a minimum of 20 percent of property parcels in any given assessment year, which is not strictly practiced though (see Figure 2 and Table 1 in the Appendix for required assessment cycle and assessment authority level of each state).

However, it appears that the relatively high expenditures in Michigan has not paid off in terms of assessing performance. Property assessment performances in Michigan are unsatisfactory in both two primarily measured aspects of accuracy: level, which refers to the overall ratio at which properties are evaluated in relation to true market value; and uniformity, which refers to the degree to which properties are evaluated at equal percentages of market value. Assessment level

[1] Calculated from data collected.

in an assessing district is generally measured by median sales ratio (assessed value/sale price) of that district. A good assessment should have a median sales ratio close to 1, so that assessed value is the true reflection of market value, minimizing systematic underestimates or overestimates. The median sales ratio of Michigan local assessing districts in 2016 ranges from 0.02 to 4, with mean ratio of 0.94, implying a slight tendency of underestimation. Only 39% of assessing districts has median sales ratio falling between 0.9 to 1. (See Figure 3 in the Appendix for 2016 Frequency Distribution of Median Ratio). Assessment uniformity is usually measured by the coefficient of dispersion (COD); the average percentage deviation of individual sales ratios from the median ratio. The smaller the measure of dispersion, the greater the uniformity of the assessment. A good assessment should have a COD less than 30, according to IAAO (1990). Yet only 16% of Michigan assessing districts can pass this threshold. (See Figure 4 in the Appendix for 2016 Frequency Distribution of COD).

In this research, I measure and analyze technical efficiency in individual assessing units within the framework of a stochastic frontier production function and to explore the potential factors of technical efficiencies.

Moreover, I will analyze how the factors impact efficiencies of different types of assessors. Michigan communities may assess property assessment using three types of assessors: In-house assessors, private assessors, and county government assessors. Traditionally, local government units (townships and cities) are more intended to hire their own local assessors as government employee, with an implicit preference to have an assessor who is a part of the community. In order to make cuts to address budget shortfalls, many local assessing offices started to contract out the assessment service to for-profit companies or stand-alone private contractors to replace the previous kept in-house local assessors.[2] Several assessing offices may hire the same contractor as a way to share the assessment cost and achieve economies of scale. Those assessing offices without either in-house assessors or private contractors can request the county equalization department to perform assessments. Currently, hiring private contractors is most common. 70.36 percent (or 985 out of 1400) townships and cities hire private contractors, 4.07 percent (or 57 out of 1400) townships and cities seeks help from county assessors, and 25.57 percent (or 358 out of 1400) townships and cities keep in-house assessors[3].

It is natural to expect different types of assessors behave differently as they have different incentives and mindsets. For example, the first and foremost priority for private contractors might be to secure and renew their contracts. County assessors might care more about county equalization other than fair evaluation within townships and cities. Thus, it is worthwhile to examine the efficiency of each type of arrangement and to consider the conditions in which each type of arrangement is most effective. In this analysis I examine whether different types of assessors have their specific comparative advantages and are thus more suitable for different types of communities. I also consider whether current matching of different types of assessors with different types of communities represent an optimum allocation.

The research seeks to answer the following questions. Does increase assessment expenditure helps to improve assessment results? Which type of assessors has expertise in assessing

[2] D. Rowley, MAAO, Michigan Assessor Association Past President, personal communication, May 1, 2018
[3] Calculated from data collected in 2017.

expensive communities? How do different types of assessors respond to larger size of assessing districts? Does composition of property parcels affect assessment efficiencies? The answers have important policy implications. For example, if the decentralization of assessment does not enhance the assessing performance, then there would be no reason for keeping such an assessing system, considering the assessment's strong economies of scale.[4]

This paper is organized as follows. The relevant literature is reviewed in Section 2. Then, section 3 summarizes the concept of technical efficiency within the stochastic production frontier framework. Section 4 discusses the specification of the property tax technical efficiency and estimation methods. Then the data set is presented in section 5, the current distribution of different types of assessors is presented in section 6, and the empirical results are discussed in section 7.

2. Literature Review

Most studies of property tax assessment so far focus on one-dimensional (either output or input) measure of assessment performance. Among all the measures, COD receives the most attention. Bowman & Mikesell (1978), Sunderman, Birch, Cannaday, & Hamilton (1990) and hundreds of other prominent studies paid attention to assessment uniformity and examined factors that could affect COD measurements. On the other hand, some studies concern about assessment costs. For example, Netzer (1966), Stocker (1973), Sjoquist & Walker (1999) estimated the costs functions of performing property tax assessments and analyzed possible policies that could reduce assessment costs.

However, to date only one paper has related the performance of the property assessments to the assessment costs for the sake of evaluating assessment efficiency: Mehta & Giertz (1996) measured the efficiency of assessment of Illinois counties by incorporating CD (what Mehta & Giertz used to denote coefficient of dispersion) and resource cost using stochastic frontier analysis and ranked them. They found out that the rankings based on both output level and input level differ greatly from the rankings based only on output level, and there is virtually no relationship between those rankings. Based on the results, they justified the innovative methodology of calculating technical efficiency combining both output and input. Their result also suggests a possible optimum relationship between efficiency and district size in property tax administration, which advocates further examination of the policy recommendation to consolidate assessing districts.

This research utilizes the framework advocated by Mehta & Giertz (1996) and to analyze and rank the importance of various determinants of the efficiency for different types of assessors. For example, this research will determine whether efficiencies in property assessment would be impacted by assessing district size, and whether such impact would be similar among the three types of assessors.

This research will contribute to the existing literature on the privatization of property assessments as well. Just like other studies of property tax assessments, the studies of assessment

[4] Yunni Deng. Determinants of Property Assessment Costs in the State of Michigan. (Working Paper)

privatization focus either on its impact on either assessment quality or cost. For example, Stocker (1973) analyzed in-house property tax assessors and private contractors in Ohio, and found out that private assessments provided 50 percent cost savings. Mikesell (1987) analyzed the data of the state of Indiana and found that contract firms do not produce a technically inferior job relative to that produced by local assessors. To my knowledge, no research has analyzed the efficiencies of privatized assessment combining both quality and cost. The present research compares the efficiencies between private assessors, county assessors and in-house assessors, as well as analyzes their comparative advantages, such as which types of assessors are more skilled at assessing larger or more expensive communities, which adds to this literature.

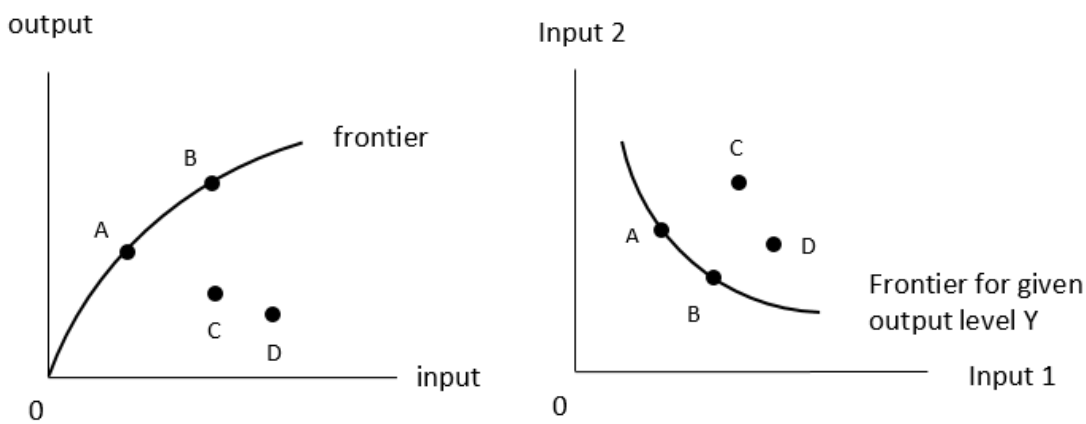
The present research also includes technical improvements to Mehta & Giertz (1996). Instead of using COD as the sole measurement of property assessment output as Mehta & Giertz did, I take both assessment level and assessment uniformity into consideration. More specifically, I will create a new performance measure, combining COD, the measurement of assessment uniformity, and median SR, the most widely used measurement of assessment level. The inclusion of assessment level measurement is particularly relevant in Michigan, as property assessment practices are more diverse in Michigan than in other states. Not only does Michigan contain jurisdictions where governments appear to systematically underestimate property values to avoid sudden tax increase and tailed aversion from taxpayers (which is almost an implicit rule nation-wide), but it also contains jurisdictions such as Detroit where the governments systematically overestimate real property to increase tax revenue intentionally (Hodge, McMillen, Sands and Skidmore, 2017). Furthermore, while Mehta & Giertz used cross-section data of 88 counties in Illinois from a single year, I use panel data from 2008 to 2016 for over 1000 local governments in Michigan. The substantial improvement of data volume and quality increases the reliability of this research. To evaluate these questions using this large panel data set, I use the Stochastic Frontier Model by Schmidt & Sickles (1984), which is essentially the same model used by Mehta & Giertz—Stochastic Frontier Model by Schmidt & Lovell (1979) but adapted for use with panel data.

Stochastic production frontier models allow to analyze technical efficiency against production frontiers—to compare actual output to the maximal amount of output obtainable from given inputs with fixed technology. The model was initially developed by Aigner, Lovell, and Schmidt (1976) and van den Broeck (1977), in an attempt to practically distinguish inefficiencies due to structural problems such as failing to utilize “best practice” technology or other factors which cause companies/organizations to produce below their maximum attainable output, and those due to random disturbances such as bad luck or systematic errors. The model was then extended by Aigner, Lovell, and Schmidt (1977), Battese and Corra (1977), Battese and Coelli (1988), Lee and Tyler (1978), Pitt and Lee (1981), Androw et al. (1982), Kalirajan and Flinn (1983), Bagi and Huang (1983), Schmidt and Sickles (1984), and Waldman (1984), etc. to allow adaption for various data complications. It is now one of the dominant models used in empirical industrial production studies, and has been used to study public production.

3. Technical Efficiency

Technical efficiency in the frontier framework is defined to be the effectiveness with which a given set of inputs is used to produce an output, with certain technology. We say technical

efficiency to be achieved when it is impossible for a firm or other producing institute to produce a larger amount of output with the same inputs or produce the same amount of output with fewer inputs. Similarly, technical inefficiency describes the situation where it is possible to produce more output from the same inputs or the same output from fewer inputs. The set of technical efficient firms or other producing units are called production frontier, and all technical inefficient units are bounded within the frontier. This framework can be described by the two graphs below. The left graph shows the case when there is one kind of inputs. Unit A and Unit B are on the production frontier, and thus technical efficient; while Unit C and Unit D are below the frontier, thus technical inefficient. The right graph shows the case when there are two kinds of inputs. Unit A and Unit B are on the production frontier, and thus technical efficient; while Unit C and Unit D are above the frontier, and technical inefficient. Note that the right graph only shows the frontier and producing units for a given level of output, and there are many different frontier curves and units representing other levels of output.



Stochastic Frontier Model models the output frontier to be a function of inputs, controls, and a symmetric error v_i as $N(0, \sigma_v^2)$, and the gap between actual output and maximum feasible output as a one-sided error $u_i \leq 0$, which comes from the concept that actual output is always smaller than or equal to maximum feasible output. To put in formula, the output frontier is structured as:

$$MY_{it} = \alpha + \sum \beta_j X_{ijt} + v_{it}; \;\; v_{it} \sim N(0, \sigma_v^2) \qquad (1)$$

Actual output function is the output frontier plus the one-sided error:

$$Y_{it} = \alpha + \sum \beta_j X_{ijt} + v_{it} + u_i; \quad u_i \leq 0, \quad E[u_i] = u < 0 \qquad (2)$$

where Y_{it} is the actual output of unit i at time t; X_{ijt} is the jth input or control variable of unit i at time t; MY_{it} is the maximum feasible output given the inputs and controls X_{ijt}; β_j is the coefficient of each input and control variable; v_{it} is the symmetric error term; and u_i is the one-sided error term. Formula (1) and (2) clearly show that the actual output of unit i is always within the frontier function as the error term $u_i \leq 0$.

There are two error terms in the output function, the symmetric error and the one-sided error, behind which is the economic logic that the production process is subject to two different

random disturbances: the one-sided error reflecting the deviation from best output due to technical or economic inefficiencies, which are usually under the unit's control; the symmetric error reflecting the fact that the frontier is stochastic due to random factors such as luck, climate, topography, which are usually not under the unit's control, and can be favorable or unfavorable.

In order to measure, compare and rank technical inefficiency, we can define technical inefficiency as $TI_i = 1 - \frac{Y_{it}}{MY_{it}}$. Apparently it will only take values from 0 to 1 inclusive, as output will take value from 0 to maximum output. TI being 0 means technical efficient as it only happens when actual output equals to maximum output feasible. Similarly, TI being 1 means most technical inefficient as it only happens when actual output equals to zero. Fixed effect analysis on the stochastic frontier model will yield efficient coefficients and give a TI score for each unit. I will elaborate on the process in latter sections.

4. Model Specification

As mentioned above, there are two primary aspects when it comes to property assessment accuracy appraisal: level and uniformity (IAAO 1990). Assessment level measures the degree to which goals or certain legal requirements are met, while uniformity measures the degree to which properties are appraised in relation to true market value. A generally preferred measure of overall appraisal level in an assessing district is median sales ratio (assessed value/sale price of each parcel) of that district, while uniformity is usually measured by the coefficient of dispersion (COD), which reflects the average percentage deviation of the ratios from the median ratio:

$$COD_{it} = 100 \frac{\sum_{h \in i} |SR_{ht} - SR_{median,it}|}{n_{it} SR_{median,it}} \qquad (3)$$

where COD_{it} is the COD of assessing district i in year t; and SR_{ht} is the sales ratio of parcel h (belonging to district i) in year t; $SR_{median,it}$ is the median sales ratio of district i in year t; and n_{it} is the number of parcels in district i in year t. Clearly, the closer median sales ratio to 1 and the closer COD to 0, the higher assessment quality is in one assessing district. The ideal assessment would have a median sales ratio of 1 and COD of 0, implying assessed value equals to market price for each parcel.

In order to evaluate and compare assessment quality for all Michigan local assessing districts in a more straight-forward way, I want to create a single measurement for assessment quality (not considering assessment costs yet), taking both assessment level and uniformity into account. The created measurement Performance score is calculated as:

$$P_{it} = \frac{100 \sum_{h \in i} |SR_{ht} - 1|}{n_{it}} \qquad (4)$$

where P_{it} is the assessment performance of assessing district i in year t. One may notice that performance is very similar to COD, only replacing the median sales ratio with the ideal sales ratio—1. So by analogy, performance measures the average percentage deviation of the ratios from the ideal sales ratio of 1.

After obtaining the performance score of each assessing district in Michigan, I then analyze the technical efficiency of Michigan assessing districts in the stochastic frontier framework, using performance score as the output, assessment cost per parcel or assessment cost in percent of total property tax collected as the input of assessment by each assessing district. In the context of property assessment, technical efficient units are defined to be the assessing districts maximizing the performance of property assessment for any given amount of assessing cost, and the performance frontier is defined to be the set of technical efficient units. To specify the performance frontier as the relationship between assessment inputs and outputs, we have

$$MP_{it} = \alpha + \beta_1 C_{it} + \beta_2 Con_{it} + v_{it};\ v_{it} \sim N(0, \sigma_v^2) \tag{5}$$

And actual performance is specified as performance frontier plus a negative disturbance:

$$P_{it} = \alpha + \beta_1 C_{it} + \beta_2 Con_{it} + v_{it} + u_i;\ \ u_i \leq 0,\ \ E[u_i] = u < 0 \tag{6}$$

where C_{it} is assessment cost per parcel of assessing district i in year t; Con_{it} is the control variables which represents the difficulty (or the workload) of the assessment of assessing district i in year t; v_{it} is the symmetric error term; and u_i is the negative error term.

I included a variety of variables to control for the difficulty or the workload for different assessing districts over time. The number of parcels is an important measurement for the assessment workload; the composition of the parcels matters because difficulty of assessing each class of parcels varies; the average property value also affects the difficulty of assessments. And then the controls are interacted with the assessor type dummies, private contract assessors, and county assessors, to analyze the difference in control's impacts for each type of assessors.

It is important to point out that u_i in equation (6) is homogeneous over time to assessing district i, reflecting the assumption that each assessing district has the same deviation from best performance due to technical inefficiencies over time. This assumption is necessary to make in order to differentiate the symmetric error term and the one-sided error term practically, and also has its economic justifications: the factors contributing to technical inefficiencies, such as insufficiently trained personnel and inadequate management system, is usually stable over time.

I define technical inefficiency of assessing district i to be the ratio of the difference between the average actual performance of that district and maximum feasible performance given its average actual assessing cost and controls to its maximum feasible performance. To be more specific,

$$TI_i = \frac{u_i}{MP(\overline{C_i}, \overline{X_i})} \tag{7}$$

Where TI_i stands for technical inefficiency of assessing unit i; $MP(\overline{C_i}, \overline{X_i})$ is the maximum feasible performance given $\overline{C_i}$, the average assessing cost of assessing unit i, and $\overline{X_i}$, the average level of controls of unit i.

To estimate technical inefficiency, we let $\alpha^* = \alpha + u, u_i{}^* = u_i - u,$ and we add u and subtract u from the right side of equation (6) to get:

$$P_{it} = \alpha + u + \beta_1 C_{it} + \beta_2 Con_{it} + v_{it} + u_i - u; \quad E[u_i] = u < 0 \qquad (8)$$

Or,

$$P_{it} = \alpha^* + \beta_1 C_{it} + \beta_2 Con_{it} + v_{it} + u_i{}^*; \qquad (9)$$

where both v_{it} and $u_i{}^*$ are iid, and $E[v_{it}] = E[u_i{}^*] = 0$. Then we let $\alpha_i = \alpha + u_i = \alpha^* + u_i{}^*$, then equation (9) becomes:

$$P_{it} = \alpha_i + \beta_1 C_{it} + \beta_2 Con_{it} + v_{it}; \qquad (10)$$

The within estimator of equation (10) estimates a separate intercept for every assessing district $\widehat{\alpha_i^*}$, i=1,…,N. Let $\widehat{\alpha^*} = \max(\widehat{\alpha_i^*})$, and $\widehat{u_i} = \widehat{\alpha^*} - \widehat{\alpha_i^*}$, then $\widehat{u_i}$ is a consistent estimator of u_i, and the consistency of the within estimator does not hinge on uncorrelatedness of the regressor and the individual effects, or the distribution of the individual effects. According to Slutsky's theorem, $\widehat{TI_i} = \frac{\widehat{u_i}}{P_{it} - \widehat{u_i}}$ is also a consistency estimator of TI_i.

5. Data

Output measurements, including performance, COD, median sales ratio, are independently calculated from individual parcel tax record and current sales data from CoreLogic. Only parcels transacted in the current year are considered in the calculation. For example, 1854 out of 43181 parcels in Lansing have been involved in transactions in 2016 (after trimming out non-arm's-length transactions, errors, and outliers), then those 1854 individual sales ratios are calculated, and then used to calculate performance, COD, and median sales ratio for Lansing in 2016. To make sure all the individual sales ratio included in the calculation are correctly recorded arm's-length, open-market transfers, the following trimming method is utilized: all individual transactions with transaction type other than "resale" and "new construction" are precluded in the calculation, as they are not standard open-market transactions, including non-arm's-length transactions, construction loan/financing, seller carrybacks, and thus their sales prices are not proper reflections of their market values; all individual transactions with sales price smaller than $1,000 or sales ratio greater than 20 are deemed as errors or outliers and precluded in the calculation. Since Michigan assesses property values at 50% of market value, the sales ratios are calculated as two times assessed value divided by market value.

In the literature of property assessment, the cost is usually measured by either assessment cost per parcel or assessment cost as a percentage of tax revenue. The two measurements both have advantages and disadvantages. Cost as a percentage of tax revenue is highly dependent on the property tax rate, which is left to the discretion of each local government and ranges from 9.3 mil to 96.6 mil.[5] In contrast, cost per parcel, although affected by inflation in panel data settings, better reflects the assessment input of each township and city. This research uses cost per parcel as the input measure, and also does a robust analysis with the alternative measure cost as a percentage of tax revenue for the purpose of confirming the results. Assessment cost per parcel is obtained by dividing total assessment costs by total property parcel counts, and assessment cost

[5] 2016 data in the dataset.

as a percentage of tax revenue is self-explanatory. Total assessment costs of each county, township, and city are obtained from the item "assessing equalization" in the Annual Financial Report (F-65). Parcel numbers of the whole state, each county, and each assessing unit (township and city) are published by Michigan Department of Treasury in L-4023 Forms annually. Total tax revenues of each county, township, and city are obtained from Ad Valorem Property Tax Report published by Department of Treasury annually, reflecting the sum of county taxes, township/city taxes, school taxes, and authority taxes if applicable.

The sizes of assessing districts are measured by the total parcel numbers. The property compositions of each assessing units are measured by the valuation of each class of parcels, including agriculture, commercial, industrial, and residential parcels, as a percentage of total parcel valuation.

Each Michigan county equalization department publishes the name of its equalization director, and each township/city publishes the name of the township/city assessor and the company he/she belongs to (if applicable). For those governments with no or underdeveloped official website, their equalization departments are contacted via phone calls and emails to obtain the assessor name list. With the assessor name list, I created the assessor employment type dummy of each township and city using the following method: if a township or city assessor is the county director or specified as "county equalization department", I deem that the assessor employment type is "county equalization assessor" for this government unit; if a township or city assessor is an employee of a private professional property assessment company, I deem that this township or city uses a "private contractor" as its assessor; if a township or city assessor is an individual whose name appears in other government unit(s), I also deem that this township or city uses a "private contractor" as assessor; I deem a township or city using "in-house assessor" if it hires a unique assessor who is not county equalization director. Except in rare cases where individual private contractor assessors only have one contract from one local government unit, this method should provide a good approximate of assessor employment type for local government units: in-house assessor, private contractor, or county equalization.

Only assessing units (townships and cities) with more than 30 correctly recorded arm's-length, open-market transfers are included in the regression, because calculating median, COD, performance from individual sales ratio is not statistically meaningful for a sample smaller than 10. Such trimming might potentially create bias because the number of transaction is highly correlated with the total parcel number in an assessing unit, and this tends to preclude the smaller assessing units. However, it is necessary to preclude the unrepresentative observations which would create more problems. If an assessing unit has more than 30 such transfers in some years, but not in other years, then only the observations in the years with more than 30 transfers are kept. This trimming method creates unbalanced panel data, but it prevents excessive or unnecessary trimming. In 2016, 762 of 1607 Michigan townships and cities used in the regression analysis. A detailed summary of statistics is provided in Table 2 in the Appendix.

6. **Distribution of Private vs. In-house vs. County**

As mentioned earlier, hiring private contractors is the current trend: 70.36 percent (985 of 1400) townships and cities hire private contractors, 4.07 percent (57 of 1400) townships and cities

seeks help from county assessors, and 25.57 percent (or 358 out of 1400) townships and cities keep in-house assessors[6].

Simple Probit regressions tells us that private contractors are more concentrated in smaller and inexpensive assessing districts. The Probit analyzes the impact of size and average value of assessing districts on the likelihood of hiring each type of assessors, and the result is shown in Table 3 in the Appendix. [7] A one percent increase in district size will bring down the probability of hiring a private contractor by 0.35, a one percent increase in average property value will bring down the probability of hiring a private contractor by 0.2. A one percent increase in district size will increase the probability of using a county assessor by 0.17, a one percent increase in average property value will increase the probability of using a county assessor by 0.36.

The rationale behind such distribution of assessors seems straightforward. Smaller assessing districts usually have a smaller budget on property assessments and higher fixed costs per parcel, and have a greater incentive to contract out for assessment services as a method of saving money. The same argument holds for less valuable assessing districts as well because they are more likely to collect less revenue.

However, such distribution might not be an optimum allocation of different types of assessors, as they might not be assessing the districts they are good at. The following section will talk about this in detail.

7. **Results and Discussion**

7.1 Overall Analysis

The specification model is first estimated for the overall Michigan, and the result is presented in Table 4 in the Appendix. The first two columns show the result of the original analysis with assessment cost per parcel as the input measurement and without eliminating the outliers. The first column shows coefficients and the second column shows the standard errors. The third and fourth columns show the results from the robust analysis with assessment cost in percent of property tax collected as the input measurement. Then it follows the results from original and robust analysis with outliers eliminated.

Overall, there is a statistically significant negative relationship between assessment cost per parcel and performance scores. To put this result in numeric term, a one percent increase in assessment cost per parcel will translate into an average 3-point deduction in assessment performance scores, or a decrease of average percentage deviation of the actual sales ratios in an assessing district from the ideal sales ratio 1 by 3. (Note that performance scores are lower for better assessments.) This confirms the intuitive speculation that more input on property assessment will bring better output.

Parcel composition also affects assessment efficiencies. Michigan groups its properties into different classifications: agricultural, commercial, industrial, residential, developmental, and

[6] Calculated from data collected in 2017.

[7] The Probit analysis uses 2016 data and does not exclude the observations with transactions less than 30.

timber cutover. This research studies the four main classes: agricultural, commercial, industrial, and residential. The result implies that agriculture, commercial, and residential parcels are easier for assessors to assess than industrial assessors, and the performance scores are statistically significantly lower when the parcels are more concentrated in these three classes. More precisely, a one percent increase of agriculture, industrial, or residential parcels composition is approximately associated with a 6-point reduction in assessment performance scores on average. This might be due to assessors' intensive experience in handling the assessments of these classes of properties.

Surprisingly, the result indicates a slight negative relationship between transaction frequencies and assessment efficiencies. Intuitively, more current property transactions can provide assessors with more information on the property market, and thus enhance assessment quality. However, the evaluation suggests that Michigan assessors did not fully take advantages of the information provided by real property sales. A possible explanation is that assessors might deliberately ignore the current sales in order to overestimate or underestimate assessments.

The overall analysis does not find interesting results for assessing district size and average property value, and the

7.2 Sensitivity Analysis for Assessor Types

As I expect that different types of assessors behave differently, it is helpful to analyze how assessor's efficiency response to different factors based on assessors' types. So I estimate to model with interactive terms of different factors with assessor type dummies. The result is shown in Table 5 in the Appendix. Again, both original analysis and robust analysis are done with and without trimming outliers.

All four analyses point to the same consistent result that in-house assessors, private contract assessors, and county assessors respond very differently to changes in assessment costs, average property values, and assessing district sizes.

County assessors appear to be much more cost-sensitive than private or in-house assessors. The performance scores of both private and in-house assessors are not significantly related to either assessment cost per parcel or assessment cost in percent of property tax collected, implying that their performances are insensitive to expenditure changes. In contrast, there is a statistically significant negative relationship between assessment cost per parcel and performance scores for county assessors, and the relationship is meaningful in terms of magnitude. A one percent increase in assessment cost per parcel translates into an average (52.43+1.36=) 54-point reduction in assessment performance scores.

The size of assessing districts also matters, but not for all assessor type. For in-house assessors, property assessment quality tends to be lower in bigger assessing districts, all else equal. More precisely, a one percent increase in assessing district size increases performance scores by 8.50-10.07 on average. County assessors are not statistically different from in-house assessors in this sense. But this unfavorableness towards larger size does not extend to private contract assessors. In fact, for them there is a slight positive relationship between size and performance quality. A

one percent increase in assessing district size will reduce the performance score for private contractors by (10.08-8.64=) 1.64, on average. A possible explanation for unfavorableness towards larger size might be that there is often greater variation in the composition of parcels in larger communities to assess. Assessors are generally more skilled in assessing markets with more homogeneous parcels, and thus larger community size usually makes assessment more difficult. Ironically, private contractors are best in assessing larger districts, although they tend to be hired by smaller assessing districts. Here is one possible explanation for this result. Private contractors or assessing companies might put much more emphasis on contracts with larger assessing districts, and conduct the assessments with more care with an intention to renew their contracts when the existing contracts end. Such emphasis offsets and even outweighs the difficulty brought by the large scale of assessments.

This result provides some rationale for Michigan's decentralized assessment scheme, as it leads to an increase in assessment quality for both in-house and county assessors. However, over 70 percent of assessing districts hire private assessors, who perform even better in larger assessing districts. And the overall analysis shows that number of parcels are not a significant factor for assessment efficiency. Considering the expenses incurred in maintaining 1607 assessing offices, with the smallest township only contains 55 parcels,[8] decentralization seems pointless as it does not add on efficiency. Moreover, when properties are segregated into many small districts, it is quite unlikely to keep assessment practices consistent among all districts, which increases the potential for unfair equalization within Michigan. Given that a large number of states in the U.S. assess real properties in a more centralized fashion effectively, it is natural to recommend Michigan to merge small assessing districts to form larger units and hire private contractors or companies for their property assessments.

Average property values affects property assessment efficiency differently for different kinds of assessors as well. In-house assessors assess more efficiently in assessing districts with larger average property value—a one percent increase in average property value leads to a 9.74-point reduction in assessment performance scores on average. Private contractors are less sensitive to average property values—a one percent increase in average property value only leads to a (9.74-8.65=) 1.09-point reduction on average. Original analysis indicates that county assessors respond in the opposite direction—a one percent increase in average property value leads to a (23.93-8.65=) 15.28-point increase in performance score on average. However, such difference is not confirmed by the robust analyses. The result implies that in-house assessors relatively more effective at assessing higher property value districts relative to private contractors. One probability is that contractors might have more incentive to please property owners in rich communities in order to securer their contract in the future, and thus deliberately underestimate their properties. Or maybe contract assessors are less familiar with highly valued and unique properties and may have less information about them, as they rotate often between different communities.

While these finding offer a number of policy implications, I acknowledge some limitations to this analysis. As discussed in the data section, eliminating all communities with fewer than 30 parcel sales may introduce selection bias. Moreover, due to data limitation, there are some

[8] Township with 55 parcels is eliminated from the dataset because the number of sales were less than 30 in all the years, and thus not shown in table 3.

potential factors of assessment efficiency not included in this research, such as assessors' certification level, and degree of local economic development of each assessing district. Looking forward, one possibility is to extend this research with a more comprehensive dataset.

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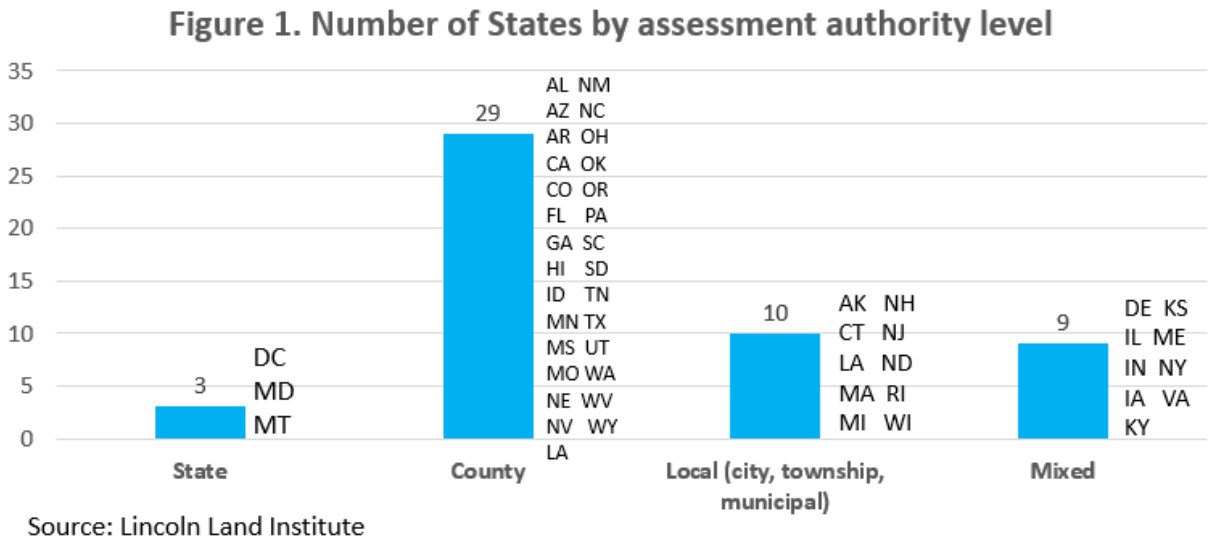
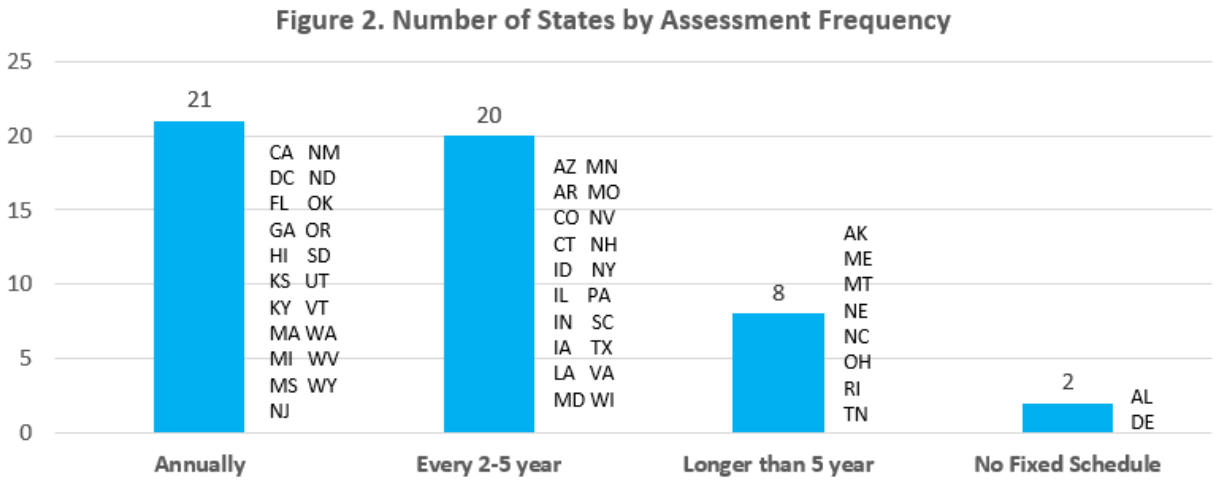
2. Appendix

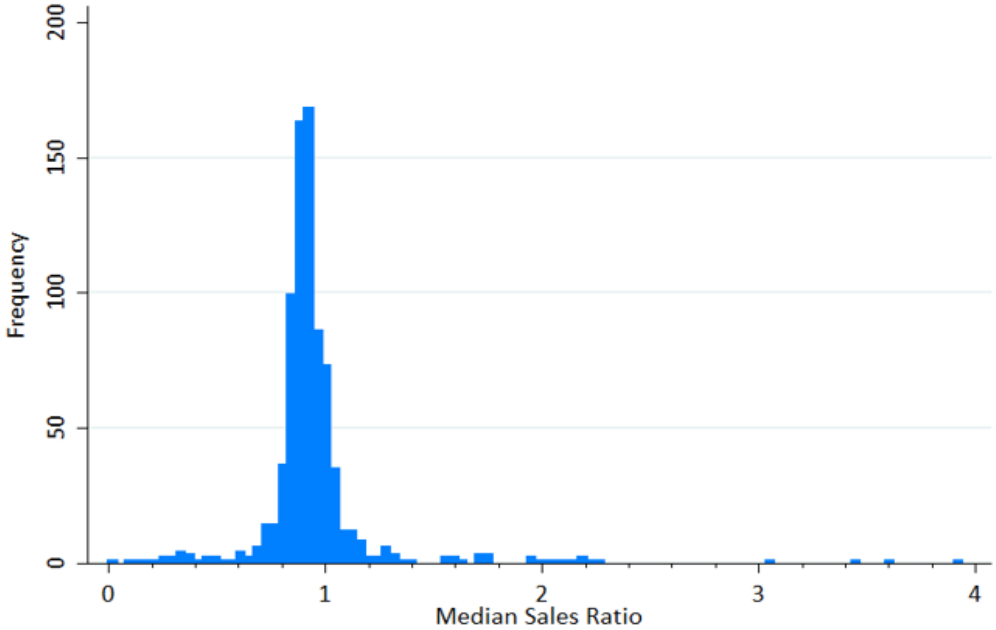


Source: Lincoln Land Institute



Source: Lincoln Land Institute

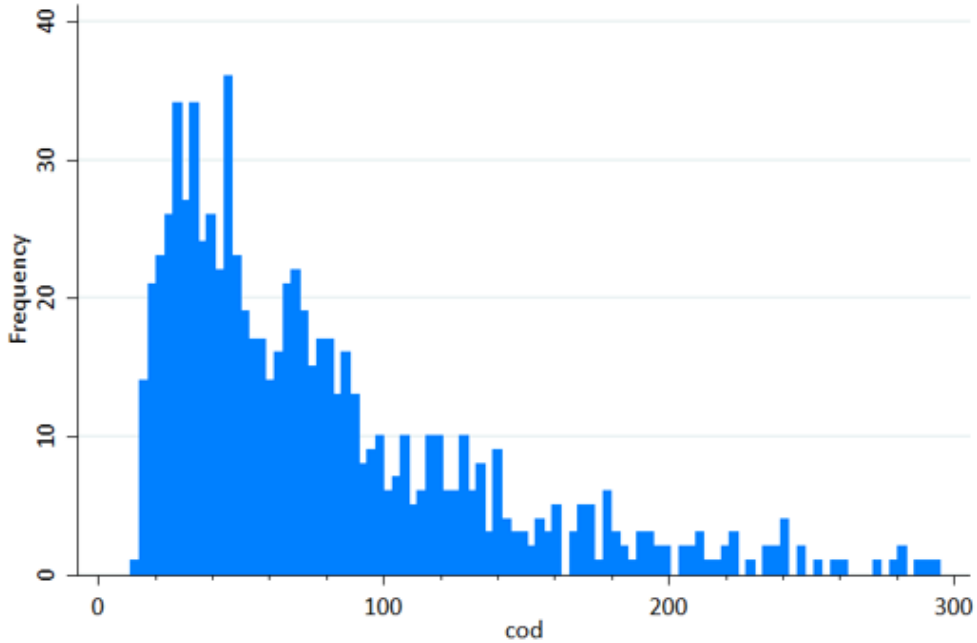
Figure 3. Michigan Frequency Distribution of Median Ratio - 2016



Source: Calculated

Note: Assessing units with transactions account less than 30 are eliminated.

Figure 4 Michigan Frequency Distribution of COD - 2016



Source: Calculated

Note: Units with transactions account less than 30 are trimmed, COD bigger than 300 are not shown.

Table 1. Assessment Cycle and Other Requirements of States in U.S.

State	Revaluation Cycle	Level of Assessing Districts
Alabama	No fixed schedule	county
Alaska	No fixed schedule, properties are revalued on locally determined cycles not to exceed 6 years.	borough and municipal
Arizona	Not to exceed 3 years For property classified as class 2, 3, or 4 property. Agricultural property an inspection is required every 4 years.	county
Arkansas	Each county must reappraise all real property every 3 or 5 years,	county
California	Most real property is revalued upon a change in ownership or upon completion of new construction. State assessed and personal property are assessed annually.	county
Colorado	Every 2 years	county
Connecticut	Assessors must revalue property at least once every 5 years. Inspections are required once during every 10 assessment years.	cities and towns
Delaware	No fixed schedule	county/local(city, own, municipality)
District of Columbia	Every year	the District
Florida	Every year	county
Georgia	Every year	county
Hawaii	Every year	county
Idaho	Property must be revaluated at least once every 5 years.	county
Illinois	Property other than farmland must be viewed, inspected, and revalued once every 4 years. Farmland is reassessed each year.	Counties and townships
Indiana	Every 5 years	Counties and townships
Iowa	Every 2 years	County or city
Kansas	Every year	county or appraisal district
Kentucky	Every year	counties, school districts, and special districts
Louisiana	Real property - no more than 4 years. Personal property - every year.	parish
Maine	at least once every 10 years.	municipality/multi-municipal area
Maryland	Every 3 years	state
Massachusetts	Each city and town must value property each year. The Department of Revenue reviews local assessing practices every 3 years.	Cities and towns
Michigan	Every year	townships, villages, and cities, and counties
Minnesota	Every 5 years	county
Mississippi	Every year	county
Missouri	Every 2 years	counties and the City of St. Louis.
Montana	Every 6 years	Department of Revenue
Nebraska	Every 6 years	county
Nevada	Every 5 years	counties and the City of Carson City
New Hampshire	At least once every 5 years, the general court may order more frequent revaluations.	Municipalities/ Cooperative Assessment Districts
New Jersey	Every year	Municipal Tax Assessor's Offices and County Tax Boards
New Mexico	Every year	county
New York	No fixed schedule. At least once every 4 years.	Counties, cities, and towns
North Carolina	At least once every 8 years, but may elect to revalue more frequently.	county
North Dakota	Every year	city and township
Ohio	At least once every 6 years.	county
Oklahoma	Every year	county
Oregon	Every year	county
Pennsylvania	Every 4 years	county/city
Rhode Island	Every city or town must conduct a revaluation within 9 years of the date of the prior revaluation and must conduct an update of real property every 3 years from the date of the last revaluation.	Cities and towns
South Carolina	Every 5 years	county
South Dakota	Every year	county
Tennessee	6-year, 5-year or 4-year inspection cycle	county
Texas	at least once every three years.	county/ multiple county appraisal district/ home-rule city
Utah	Every year	county
Vermont	Every year	municipality and town
Virginia	Every 2 years in cities, every 4 years in counties, every 4 years in towns.	Counties, cities, and towns
Washington	Every year	county
West Virginia	Every year	county
Wisconsin	At least once every 5 years	any subdivision of territory
Wyoming	Every year	

Source: Lincoln Land Institute

Table 2. Summary of Statistics

		Performance				COD				Median Sales Ratio			
Year	Obs.	Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max
2016	762	89.0	92.6	16.7	844	91.1	97.9	14.4	1653	0.94	0.29	0.03	3.94
2015	747	77.7	72.8	14.1	761	75.1	51.8	10.4	283	0.96	0.32	0.19	5.14
2014	691	81.6	90.8	18.0	1159	77.0	55.1	12.6	393	0.98	0.55	0.09	10.17
2013	660	89.9	92.7	15.5	1220	83.2	77.4	13.4	894	1.05	0.57	0.08	10.89
2012	631	103.5	103.4	16.1	993	83.0	77.4	16.1	1542	1.19	0.64	0.04	9.20
2011	541	113.4	105.8	18.8	986	80.8	58.5	16.9	698	1.29	0.62	0.11	7.03
2010	496	112.7	103.3	19.3	1133	76.0	50.2	17.7	378	1.34	0.77	0.20	9.48
2009	434	138.9	129.0	20.8	1234	78.6	56.6	17.3	456	1.54	0.92	0.22	10.68
2008	326	124.0	138.2	18.2	880	88.6	234.2	11.9	3850	1.42	0.86	0.00	7.84
		Assessment Cost per Parcel				**Cost (percent of Tax Revenue)**				**Number of Parcels**			
		Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max
2016		17.5	9.1	0.1	73.6	1.05	0.56	0.01	6.91	4599	6700	683	66245
2015		17.1	11.7	0.0	185.2	1.03	0.55	0.00	4.32	4701	6619	710	67165
2014		16.5	9.5	0.1	87.5	1.01	0.52	0.00	3.67	4962	6904	717	67395
2013		20.8	35.1	0.1	389.2	1.01	0.52	0.00	4.12	4900	6955	129	67421
2012		16.6	10.4	0.1	126.6	1.05	0.60	0.00	5.64	5273	7200	717	67459
2011		16.7	9.9	0.1	84.8	1.02	0.58	0.01	5.05	5892	7778	776	67581
2010		17.8	11.9	0.2	123.7	1.02	0.57	0.01	4.68	6116	7890	726	68107
2009		17.9	11.0	0.2	90.3	0.95	0.58	0.01	5.60	6712	8322	651	68165
2008		19.1	12.1	0.2	113.3	0.93	0.70	0.01	8.76	7775	9234	166	68075
		Number of Transactions				**Total Tax Rate (‰)**				**Average Parcel Assessed Value**			
		Mean	Std. Dev.	Min 1/	Max	Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max
2016		183	322	30	4196	31.6	9.9	16.0	96.6	78207	46025	18458	695846
2015		165	283	30	3401	30.9	9.2	15.9	74.8	73268	36285	9473	335233
2014		153	251	30	2748	30.6	9.2	15.9	82.2	70912	40980	10441	660082
2013		146	243	30	2549	30.2	9.1	15.6	81.7	86757	121451	9847	1035049
2012		138	238	30	2838	30.3	9.2	16.3	85.6	67175	31843	9703	283632
2011		128	195	30	1667	30.2	9.0	11.1	78.1	70487	32186	9544	298398
2010		131	204	30	1802	30.3	8.8	15.7	77.5	74550	33527	15775	306417
2009		135	210	30	1887	30.0	8.8	15.5	73.2	84025	39282	18998	453901
2008		139	193	30	1661	31.0	9.0	15.3	69.2	92576	42496	19402	357048

1/ Townships and cities with number of sales less than 30 are eliminated from the dataset.

Table 3. Results of Probit Analysis

	Coefficients	Std. Err.
In-House Assessors (dummy)		
Number of Parcels (log)	0.32**	0.04
Average Assessed Value (log)	0.11	0.07
Private Contractors (dummy)		
Number of Parcels (log)	-0.35**	0.04
Average Assessed Value (log)	-0.20**	0.07
County Assessors (dummy)		
Number of Parcels (log)	0.17**	0.06
Average Assessed Value (log)	0.36**	0.12

* indicates statistically significance with 0.1 level, ** indicates statistically significance with 0.05 level.

Table 4. Coefficients of Assessment Cost and Other Factors of Assessment Efficiency

	Original Analysis		Robust Analysis		Original Analysis (w/o outliers) 1/		Robust Analysis (w/o outliers) 1/	
Performance Score	Coefficients	Std. Err.	Coefficients	Std. Err.	Coefficients	Std. Err.	Coefficients	Std. Err.
Assessment Cost per Parcel (log)	-3.00*	1.73			-2.98*	1.73		
Assessment Cost (in percent of property tax collected)			0.33	1.73			0.35	1.68
Average Assessed Value (log)	-5.87	4.53	-7.48*	4.45	-4.58	4.45	-6.17	4.34
Number of Parcels (log)	2.94	3.50	3.32	3.49	3.54	3.49	3.92	3.39
Agriculture Parcels (in percent of Total Parcels)	-5.96*	3.20	-6.07*	3.20	-6.40**	3.20	-6.51**	3.11
Commercial Parcels (in percent of Total Parcels)	-6.36**	3.21	-6.53**	3.21	-6.77**	3.21	-6.95**	3.12
Industrial Parcels (in percent of Total Parcels)	-4.13	3.23	-4.28	3.23	-4.56	3.23	-4.71	3.13
Residential Parcels (in percent of Total Parcels)	-5.69*	3.21	-5.83*	3.21	-6.15**	3.21	-6.29**	3.11
Number of Transactions (in percent of total parcels)	1.24**	0.16	1.23**	0.16	1.24**	0.16	1.24**	0.16
Number of Observations	5288		5288		5274		5274	
Number of Cross Sections	959		959		958		958	

* indicates statistically significance with 0.1 level, ** indicates statistically significance with 0.05 level.

Assessing districts with number of transactions less than 30 are eliminated.

1/ observations with COD>500 are deemed as outliers.

Table 5. Coefficients of Factors for Different Types of Assessors

	Original Analysis		Robust Analysis		Original Analysis (w/o outliers) 1/		Robust Analysis (w/o outliers) 1/	
Performance Score	Coefficients	Std. Err.	Coefficients	Std. Err.	Coefficients	Std. Err.	Coefficients	Std. Err.
Assessment Cost per Parcel (log)	-1.36	2.78			-1.55	2.71		
Assessment Cost (in percent of property tax collected)			3.27	2.83			3.00	2.75
Number of Parcels (log)	8.64**	4.30	8.5**	4.28	10.07**	4.17	9.98**	4.16
Average Assessed Value (log)	-9.74*	5.14	-11.34**	4.98	-8.82*	5.00	-10.49**	4.85
Agriculture Parcels (in percent of Total Parcels)	-6.11*	3.20	-6.20*	3.20	-6.57**	3.11	-6.66**	3.11
Commercial Parcels (in percent of Total Parcels)	-6.34**	3.22	-6.53**	3.21	-6.76**	3.12	-6.93**	3.12
Industrial Parcels (in percent of Total Parcels)	-4.12	3.23	-4.28	3.23	-4.56	3.14	-4.72	3.14
Residential Parcels (in percent of Total Parcels)	-5.70*	3.21	-5.84*	3.21	-6.17**	3.12	-6.30**	3.11
Number of Transactions (in percent of total parcels)	1.23**	0.16	1.23**	0.16	1.24**	0.16	1.24**	0.16
Interactive Terms 2/								
Interactive with County Assessor (dummy)								
Assessment Cost per Parcel * County Assessor	-52.43**	16.76			-52.41**	16.30		
Assessment Cost (to property tax collected) * County Assessor			-26.82*	16.26			-25.49	15.81
Number of Parcels * County Assessor	-13.44	9.06	-2.13	8.72	-13.95	8.80	-2.80	8.48
Average Assessed Value * County Assessor	23.93**	9.02	1.70	6.55	24.47**	8.77	2.38	6.37
Interactive with Contract Assessor (dummy)								
Assessment Cost per Parcel * Contract Assessor	-1.73	3.52			-1.39	3.43		
Assessment Cost (to property tax collected) * Contract Assessor			-4.21	3.55			-3.73	3.45
Number of Parcels * Contract Assessor	-10.08**	5.16	-9.68**	5.11	-11.44**	5.01	-11.11**	4.97
Average Assessed Value * Contract Assessor	8.65**	3.95	7.85**	3.72	9.73**	3.84	9.07**	3.62
Number of Observations	5288		5288		5274		5274	
Number of Cross Sections	959		959		958		958	

* indicates statistically significance with 0.1 level, ** indicates statistically significance with 0.05 level.

Assessing districts with number of transactions less than 30 are eliminated.

1/ observations with COD>500 are deemed as outliers.

2/ controls in the interactive terms are of the same unit with the controls themselves, such as in logs, in percent.