UC Irvine
UC Irvine Electronic Theses and Dissertations

Title
Essays on Policy Incentives and Labor Economics

Permalink
https://escholarship.org/uc/item/3vs7b4t1

Author
Williams, Katherine Ellyn

Publication Date
2016

Peer reviewed|Thesis/dissertation

eScholarship.org Powered by the California Digital Library
University of California

UNIVERSITY OF CALIFORNIA,
IRVINE

Essays on Policy Incentives and Labor Economics

DISSERTATION

submitted in partial satisfaction of the requirements
for the degree of

DOCTOR OF PHILOSOPHY

in Economics

by

Katherine Williams

Dissertation Committee:
Professor David Neumark, Chair
Professor Marianne Bitler
Assistant Professor Damon Clark

2016

© 2016 Katherine Williams

DEDICATION

To my parents, Brad and Nancy Williams, for their endless love, patience, support, and encouragement.

TABLE OF CONTENTS

	Page
LIST OF FIGURES	iv
LIST OF TABLES	vi
ACKNOWLEDGMENTS	viii
CURRICULUM VITAE	ix
ABSTRACT OF THE DISSERTATION	xi
CHAPTER 1: Teacher Early Retirement Incentives and the Effect on Student Performance	1
CHAPTER 2: Child Evaluating the "Cliff Effect" and Female Labor Supply: Evidence from Child Care Subsidies	46
CHAPTER 3: Do State Earned Income Tax Credits Increase Program Participation at the Federal Level?	92
REFERENCES	135
APPENDIX CHAPTER 1	141
APPENDICES CHAPTER 2	148
APPENDIX CHAPTER 3	167

LIST OF FIGURES

		Page
Figure 1.1	CA Enrollment, FTE Teachers, and Pupil-Teacher Ratio	29
Figure 1.2	Annual Teacher Retirements by Years of Service at Retirement	29
Figure 1.3	Share of Districts in Sample Offering an ERI, by Year	30
Figure 1.4	Number of LEAs Participating in CalSTRS' "Golden Handshake" Program	30
Figure 1.5	Event Study: Enrollment and Pupil-FTE Teacher Ratio	31
Figure 1.6	Event Study: Teacher Experience (30+, 25-29, 20-24, 15-9)	32
Figure 1.7	Event Study: Teacher Experience (1, 2, <5) and Total Number of Teachers	33
Figure 1.8	Event Study: Number of Teachers with 30+ Years of Experience, by Experience in District	34
Figure 1.9	Event Study: CST English-Language Arts Test Scores	34
Figure 1.10	Event Study: CST Math Test Scores	35
Figure 1.11	Event Study: CST Science Test Scores	35
Figure 1.12	Event Study: CST History and CST US History Test Scores	36
Figure 1.13	Event Study: Number of FTE Teachers with 20+ Years of Experience, by Grade	37
Figure 2.1	Child Care Subsidy Budget Set	73
Figure 2.2	Average Maximum Income Threshold per HHS Guideline	73
Figure 2.3	Average Maximum Income Threshold for Family Size = 3, by Year and State	74
Figure 2.4	Histogram of Hours, Single Mothers	75
Figure 2.5	Average Weighted Simulated % Eligible, Single Mothers	75
Figure 2.6	Average Simulated % Eligible, Single Mothers	76

Figure 2.7	Average Maximum Income Amongst Eligible Families	76
Figure 3.1	Federal and State EITC Parameters, 2008	119
Figure 3.2	Average EITC Supplement (Fraction of Federal EITC), 1997-2008	119
Figure 3.3	Average EITC Supplement (Fraction of Federal Credit), by State and Year	120
Figure 3.4	Number of State EITCs and Federal EITC Filers per Potentially Eligible Population, 1997-2008	121
Figure 3.5	Federal and State EITCs and the Budget Line	121
Figure 3.6	State EITC Induces Labor Market Entry	122
Figure 3.7	State EITC Decreases Hours Worked	122

LIST OF TABLES

		Page
Table 1.1	Summary Statistics	38
Table 1.2	Effect of ERIs on Average Years Experience and Number of FTE Teachers	39
Table 1.3	Effect of ERIs on Total # of FTE Teachers with Varying Levels of Experience	40
Table 1.4	Effect of Multiple ERIs on Total # of FTE Teachers with Varying Levels of Experience	41
Table 1.5	Effect of ERIs on CST English-Language Arts Test Scores	42
Table 1.6	Effect of ERIs on CST Mathematics, CST Algebra 1, and CST Geometry Test Scores	43
Table 1.7	Effect of ERIs on CST Science Test Scores	44
Table 1.8	Effect of ERIs on CST History-Social Science and CST U.S. History Test Scores	45
Table 2.1	Summary of State Child Care Policies, January 2009 - October 2012	77
Table 2.2	Single Mothers Summary Statistics, January 2009-October 2012	78
Table 2.3	Married Mothers Summary Statistics, January 2009-October 2012	79
Table 2.4	Effect of Simulated % Eligible (Weighted) on Single Mother Labor Supply, Longitudinal Panel Weights	80
Table 2.5	Effect of Simulated % Eligible on Single Mother Labor Supply, Longitudinal Panel Weights	82
Table 2.6	Effect of Simulated % Eligible (Weighted) on Married Mother Labor Supply, Longitudinal Panel Weights	84
Table 2.7	Effect of Simulated % Eligible on Married Mother Labor Supply, Longitudinal Panel Weights	86
Table 2.8	Effect of Simulated % Eligible (Weighted) on Single Mother Labor Supply, Wave 1 Person Weights	88

Table 2.9	Effect of Simulated % Eligible on Single Mother Labor Supply, Wave 1 Person Weights	90
Table 3.1	Summary Statistics, 1997-2008	123
Table 3.2	Estimated EITC Effects on Employment, Single and Married Women, Aged 21-44, 1997-2008	124
Table 3.3	Estimated EITC Effects on Employment, Single Individuals with Children, Aged 21-44, 1997-2008	125
Table 3.4	Estimated EITC Effects on Employment, Single Women with Children, Aged 21-44, 1997-2008	126
Table 3.5	Estimated EITC Effects on Employment, Married Individuals with Children, Aged 21-44, 1997-2008	127
Table 3.6	: Estimated EITC Effects on Employment, Married Women with Children, Aged 21-44, 1997-2008	128
Table 3.7	Estimated EITC Effects on Employment, Childless Individuals, Aged 21-34, 1997-2008	129
Table 3.8a/b	Estimated State EITC Effects on Federal EITC Participation, Single Filers with Children, 1997-2008	130
Table 3.9a/b	Estimated State EITC Effects on Federal EITC Filers, Married Filers with Children, 1997-2008	132
Table 3.10	Estimated State EITC Effects on Federal EITC Participation and Expenditures, Childless Filers, 1997-2008	134

ACKNOWLEDGMENTS

I would like to express the deepest appreciation to my committee members, Professor David Neumark, Professor Marianne Bitler, and Assistant Professor Damon Clark, for their guidance on my dissertation and assistance throughout my graduate studies. Specifically, I would like to thank David Neumark for helping to inspire my interest in labor economics since my undergraduate studies and for his thoughtful insight and dedication to my academic and professional development. I cannot thank him enough for his support and for always motivating me to improve my knowledge and work.

I am thankful to Marianne Bitler for her invaluable guidance on my first two papers, which has improved the quality of my research in all aspects. Her positive support has helped me to stay motivated throughout my graduate career. Additionally, I would like to acknowledge Damon Clark for his thoughtful feedback and for helping to improve my writing.

I am very grateful to my parents (Brad and Nancy Williams), my sister (Mari Gottlieb), and brother-in-law (Dave Gottlieb). Without their support and encouragement, I would not be where I am today.

Finally, I would like to thank my classmates and friends. I am especially grateful to Michael Gou, Paul Lombardi, and Sierra Moussatche for helping me to get through the many late nights of studying and for the thoughtful discussions. I am thankful to Alli Tung for always being so considerate and for putting up with me and Luna. And I'd like to thank Gerardo Franco, for his support and for making sure I'm still having fun.

Summer funding was provided by the Department of Economics, School of Social Sciences, University of California Irvine.

CURRICULUM VITAE

Katherine Williams

Education

Ph.D. Economics, University of California, Irvine, 2016

M.A. Economics, University of California, Irvine, 2013

B.A. Business Economics, *cum laude*, University of California, Irvine, 2011

Research Interests

Labor Economics, Public Economics, Economics of Education, Applied Microeconomics

Research Experience

Research Assistant, Professor David Neumark, Economics (2014)

Research Assistant, Professor Marianne Bitler, Economics (2014)

Undergraduate Research Assistant, Professor Gary Richardson, Economics (2010-11)

Teaching Experience

Teaching Assistant, University of California, Irvine (2011-2016)

Courses: Computer Based Research in the Social Sciences, Basic Economics I, Basic Economics II, Intermediate Economics I, Intermediate Economics II, Applied Econometrics I, Applied Econometrics II, Labor Economics & Human Resources, Economics of Education, Probability & Statistics for Economics I

Non-Academic Experience

Economic Consultant, Chicago Lawyers' Committee (2015)

Economic Consultant, Capitol Matrix Consulting (2014 and 2015)

Communications and Social Media, Center for Economics & Public Policy, University of California, Irvine (2014-2016)

Representative, Graduate Student Council, University of California, Irvine (2015-2016)

Grants and Awards

Summer Research Fellowship, University of California, Irvine (2015)

Borowski Fellow, Center for Economics & Public Policy, University of California, Irvine (2014-2015)

Summer Research Fellowship, University of California, Irvine (2014)

Seminars and Presentations

Department of Economics Graduate Poster Session, University of California, Irvine (November 2015)

Applied Microeconomics Seminar, University of California, Irvine (October 2015)

Center for Economics and Public Policy, University of California, Irvine (October 2015)

All-California Labor Economics Conference, Poster Session, University of California, Irvine (September 2015)

Applied Microeconomics Seminar, University of California, Irvine (June 2015)

Applied Microeconomics Seminar, University of California, Irvine (January 2015)

Department of Economics Graduate Poster Session, University of California, Irvine (October 2014)

Computer Skills

Stata, MATLAB, EViews, Excel

Professional Affiliations

American Economic Association

ABSTRACT OF THE DISSERTATION

Essays on Policy Incentives and Labor Economics

By

Katherine Williams

Doctor of Philosophy in Economics

University of California, Irvine, 2016

Professor David Neumark, Chair

My dissertation broadly examines how individuals respond to incentives imbedded in various policy designs. I study a variety of policies, including teacher retirement incentives, child care subsidies, and the Earned Income Tax Credit. The evidence presented here is of importance to policy analysis and design.

In the first chapter, I examine what types of teachers respond to early retirement incentives (ERIs). In recent years, many education programs have been faced with steep budget cuts. In response to these budget shortfalls, many school districts have turned to the use of ERIs to induce higher cost, but highly experienced teachers to retire. A key question is how these incentives affect students. Using a newly assembled panel dataset of school district ERI policies in California. I employ a difference-in-differences strategy and find that after districts offered retirement incentives, student test scores improved. These results suggest that less-effective, but highly experienced teachers respond the most to the retirement incentives.

The second chapter examines whether child care subsidy policies, which are intended to provide work-related support for low income families, can actually discourage

work. Specifically, I examine how the sharp phase-out of subsidy benefits creates an incentive for parents near the maximum income eligibility limits to lower their labor supply in order to qualify for the subsidy. I exploit recent changes in income eligibility thresholds across different states and years and construct an exogenous simulated measure of eligibility. I find that an increase in subsidy generosity, primarily driven by an increase in maximum income eligibility thresholds, has a net negative effect on hours and earnings for single mothers.

Finally, the third chapter, which is joint work with Professor David Neumark, studies whether state supplemental Earned Income Tax Credits (EITCs) encourage participation in the federal EITC. The EITC provides refundable tax credit for working families with low to moderate income. Existing research has linked the EITC to many positive labor supply and welfare outcomes for low to moderate income families. States and local governments should be interested in maximizing participation of their constituents in the federal EITC not only because of these anti-poverty effects, but because of other economic benefits, such as an increase federal tax dollars being spent by EITC recipients. We use recent state EITC policy variation during a period when there were no major changes in the federal EITC to explore whether state EITCs actually influence participation in the federal program. We find evidence that state EITCs encourage federal participation for single filers with children.

CHAPTER 1

Teacher Early Retirement Incentives and the Effect on Student Performance

Introduction

Since the onset of the Great Recession, education programs across the United States have faced steep budget cuts, resulting in widespread reductions in staffing levels. In response to financial problems and rising layoffs, many states and school districts sought alternative methods to achieve cost savings. One option that has gained popularity across the United States is the use of financial incentives to encourage early retirement (Kowalski & Sweetland, 2005). Senior-level teachers comprise an expensive portion of school district budgets, with nearly 30 percent of teachers in the United States over the age of 50 in 2012 (National Center for Education Statistics, 2012). Early retirement incentives (ERIs) enable schools to reduce the number of more experienced and higher paid staff, resulting in salary savings, and thus, fewer layoffs. Proponents of ERI programs argue that ERIs not only reduce costs and allow school districts to avoid reductions-in-force, but they also provide opportunities for schools to restructure education programs and encourage ineffective teachers to retire (Auriemma, Cooper, & Smith, 1992; Kowalski & Sweetland, 2005). Yet, despite the growing popularity of ERIs, little is known about their effect on students.

In this paper, I study the impact of ERIs on student performance in California. California offers an interesting setting for studying the effects of ERIs in part due to its laws related to layoffs and ERI implementation. During the Great Recession, school districts in California experienced extensive budget cuts, leading to mass reductions-in-force. As of 2013, California is one of 11 states that require school districts to follow a "last in, first out"

(LIFO) policy for teacher layoffs, letting go of teachers with the least amount of experience.[1] Yet, not all districts reduced their workforce through LIFO layoffs. About 30% of all California school districts provided extra financial incentives from 2009 to 2011 to encourage teachers to retire early (Legislative Analyst Office, 2012). As a result, instead of laying off only the least experienced teachers, some school districts removed their most experienced teachers through ERIs.

Using a newly assembled panel dataset of ERI usage by school districts in California and data from the California Department of Education, I explore the effects of school districts removing their most experienced teachers on student achievement. I exploit within-state variation in the timing of ERI introduction using a difference-in-differences specification and find that California school districts that offered ERIs reduced their numbers of highly experienced teachers. I find that offering an ERI generally had a positive impact on California Standards Test (CST) scores, particularly among high school students. Furthermore, to explore the exogeneity of ERI adoption, I use event-study analyses to show the absence of pre-trends in my outcome variables and to show changes in test scores after the introduction of ERIs.

As discussed in more detail below, this paper builds on two strands of the existing literature. In the teacher experience and quality literature, most studies find a positive relationship between teacher experience and effectiveness, but the benefits of experience level off after three to five years (Harris & Sass, 2011; Rivkin, Hanushek, & Kain, 2005; Rockoff, 2004). If on average, the most experienced teachers are more productive than the

[1] Junior employees can be "skipped" for a number of exceptions including, but not limited to: special education credential, language specialization credential, specialized training in high-need programs, math/sciences specialization credential, or equal protection laws.

least experienced teachers, then offering an ERI may result in declining test scores. Yet, if teacher experience does not matter after a few years, or if only lower quality teachers respond to retirement incentives, student test scores may not change, or even increase. Therefore, a key question is whether removing the least experienced teachers through LIFO layoffs or the most experienced teachers through ERIs has a positive or negative effect on student performance. I find evidence suggesting that the highly experienced teachers responding to retirement incentives are generally less effective.

In the teacher retirement literature, only one existing study evaluates the effects of ERIs on student performance. Fitzpatrick and Lovenheim (2014) examine the effect of an ERI program offered to all teachers in the state of Illinois in the mid-1990s on student achievement. The authors exploit differences in the pre-treatment experience composition and find the ERI program had small, non-negative effects on math and English test scores, with positive effects most pronounced among disadvantaged schools. Although my paper builds on this work, there are several important differences. First, instead of examining an ERI offered broadly at the state level, this study focuses on school districts in California, which are able to decide individually whether to offer these retirement incentives. As a result, there is potentially greater treatment variation across more homogenous geographical regions, allowing for a more precise estimate of the impact of ERIs on student achievement. Second, I am able to examine the effect of ERIs on more test subjects and grades than the previous literature. Finally, California's LIFO layoff policy and the recent financial crisis allows me to explore the effects of removing highly experienced older teachers instead of only removing less experienced younger teachers. Previous work has not examined the effects of ERIs offered during a period of high layoffs.

Overall, I find evidence that ERIs improved test scores. Although effects varied across subjects and grades, the improvements were on the order of 0.1 standard deviations in school-grade test scores for high school students, a relatively large effect. This paper provides new evidence suggesting that lower-quality but higher-experienced teachers are responding to these retirement incentives, resulting in positive impacts on student achievement. Thus, a potential benefit of incentivizing voluntary early retirement is "positive" self-selection out of the workforce.

Literature Review

There is extensive literature examining how teacher turnover or teacher quality affect student outcomes (Chetty, Friedman, & Rockoff, 2011; Loeb, Darling-Hammond, & Luczak, 2005; Ronfeldt, Loeb, & Wyckoff, 2012), but few studies examine the impact of retirements on students. Within the teacher retirement literature, existing work largely focuses on the structure of pension plans and the effect on teacher retirement decisions (Brown, 2012; Costrell and Podgursky, 2008; Friedberg and Turner, 2011; Furgeson, Strauss, and Vogt, 2006). These studies generally show that teachers respond to financial incentives to retire, but do not evaluate the impact on students.

The first study examining the effects of ERIs on student achievement is a recent paper by Fitzpatrick and Lovenheim (2014). The authors evaluate the impact of large-scale teacher retirements driven by an early retirement incentive program offered in Illinois in the mid-1990s. In the 1992-93 and 1993-94 school years, Illinois offered a temporary ERI called "the 5+5" that allowed employees to purchase an extra five years of age and experience to be counted as creditable service for calculating their retirement benefit. The program resulted in a threefold increase in retirements. The authors use administrative

and school-level data from 1990-1997 on 3rd, 6th, and 8th grade standardized math and English test scores and a difference-in-differences specification to evaluate the impact of ERIs on student outcomes. They find the ERI program did not reduce test scores. Their point estimates are positive, and in some cases statistically significant. The authors find effects to be more pronounced in low supplemental education system (SES) and lower-performing schools.

Due to data limitations, the authors cannot observe which teachers take up the ERI or teacher ages. Therefore, they create a measure of treatment intensity using the pre-treatment average number of teachers in a given grade with at least 15 years of experience. The authors compare changes in test scores when the ERI is introduced across school-grades with fewer or more potentially affected teachers. By fixing experience levels using pre-treatment data, they avoid any endogeneity issues created by teachers potentially responding to the ERI for reasons related to student achievement. Their main identifying assumption is that trends in student test scores among schools with fewer experienced teachers are an accurate counterfactual for schools with more experienced teachers.

Contrary to previous studies, Fitzpatrick and Lovenheim's results suggest that teachers with high experience are less productive. However, the authors' study is limited by the fact that the ERI program that they evaluate was offered to all of the teachers in the state. The authors cannot clearly attribute their results to the implementation of the ERI since all schools received treatment. Instead, the authors compare test scores in school-grades with different pre-treatment experience levels. It is harder to maintain the common trends assumption when evidence suggests that urban and low income schools have lower quality and inexperienced teachers, and that teachers tend to move to higher achieving and

higher income schools as they gain experience (Lankford, Loeb, & Wyckoff, 2002; Boyd, Grossman, Lankford, Loeb, & Wyckoff, 2009). This suggests that the authors are comparing trends in lower versus higher income schools. If an ERI led to an increase in retirements in high income schools, the best teachers from the low income schools could move to the higher income schools, violating the common trends assumption.

While my findings generally support Fitzpatrick and Lovenheim's results, my analysis differs in several ways. I study the effects of ERIs offered at the district level in California. As a result, I have greater treatment variation within state, allowing for more precise estimates. My analysis includes data on test scores for grades two through 11 and for additional test subjects (Algebra 1, Geometry, Science, History, and U.S. History). I consistently find positive and significant effects, especially among higher grades that are not included in Fitzpatrick and Lovenheim's data.[2] Finally, I examine the effects of ERIs offered in a different environment. ERI programs in the 1990s were primarily used to reduce the growing number of costly senior-level teachers (Auriemma, Cooper, & Smith, 1992). Illinois' mid-1990s program was created in response to its aging teacher workforce, and the state was not undergoing layoffs. I study the effects of increased ERI usage by school districts in California during a difficult financial climate and period of high layoffs. I find evidence suggesting that lower quality teachers respond the most when districts offer ERIs, with positive effects on student performance.

California Teachers and Early Retirement Incentives

[2] Unfortunately, I cannot directly compare my estimates to Fitzpatrick and Lovenheim's (2014) estimates due to data differences. The 2014 authors interpret their results as the effect of having one more experienced teacher pre-ERI on school-grade standardized test scores, and the authors informally convert their estimates to be the effect per exiting teacher. I cannot easily compare my estimates since I am unable to determine the effect of ERIs on those who retired. However, similar to the authors, I find positive effects.

California public school teachers are covered by a defined benefit retirement plan, provided by CalSTRS, the largest teachers' retirement fund in the United States. Participation is mandatory for all full-time teachers in California public schools. The system is funded by active member contributions, employing school districts, investment earnings, and the State of California's General Fund. Under CalSTRS rules, teachers are eligible for retirement at age 55 with at least five years of service credit, or at age 50 with at least 30 years of service credit (CalSTRS 2012b). The average retirement age for teachers in California is about 61 years.

In California, individual school districts are able to offer ERIs to encourage teachers to retire earlier than they would have retired in the absence of the incentives. In order to offer a retirement incentive, school districts must be able to demonstrate cost savings, usually through a reduction in salary expenses. These incentives generally include cash bonuses, the availability of retirement benefits at a lower age, or other benefits such as health insurance continuing after employment ends (Tarter & McCarthy, 1989). To be eligible for an ERI, teachers must already meet CalSTRS' age and service eligibility requirements for retirement. California school districts can offer ERIs either through CalSTRS' Early Retirement Incentive Program or through private financial agencies.

Through CalSTRS' Early Retirement Incentive Program, also referred to as the "Golden Handshake", eligible employees receive two additional years of service credit for their retirement benefit calculation. School districts are responsible for the benefit costs, paid to CalSTRS. While the "Golden Handshake" program has existed in some form in California since the late 1970's, AB1207 made the incentive granting 2 years of service credit permanent in 2003 (CalSTRS 2012b). Additionally, a new, temporary "2+2"

program ending January 1, 2005, allowed school districts (that pay the benefit costs) to add two additional years of service credit, plus two years to the age factor used to calculate the retirement benefit. The "Golden Handshake" and more generous "2+2" program encourage eligible teachers to retire earlier by increasing the factors used to calculate an individual's retirement benefit.

Alternatively, school districts can offer ERIs administered by agencies outside of CalSTRS. These incentives provide supplemental benefits to CalSTRS retirement benefits in lieu of "Golden Handshake" deals, and are often called supplemental early retirement plans (SERP).[3] They are administered by private agencies such as the Public Agency Retirement Services (PARS) or other financial services firms. These types of SERP incentives are often preferred by school districts because they allow more flexibility in plan design, benefit level, and funding.

Although "Golden Handshakes" have been available for several decades, most school districts did not negotiate early retirement incentives until faced with recent budget cuts and they most often offered ERIs through private agencies. Since the Great Recession, school districts in the California have faced substantial budget reductions. Despite receiving one-time federal aid of about $7.3 billion between 2009 and 2010, real expenditures per average daily attendance declined by about 10% between 2007-08 and 2011-12 (California Department of Education, School Fiscal Services Division, 2013). To overcome budget shortfalls, many school districts reduced staffing levels. As shown in figure 1, the size of California's teacher workforce has declined by more than 10% over this

[3] For example, Los Angeles Unified School District offered a retirement incentive bonus of 40% of final year salary to certificated teachers who retired at the end of the 2008-09 academic year. The benefit could be paid in the form of an annuity with lifetime monthly payments or fixed term monthly payments ranging from 5 to 15 years.

same time period, while enrollment has remained relatively steady.[4] While many school districts were forced to reduce their workforce through LIFO layoffs, about 30% of California school districts implemented ERIs in 2009-2011 to encourage teachers to retire early (Legislative Analyst Office, 2012).[5]

Figure 2 shows the annual number of teacher retirements in California by years of service at retirement. There is an increase in retirements in 2003-04 following the "2+2" incentive. There is also a spike in retirements after 2007, peaking in 2009-10. The spike in retirements explains a portion of the decline in the teacher workforce and coincides with the timing of budget cuts and increased ERI usage by districts in my sample. Figure 3 displays the share of districts in my sample offering an ERI, by year. School districts in my sample increasingly offered ERIs in the years following the Great Recession.

As ERI usage increased during this period, the number of school districts offering ERIs through CalSTRS declined. Figure 4 displays the number of local educational agencies (LEAs), consisting of districts and county offices of education, that participated in CalSTRS' Early Retirement Incentive Program for years 2001-2011. The number of participating LEAs peaks at 84 in 2003-04, corresponding with the "2+2" offer, then declines. There are approximately 950 LEAs in California, suggesting that only a small percentage of California school districts participate in the "Golden Handshake" deal.

This evidence implies that California's public school teaching workforce declined following the Great Recession and ensuing budget cuts. Retirement numbers rose as ERI usage in California increased, and these ERIs were increasingly offered through agencies

[4] The California Department of Education does not collect data on layoffs, but only collects data on the number of teachers in the state.
[5] The LAO survey data provides interesting insight into the growing popularity of ERI usage in California. However, the LAO does not release disaggregated survey respond data to the public.

outside of CalSTRS.[6] Despite the growing popularity of ERI usage in California, little has been done to study the impact on student achievement. Using data described in detail below, I examine the effects of increased ERI usage in California during a period of high layoffs.

Data

Data for this study come from a variety of sources. I constructed a dataset on ERIs offered by the 55 largest California public school districts for the academic years 2003-04 to 2011-12, accounting for approximately 45% of total student enrollment in California. I matched these data to school-grade California Standards Test (CST) scores for English, Mathematics, Algebra I, Geometry, Life Sciences, History, and U.S. History, for the academic years 2004-05 to 2012-13, available from the California Department of Education (CDE).[7] I combined these data with additional district, school, and grade-level demographic data for both students and teachers, also released by the CDE.

District Early Retirement Incentive Offerings

To evaluate the effect of ERIs in California, I first collected data on ERI usage by the 55 largest school districts in California for academic years 2003-04 to 2011-12.[8] Primarily, ERI information came from district audited financial reports. However, not all school districts clearly reported their ERIs, and many districts do not release their audited financial reports for all of the years in my sample. Alternatively, I collected data on ERI usage through district board meeting minutes and district and teaching association

[6] Only three districts in my sample offered ERIs through CalSTRS.
[7] Charter schools and private schools are excluded from the sample because not all data are available.
[8] Although data were collected for the 55 largest school districts, only 54 are included in the final sample since one school district opened in 2008 (Twin Rivers School District).

websites. ERI information could also be found in contracts with teaching unions and through news reports.[9]

While I was able to determine whether a district offered an ERI, I was not always able to determine the exact terms of the incentive, such as the amount offered. Most districts announced that they were offering an ERI as a way to cut costs and minimize layoffs. As discussed previously, figure 3 shows the share of districts in my sample offering an ERI, by year. Prior to 2007, few districts in my sample offered ERIs. However, after the recession, the share of districts offering ERIs in my sample increases, peaking in 2009-10 with almost half of the districts in my sample offering an ERI. Table 1 displays summary statistics for my sample. There are eight school districts that offered no ERIs during my sample period, 28 school districts that offered one ERI, and 18 school districts that offered multiple ERIs.

Table A.1 in the Appendix also reports summary statistics for the first year in my sample (2004-05), and the peak ERI usage year (2008-09). These summary statistics are reported for the full sample, for the districts that offered at least one ERI, and for districts that never offered an ERI. Generally, the means of the demographic variables in districts that offered ERIs were similar to the means of the demographic variables in districts that never offered ERIs.

Test Scores and Demographic Data

[9] I cross-checked the gathered data with annual district-level financial data collected from the CDE standardized account code structure (SACS) unaudited actual data for fiscal years 2004-2011. If a school district offered an ERI, their accounting data should reflect an increase in expenditures coded under Object code 3901 "Other Benefits, Certificated Positions" and unrestricted resources. Object code 3901 does not uniquely identify ERIs, so I also verified my ERI data with other sources. I checked audited financial reports from the Public Agency Retirement Services (PARS), a main outside contractor for ERIs. I also compared my ERI data to data on total number of teachers with high levels of experience in each district.

My remaining data come from multiple datasets available from the CDE. Student performance is measured by California Standardized Testing and Reporting (STAR) CST test scores for years 2004-05 to 2012-13. The STAR program administers standardized tests every spring to all California public school students in grades two through 11, with the program ending in July 2013. The test subjects used in this analysis are the CSTs for English-Language Arts, Mathematics, Algebra 1, Geometry, Life Sciences, History-Social Science, and U.S. History. All students grades two through 11 take the CST English-Language Arts test. Students in grades two through seven take the CST Mathematics test, and then can take other math-related subject tests beginning in grade eight. The CST Science tests are taken in grades five, eight, and ten.[10] The CST History-Social Science test is taken in grade eight and the CST U.S. History test is taken in grade 11. Test score data are reported as mean test scores for each school-grade for each year, which I scale to have a mean of zero and standard deviation of one in each year, grade, and test subject to reduce possible biases due to changes in the exam over time. I exclude schools that have no valid test scores, charter schools, and private schools from my sample.

Information on all public schools and districts in California, including district types, names, and location are available from the CDE's public schools database (2015). Student demographic data were collected from a variety of datasets provided by the CDE, including English Language Learners (ELL) by school-grade and language for academic years 2004-05 to 2012-13, school-grade level enrollment by racial/ethnic designation for academic years 2004-05 to 2012-13, and school level percent free and reduced price meals for academic years 2004-05 to 2012-13.

[10] Grades 8 and 10 were added beginning in spring 2006.

I use data from the CDE on school staff demographics and teaching assignments, which includes information on individual teachers within each school and year. The data are not a panel, so there are no consistent identifiers that can be used to match teachers over time. The CDE staff data includes information on total years of teaching service (experience), total years of service within a district (creditable service), gender, credentials, subjects and grades taught, and more.

The data do not include information on age or retirement, but I use data on years of experience as a proxy for age. Since I know the total numbers of teachers with various levels of experience within each school and year, I can see how the composition of teachers with different levels of experience changes over time at each school. All of my analyses focus on full-time equivalent (FTE) teachers only.[11]

I am interested in the increased number of ERIs offered by districts during a period of high layoffs (and for which test score data is available), so my analysis focuses on the academic years 2004-05 through 2012-13.

Empirical Framework

I analyze the impact of ERIs on student achievement in California during the period 2004-05 to 2012-13 using panel data and a difference-in-differences specification. I exploit the variation in average STAR subject test scores between school-grades that are in districts that offered ERIs and school-grades in districts that did not offer an ERI. Additional variation comes from school districts offering ERIs at different times during my sample period. The key identifying assumption in my analysis is that school-grades in

[11] Full-time equivalent (FTE) is defined as the percentage of time a staff member works. If a teacher was reported to have multiple assignments, I considered them to be a FTE if the sum of their assignment FTE percentages was greater or equal to one.

districts that offered ERIs, had they not been treated (no ERI offered), would have similar trends in test scores as other school-grades in districts without ERIs. This assumption is conditional on school-grade fixed effects and a variety of time-varying controls. My basic specification is of the following form:

$$Y^i_{gsdt} = c + \beta_1 ERI_{dt-1} + \boldsymbol{X}_{gsdt}\delta + \gamma_{sg} + \rho_{gt} + \mu^i_{gsdt} \quad (1)$$

Y^i_{gsdt} is the standardized average test score for subject i in grade g in school s within district d and year t. ERI_{dt-1} is a dummy variable equal to zero if no ERI had ever been offered in the district, and equal to one in the year the district offered the first ERI, and every following year. The ERI variable is lagged because nearly all ERIs resulted in teachers retiring at the end of the school year. Thus, the impact on student test scores is unlikely to occur until the following academic year.[12]

$\boldsymbol{X}_{gsdt}$ is a vector of grade by school by year control variables, including percent English learners, percent black, percent Hispanic, percent Asian, and a quadratic in school-grade-year enrollment. I control for enrollment to account for possible trends in enrollment. I also control for school-year percent free and reduced price meal. My specification includes both school-by-grade (γ_{sg}) and grade-by-year (ρ_{gt}) fixed effects. School-by-grade fixed effects control for unobservable characteristics of grades within the same schools.[13] Grade-by-year fixed effects control for annual unobservable characteristics specific to each grade level which may affect California student outcomes.

[12] Although not shown, estimates for regressions with the ERI variable(s) not lagged are of the same sign, but smaller in magnitude and generally statistically insignificant.

[13] An alternative specification includes district-by-grade or district fixed effects to better control for unobserved heterogeneity at the district level that may affect test scores and my treatment variable. However, these specifications do not adequately control for omitted school-grade characteristics that are more likely to affect school-grade test scores and my other explanatory variables than district unobservables. Additionally, using school-by-grade fixed effects controls for district fixed effects, since within school-grade variation is not subject to district fixed effects (assuming schools and school-grades are not leaving or

Regressions are weighted by school-grade enrollment to represent the effect for an average student. To account for within district serial correlation of errors, I cluster my standard errors at the district level for all of my regressions. There are 11,969 school-grades, 2,922 schools, and 54 districts in my total sample.

The remainder of the paper is organized as follows: In section VI, I explore how ERIs altered the composition of teachers with varying levels of experience. First, I conduct event-study analyses to check the pre-trend differences and the changes in my outcome variables, post-ERI introduction. I then report regression results for a similar model. The purpose of this section is to show the implied first stage, or how teachers responded to the ERI policy. In section VII, I present my main results showing the effect of ERIs on student performance. First, I use event-study analyses to explore the exogeneity of ERI adoption and the effect of ERIs post-introduction on student test scores. I then report my main regression results. In section VIII, I discuss my results and explore the relationship between the teacher experience distribution and test scores. Finally, in section IX I conclude.

The Effect of ERIs on Enrollment, Pupil-Teacher Ratios, and Teachers

Event Studies: Exploring the Effect of ERIs on Enrollment, Pupil-Teacher Ratios, and Teachers

Before discussing the effect of ERIs on test scores, it is important to better understand how ERIs affect the composition of teachers within schools. I first explore the pre- and post- effects of ERIs on enrollment, pupil-FTE teacher ratios, and the number of

entering my sample). Although not shown, results with district-by-grade fixed effects are less precise, but similar in sign and magnitude to my results with school-by-grade fixed effects.

FTE teachers by years of experience. To conduct an event-study examining the effects of ERIs on enrollment and teachers, I estimate the following model:

$$Y_{sdt} = c + \sum_{r=min}^{r=-1} \alpha_r I_r * ERI_d + \sum_{r=1}^{r=max} \alpha_r I_r * ERI_d + \boldsymbol{X}_{sdt}\delta + \gamma_s + \rho_t + \mu_{sdt} \quad (2)$$

Y_{sdt} represents the outcome variable of interest (enrollment, pupil-FTE teacher ratio, and number of teachers with differing levels of experience) for school s within district d and year t. Here, r is event time, with $r = 0$ corresponding to the academic year in which the ERI is first offered, min is the earliest event time that we see, and max is the latest event time that we see. I_r is an event-time indicator variable and ERI_d is variable equal to 1 if a school district ever offered an ERI, 0 otherwise. $\boldsymbol{X}_{sdt}$ is a vector of school-level demographic controls described in equation (1), although the regressions with enrollment and pupil-FTE teacher ratio as the dependent variables do not control for enrollment. I also include γ_s school fixed effects and ρ_t year fixed effects. The α_r coefficients are measured relative to the omitted coefficient ($r = 0$).

For all event studies, I restricted my sample to districts with at least two pre and two post periods. I included districts that offered only one ERI during my sample period, or two ERIs if they were offered in consecutive years.[14] Standard errors are clustered at the district level.

Figure 5 displays the α_r coefficients for the dependent variables of enrollment and pupil-FTE teacher ratio.[15] For $r \leq 0$, trends in both enrollment and pupil-FTE teacher ratios appear to be unrelated to ERI adoption, with zero contained in the 95% confidence interval up to four years prior to the event. After the ERI was adopted, the pupil-FTE

[14] There are 35 districts included in my event studies, accounting for about 30% of California student enrollment, or about 70% of my original sample.

[15] For additional regression results for the effect of ERIs on the number of teachers and pupil-teacher ratios, see Appendix Table A.2.

teacher ratio increases, while enrollment remains close to zero for the first few years after the ERI was offered. This suggests that in the years immediately following ERI implementation, ERIs reduced the number of teachers, while enrollment remained generally unchanged.

Figure 6 displays the event study analyses for the number of teachers with 30+ years of experience, 25-29 years of experience, 20-24 years of experience, and 15-19 years of experience. For $r = -1$, all pre-trends are close to zero. After ERI adoption, the number of teachers with 30+, 25-29, and 20-24 years of experience declines. The number of teachers with 15-19 years of experience is unchanged in $r = 1$, but then increases. These event studies imply that ERIs are reducing the number of highly experienced, older teachers, which is to be expected.

Figure 7 displays the results for the number of teachers with low levels of experience (1 year, 2 years, <5 years) and the total number of FTE teachers. For first-year teachers, there is no significant evidence of a pre-trend. After ERI adoption, schools that offered ERIs increased the number of first year teachers, although the effect is not significant. The trend in the number of teachers with 2 years of experience is largely unchanged over time, but estimates are outside of zero. The event study with the number of teachers with less than 5 years of experience shows that in the years immediately prior to ERI adoption and after ERI adoption, there isn't a significant change in trends for the numbers of low experienced teachers related to ERI adoption. The total number of FTE teachers fell in districts that offered ERIs, primarily due to reductions in highly experienced teachers.

Figure 8 shows the relative change in highly experienced teachers (30+ years) by teachers with five or more years of service in the district versus teachers with less than five years of service in the district. The highly experienced, but relatively newer to the district teachers would not qualify for an ERI based on the CalSTRS retirement rules. As shown in figure 8, the reduction in highly experienced teachers is coming from teachers qualified for retirement.

These event studies show that there is a reduction in the number of FTE teachers following ERI adoption. The reduction in teachers is coming primarily from a reduction in highly experienced, older teachers, and not due to the layoff of newer teachers. In fact, the number of first-year hires increases, but the effect is not statistically significant. Since California is a LIFO state, this suggests that school districts that offer ERIs are reducing the number of more costly and higher experienced teachers and are possibly replacing some of these teachers with less experienced hires.

Results: The Effect of ERIs on Enrollment, Pupil-Teacher Ratios, and Teachers

I also report estimates for a more restrictive version if equation (2). Instead of allowing β_1 (the effect of the ERI) to vary with time, I estimate the following specifications:

$$Y_{sdt} = c + \beta_1 ERI_{dt-1} + \boldsymbol{X}_{sdt}\delta + \gamma_s + \rho_t + \mu_{sdt} \quad (3)$$

$$Y_{sdt} = c + \beta_1 ERI_{dt-1} + \beta_2 SecERI_{dt-1} + \beta_3 ThirdERI_{dt-1} + \boldsymbol{X}_{sdt}\delta + \gamma_s + \rho_t + \mu_{sdt} \quad (4)$$

where I include the same school-level controls as in (2). ERI is the same dummy variable included in specification (1). In specification (4), I estimate the effect of districts offering multiple ERIs. $SecERI$ is a dummy variable, equal to zero if a district had not offered a second ERI, and equal to one in the year the second ERI was offered and every following year. $ThirdERI$ is another dummy variable equal to one in the year a district offered a

third ERI (or more) and every following year, and zero otherwise. Thus, compared to a district that never offered an ERI, all else equal, the average additional effect of a district offering three or more ERIs on Y_{sdt} would be: $\beta_1 + \beta_2 + \beta_3$. All ERI related dummy variables are lagged by one year.

Table 2 shows the results of specifications (3) and (4), where Y_{sdt} represents the average years of experience or the number of FTE teachers. Results weighted by school enrollment and unweighted results are displayed. Offering an ERI decreased the average years of experience teaching by .219 years, but the result is not statistically significant from zero (Table 2, column 2). Offering a second ERI decreased average experience and offering three or more ERIs increased experience, but these results are not statistically significant (Table 2, column 4). The effect of ERIs on the number of FTE teachers is also negative for the first two ERIs offered, significant at the 10% level. Offering an ERI reduced the number of FTE teachers in a school by about 1.829 teachers, or by about 4.2% (Table 2, column 6). These results suggest that ERIs decreased the overall number of teachers, with more retirements than new hires.

Table 3 displays the results from regressing the number of teachers with varying levels of experience on lagged ERI adoption.[16] Similar to the event studies, the results suggest that ERIs reduced the numbers of highly experienced teachers, although results are only statistically significant at the 5% and 10% levels for the number of FTE teachers with

[16] In specifications (3) and (4), the ERI dummy variables capture the overall post-event time effects of ERIs on the dependent variable, since the ERI variables are equal to one in the period the ERI was offered and all following periods. This assumes that the effect of ERIs persists beyond one year. In Appendix Table A.3, I report results from regressing the number of teachers with varying levels of experience on lagged ERI adoption, restricting the ERI dummy variable to equal one only in the period that the ERI was offered. This captures the non-cumulative effect of a school district offering an ERI on the total number of teachers in the following year, relative to all other years. Compared to Table 3, the effects are qualitatively similar, but larger in magnitude. ERIs induced highly experienced teachers to leave.

25-29 and 20-24 years of experience, respectively (Table 3, columns 2 and 3). While negative, the estimated effect on the number of teachers with 30+ years of experience is statistically insignificant and small in magnitude. This may be due to highly experienced teachers having other characteristics that incentivize them to teach other than financial motives. On average, offering an ERI reduced the number of FTE teachers with 25-29 years of experience by .261 teachers per school and reduced the number of FTE teachers with 20-24 years of experience by .358 teachers. The mean of the dependent variables are also displayed. Each school in my sample had an average of 2.90 teachers with 25-29 years of experience and 4.01 teachers with 20-24 years of experience, so this represents approximately a 9% reduction for both groups.

The effect of ERIs on the number of teachers with less than five years of experience is negative, but statistically insignificant (Table 3, column 7). The positive coefficient on the number of teachers with one year of experience suggests that school districts may be replacing the retirees with lower cost, less experienced teachers, but the estimates are statistically insignificant.

Results are similar when accounting for multiple ERIs (see Table 4). However, now offering a second ERI results in a statistically significant and negative coefficient on the number of teachers with 30+ years of experience (Table 4, column 1). This may be due to districts offering stronger financial incentives as their second ERI to encourage larger numbers of teachers to retire.

Overall, I find that ERIs did have an impact on teacher experience composition within school districts. There is a negative effect on teachers with over 20 years of experience, but initially no significant effect on extremely experienced teachers (30+

years). This suggests that teachers generally responded to incentives, but very highly experienced teachers likely had different motives to keep teaching, other than financial incentives. Additionally, ERIs had a negative impact on the overall number of teachers, which suggest that districts were not immediately filling the job vacancies. The positive coefficient on the number of teachers with one year of experience indicates that if they did fill the job vacancies, it was with relatively less costly newer teachers, but I do not generally find any significant effect of ERIs on the number of low experienced teachers.

The Effect of ERIs on Student Performance

Event Studies: Exploring the Exogeneity of ERI Adoption and Post-Treatment Effects

Before presenting my main results from (1), I first present event study estimates of the effect of ERIs on test scores. As stated previously, my main identification strategy is based on the assumption that there are no underlying trends in school-grade test scores that are correlated with ERI adoption. The purpose of this analysis is to explore the exogeneity of ERI adoption with respect to test scores and to estimate the effect of ERIs in the periods post-adoption. I estimate the following equation:

$$Y_{gsdt}^{i} = c + \sum_{r=min}^{r=-1} \alpha_r I_r * ERI_d + \sum_{r=1}^{r=max} \alpha_r I_r * ERI_d + \boldsymbol{X}_{gsdt}\delta + \gamma_{sg} + \rho_{st} + \mu_{gsdt}^{i} \qquad (5)$$

Equation (5) is similar to the event study presented in equation (2), but the outcome variable, Y_{gsdt}^{i}, is the standardized average test score for subject i in grade g in school s within district d and year t. Equation (5) controls for the same variables as in (1) and is weighted by school-grade enrollment.

Figure 9 shows the event study results for CST English test scores. There is strong evidence supporting the exogeneity of ERI adoption. There is no evidence of a pre-trend in

English test scores, suggesting no differences in test score trends related to ERI adoption. Then, following the introduction of the ERI, English test scores increase.

Figure 10 displays the results for the pooled math subject test scores. The math tests include CST Mathematics, CST Algebra 1, and CST Geometry.[17] While there is no significant evidence of a pre-trend in math scores, it also appears that math scores do not respond to ERI implementation. There is a slight positive effect 2 years after the ERI was adopted, but the effect is not statistically significant.

Figures 11 and 12 display the event study results for CST Science, CST History, and CST US History test scores. For CST Science and CST US History test scores, there is no evidence of a pre-trend and test scores increase following ERI adoption. Although the pre-trend is slightly negative for the CST History test, taken in grade eight, the pre-trend is close to zero for $r = -1$ and test scores increase after the ERI is implemented. There is stronger evidence supporting the exogeneity of ERI adoption is for higher grades.

Taken together, the test score event studies largely suggest that my identification assumptions are valid for most test subjects. Generally, ERIs appear to have a positive impact on student test scores.

Main Results: The Effect of ERIs on Student Performance

[17] As stated previously, the mathematics tests are subject specific beginning in grade 8. I pool the mathematics tests across different subjects and grades to increase the sample size and precision of the estimates. I assume that the effect of ERIs does not vary across mathematics subjects (and grades), which may be a fair assumption given that the test scores are standardized by year, grade, and test subject.

However, it is possible that there may be changes in the student population taking different mathematics subject tests at the same time that ERIs were offered, biasing my results. To address this issue, I look at the share of students taking the CST Algebra 1 and Geometry subject tests during my sample period, shown in Appendix figure A.1. There is generally no significant change in the share of students taking Geometry. It appears that there is a slight increase over time in the share of students taking Algebra 1 in Grade 8 instead of in High School. Yet, even in non-pooled specifications, I find qualitatively similar, but less precise results (results not shown). I find that ERIs have a positive effect on Algebra 1 and Geometry test scores, with larger effects on mathematics tests in higher grades.

Regression results for my primary specification (1) with the CST English-Language Arts standardized test scores as the dependent variable are displayed in Table 5. Offering an ERI increased English test scores, although results are only statistically significant for high school students (grades 9 through 11). On average, offering an ERI increased English test scores by .04 standard deviations, but this effect is not statistically significant. Offering an ERI increased high school CST English-Language Arts test scores by .103 standard deviations, significant at the 1% level (Table 5, column 4). When I examine the effect of offering multiple ERIs (columns 5-8), the effect increases to .048 standard deviations for all grades, and by .114 standard deviations for high school students. Only the first ERI had a statistically significant effect. This may be due to the least motivated teachers responding to the first option to retire early.

Table 6 displays the results for the pooled math test scores. Generally, the effect of ERIs on math test scores is positive, with the largest effects among high school students (.092 standard deviations, significant at the 10% level, column 4). The estimate for elementary math test scores is negative, but statistically insignificant with a large standard error (Table 6, column 2). Results also suggest that the first ERI is the most effective.

Table 7 displays the results for the regressions with CST Science test scores as the dependent variable. The story is similar; offering an ERI had a positive effect on student test scores, with effects most pronounced among higher grades. The effect for all grades is a .069 standard deviation increase in Science test scores (Table 7, column 1). Offering the first ERI increased test scores for high school students by .134 standard deviations (or .148 standard deviations in the specifications with multiple ERIs), and the first ERI offered was the most effective (Table 7, columns 4 and 8).

Finally, Table 8 displays the results for the regressions with CST History and CST U.S. History test scores. On average, offering the first ERI increased history tests scores by .104 standard deviations (Table 8, column 1). The effect is larger for high school students, and the first ERI offered was the most effective. The estimated effect of the second ERI is negative, but is small in magnitude and statistically insignificant. The net effect of multiple ERIs ($\beta_1 + \beta_2 + \beta_3$) is positive.

The clear pattern in my findings is that ERIs improved school-grade test scores, especially among high school students. Ideally, to be consistent with the existing student achievement literature, I would like to express my results in student-level standard deviations, as opposed to school-grade level standard deviations. Similar to Fitzpatrick and Lovenheim (2014), I do not have student-level test score data. Without this data, I cannot accurately determine the ratio of school-grade-level test score standard deviations to student-level test score standard deviations. However, making the same assumption as Fitzpatrick and Lovenheim (2014) that the variance in school-grade test scores is approximately 15 percent of the variance in student-level test scores, I can roughly convert my estimates. Offering an ERI increased school-grade level test scores by about 0.1 standard deviations, which is approximately equal to a 0.04 standard deviation increase in student-level test scores. These student-level estimates are small, but likely closer to the existing estimates.

To evaluate how ERIs may disproportionally affect high school students, I conduct an event-study estimating the effect of ERIs on the total number of FTE teachers with 20+ years of experience, by elementary, middle, and high schools. Results are displayed in figure 13. I find that ERIs led to a larger decrease in the number of high school teachers,

relative to other grades. Additionally, it is possible that ERIs have a larger effect for high school students because a high school teacher can affect up to six classrooms, while elementary school teachers typically teach in a self-contained classroom. The retirement of one high school teacher therefore affects a larger number of students.

Discussion

My previous findings show that highly experienced teachers responded to ERIs with positive impacts on student achievement. This effect seems contrary to findings in the existing literature that experienced teachers are generally more effective than the least experienced teachers. If more experience is better (on average), we would expect test scores to decline when districts offer ERIs. One possible explanation for my findings is that while teachers with higher levels of experienced are of better quality relative to new teachers, it is the low quality, yet higher experienced teachers that respond to incentives. I can provide some evidence on whether more experience is good on average by examining the relationship between test scores and the experience distribution. I regress test scores on the share of teachers with varying levels of experience (as well as the same demographic controls and fixed effects used in previous regressions) to explore how the experience profile of teachers in my sample are related to student test scores. Results are displayed in Appendix tables A.4-A.6. All estimates are relative to the omitted category, the share of teachers with 10-14 years of experience.

Table A.4 shows the results for regressions with CST English-Language Arts test scores as the dependent variable. Schools with greater shares of experienced teachers tended to have higher test scores, but it is the extremely experienced (30+ years) or mid-range experienced (15-19 years) teachers that have the greatest effect on student

outcomes. Schools with a greater share of teachers with less than five years of experience had relatively slightly lower test scores.

The results for CST Math test scores are displayed in Table A.5. Schools with a greater share of highly experienced teachers had higher test scores. Finally, Table A.6 displays the results for CST Science and CST History and US History test scores. Results are somewhat mixed, but generally, highly experienced teachers had a positive impact on test scores and schools with a greater share of new teachers had relatively lower test scores. Focusing on high school students (Table A.6, columns 4 and 7), schools with a greater share of teachers with 15-19 years of experience had higher test scores.

These estimates are consistent with evidence from the existing teacher experience literature that finds that the least experienced teachers are less effective, on average. I generally find that higher experienced teachers are more effective, with positive effects on test scores. Although offering an ERI reduced the number of highly experienced teachers, I find no negative effects on student performance. This suggests that the least productive teachers are the ones that respond to ERIs, resulting in improved student achievement.[18]

Conclusion

To overcome budget shortfalls, many states or school districts have adopted ERIs to reduce costs by inducing early retirements. In the limited existing ERI literature, the evidence suggests that ERIs have a positive effect on student outcomes. However, other studies find that more experienced teachers are more effective in classrooms, so

[18] It is also possible that the positive effect on test scores is due to the non-ERI school districts implementing some other type of cost-saving program that negatively affected student performance at the same time ERIs were introduced. For instance, it is possible that these non-ERI districts laid off administrators or pupil services staff to reduce their payrolls and save money. In Appendix figure A.2, I show the estimated effect of ERIs on the total number of FTE pupil services and administrative staff. I find no relationship between ERIs and the total number of non-teaching staff.

retirements are likely to have a negative impact on students. California provides an interesting case study during a time when funding cuts led to massive layoffs and an increased usage of early retirement incentives. School districts in California implemented ERIs to reduce their supply of teachers, removing their most experienced teachers instead of only removing their least experienced teachers.

Using a difference-in-differences specification, I find that ERIs affected the composition of teacher experience in schools. School districts that offered ERIs reduced their numbers of highly experienced teachers, especially in the 20-29 years of experience range. I find evidence that school districts did not immediately fill the job vacancies, and the overall number of teachers declined. Although school districts that offered ERIs reduced the number of their most experienced teachers, I did not find a negative effect on student performance. Rather, I find that ERIs generally increased test scores.

I use event-study analyses to provide evidence supporting the exogeneity of ERI adoption. Although the effect of ERIs on test scores varies across subjects and grades, I consistently find evidence of a positive effect, particularly among high school students. A possible explanation for why the effect may be larger for high school students is that more high school teachers are responding to the retirement incentives. My data support this claim.

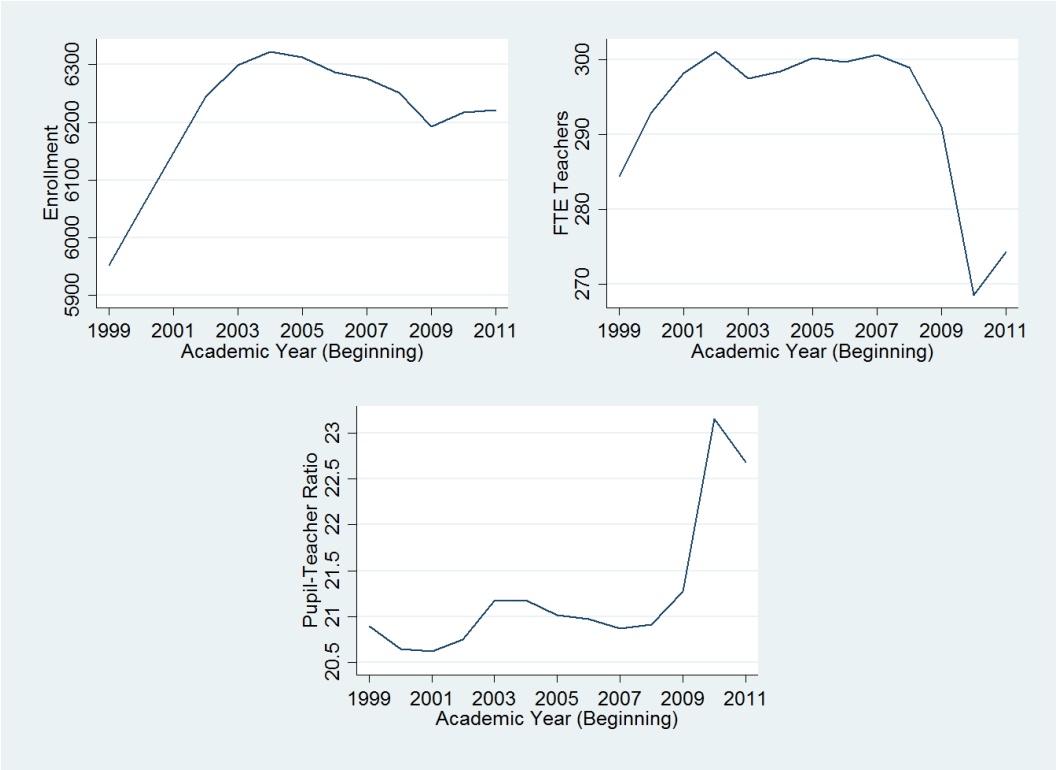
Furthermore, the first ERI offered by school districts often had the greatest impact on the number of teachers and test scores, suggesting that teachers with the lowest motivation are often the ones that respond first to incentives to retire early. I explore the relationship between the experience distribution and student test scores and, similar to the literature, find that the least experienced teachers are on average less effective than more

experienced teachers. Because I find that ERIs had a positive effect on test scores, this provides further evidence that it is the lower quality, but highly experienced teachers responding to these incentives.

Given the high cost of experienced teachers relative to new hires and aging teacher workforce, school districts often find that ERIs are an attractive policy to achieve cost savings. This paper provides evidence that by allowing teachers to select into early retirement through incentives, student test scores increase. The policy implications are that if districts must reduce their workforce, allowing teachers to self-select out of the teaching workforce, as opposed to LIFO layoffs, can improve student outcomes. Furthermore, the cost-benefit calculations regarding the impacts of ERIs ought to take the effects on student outcomes into consideration.

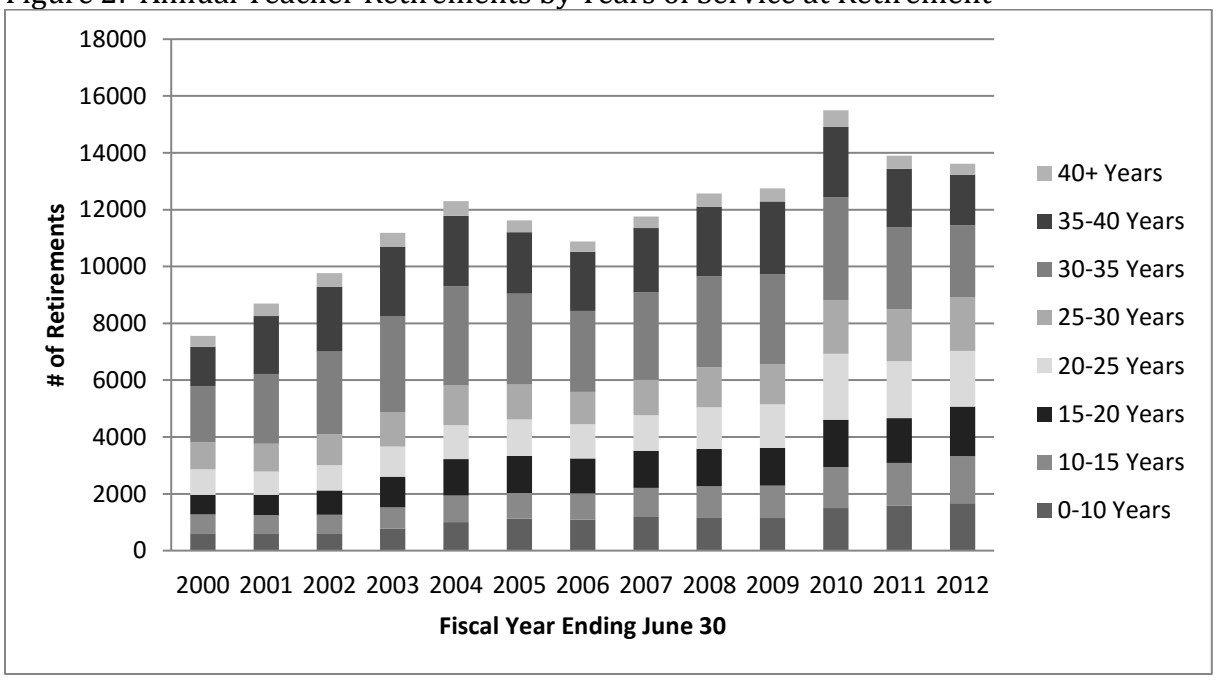
Future research will involve examining the long-run effect of ERIs on student achievement. Specifically, I will use data on California High School Exit Examination (CAHSEE) test scores, which is a requirement for high school graduation in the state of California. The CAHSEE test is first taken in grade 10. I will examine whether being subjected to an ERI in earlier grades had an impact on a student's future test score. Additionally, I plan on examining the relationship between teacher quality and early retirement incentives using data on teacher value-added measures in Los Angeles Unified School District.

Figure 1: CA Enrollment (thousands), FTE Teachers (thousands), and Pupil-Teacher Ratio



Source: California Department of Education, Educational Demographics Office (CBEDS), 2012

Figure 2: Annual Teacher Retirements by Years of Service at Retirement



Source: CalSTRS Comprehensive Annual Financial Reports, 1999-2012

Figure 3: Share of Districts in Sample Offering an ERI, by Year

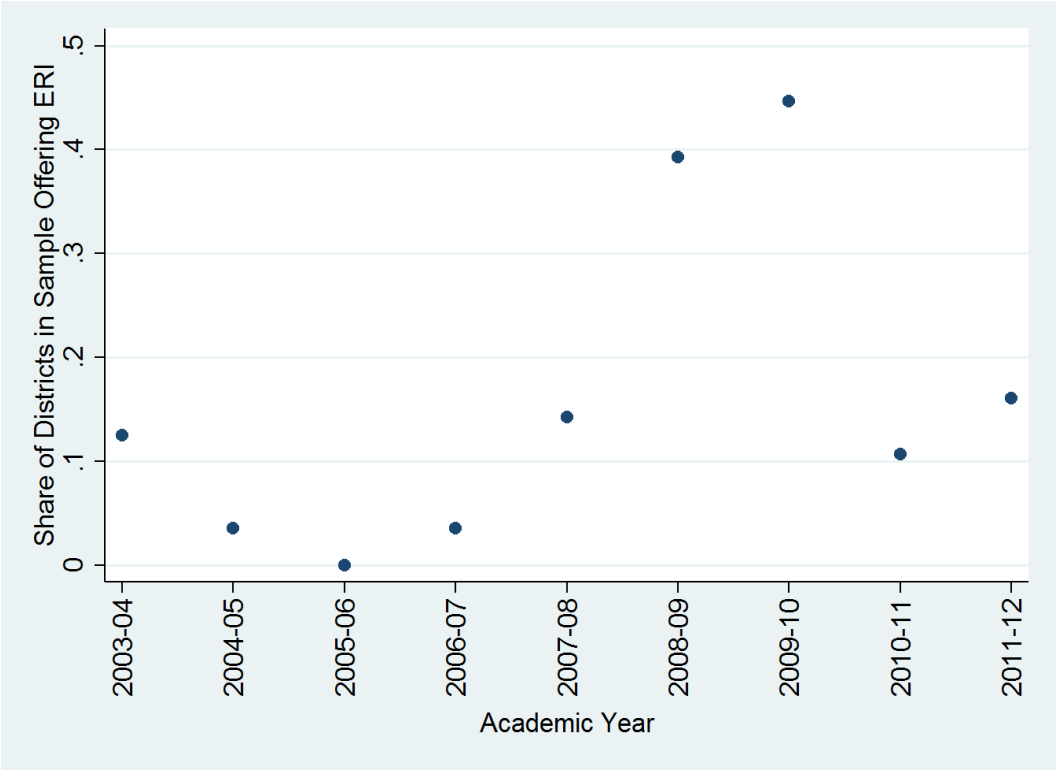
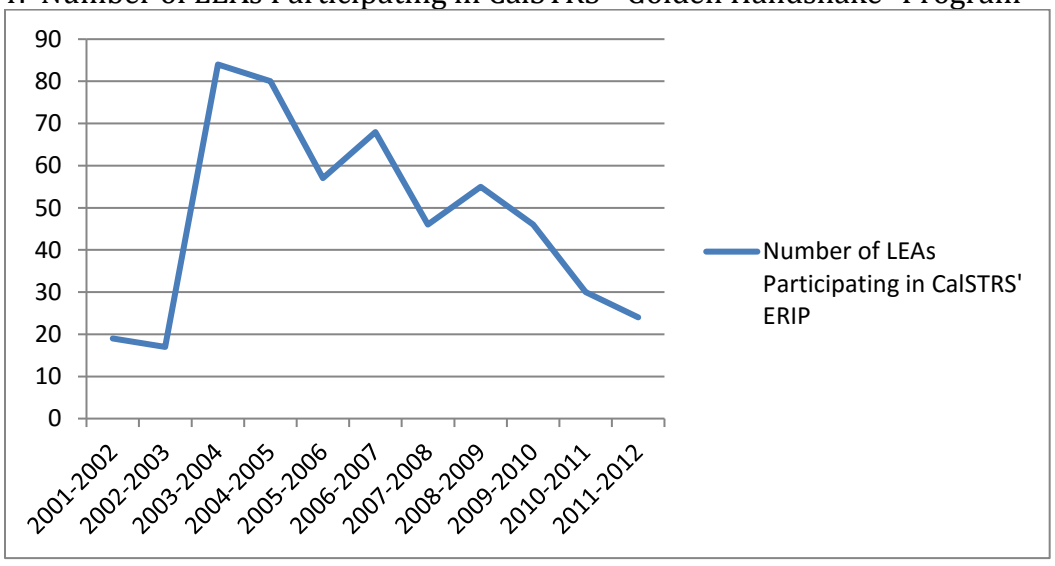


Figure 4: Number of LEAs Participating in CalSTRS' "Golden Handshake" Program



Source: State Controller Cost Analysis Report, 2001-2012

Figure 5: Event Study: Enrollment and Pupil-FTE Teacher Ratio

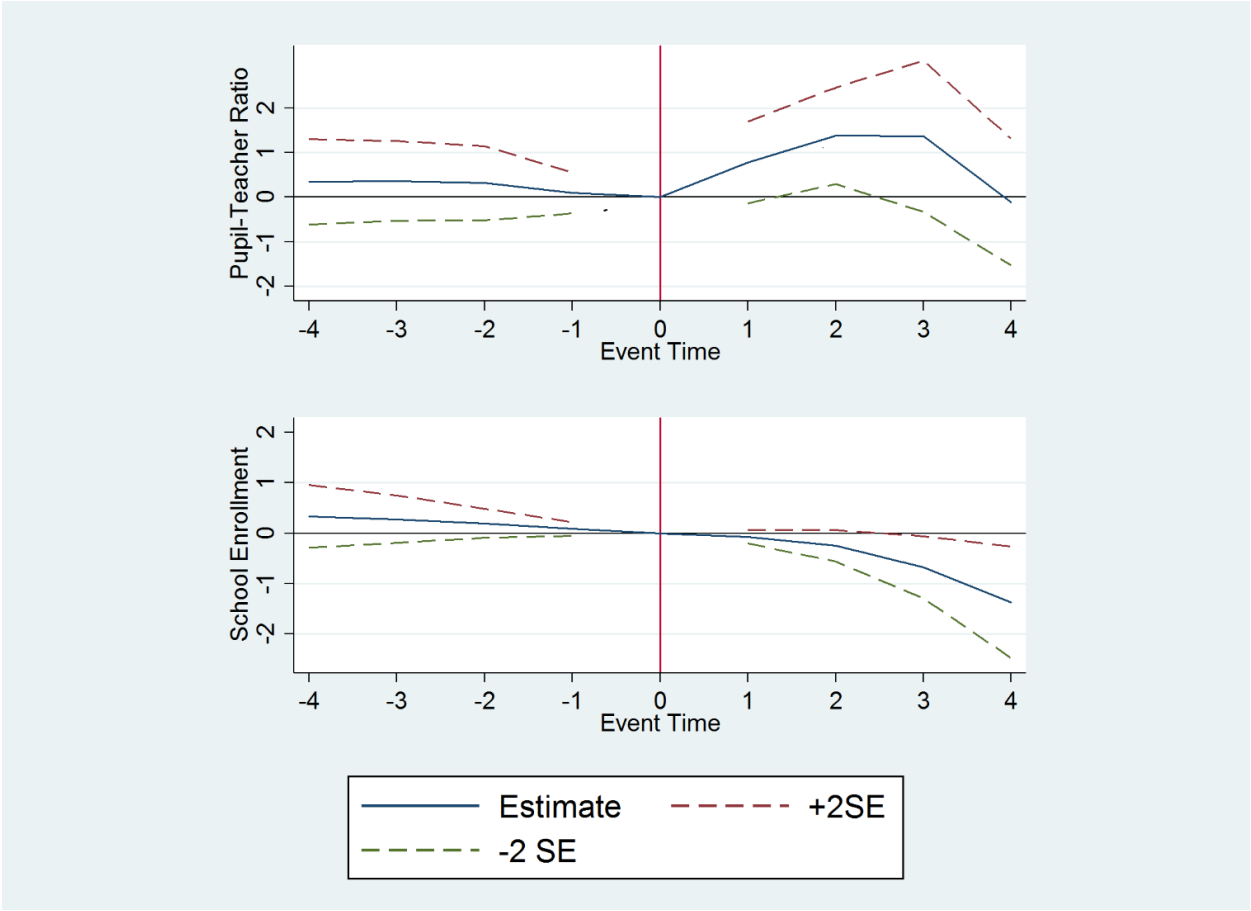


Figure 6: Event Study: Teacher Experience (30+, 25-29, 20-24, 15-9)

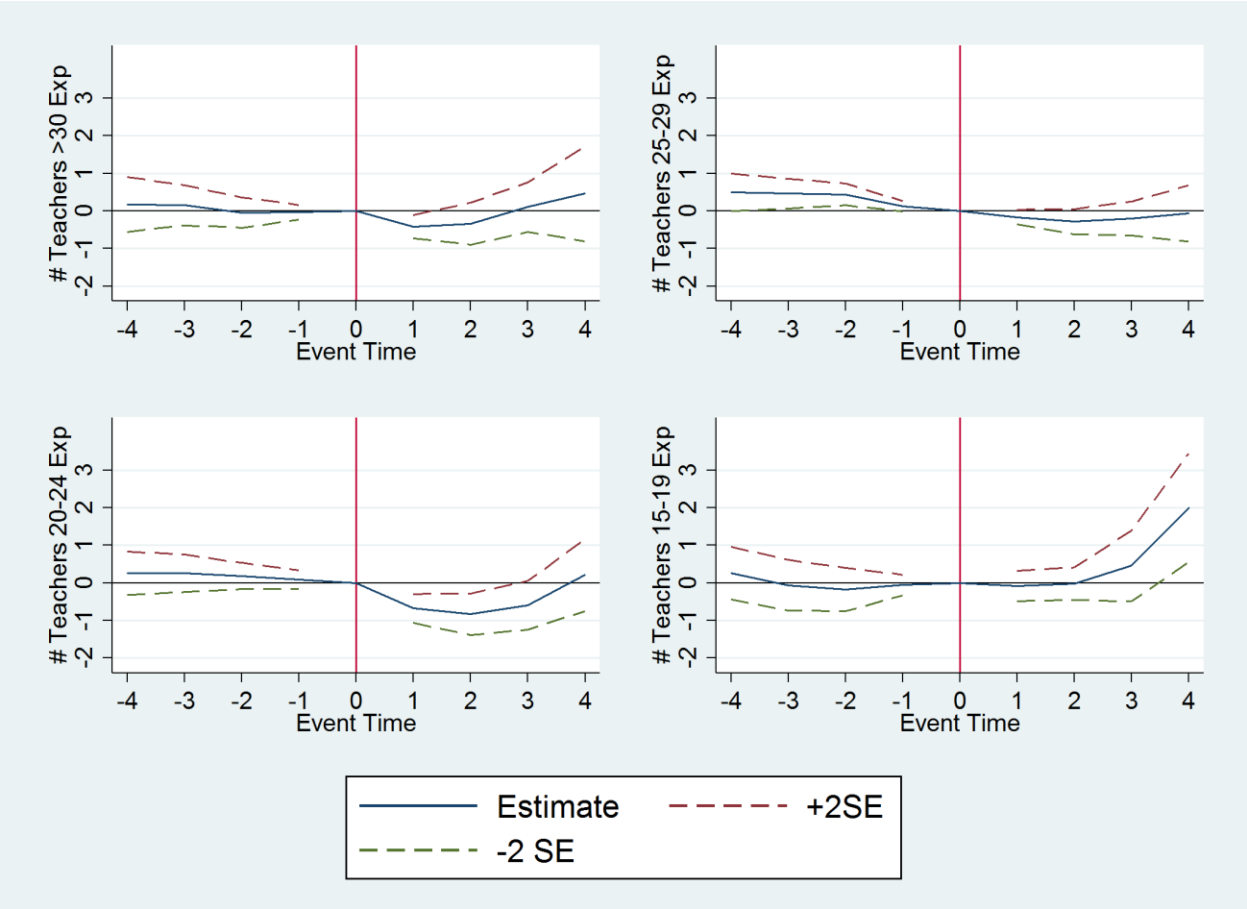


Figure 7: Event Study: Teacher Experience (1, 2, <5) and Total Number of Teachers

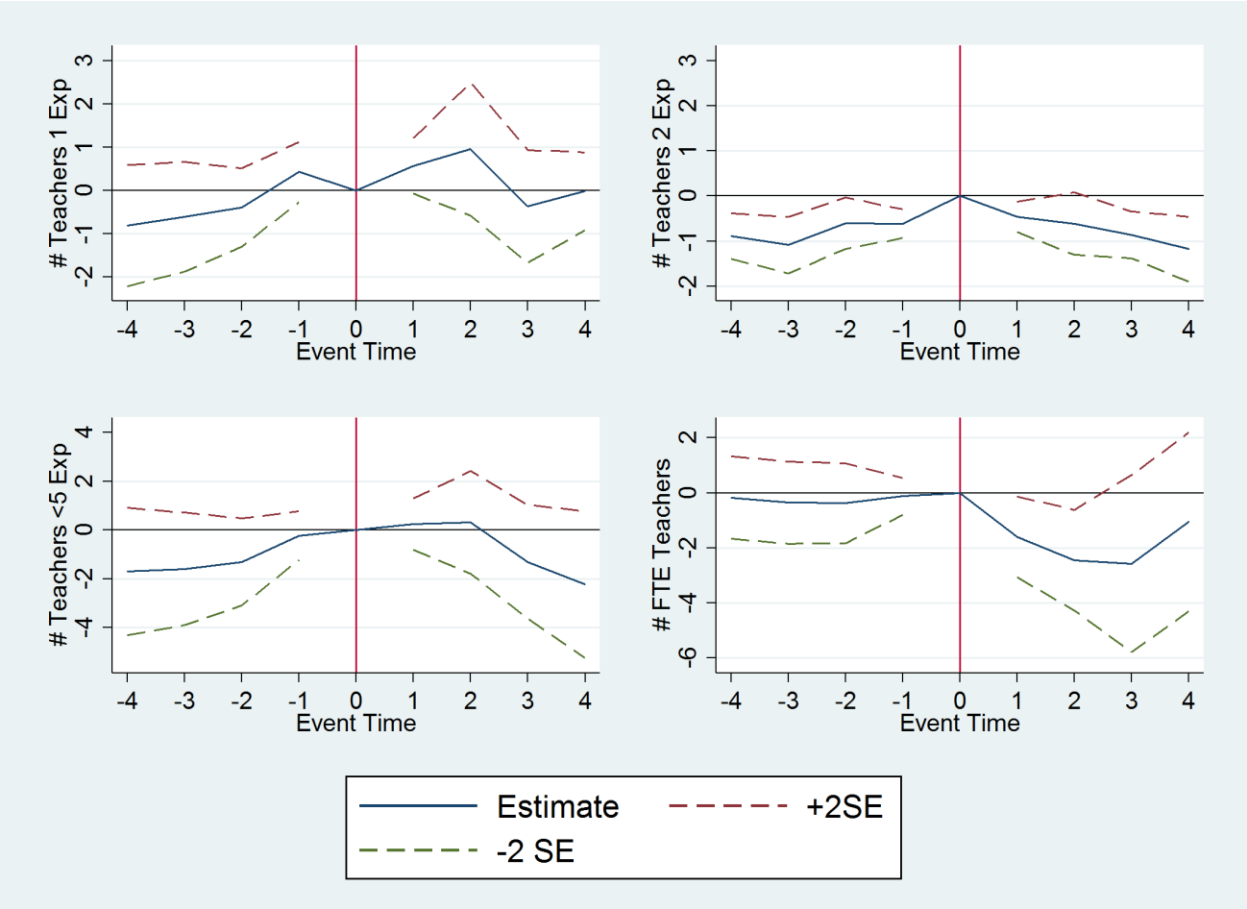


Figure 8: Event Study: Number of Teachers with 30+ Years of Experience, by Exp. in District

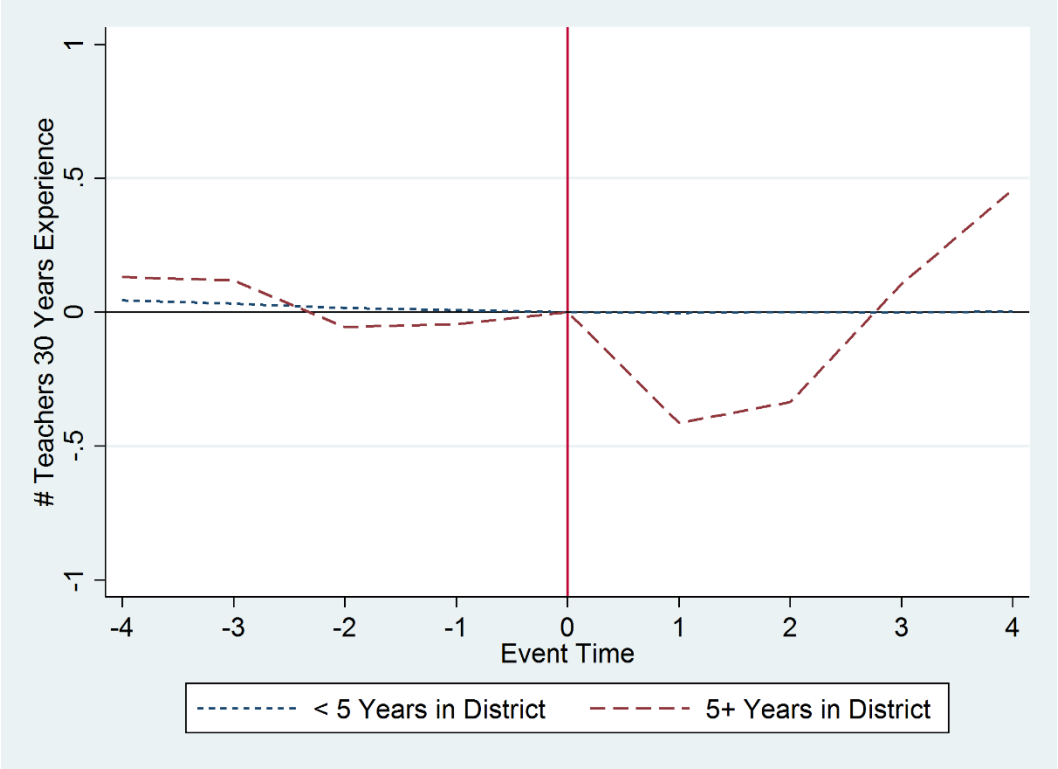


Figure 9: Event Study: CST English-Language Arts Test Scores

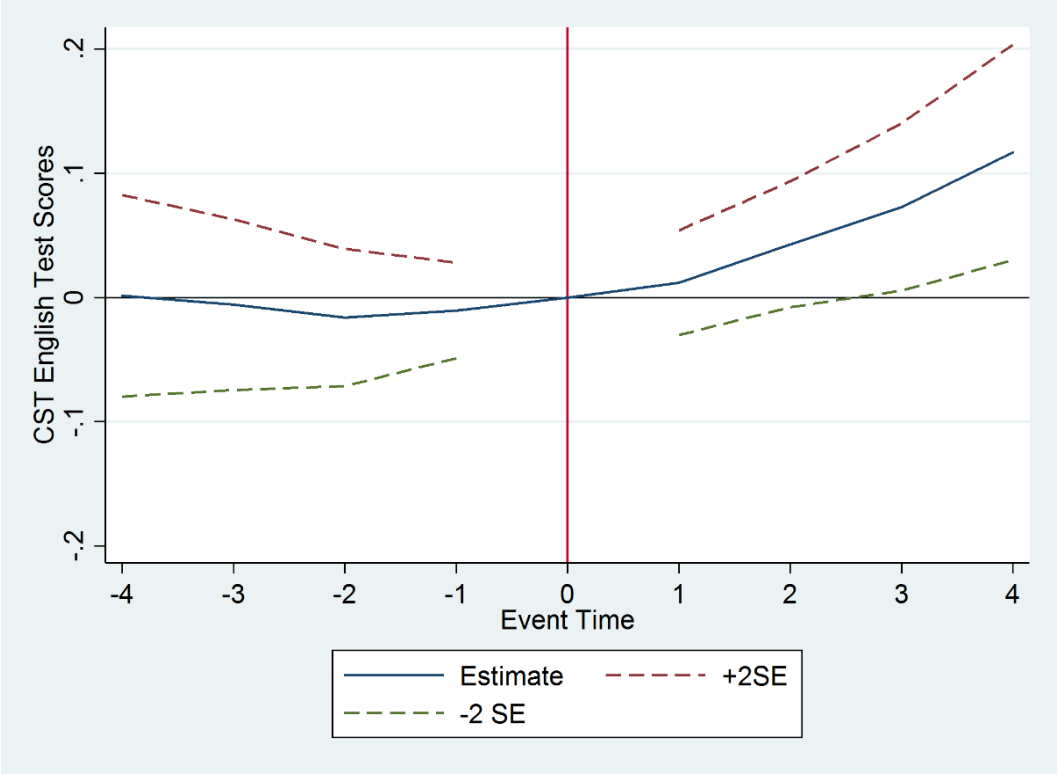


Figure 10: Event Study: CST Math Test Scores

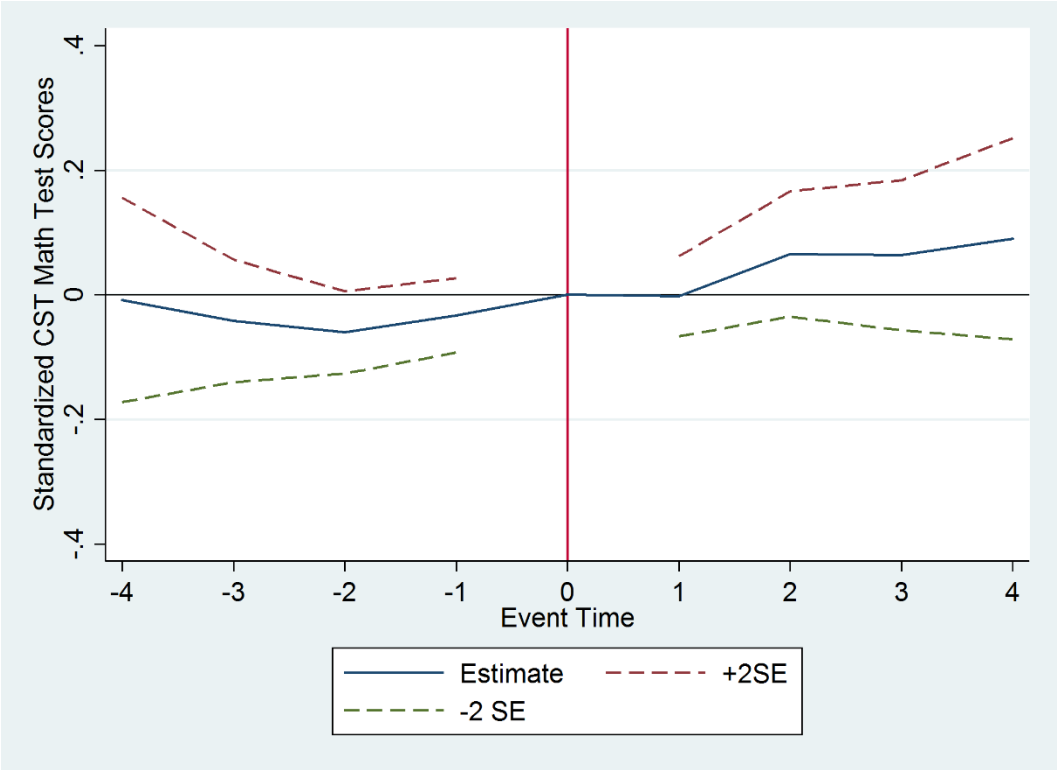


Figure 11: Event Study: CST Science Test Scores

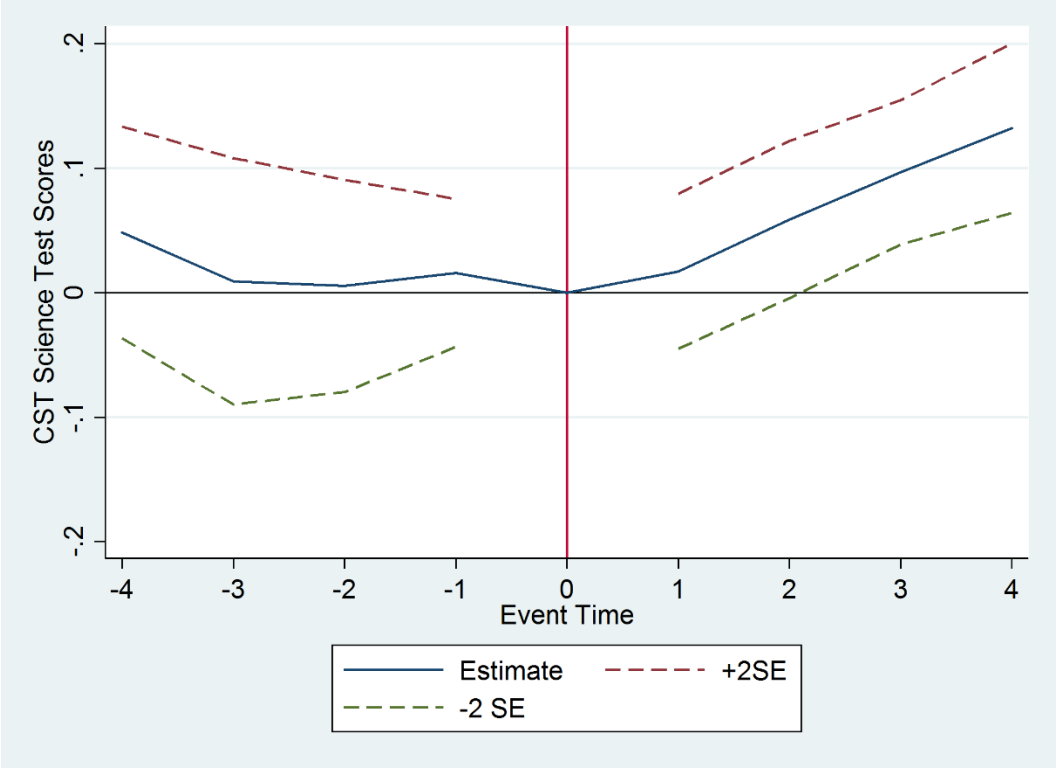


Figure 12: Event Study: CST History and CST US History Test Scores

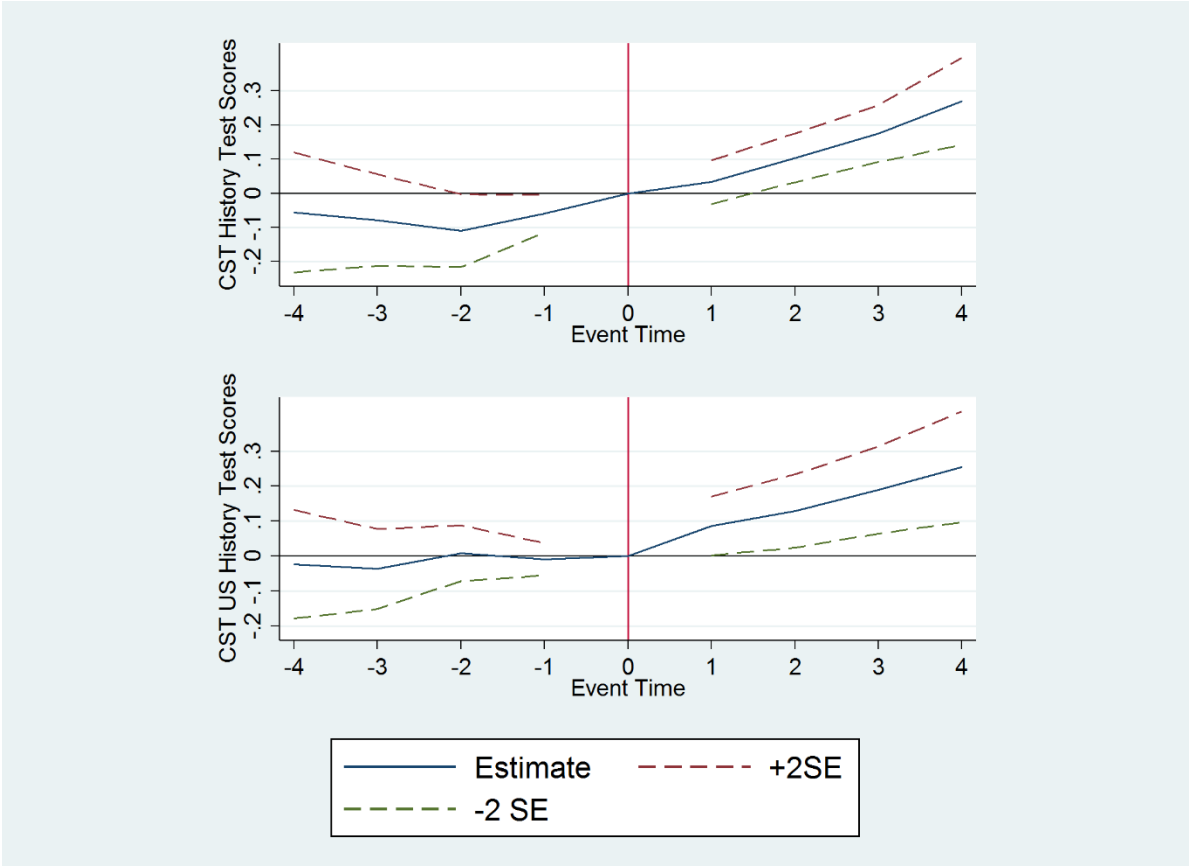


Figure 13: Event Study: Number of FTE Teachers with 20+ Years of Experience, by Grade

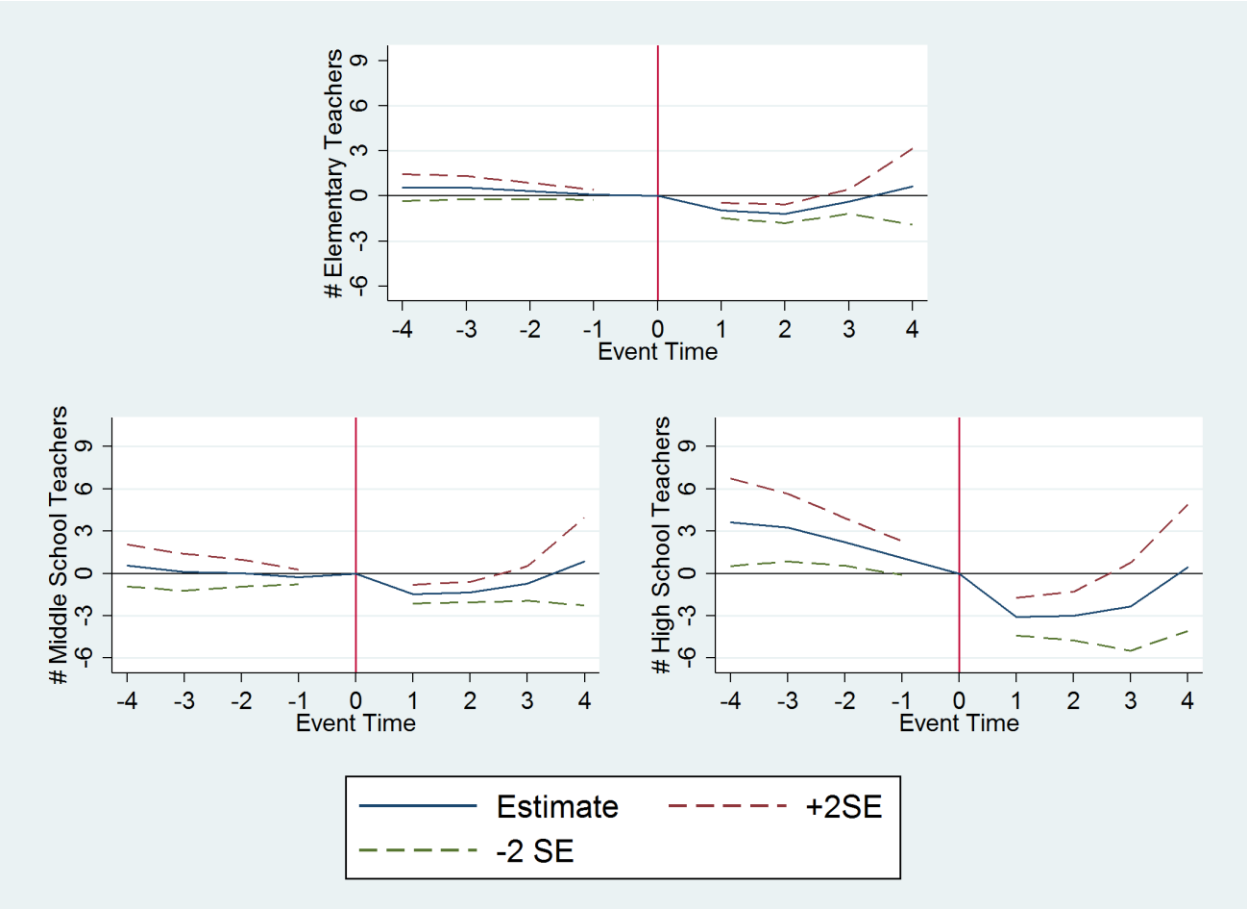


Table 1: Summary Statistics

Variable	Mean	Std. Dev.	Min	Max
School-Level				
% Free or Reduced Price Meal	60.34	29.66	0	100.00
% English Language Learners	27.98	20.22	0	200.96
% Black	9.63	12.30	0	97.00
% Hispanic	52.21	28.95	0	100.00
% Asian	10.04	14.00	0	100.00
Pupil-FTE Teacher Ratio	22.75	4.24	2.8	137.60
Enrollment/100	10.02	7.81	0.31	53.36
School-Grade Level				
% English Language Learners	25.46	20.16	0	200.00
% Black	9.98	12.83	0	100.00
% Hispanic	52.19	29.21	0	100.00
% Asian	10.57	14.47	0	100.00
Enrollment/100	2.29	2.38	0.01	23.34
# FTE Teachers, School Level				
30+ Years Exp	3.19	3.82	0	35.00
25-29 Years Exp	2.90	3.08	0	29.00
20-24 Years Exp	4.01	3.59	0	31.00
15-19 Years Exp	5.90	4.88	0	46.00
10-14 Years Exp	8.93	6.92	0	54.00
6-9 Years Exp	8.42	7.46	0	57.00
<5 Years Exp	9.75	11.32	0	129.00
2 Years Exp	1.90	3.05	0	58.00
1 Year Exp	1.87	3.23	0	62.00
Average Experience	13.53	3.51	1	29.90
Total (School)	43.29	31.22	0	233.00
Total (District)	7920.77	11873.66	0	34393.00

Number of Districts, by ERIs Offered	
0	8
1	28
2	7
3+	11
Total # of Districts	54

Observations	262,665
Number of schools	2,922
Number of school-grades	11,969

Table 2: Effect of ERIs on Average Years Experience and Number of FTE Teachers

VARIABLES	(1) Avg. Years Teaching	(2) Avg. Years Teaching	(3) Avg. Years Teaching	(4) Avg. Years Teaching	(5) # FTE Teachers	(6) # FTE Teachers	(7) # FTE Teachers	(8) # FTE Teachers
lag(First ERI)	-0.168	-0.219	-0.049	-0.164	-1.089*	-1.829*	-1.154*	-1.724*
	(0.379)	(0.327)	(0.368)	(0.307)	(0.610)	(1.012)	(0.594)	(0.950)
lag(Second ERI)			-0.129	-0.234			-1.590**	-1.925*
			(0.416)	(0.386)			(0.659)	(1.146)
lag(Third ERI +)			0.782**	0.454			0.446	1.656
			(0.374)	(0.376)			(0.675)	(1.148)
Mean of Dependent Variable	13.53	13.53	13.53	13.53	43.29	43.29	43.29	43.29
Weighted by school enrollment		Y		Y		Y		Y
School FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Observations	24,436	24,436	24,436	24,436	24,494	24,494	24,494	24,494
R-squared	0.799	0.805	0.800	0.805	0.976	0.978	0.976	0.978

Clustered standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Notes: All regressions control for % eligible for free or reduced price lunch, % English language learner, % black, % Hispanic, % Asian, enrollment, and enrollment squared. All controls vary at the school level. Standard errors clustered at the district level.

Table 3: Effect of ERIs on Total # of FTE Teachers with Varying Levels of Experience

VARIABLES	(1) 30+ Years Exp	(2) 25-29 Years Exp	(3) 20-24 Years Exp	(4) 15-19 Years Exp	(5) 10-14 Years Exp	(6) 6-9 Years Exp	(7) <5 Years Exp	(8) 1 Year Exp	(9) 2 Years Exp
lag(First ERI)	-0.126	-0.261**	-0.358*	0.248	-0.223	-0.214	-0.491	0.092	-0.332
	(0.177)	(0.117)	(0.181)	(0.274)	(0.361)	(0.439)	(0.737)	(0.289)	(0.208)
Mean of Dependent Variable	3.19	2.90	4.01	5.90	8.93	8.42	9.75	1.87	1.90
School FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	24,494	24,494	24,494	24,494	24,494	24,494	24,461	24,461	24,494
R-squared	0.804	0.741	0.743	0.740	0.776	0.786	0.853	0.554	0.593

Clustered standard errors in parentheses

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

Notes: All regressions control for % eligible for free or reduced price lunch, % English language learner, % black, % Hispanic, % Asian, enrollment, and enrollment squared. All controls vary at the school level. Standard errors clustered at the district level.

Table 4: Effect of Multiple ERIs on Total # of FTE Teachers with Varying Levels of Experience

VARIABLES	(1) 30+ Years Exp	(2) 25-29 Years Exp	(3) 20-24 Years Exp	(4) 15-19 Years Exp	(5) 10-14 Years Exp	(6) 6-9 Years Exp	(7) <5 Years Exp	(8) 1 Year Exp	(9) 2 Years Exp
lag(First ERI)	-0.124	-0.277**	-0.357*	0.328	-0.174	-0.315	-0.593	0.092	-0.321*
	(0.177)	(0.121)	(0.197)	(0.247)	(0.369)	(0.417)	(0.697)	(0.286)	(0.183)
lag(Second ERI)	-0.441*	0.023	-0.154	-0.544	-0.544	0.254	0.031	0.161	-0.153
	(0.234)	(0.166)	(0.187)	(0.445)	(0.483)	(0.450)	(0.901)	(0.263)	(0.236)
lag(Third or More ERI)	0.241	-0.106	0.084	0.765*	0.581	-0.740	-0.627	-0.083	0.148
	(0.272)	(0.177)	(0.242)	(0.449)	(0.557)	(0.593)	(1.148)	(0.307)	(0.327)
Mean of Dependent Variable	3.19	2.90	4.01	5.90	8.93	8.42	9.75	1.87	1.90
School FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	24,494	24,494	24,494	24,494	24,494	24,494	24,461	24,461	24,494
R-squared	0.804	0.741	0.743	0.741	0.776	0.786	0.853	0.554	0.593

Clustered standard errors in parentheses

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

Notes: All regressions control for % eligible for free or reduced price lunch, % English language learner, % black, % Hispanic, % Asian, enrollment, and enrollment squared. All controls vary at the school level. Standard errors clustered at the district level.

Table 5: Effect of ERIs on CST English-Language Arts Test Scores

VARIABLES	(1) All Grades	(2) Elementary	(3) Middle	(4) High School	(5) All Grades	(6) Elementary	(7) Middle	(8) High School
lag(First ERI)	0.040	0.005	0.008	0.103***	0.048*	0.010	0.012	0.114***
	(0.027)	(0.028)	(0.036)	(0.032)	(0.028)	(0.028)	(0.038)	(0.031)
lag(Second ERI)					0.020	-0.003	0.000	0.064
					(0.032)	(0.031)	(0.033)	(0.056)
lag(Third ERI +)					0.039	0.032	0.020	0.052
					(0.036)	(0.039)	(0.047)	(0.047)
School-by-grade FE	Y	Y	Y	Y	Y	Y	Y	Y
Grade-by-year FE	Y	Y	Y	Y	Y	Y	Y	Y
Observations	92,027	72,354	9,477	10,196	92,027	72,354	9,477	10,196
R-squared	0.940	0.920	0.962	0.959	0.940	0.920	0.962	0.959

Clustered standard errors in parentheses

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

Notes: All regressions control for % eligible for free or reduced price lunch, % ELL, % black, % Hispanic, % Asian, enrollment, and enrollment squared. All controls vary at the school-grade-year level, except for % free or reduced price lunch, which varies at the school-year level. Regressions weighted by school-grade enrollment. Standard errors clustered at the district level.

Table 6: Effect of ERIs on CST Mathematics, CST Algebra 1, and CST Geometry Test Scores

VARIABLES	(1) All Grades	(2) Elementary	(3) Middle	(4) High School	(5) All Grades	(6) Elementary	(7) Middle	(8) High School
lag(First ERI)	0.027	-0.047	0.004	0.092*	0.039	-0.035	0.002	0.107*
	(0.035)	(0.043)	(0.057)	(0.053)	(0.035)	(0.042)	(0.061)	(0.054)
lag(Second ERI)					0.012	-0.001	-0.072	0.065
					(0.042)	(0.055)	(0.058)	(0.068)
lag(Third ERI +)					0.072	0.070	0.036	0.082
					(0.053)	(0.071)	(0.114)	(0.065)
School-by-grade FE	Y	Y	Y	Y	Y	Y	Y	Y
Grade-by-year FE	Y	Y	Y	Y	Y	Y	Y	Y
Observations	104,149	72,354	13,229	18,566	104,149	72,354	13,229	18,566
R-squared	0.799	0.857	0.787	0.765	0.800	0.857	0.787	0.765

Clustered standard errors in parentheses

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

Notes: All regressions control for % eligible for free or reduced price lunch, % ELL, % black, % Hispanic, % Asian, enrollment, and enrollment squared. All controls vary at the school-grade-year level, except for % free or reduced price lunch, which varies at the school-year level. Regressions weighted by school-grade enrollment. Standard errors clustered at the district level.

Table 7: Effect of ERIs on CST Science Test Scores

VARIABLES	(1) All Grades CST: Science	(2) Grade 5 CST: Science	(3) Grade 8 CST: Science	(4) Grade 10 CST: Life Science	(5) All Grades CST: Science	(6) Grade 5 CST: Science	(7) Grade 8 CST: Science	(8) Grade 10 CST: Life Science
lag(First ERI)	0.069*	0.013	0.044	0.134***	0.081**	0.027	0.052	0.148***
	(0.040)	(0.039)	(0.048)	(0.049)	(0.038)	(0.038)	(0.048)	(0.047)
lag(Second ERI)					0.020	0.032	-0.000	0.031
					(0.045)	(0.042)	(0.046)	(0.062)
lag(Third ERI +)					0.063	0.065	0.046	0.076
					(0.042)	(0.046)	(0.056)	(0.055)
School-by-grade FE	Y	Y	Y	Y	Y	Y	Y	Y
Grade-by-year FE	Y	Y	Y	Y	Y	Y	Y	Y
Observations	22,802	15,523	4,226	3,053	22,802	15,523	4,226	3,053
R-squared	0.926	0.904	0.930	0.946	0.926	0.905	0.930	0.946

Clustered standard errors in parentheses

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

Notes: All regressions control for % eligible for free or reduced price lunch, % ELL, % black, % Hispanic, % Asian, enrollment, and enrollment squared. All controls vary at the school-grade-year level, except for % free or reduced price lunch, which varies at the school-year level. Regressions weighted by school-grade enrollment. Standard errors clustered at the district level.

Table 8: Effect of ERIs on CST History-Social Science and CST U.S. History Test Scores

VARIABLES	(1) All Grades	(2) Grade 8 CST: History	(3) Grade 11 CST: U.S. History	(4) All Grades	(5) Grade 8 CST: History	(6) Grade 11 CST: U.S. History
lag(First ERI)	0.104***	0.083*	0.122***	0.117***	0.092*	0.137***
	(0.036)	(0.049)	(0.037)	(0.036)	(0.054)	(0.034)
lag(Second ERI)				-0.018	-0.019	-0.016
				(0.051)	(0.041)	(0.068)
lag(Third ERI +)				0.092*	0.063	0.122**
				(0.050)	(0.067)	(0.057)
School-by-grade FE	Y	Y	Y	Y	Y	Y
Grade-by-year FE	Y	Y	Y	Y	Y	Y
Observations	8,067	4,696	3,371	8,067	4,696	3,371
R-squared	0.939	0.943	0.934	0.939	0.943	0.935

Clustered standard errors in parentheses

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

Notes: All regressions control for % eligible for free or reduced price lunch, % ELL, % black, % Hispanic, % Asian, enrollment, and enrollment squared. All controls vary at the school-grade-year level, except for % free or reduced price lunch, which varies at the school-year level. Regressions weighted by school-grade enrollment. Standard errors clustered at the district level.

CHAPTER 2

Child Evaluating the "Cliff Effect" and Female Labor Supply: Evidence from Child Care Subsidies

Introduction

Demand for child care has risen in the past several decades, in part due to the rising trend in female labor force participation rates (Bureau of Labor Statistics, 2007). As the number of working parents has increased, so has the cost of child care. According to data from the U.S. Census Bureau's Survey of Income and Program Participation (SIPP), real out-of-pocket costs of child care have nearly doubled in the last quarter of a century. Low income families are disproportionately affected by these rising costs, with families below the federal poverty line spending roughly four times the percentage of their income on child care, compared to families not in poverty (Laughlin 2013). These high costs can make it difficult for low-income families to access quality care and may prevent parents from working. Child care subsidies, funded in part by the Child Care and Development Fund (CCDF), can help to offset these costs, enabling low income parents to work, receive job training, or to further their education. However, child care subsidies may also encourage parents to restrict their labor supply so that they meet income qualifications for the subsidy benefits.

The CCDF is a federal block grant, created following the passage of the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) in 1996.[19] The stated goal of the CCDF is to help families transition from welfare to work and to keep employed

[19] The CCDF block grant consolidated several previous government programs that provided means-tested child care subsidies.

workers from becoming welfare dependent. Eligible families can receive subsidized child care services, paying only a portion of the full cost of child care. However, child care subsidies under this program only help families with incomes up to a certain income level.

Although exact rules vary from state to state, the subsidy benefit is a decreasing function of family income. Once a family's income exceeds the state's maximum income threshold, they no longer qualify for assistance and become responsible for the full cost of child care. This subsidy benefit structure creates a "cliff effect", where the increase in price of child care can greatly exceed the corresponding increase in income.[20] This "cliff effect" may have adverse effects on labor supply, particularly for single mothers. For instance, parents may restrict their work hours to avoid losing their benefit eligibility. Child care subsidies are supposed to encourage employment and decrease welfare usage, but may have an opposite effect for parents near the maximum income threshold. Thus, it is important to examine empirically whether income eligibility thresholds adversely affect income, wages, and hours of work for people near the threshold limit.

The existing literature related to child care subsidies and labor supply largely focuses on the extensive margin. In general, these studies consistently find a negative relationship between child care costs and mothers' employment (Anderson and Levine, 2000; Blau and Robbins, 1991; Blau and Tekin, 2007; Herbst, 2010) They find that receiving a child care subsidy increases the probability of parental employment, with larger effects for single mothers. However, there is a lack of research on the disincentive effects of child care subsidies for mothers near eligibility limits. Very few studies evaluate the effect of child care subsidies on other labor supply outcomes, such as hours of work, earnings,

[20] Depending on the cost of child care, marginal rates can exceed well over 100% in many states. (Child Care Policies Database, October 1, 2012 Data).

and wages. While evidence suggests that child care subsidies encourage employment, it is important to examine whether there are any negative effects on earnings for parents who want to remain eligible for their subsidies. Additionally, to my knowledge, no studies use recent state policy variation in child care subsidy rules to examine the effects of child care policy on labor market outcomes.

Using data on state child care policies and data on single and married mothers from the Survey of Income and Program Participation, I examine the "cliff effect" empirically. A key identification issue is that child care subsidy eligibility is endogenous to labor supply outcomes. Subsidies lower the cost of care, helping parents to work. However, parents may also adjust their earnings so that they qualify for the subsidies. To overcome this problem, I create an exogenous measure of eligibility by exploiting recent policy changes in income eligibility thresholds across different states to construct a simulated eligibility measure.[21] I use data from the Survey of Income and Program Participation for the years 2009-2012 (the years that I have data on child care subsidy policies).

When an income eligibility threshold is raised, mothers just above the new income eligibility threshold may lower their earnings to qualify for a subsidy. Alternatively, mothers who were previously restricting their earnings to remain eligible for the subsidy may increase their earnings after the threshold rises. I find that an increase in a state's child care subsidy generosity, primarily driven by an increase in maximum income eligibility thresholds, has a net negative effect on hours, wages, and earnings for single mothers and generally no effect for married mothers. My results suggest that when an income eligibility threshold is raised, the net effect is dominated by single mothers

[21] This measure is similar to Cutler and Gruber's (1996) simulated Medicaid eligibility instrument.

previously above the new threshold restricting their labor supply. The results on schooling and labor force participation are mixed. Taken together, I find that although child care subsidies are intended to bring parents into the workforce, the existing subsidy structure discourages work for those near the threshold limits.

Child Care and Development Fund

The passage of the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) in 1996 led to the creation of the Child Care and Development Fund (CCDF). Through this law, Congress consolidated and expanded the previously fragmented child care subsidy system into a single block grant. The CCDF was created during welfare reform to help move disadvantaged families from "welfare to work" by providing low income families child care assistance while they participate in a state-defined work activity.[22] In fiscal year 2014, the CCDF made available $5.3 billion to states, territories, and tribes. Additionally, approximately 1.41 million children and 852,900 families per month received child care assistance in 2014 (U.S. Department of Health and Human Services, Office of Child Care 2015).

The CCDF is not an entitlement program. It is part of a federal block grant and is also funded with both federal and state match funding. There are broad federal guidelines for eligibility, including income below 85% of state median income, having a child between the ages of 0-12 or with special needs, and work requirements. However, states are given flexibility in designing rules governing eligibility and benefits. For instance, states can set higher maximum income eligibility thresholds, set copayment and reimbursement rates,

[22] Work activities include employment, education, job training, and even job search in some states.

decide rules for income deductions and disregards, and define work-acceptable activities.[23] They can also decide the process for applying and re-determining eligibility.[24] As a result, there is substantial variation in subsidy benefits and rules across states and over time.

CCDF Subsidy Benefit Structure and the "Cliff Effect"

Child care subsidies feature a non-linear benefit structure. Part of the portion of care is paid by parents, called a copayment. Copayment rules vary by state, but are generally based on a sliding scale fee that rises with income.[25] The maximum amount that states pay families or child care providers for child care expenses is called the reimbursement rate. This rate depends on state rules based on income, number and age of children, and type of care. State policies distribute subsidy benefits as a decreasing function of family income, creating a non-linear benefit structure. While subsidies create a positive incentive to enter the labor force, the non-linear structure may also provide an incentive for workers to reduce their labor supply in order to qualify for a higher subsidy (Herbst, 2010; Blau, 2000).

In particular, if family income exceeds the maximum income threshold, families lose their entire subsidy eligibility and become responsible for the full cost of care. Often, the marginal increase in income is much smaller than the corresponding increase in child care cost, creating a "cliff effect". It is possible that families restrict their earnings so that they do not exceed the maximum income threshold and go over the "cliff".

[23] Federal guidelines also require states to reimburse providers at the 75th percentile of the state's market rate for child care.
[24] Re-determination for subsidy eligibility typically occurs every 6-12 months or when an individual experiences a change in service need, such as loss of employment or significant changes to income.
[25] Copayment can be calculated as a flat dollar amount, percent of income, percent of child care cost, or percent of maximum rate. Copayments vary by family size and income.

Figure 1 illustrates the "cliff effect" with a stylized non-linear child care subsidy budget set based on Blau (2000) and Yelowitz (1995). On this graph, the Y-axis represents consumption not spent on child care, and the X-axis represents leisure hours. The model assumes a mother faces a budget constraint given in equation (1):

$$C = I = Y + (w - p + s_i) * H \quad (1)$$

C is other consumption and I is income net of child care expenditures. The mother's income net of child care expenditures is a function of non-labor income (Y), hourly wage rate (w), the hourly price of child care (p), hourly subsidy rate (s_i), and hours worked (H).[26] This model assumes child care is homogenous in quality and costs are determined based on hours of care per child. As income rises, the subsidy rate decreases ($s_1 > s_2 > s_3$), creating the non-linearities in the budget set.

The mother is eligible for a subsidy up to a maximum income threshold (I_3). If income exceeds I_3, income net of child care expenditures falls sharply, creating a notch, or "cliff", on the budget set.[27] Figure 1 also illustrates the budget set after the maximum income eligibility threshold is raised, which shifts the notch further to the left to I_3'. This model yields several predictions of the effect of an increase in the maximum income eligibility threshold on labor supply decisions for people near the threshold limits.

For people just below the previous eligibility threshold (near "1" on Figure 1), they may raise their hours up to H_3' (up to "2") if they were previously restricting their earnings to stay eligible for the subsidy. People with income just above the new threshold (near "3") may reduce hours to below H_3' in order to increase their income net of child care

[26] Here, the price of care net of subsidy is the copayment amount. Generally, copayments are specific dollar amounts or percentages of income pre-set according to specific income ranges. However, a few states set copayments at a percentage of the cost of care.
[27] People with different wages face the "cliff" at different numbers of hours.

expenditures by qualifying for a subsidy. If only the maximum income threshold changes, it is unlikely that there will be an effect on employment, since the slope of the budget constraint does not change when you increase the maximum threshold. Although this simplified model assumes wage is held constant, it is feasible that parents are adjusting their earnings also through their wages.

Card and Hyslop (2004) describe a dynamic search model for welfare recipients in the Self Sufficiency Project where work subsidies can affect wages. In their model, work subsidies provide an incentive for women to accept jobs with lower reservation wages in order to meet work eligibility requirements. In the context of child care subsidies, it is possible that higher earners may choose to work at jobs with lower reservation wages in order to meet maximum income eligibility requirements. Lower wages also could come from workers wanting to lower their incomes, so they switch to lower effort jobs with more attractive conditions and better schedules.

Literature Review

The existing literature related to child care subsidies and labor supply largely focuses on the effects of child care costs and subsidies on female labor force participation rate. These studies consistently find a negative relationship between the price of child care and mothers' employment (Anderson and Levine, 2000; Blau & Robbins, 1991; Connelley, 1992; Ribar, 1992). These estimated price elasticities range from -.02 to -1.36, and tend to find larger labor supply effects for low income single mothers. The studies that examine the effects of actual subsidy eligibility (as opposed to indirectly capturing this effect through the cost of care) on employment also find a negative relationship (Berger & Black, 1992; Blau & Tekin, 2002; Herbst, 2010; Meyers, Heinze, & Wolf, 2002; Tekin, 2005).

Blau and Tekin (2007) provide an example of the standard approach in this literature on determining the effects of child care subsidy receipt on single mothers.[28] The authors use data from the National Survey of America's Families in 1999 to analyze the determinants of receipt of child care subsidies and the effects of subsidy receipt on employment. In a first-stage, the authors predict subsidy receipt using observed family characteristics, state and local child care subsidy features (county dummies), and state and local economy characteristics. Next, they use their predicted subsidy receipt to estimate the effect on employment status. They find an effect of subsidy receipt on mother's employment of 13%. However, this paper only considers the impact of subsidies on labor force participation, and does not look at any other measures of labor supply. The authors primarily use demographic correlates of subsidy receipt, but ignore different state rules.

Very few papers evaluate the effect of child care subsidies on hours of work.[29] Berger and Black (1992) use survey data from two day care subsidy programs in Kentucky to compare subsidy recipients to those on a waiting list. The authors estimate the impact of child care subsidies on labor supply and utilize Heckman's sample selection model to attempt to correct for self-selection. They find no effect on hours. However, they ignore the fact that the hour response to subsidy receipt depends on income and where the recipient is located on their budget line. More importantly, they use pre-PRWORA data, so their results can only be applied to child care subsidies before welfare reform and not to present-day CCDF subsidies.

[28] Similar papers evaluating the effect of subsidy receipt on single mothers' employment find a positive impact on labor force participation (Meyers et al., 2002; Tekin 2005, 2007).
[29] Averett, et al. (1997) evaluate the effect of the Child Care Tax Credit on labor supply decisions of married women using a structural model and find no effect on hours.

The most commonly addressed identification issue in the child care subsidy literature is that subsidy receipt is endogenous to labor supply outcomes. Subsidies lower the cost of care, thus incentivizing parents to work. However, parents may adjust their labor supply so that they qualify for the subsidy. There are also sample selection issues since parents who are more likely to seek out child care subsidies are already more likely to work and because some mothers were given higher priority based on need. Omitting factors such as motivation may bias results.

Very few papers examine recent changes in state-specific child care policies and the impact on parents. Weber, Grobe, and Davis (2014) use changes in the generosity of child care policies in Oregon from 2007-2009 to examine the effect on program outcomes. Comparing subsidy recipients before and after a dramatic expansion in Oregon's child care subsidy program, the authors find positive effects of program generosity on subsidy duration and child care usage. However, recipients prior to the program expansion may not be an adequate control group for recipients after the policy change due to the recession. Also, the authors only look at Oregon, so it is likely that their results are not externally valid since subsidy rules are very different across states.

I contribute to the literature by evaluating parent's labor supply responses to changes in state child care subsidy policies. Instead of focusing on the impact of subsidy receipt on labor force participation, I examine the effects of changes in maximum income eligibility thresholds on both single and married mothers' hours, earnings, wages, employment, and other labor supply measures. I exploit across-state variation in maximum income threshold rules for the years 2009-2012.

Data

For my analysis, I utilize data from multiple sources. Data on state child care policies come from the Urban Institute's Child Care and Development Fund Database (CCDFD), 2012. This database provides up-to-date child care subsidy policies for the 50 states, District of Columbia, and U.S. Territories and outlying areas. It includes detailed state CCDF policy information on topics related to income eligibility thresholds and other eligibility rules, copayment and reimbursement rate information, and more. The CCDFD data were collected from states' caseworker policy manuals for the years 2009-2012.[30]

To determine potential eligibility, I limit my sample to the 50 states and the District of Columbia. I use CCDFD data on family size definitions, initial income eligibility thresholds, acceptable work activities, and data on countable income and disregards. Table 1 provides summary details on state child care policies related to eligibility determination. The majority of the variation in state eligibility rules comes from changes in the maximum income thresholds since most states do not adjust other rules regarding eligibility determination during the sample period.

Figures 2 and 3 show the policy variation in maximum income thresholds across states and time. Figure 2 displays the average real maximum income eligibility threshold for each month from January 2009-October 2012, by family size. The thresholds for each family size are divided by the appropriate poverty guideline for scale. Figure 3 shows each state's annual average maximum income threshold for a family size of three. There is substantial variation in maximum income thresholds not only across states, but also over

[30] The database reports policies for most states as early as 2007.

time during my sample period. This variation in income thresholds drives the differences in my simulated eligibility measure from state-to-state over the period of 2009-2012.

Data on individuals come from the Survey of Income and Program Participation (SIPP). I use data from the 2004 wave to create my simulated eligibility measure and data from 2008 wave to evaluate the effect of child care policies. The SIPP is a series of national panels with sample sizes ranging from 14,000-37,000 households. The 2008 panel covers the years 2008-2013 and includes data on income, demographic characteristics, employment, and child care expenditures. An advantage to using SIPP data is that it tracks individuals at the month level. Subsidy spells are generally short in length and typically last less than a year (Meyers et al., 2002; Ha, 2009). It is useful to have data at the month level for my analysis to capture more frequent changes in labor supply responses and child care eligibility policies that annual data may miss. Additionally, some child care policies changed multiple times within the same year.

I restrict the SIPP to a sample of mothers with at least one child under the age of 13 for the period of January 2009-October 2012 (the period for which I have CCDF data). I first analyze the sample of single mothers over the age of 14, since previous literature suggests that they tend to respond more strongly to child care subsidies than married women. I also analyze a sample of married women over the age of 14. Mothers must live in the same household as their child. I further restrict the sample to those who are not self-employed (many self-employed had negative earnings and low reported hours), with either zero or positive monthly earnings, and with reported hours.

I am primarily interested in the outcome variables of weekly hours worked, monthly earnings, and hourly wages. In a static labor supply model where wages are fixed, women

theoretically only adjust their hours to meet income requirements. For an increase in maximum income threshold, I expect hours for mothers just above the new threshold to decrease so that their income is below the qualifying level. For mothers previously just below the threshold, when the threshold is raised, I expect their hours to increase.

There may also be an effect on wages, especially if women are unable to easily adjust their hours. Figure 4 shows the distribution of hours in my sample of single mothers. Many single mothers in my sample are working full-time (40 hours) and may be unable to adjust their hours in their current job. With large subsidy cliffs, mothers may be willing to accept lower-paying jobs to qualify for the subsidy, especially since their after-subsidy income can be higher, even with a lower wage.

I also examine the effect of subsidy policy changes on labor force participation and schooling. An individual is considered employed if they reported working at least one week in the month and had positive earnings. In most analyses, I examine whether changes in child care subsidy policies affect the probability of mothers working within the past four months.[31] I look at whether mothers were employed in the previous four months because low income families often experience frequent changes in employment over short periods of time unrelated to child care policy changes. I use the four month measure to better capture the more permanent effects of child care subsidy policy changes on employment.[32]

[31] Results for specifications estimating the effect of child care eligibility policy changes on the probability of working in the past month (as opposed to working within the past four months) are displayed in Appendix A, Table A.1 for the sample of single mothers. Although not shown, the estimates for the sample of married mothers are not sensitive to using either employment measure.

[32] Parents receiving CCDF subsidies are supposed to report changes in employment (and usually income) to their CCDF provider, typically within 10 days. Failure to report a required change in service need may result in repayment of the child care subsidy. However, job loss does not automatically disqualify a parent from the subsidy if the parent can demonstrate continued service need. Most states (38) allow parents to engage in job search if a parent loses their job after their initial eligibility determination, and parents could also enroll in school or job training (CCDF Policies Database, 2012).

I do not condition on employment when analyzing hours and earnings, so that I estimate the overall effects of the child care subsidy policy changes on these variables. Hours are defined as hours typically worked in a week at all jobs. Hours are equal to 0 for all women not working in my sample. Hours are recoded to 0 if the individual was unemployed but reported positive working hours.[33]

In the wage analyses, I condition on working and limit the sample to mothers earning an hourly wage.[34] If the individual reported two jobs with hourly wages, I used the average wage. Wages were recoded to $0 if the individual reported a wage, but also reported being unemployed (so these individuals are not included in the main wage analysis). To eliminate outliers in my sample, real wages for employed workers are restricted to be between $2-$100 and real earnings are restricted to be between $0-$10,000. See Figures A.1A-1G in Appendix A for graphs of the averages of the outcome variables over my sample period for single mothers.

Table 2 displays summary statistics for the single mothers in my sample. There are a total of 3,190 single mothers in my sample. 70% of the single mothers were employed in the past four months (64% in the past month). On average, the mothers worked 23 hours a week and earned a wage of $12.21 an hour. 14% were in school either full time or part time. The average age was 34, and the mothers were predominately white with two children. Over half of the single mothers in my sample had more education than a high school diploma.

[33] Individuals in the SIPP are asked about "typical" weekly hours worked at all jobs during the reference period. Since this variable covers a four month reference period, some mothers reported positive hours but were unemployed. Individuals were also asked to report hours typically worked at up to two different jobs. If an individual reported multiple jobs, hours were counted only for jobs from which they earned positive income.

[34] Although not shown, analyses not conditioning on work are qualitatively the same.

Table 3 displays summary statistics for the 6,883 married mothers in my sample. Married mothers in my sample were less likely to be working (62%), less likely to be in school (7%), were more educated, and had more children (2.2) than the single mothers in my sample. On average, they worked 21 hours a week and earned a wage of $15.99 an hour. The average age was 37 and the married mothers were primarily white.

Finally, to help control for business cycles, I use data on unemployment statistics from the Bureau of Labor Statistics.[35] All dollars are reported in real October 2012 dollars using the Consumer Price Index (CPI).

Method

In this paper, I examine the effect of changes in child care subsidy eligibility (driven by changes in child care subsidy eligibility policies) on single and married mother labor supply. The key problem facing my analysis is that an individual's child care subsidy eligibility is endogenous to their labor supply outcomes. Subsidy eligibility may enable more parents to work or to increase their hours since they can better afford child care. However, subsidy eligibility is also a function of earnings, wages, and hours worked. Parents may adjust their hours, wages, or earnings downwards so that they qualify for the subsidy. This endogeneity problem makes it difficult to determine the causal effect of subsidy policies on labor supply. To address this endogeneity problem, I use a type of instrument commonly used in the Medicaid literature.

Specifically, I replace actual individual eligibility with a simulated eligibility measure similar to Cutler and Gruber's (1996) simulated Medicaid eligibility instrument. I take a nationally representative sample from the 2004 SIPP and apply each state's rules from

[35] The U.S. Bureau of Labor Statistics provides data on many economic trends, including inflation and prices, unemployment statistics, productivity, and more.

2009-2012 to the same sample to determine monthly potential eligibility. My simulated eligibility variable is the total share of my sample potentially eligible for each state and month-year, determined by state rules.[36] I use data on individuals from 2004 to isolate the child care policy variation in 2009-2012, since these mothers are not adjusting their labor supply to child care policy rules from the later period.

$$Simulated\ \%\ Eligible_{st} = \frac{Total\ Potentially\ Eligible}{Total\ \#\ of\ Mothers} * 100 \qquad (2)$$

This approach is useful for several reasons. It relies only on state policy variation and is not based on variation in individual characteristics of people across states. This measure eliminates bias due to individual factors since it is not individual specific. And, it takes complicated eligibility rules and provides a simpler way to show the overall effect of the subsidy policy.

My simulated instrument is a measure of state subsidy generosity and captures the effect of policy changes in income eligibility thresholds across states and over time. See Appendix B for more details on how my simulated eligibility measure is constructed.

Since not all who are eligible receive the subsidy, I also construct a weighted measure of eligibility to help capture the actual number of slots. According to the Assistant Secretary for Planning and Evaluation at the U.S Department of Health and Human Services (2010), using CCDF state defined rules, only 28% of children receiving subsidies among the potentially eligible actually received a subsidy in an average month in 2009.[37] This is likely

[36] My simulated eligibility instrument is similar to actual technical eligibility. Table A.2 in Appendix A displays first stage results. For the states with more than 50 single mothers, actual eligibility and simulated eligibility are highly correlated.

[37] Herbst (2008) uses data on mothers from the National Survey of America's Families in 2002 to simulate state eligibility rules for 2001. He finds that 12-15% of eligible children receive assistance and recipients are more likely to be African-American, single, below the federal poverty line, and more educated. Recipients were also more likely to have young children, be employed, and work part-time.

due to lack of knowledge of subsidies and, in some states, waiting lists. State spending on child care subsidies is limited, so not all who apply for a subsidy get one. To avoid using an endogenous measure of available subsidies, I estimate the number of actual slots using data from 2004.

I weight my *Simulated % Eligible* variable by the estimated number of slots available divided by the number of children under the age of 12 with single mothers (or married mothers, for the specifications with married mothers). This weight helps to estimate the effective subsidy eligibility.

$$Weight_s = \frac{Total\ Slots\ Available_s}{Total\ Children\ Under\ 12\ With\ Single\ Mothers_{s,2005}} \quad (3)$$

where,

$$Total\ Slots\ Available_s = \frac{CCDF\ Direct\ Expenditures_{s,2004}}{Maximum\ Reimbursement\ Rate_{s,2004}} \quad (4)$$

I determine total slots available by dividing a state's CCDF direct expenditures in fiscal year 2004 by the state's monthly maximum reimbursement rate (multiplied by 12) in 2004.[38] Using the maximum reimbursement rate provides the most conservative estimate for the number of available subsidies and captures the fact that actual subsidy take-up is low. Reimbursement rates are based on a per child basis, so I divide the number of subsidies available by the state's number of children with single (or married) mothers, using data from the American Community Survey in 2005. Figure 5 shows the average *Simulated % Eligible*Weight* for each month-year for my sample of single mothers.

[38] Reimbursement rates vary based on the state, age of child, and type of care. I use the reimbursement rate for before-and-after center based care since this is the most common type of care used. If the reimbursement rate varied at the sub-state level, I used the reimbursement rate for the state's most populous region.

I regress labor market outcome variables on the weighted $Simulated\ \%\ Eligible$ variable in a reduced form model. My main specification using this weighted measure of simulated eligibility is given in equation (5):

$$Y_{ist} = \beta_0 + \beta_1 Simulated\%Eligible_{st} * Weight_s + \pi X_{it} + \rho Z_{st} + \delta_i + \theta_s + \gamma_t + \varepsilon_{ist} \quad (5)$$

Y_{ist} is the outcome variable for individual *i*, in state *s*, within month-year *t*. $Simulated\%Eligible_{st}$ is the simulated percent potentially eligible for child care subsidies for each state and month-year. My specification includes individual (δ_i), state (θ_s), and month-year (γ_t) fixed effects.[39] X_{it} is a vector of individual-by-month-year control variables, including age, age squared, number of children under the age of five, number of disabled children (defined as children receiving Supplemental Security Income (SSI)), marital status dummies, and education dummies.[40] Z_{st} is a set of state controls which vary by month and year, including the state unemployment rate.

The analysis covers the time period from January 2009 to October 2012. The outcome variables of interest are weekly hours, monthly real earnings, real hourly wage, whether the individual attends school, whether the individual is working, and whether they work part time (more than 20 hours a week), or full time (more than 40 hours a week). I also look at the log of 1 + earnings and the log of 1 + wages, to mitigate any potential problems with outliers. Other specifications control for month and year fixed effects

[39] Although not shown, specifications without individual fixed effects were qualitatively similar to specifications with individual fixed effects.

[40] Marital status dummies for the single mother sample include "Never Married", "Widowed", "Divorced", etc. Marital status dummies for the married mother sample include "Married, spouse present" versus "Married, spouse absent".

separately. Most specifications are weighted using longitudinal panel weights, although I also use SIPP Wave 1 panel weights as a robustness check.

A critical issue with using the SIPP is that it has a problem with "seam bias". Since individuals are asked information about the previous four months, there may be reporting errors. Survey responses in the SIPP tend to be the most accurate in reporting months (Moore, 2008). To address this issue, I include a dummy for whether the month reported was a seam month.

For robustness, I also use a specification without the weight:

$$Y_{ist} = \beta'_0 + \beta'_1 Simulated\%Eligible_{st} + \pi' X_{it} + \rho' Z_{st} + \delta_i + \theta_s + \gamma_t + \varepsilon'_{ist} \qquad (6)$$

Figure 6 shows the average $Simulated\ \%\ Eligible$ for each month-year, and follows a similar pattern as my weighted measure (see Figure 5). Figure 7 displays the average of the state's maximum income threshold amongst the potentially eligible in my sample for each month-year. More single mothers in my sample become technically eligible for subsidies during the recession and changes in the maximum income threshold generally correspond with changes in my simulated eligibility measures.[41]

The coefficient on $Simulated\%Eligible$ in specifications (5) and (6), β_1, is the coefficient of interest. It provides an estimate of the net effect of raising the maximum income eligibility threshold on labor supply.[42] As explained by the budget set in (1), the sign of β_1 depends on how mothers react on either side of the maximum income threshold. I expect hours, earnings, and possibly wages to decline for mothers above the new threshold. I expect hours, earnings, and possibly wages to increase for mothers below the

[41] Although not shown, the variation in simulated eligibility for married mothers is similar to the sample of single mothers.

[42] Although other state policies on acceptable work activities and counted income and disregards are used to determine potential eligibility, the majority of the policy variation comes from changes in maximum income thresholds. See Table 1.

eligibility threshold if the threshold is raised. *Ex ante*, the net effect is unclear. All states except for Utah and Oregon define school as an acceptable work activity, so I would expect to see a positive effect on schooling if parents are decreasing their income by working less and enrolling in school.[43] Finally, I expect to see no effect on employment for single mothers since changes in the maximum income threshold affect those who are already working. However, in a dual-earner household, a married mother above the new eligibility threshold could leave the labor force so that their family income would qualify for the subsidy (or for families below the new threshold, the married mother could enter the labor force).

Ideally, I would like to account for the size of the "cliff" since states with larger "cliffs" should experience stronger responses to changes in their eligibility policies relative to states with little to no "cliffs". However, it is difficult to determine the exact size of the "cliff" since it depends on the difference between the full price of care and maximum copayment amount paid, both of which are difficult to estimate. The full price of care varies widely within each state, and depends on location, the amount of child care used (full or part-time), the type of child care provider used (center-based, in-home, or other types), the number and age of children, and many other factors. It is possible that parents may switch their children to lower quality and lower cost child care if they lose their subsidy eligibility, making it difficult to determine the true price parents would pay if they exceed the income eligibility limit.

[43] Herbst and Tekkin (2011) find that child care subsidies influence single mother's decisions to invest in human capital. They find that subsidies encourage single mothers to go to school or participate in job training activities.

There is also variation in state copayment calculation policies. Most states determine the copayment based on a dollar amount or percent of income (for each family size and income category). However, a small number of states determine the copayment based on a percent of child care cost. For higher levels of income, the copayment is set at a higher percentage of the price of child care, so at the maximum income eligibility threshold, the maximum copayment paid is close to the full price of care, resulting in a smaller "cliff". Although the results are not shown, my estimates are robust to omitting these states that determine copayments as a percentage of child care costs (and generally slightly larger in magnitude).[44] My main results include all states in the analyses since even if a state determines the copayment as a percentage of child care costs, it is still likely that there is a small "cliff" since the maximum copayment does not fully equal 100% of the child care price.

Results

Single Mothers

Table 4 displays my main results for specification (5) with my sample of single mothers. Estimates are weighted using longitudinal panel weights. I present results with both month and year fixed effects and month-year fixed effects. The results are similar for both uses of fixed effects. For simplicity, I focus my discussion on the results with month-year fixed effects. For each regression, I also include the average effect (similar to an elasticity), calculated by the estimated coefficient on the weighted $Simulated\%Eligible$ variable ($\hat{\beta}_1$) multiplied by the mean of the weighted $Simulated\%Eligible$ variable and divided by the mean of the dependent variable.

[44] The states that determine copayments as a percentage of child care price in 2012 are Arkansas, Delaware, Idaho, Louisiana, Nevada, and Vermont.

Simulated%Eligible is on a 0-100 scale, so a 1% increase in weighted simulated percent eligible (driven by an increase in maximum income threshold) is associated with a 1.08 reduction in weekly hours and a $58 decrease in real monthly earnings, significant at the 5% level (Table 4, columns 2 and 6, respectively). A 1% increase in weighted simulated percent eligible is associated with a 17 cent decrease in real hourly wage, but the effect is not statistically significant (Table 4, column 10). Estimates using the log of monthly earnings and the log of real hourly wages are qualitatively similar to the estimates of the corresponding level variables, with similar average effects.

I find negative, but statistically insignificant effects of increased subsidy eligibility on the probability of going to school and the probability of working, and the average effect on the probability of working is small in magnitude (Table 4, columns 12 and 14, respectively). As mentioned previously, I define working as having a job within the previous four months to better evaluate the longer-run effects of subsidy policy changes on employment, since many low income families experience job instability and frequent employment changes over short periods of time.

Table A.1 in Appendix A displays results when defining working as having a job in the previous month. In these specifications, I find that an increase in simulated eligibility is associated with a 2.4% decrease in the probability of working (Table A.1, column 2). While I find a negative effect on the probability of working within the past month, it appears these unemployment spells are only temporary. It is possible that single mothers are leaving their jobs to find jobs with more flexible hours or wages so that they can qualify for the subsidy. Since subsidy recipients are able to engage in job training or job search in some

states, it is also possible that mothers are leaving their jobs to participate in these other activities to improve human capital investment.[45]

Single mothers are also 3.1% less likely to work part time, and I find no significant effect on the probability of working full time (Table 4, columns 16 and 18, respectively). As stated previously, figure 4 shows the distribution of hours for single mothers in my sample. There is a spike in hours at 40, suggesting that many full-time workers cannot adjust their hours. It is primarily the part-time workers, working less than 40 hours a week, who are responding to changes in the maximum income thresholds.

Results for specification (6) without weighting my $Simulated\%Eligible$ variable are presented in Table 5. Although the results change in magnitude from Table 4, estimated coefficient signs and significance levels are generally the same. I find that a 1% increase in $Simulated\%Eligible$ is associated with a .2 decrease in weekly hours and an $11 decrease in monthly earnings, significant at the 5% level, and wages decrease by $0.04, significant at the 10% level (Table 5, columns 2, 6, and 10, respectively). The elasticities for these coefficients are similar to Table 4. I find no significant effect on the probability of going to school or working, and a negative effect on the probability of working part-time.

Overall, I find that an increase in weighted (and unweighted) simulated eligibility is correlated with a decrease in hours, earnings, and wages for single mothers. Although I find a negative effect on employment when I define employment as having a job in the past month, I find evidence that suggests that these employment effects are temporary. Changes in eligibility rules do not have a significant effect on employment when employment is defined as having a job in the previous four months. These estimates represent the net

[45] The SIPP includes variables for job search and job training, but are highly underreported. Future analysis will involve looking at these variables with CPS data.

effect on raising the maximum income threshold, suggesting that single mothers above the income threshold have a stronger response to threshold changes relative to single mothers below the threshold limit.

Married Mothers

I also estimate the effect of changes in child care eligibility policies on the labor supply of married mothers. I analyze a sample of married mothers since it is feasible that they can more easily adjust their earnings if they are in a dual-earner household. The results for specification (5) incorporating a weighted measure of $Simulated\%Eligible$ are displayed in Table 6. I find no significant effects on hours, earnings, wages, or employment for married mothers. These coefficients are small in magnitude, and statistically insignificant. My results are consistent with previous child care literature that has found that single mothers tend to respond more to subsidies than married mothers (Anderson and Levine, 2000; Blau and Tekin, 2007; Herbst, 2008, etc). Child care subsidies tend to be used primarily by low-income single mothers and the married women in my sample tend to be higher earners relative to the single mothers.

I find that an increase in simulated eligibility is associated with a 3.2% increase in the probability that married mothers go to school, significant at the 1% level (Table 6, column 12). It is possible that married mothers are using the subsidy to improve human capital investment. Although not shown, these results are robust to omitting states where schooling is not an acceptable work activity.

Table 7 displays results for married mothers and the unweighted simulated eligibility measure. Again, I find no significant effects on hours, earnings, or wages for married mothers, with elasticities close to 0. An increase in simulated eligibility is

associated with an increase in the probability of going to school (Table 7, column 12). Using the unweighted measure of eligibility, I find a small positive effect on the probability of married mothers working full time (significant at the 10% level), but the elasticity is small in magnitude (Table 7, column 18).

Overall, for my sample of married mothers, I find that changes in child care eligibility policies have no net effect on the hours, earnings, or wages of married mothers. I find evidence that married mothers are more likely to go to school. Again, these estimates represent the net effect of the policy change. Finding effects close to zero does not necessarily mean that married mothers are not responding to the policy. It is possible that married mothers above and below the threshold are responding, and on net the effect is zero.

Robustness Checks

In my main specifications, I use the SIPP's longitudinal panel weights, which place more weight on the people who remain in the sample, to help account for attrition problems. As a robustness check, I also use person weights from the first wave and limit my sample to mothers that never attrit from the sample. These results using first wave panel weights are shown in tables 8 and 9 for my sample of single mothers.

Table 8 displays results for the weighted simulated eligibility measure, and Table 9 shows results for the unweighted simulated eligibility measure. In Table 8, I find a negative effect on hours and earnings, but these results are only significant at the 10% level. Using the unweighted simulated eligibility measure generally does not affect the average effect of the results. Similar to the estimates weighted using the longitudinal panel weights, I find evidence that single mothers are less likely to work part-time.

Using the first wave panel weights does not drastically change my results. Overall, the estimates are qualitatively similar, but slightly smaller in magnitude to the results weighted by the longitudinal panel weights. However, using longitudinal panel weights is more appropriate since they weight those who remain in the sample to be nationally representative. See Figure A.2 for how the sample changes annually using longitudinal panel weights.

A concern using the SIPP is that some of the individuals leave the sample. Even when using longitudinal panel weights, if attrition is related to my variable of interest ($Simulated\%Eligible$), my results will be biased. To check this, I regress whether a person attrits from the sample on my variable of interest, demographic and other controls. I use both controls that vary over time and baseline controls from the first period. Table A.3 displays the results for the weighted simulated eligibility measure, and Table A.4 displays the results for the unweighted simulated eligibility measure. I find that after controlling for individual characteristics, there is no significant relationship between (weighted and unweighted) $Simulated\%Eligible$ and attrition. The coefficients are close to 0 and insignificant, regardless of the type of control used (see columns 2-5). I conclude that attrition is not an issue when I control for individual characteristics.

Another concern using SIPP data is that some of the data are imputed and may be imputed incorrectly. As a robustness check, I leave out all imputed data. Tables A.5-A.8 present results without imputed data for my sample of single mothers.[46] I find that my results are very similar to specifications with imputed data. For instance, Table A.5 presents results for specification (5) for my sample of single mothers, without imputed

[46] Although not shown, I find that leaving out imputed data also does not affect the qualitative results for my sample of married mothers.

data, and using longitudinal panel weights. Similar to Table 4, I find a negative, and statistically significant effects for hours, earnings, and the probability of working part-time, and no effect on wages. I conclude that my analysis is not sensitive to imputation.

Conclusion

Child care subsidies seek to help low income parents to better afford child care so that they can work, go to school, or participate in other job-related activities. Although the effect of receiving a subsidy on labor force participation has been extensively studied, little is known about how subsidy policies affect labor supply in the phase-out region. In particular, the possibility of losing a subsidy due to exceeding a maximum income eligibility requirement can be a deterrent to work. This paper evaluates how an increase in child care subsidy maximum income thresholds affect the labor supply responses of single and married mothers near the income threshold.

To examine how child care subsidy maximum income qualifications affect mothers' labor supply decisions, I use data on individuals from the SIPP and exploit recent CCDF policy variation in eligibility requirements across states and over the years 2009-2012. To overcome the endogeneity of child care subsidy eligibility, I create a simulated measure of potential eligibility using a nationally representative sample of single and married mothers from the 2004 SIPP. This measure of eligibility is not individual specific, but captures the effect of changes in state child care subsidy policies.

An increase in a maximum income eligibility threshold has opposing incentives for mothers just above and just below the threshold. Thus, ex ante, the net effect on labor supply is unclear. I find that an increase in child care subsidy generosity, generally driven by an increase in maximum income thresholds, has a net negative effect on hours, earnings,

and wages for single mothers. I find some evidence that an increase in subsidy eligibility leads to a decrease in the probability of employment, but these effects are temporary. I find no significant effect on employment when employment is defined as having a job within the past four months. It is possible that single mothers are experiencing short-term unemployment while looking for jobs with more flexible hours in order to qualify for child care subsidies.

For married mothers, I find that changes in subsidy eligibility policies generally have no net effect on hours, earnings, wages, or employment. Since I am only able to identify the net effect, I cannot determine whether parents above and below the eligibility thresholds are both responding to the policy change, or whether neither groups are responding. However, from a policy perspective, it is interesting to examine the overall effect of a policy change. This paper provides evidence suggesting that when states raise an income eligibility threshold, single mothers above the threshold are more responsive to the policy change than single mothers below the threshold, resulting in an estimated overall decline in work effort.

Child care subsidies are intended to encourage work, yet the existing subsidy benefit structure creates a disincentive to work for parents near the maximum income eligibility threshold. This paper provides evidence showing that single mothers are responding to the large "cliffs" prevalent in many existing benefit structures by reducing their hours, earnings, and wages to become qualified for child care subsidies, thus increasing their income net of child care costs. To lessen these work disincentives, a less sharp phase-out of benefits (similar to the EITC) may be a better policy alternative.

Figure 1: Child Care Subsidy Budget Set

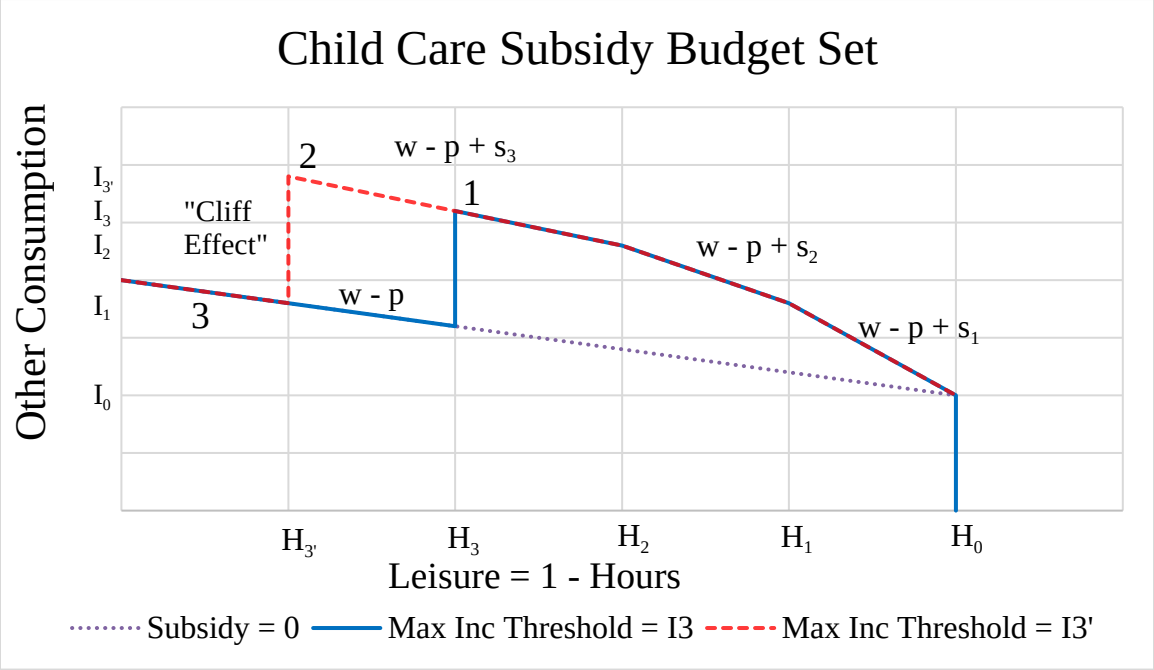


Figure 2: Average Maximum Income Threshold per HHS Guideline

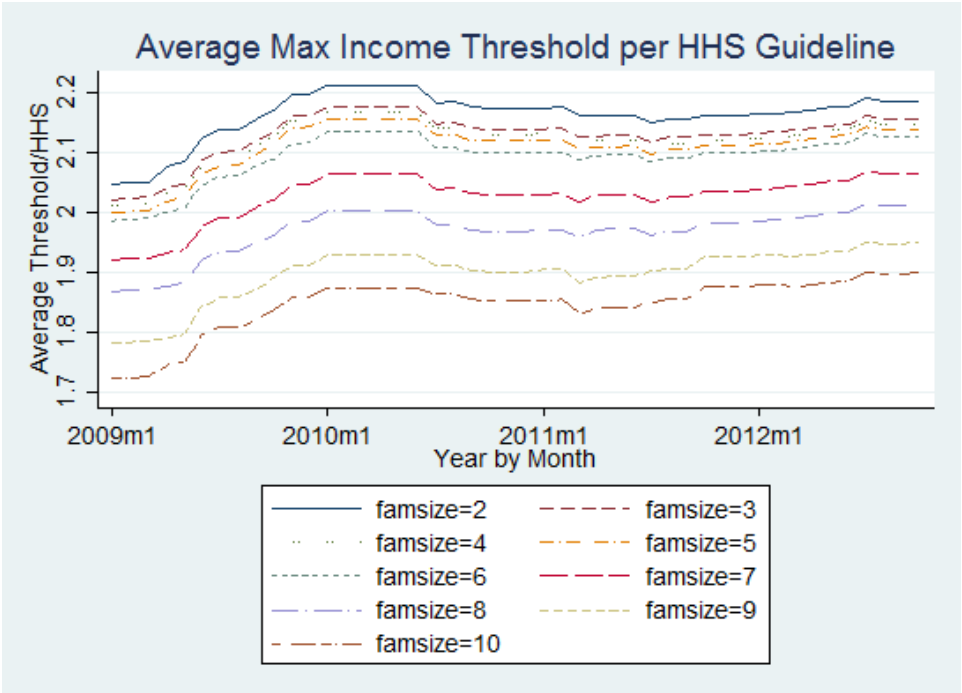


Figure 3: Average Maximum Income Threshold for Family Size = 3, by Year and State

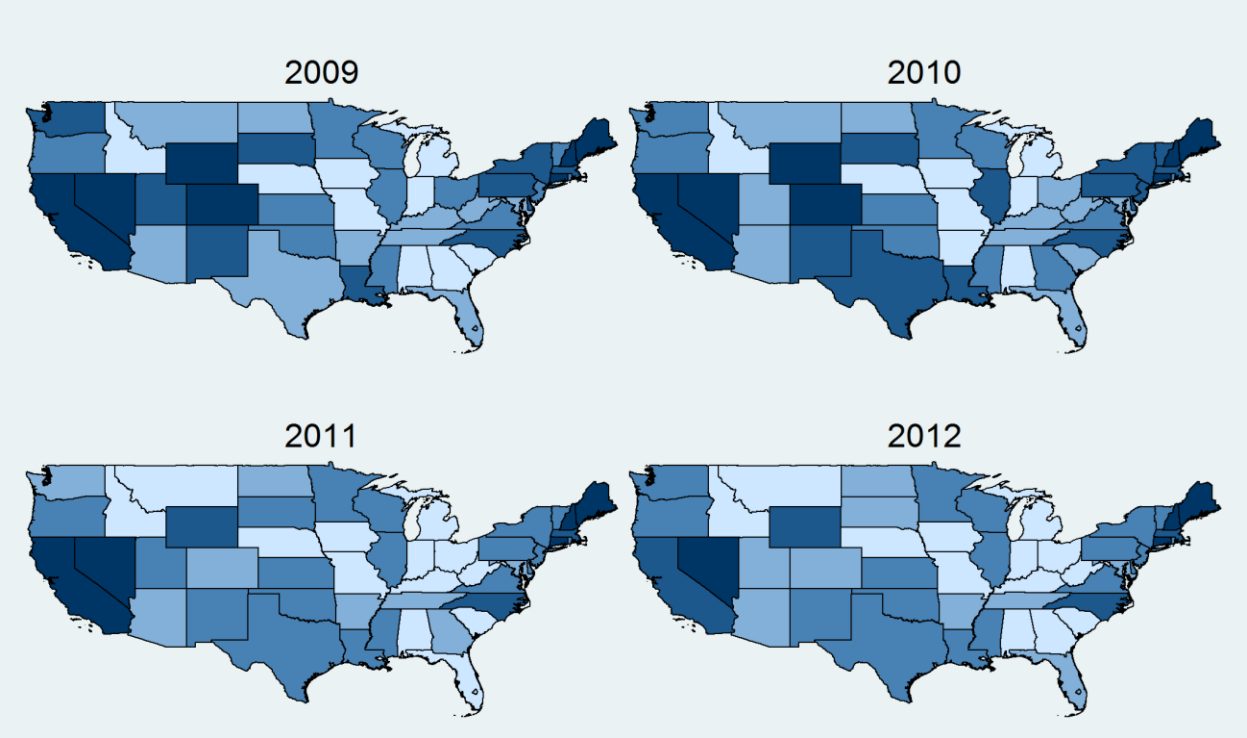


Figure 4: Histogram of Hours, Single Mothers

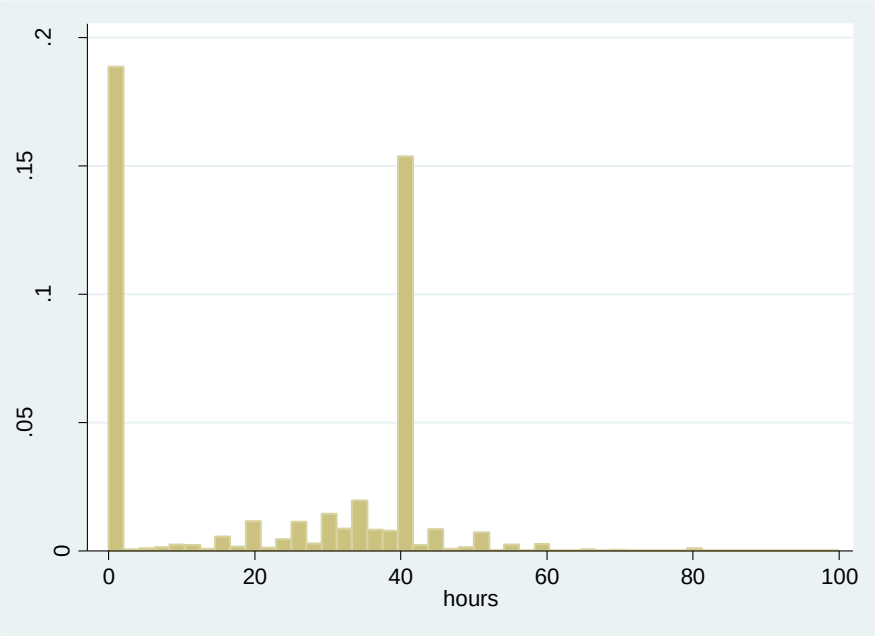


Figure 5: Average Weighted Simulated % Eligible, Single Mothers

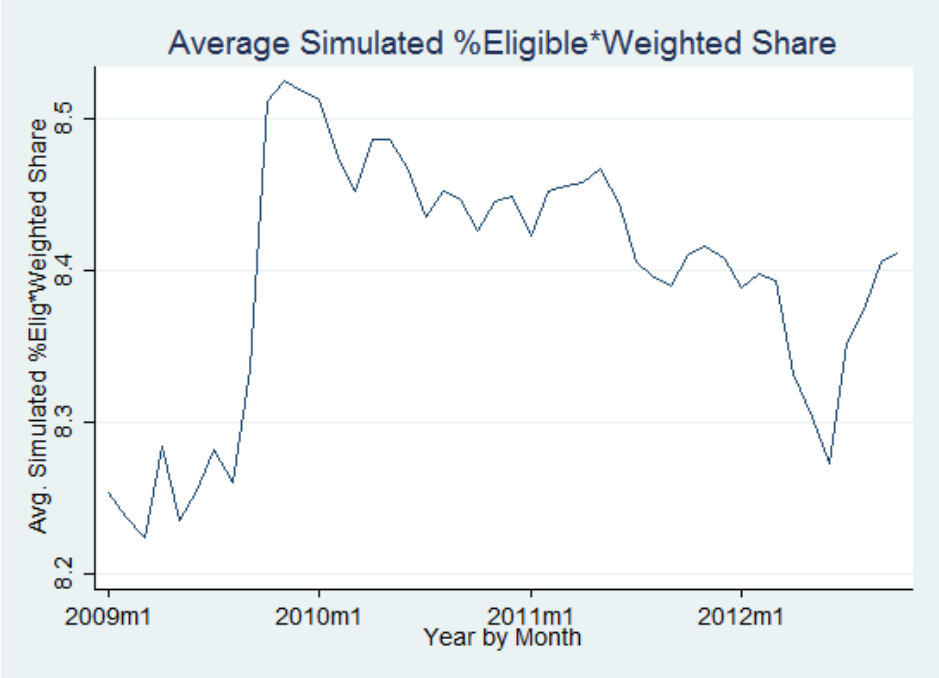


Figure 6: Average Simulated % Eligible, Single Mothers

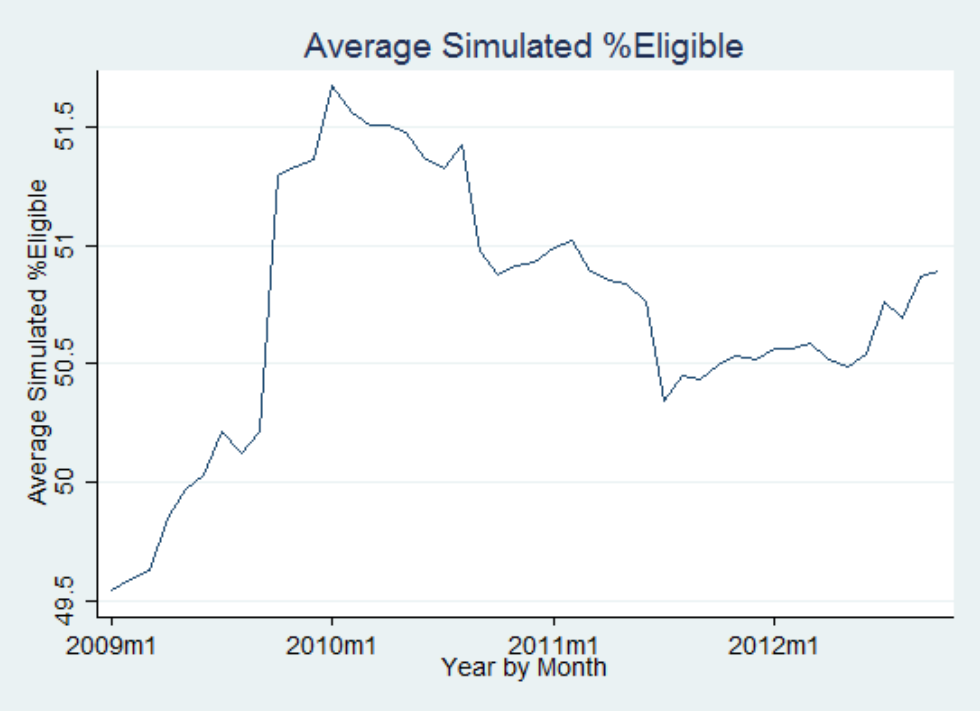


Figure 7: Average Maximum Income Amongst Eligible Families

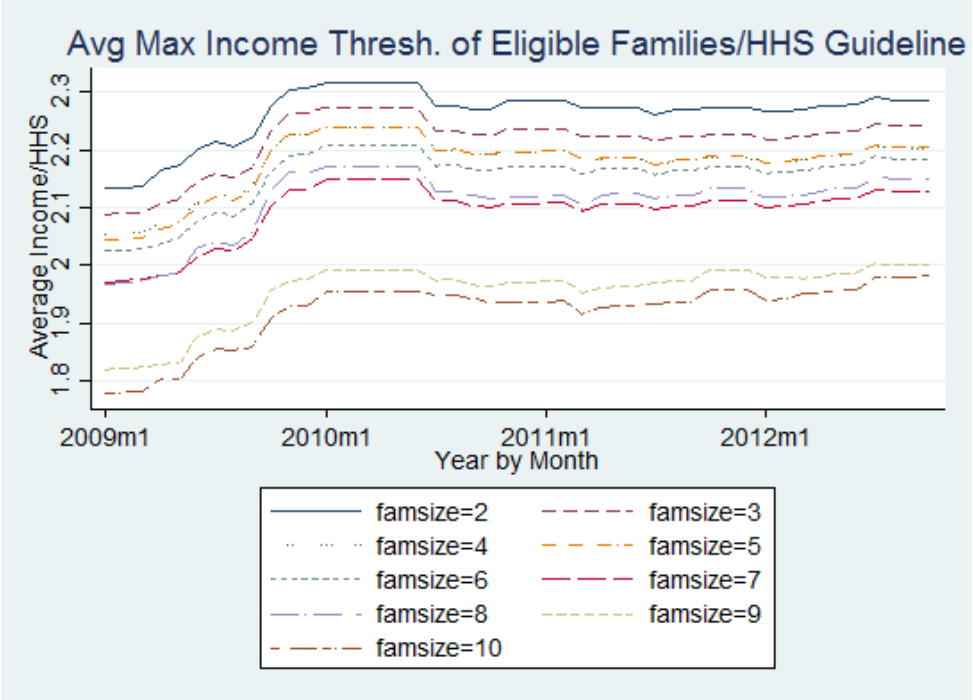


Table 1: Summary of State Child Care Policies, January 2009 - October 2012

Summary Statistics

Maximum Eligibility Threshold, Family Size = 3

Year	Mean	Std. Dev.	Min	Max
2009*	2773.60	576.11	1759.99	5129.67
2010	2865.33	620.12	1830.99	5129.67
2011	2838.99	603.94	1830.99	4524
2012	2860.47	610.52	1853.99	4524

Total # of States With Policy

Acceptable Work Activities	Year	Work Requirement	Job Search	GED	Post-secondary	Job Training
	2009**	25	21	49	44	46
	2010	22	22	49	44	46
	2011	23	22	49	44	46
	2012	24	22	49	44	46

Income and Disregards	Year	TANF	SSI	SSDI	Child Support	SNAP	General Assistance	Disregard
	2009***	24	28	42	48	1	28	4
	2010	24	28	43	48	1	27	4
	2011	24	27	43	48	1	27	4
	2012	24	27	43	48	1	27	4

Notes: Data are from the CCDF Policies Database, 2012. Sample includes 50 states and District of Columbia.

*Missing data on eligibility thresholds for 3 states for part of 2009.

**Missing data on work requirements for 4 states for part of 2009.

***Missing income/disregard data for 5 states for part of 2009.

Table 2: Single Mothers Summary Statistics, January 2009-October 2012

Variable	Mean	Std. Err.	Min	Max
Single Mothers (N=3190)				
Simulated % Eligible	46.180	0.044	33.76	62.19
Simulated % Eligible*Weighted Share	10.774	0.032	3.13	27.83
Outcome Variables				
Weekly Hours	23.298	0.119	0	100
Log(1 + Real Monthly Earnings)	4.821	0.023	0	9.21
Real Monthly Earnings	1511.873	10.564	0	10000
Log(1 + Real Hourly Wage)	1.703	0.008	0	4.52
Real Hourly Wage*	12.205	0.051	2.13	81.67
School	0.136	0.002	0	1
Working**	0.704	0.003	0	1
Part Time >= 20 Hours/Week	0.595	0.003	0	1
Full Time >= 40 Hours/Week	0.404	0.003	0	1
Age	33.519	0.048	15	80
Urban	0.800	0.002	0	1
Race				
White	0.615	0.003	0	1
Black	0.332	0.003	0	1
Asian	0.012	0.001	0	1
Other Minority	0.041	0.001	0	1
Children				
# of Children <5 Years Old	0.514	0.004	0	5
# of Children < 13 Years Old	1.616	0.005	1	8
# of Children < 18 Years Old	1.919	0.006	1	8
# of Disabled Children	0.047	0.002	0	6
Education				
Less than High School	0.164	0.002	0	1
High School Diploma	0.253	0.003	0	1
Some College	0.435	0.003	0	1
Bachelors	0.110	0.002	0	1
Post-Graduate	0.038	0.001	0	1

Notes: Data are from the Survey of Income and Program Participation, 2008. The sample includes single mothers over the age of 14. The sample excludes the self-employed, observations with undetermined work hours, and negative earnings. Furthermore, real hourly wage is restricted be $0 or between $2 and $100 and real monthly earnings are restricted to be below $10,000. Statistics are weighted by individual longitudinal weights.
* The sample restricted to working mothers
** Working is defined as having a job within the past four months. When defined as having a job in the past month, the mean is .644

Table 3: Married Mothers Summary Statistics, January 2009-October 2012

Variable	Mean	Std. Err.	Min	Max
Married Mothers (N=6833)				
Simulated % Eligible	16.851	0.019	8.43	36.86
Simulated % Eligible*Weighted Share	1.331	0.002	0.31	5.19
Outcome Variables				
Weekly Hours	20.564	0.066	0	100
Log(1 + Real Monthly Earnings)	4.493	0.013	0	9.21
Real Monthly Earnings	1776.065	7.195	0	10000
Log(1 + Real Hourly Wage)	1.687	0.005	0	4.61
Real Hourly Wage*	15.991	0.048	2.03	75.40
School	0.067	0.001	0	1
Working**	0.624	0.002	0	1
Part Time >= 20 Hours/Week	0.529	0.002	0	1
Full Time >= 40 Hours/Week	0.353	0.002	0	1
Age	36.662	0.024	17	79
Urban	0.811	0.001	0	1
Race				
White	0.838	0.001	0	1
Black	0.076	0.001	0	1
Asian	0.056	0.001	0	1
Other Minority	0.030	0.001	0	1
Children				
# of Children <5 Years Old	0.617	0.003	0	4
# of Children < 13 Years Old	1.873	0.003	1	9
# of Children < 18 Years Old	2.199	0.003	1	11
# of Disabled Children	0.006	0.000	0	4
Education				
Less than High School	0.109	0.001	0	1
High School Diploma	0.181	0.001	0	1
Some College	0.344	0.002	0	1
Bachelors	0.251	0.001	0	1
Post-Graduate	0.115	0.001	0	1

Notes: Data are from the Survey of Income and Program Participation, 2008. The sample includes married mothers over the age of 14. The sample excludes the self-employed, observations with undetermined work hours, and negative earnings. Furthermore, real hourly wage is restricted be $0 or between $2 and $100 and real monthly earnings are restricted to be below $10,000. Statistics are weighted by individual longitudinal weights.
* The sample restricted to working mothers
** Working is defined as having a job within the past four months. When defined as having a job in the past month, the mean is .579

Table 4: Effect of Simulated % Eligible on Single Mother Labor Supply, Longitudinal Panel Weights

VARIABLES	(1) Hours	(2) Hours	(3) Log (Earnings)	(4) Log (Earnings)	(5) Real Monthly Earnings	(6) Real Monthly Earnings	(7) Log(Real Hourly Wage)	(8) Log(Real Hourly Wage)	(9) Real Hourly Wage	(10) Real Hourly Wage
Simulated %Eligible*Weighted Share	-1.0860**	-1.0784**	-0.1873**	-0.1821**	-58.3139**	-57.6261**	-0.0157	-0.0167	-0.1602	-0.1745
	(0.5027)	(0.4845)	(0.0858)	(0.0852)	(23.6548)	(24.4922)	(0.0111)	(0.0115)	(0.1062)	(0.1134)
$Avg.Effect = \hat{\beta} * \bar{X}/\bar{Y}$	-0.502	-0.499	-0.419	-0.407	-0.416	-0.411	-0.067	-0.072	-0.141	-0.154
Month FE	Yes		Yes		Yes		Yes		Yes	
Year FE	Yes		Yes		Yes		Yes		Yes	
Month-Year FE		Yes		Yes		Yes		Yes		Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Single Mothers (N)	3190	3190	3190	3190	3190	3190	2022	2022	2022	2022
Observations	33,748	33,748	33,748	33,748	33,748	33,748	14,472	14,472	14,472	14,472
R-squared	0.6842	0.6848	0.7028	0.7033	0.8481	0.8482	0.8744	0.8748	0.8909	0.8912

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

Notes: Regressions include education dummies, age, age squared, marital status dummies, urban/rural status, number of children under 5, number of disabled children, and monthly state unemployment rate. Regressions also include a dummy for seam month. The sample is restricted to single mothers who are not self-employed, with determined hours, and non-negative earnings. Real hourly wage is restricted to working mothers with real wages between $2 and $100. Real monthly earnings are restricted to be $0 or up to $10,000. Part Time = Hours > 20. Full Time = Hours > 40. Simulated % Eligible*Weighted Share is on a 0 to 100 scale. All standard errors are clustered at the state level.

Table 4 (Continued): Effect of Simulated % Eligible on Single Mother Labor Supply, Longitudinal Panel Weights

VARIABLES	(11) P(School = 1)	(12) P(School = 1)	(13) P(Working = 1)	(14) P(Working = 1)	(15) P(Part Time = 1)	(16) P(Part Time = 1)	(17) P(Full Time = 1)	(18) P(Full Time = 1)
Simulated %Eligible*Weighted Share	-0.0091	-0.0092	-0.0149	-0.0134	-0.0310**	-0.0308**	-0.0126	-0.0124
	(0.0088)	(0.0087)	(0.0108)	(0.0104)	(0.0135)	(0.0125)	(0.0111)	(0.0104)
Avg. Effect = $\hat{\beta}^* \bar{X}/\bar{Y}$	-0.722	-0.730	-0.228	-0.205	-0.561	-0.558	-0.336	-0.330
Month FE	Yes		Yes		Yes		Yes	
Year FE	Yes		Yes		Yes		Yes	
Month-Year FE		Yes		Yes		Yes		Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Single Mothers (N)	3190	3190	3190	3190	3190	3190	3190	3190
Observations	33,748	33,748	33,748	33,748	33,748	33,748	33,748	33,748
R-squared	0.5163	0.5167	0.7042	0.7047	0.6523	0.6529	0.6530	0.6534

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

Notes: Regressions include education dummies, age, age squared, marital status dummies, urban/rural status, number of children under 5, number of disabled children, and monthly state unemployment rate. Regressions also include a dummy for seam month. The sample is restricted to single mothers who are not self-employed, with determined hours, and non-negative earnings. Real hourly wage is restricted to working mothers with real wages between $2 and $100. Real monthly earnings are restricted to be $0 or up to $10,000. Part Time = Hours > 20. Full Time = Hours > 40. Simulated % Eligible*Weighted Share is on a 0 to 100 scale. All standard errors are clustered at the state level.

Table 5: Effect of Simulated % Eligible on Single Mother Labor Supply, Longitudinal Panel Weights

VARIABLES	(1) Hours	(2) Hours	(3) Log (Earnings)	(4) Log (Earnings)	(5) Real Monthly Earnings	(6) Real Monthly Earnings	(7) Log(Real Hourly Wage)	(8) Log(Real Hourly Wage)	(9) Real Hourly Wage	(10) Real Hourly Wage
Simulated %Eligible	-0.1998**	-0.1961**	-0.0369**	-0.0355**	-11.3670**	-11.0957**	-0.0033	-0.0034	-0.0332*	-0.0355*
	(0.0832)	(0.0816)	(0.0159)	(0.0159)	(4.5399)	(4.7227)	(0.0020)	(0.0021)	(0.0189)	(0.0205)
Avg. Effect = $\hat{\beta}^* \overline{X}/\overline{Y}$	-0.396	-0.389	-0.353	-0.340	-0.347	-0.339	-0.061	-0.063	-0.126	-0.134
Month FE	Yes		Yes		Yes		Yes		Yes	
Year FE	Yes		Yes		Yes		Yes		Yes	
Month-Year FE		Yes		Yes		Yes		Yes		Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Single Mothers (N)	3190	3190	3190	3190	3190	3190	2022	2022	2022	2022
Observations	33,748	33,748	33,748	33,748	33,748	33,748	14,472	14,472	14,472	14,472
R-squared	0.6842	0.6847	0.7029	0.7033	0.8481	0.8482	0.8744	0.8748	0.8909	0.8912

Clustered standard errors in parentheses

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

Notes: Regressions include education dummies, age, age squared, marital status dummies, urban/rural status, number of children under 5, number of disabled children, and monthly state unemployment rate. Regressions also include a dummy for seam month. The sample is restricted to single mothers who are not self-employed, with determined hours, and non-negative earnings. Real hourly wage is restricted to working mothers with real wages between $2 and $100. Real monthly earnings are restricted to be $0 or up to $10,000. Part Time = Hours > 20. Full Time = Hours > 40. Simulated % Eligible is on a 0 to 100 scale. All standard errors are clustered at the state level.

Table 5 (Continued): Effect of Simulated % Eligible on Single Mother Labor Supply, Longitudinal Panel Weights

VARIABLES	(11) P(School = 1)	(12) P(School = 1)	(13) P(Working = 1)	(14) P(Working = 1)	(15) P(Part Time = 1)	(16) P(Part Time = 1)	(17) P(Full Time = 1)	(18) P(Full Time = 1)
Simulated %Eligible	-0.0023	-0.0024	-0.0029	-0.0026	-0.0057***	-0.0056***	-0.0022	-0.0021
	(0.0015)	(0.0015)	(0.0019)	(0.0018)	(0.0021)	(0.0020)	(0.0019)	(0.0017)
Avg. Effect = $\hat{\beta}^* \overline{X}/\overline{Y}$	-0.782	-0.816	-0.190	-0.171	-0.442	-0.435	-0.251	-0.240
Month FE	Yes		Yes		Yes		Yes	
Year FE	Yes		Yes		Yes		Yes	
Month-Year FE		Yes		Yes		Yes		Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Single Mothers (N)	3190	3190	3190	3190	3190	3190	3190	3190
Observations	33,748	33,748	33,748	33,748	33,748	33,748	33,748	33,748
R-squared	0.5164	0.5167	0.7043	0.7047	0.6522	0.6528	0.6530	0.6533

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

Notes: Regressions include education dummies, age, age squared, marital status dummies, urban/rural status, number of children under 5, number of disabled children, and monthly state unemployment rate. Regressions also include a dummy for seam month. The sample is restricted to single mothers who are not self-employed, with determined hours, and non-negative earnings. Real hourly wage is restricted to working mothers with real wages between $2 and $100. Real monthly earnings are restricted to be $0 or up to $10,000. Part Time = Hours > 20. Full Time = Hours > 40. Simulated % Eligible is on a 0 to 100 scale. All standard errors are clustered at the state level.

Table 6: Effect of Simulated % Eligible on Married Mother Labor Supply, Longitudinal Panel Weights

VARIABLES	(1) Hours	(2) Hours	(3) Log (Earnings)	(4) Log (Earnings)	(5) Real Monthly Earnings	(6) Real Monthly Earnings	(7) Log(Real Hourly Wage)	(8) Log(Real Hourly Wage)	(9) Real Hourly Wage	(10) Real Hourly Wage
Simulated %Eligible*Weighted Share	-0.1115	-0.0799	0.0216	0.0261	-11.5374	-10.5806	-0.0006	0.0005	-0.0906	-0.0765
	(0.5221)	(0.5362)	(0.1017)	(0.1032)	(33.7249)	(33.1992)	(0.0154)	(0.0146)	(0.3114)	(0.3024)
Avg. Effect = $\hat{\beta}^* \bar{X}/\bar{Y}$	-0.007	-0.005	0.006	0.008	-0.009	-0.008	0.000	0.000	-0.008	-0.006
Month FE	Yes		Yes		Yes		Yes		Yes	
Year FE	Yes		Yes		Yes		Yes		Yes	
Month-Year FE		Yes		Yes		Yes		Yes		Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Married Mothers (N)	6883	6883	6883	6883	6883	6883	3429	3429	3429	3429
Observations	103,526	103,526	103,526	103,526	103,526	103,526	31,321	31,321	31,321	31,321
R-squared	0.7961	0.7962	0.8110	0.8111	0.8819	0.8820	0.9321	0.9322	0.9438	0.9439

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

Notes: Regressions include education dummies, age, age squared, marital status dummies, urban/rural status, number of children under 5, number of disabled children, and monthly state unemployment rate. Regressions also include a dummy for seam month. The sample is restricted to married mothers who are not self-employed, with determined hours, and non-negative earnings. Real hourly wage is restricted to working mothers with real wages between $2 and $100. Real monthly earnings are restricted to be $0 or up to $10,000. Part Time = Hours > 20. Full Time = Hours > 40. Simulated % Eligible*Weighted Share is on a 0 to 100 scale. All standard errors are clustered at the state level.

Table 6 (Continued): Effect of Simulated % Eligible on Married Mother Labor Supply, Longitudinal Panel Weights

VARIABLES	(11) P(School = 1)	(12) P(School = 1)	(13) P(Working = 1)	(14) P(Working = 1)	(15) P(Part Time = 1)	(16) P(Part Time = 1)	(17) P(Full Time = 1)	(18) P(Full Time = 1)
Simulated %Eligible*Weighted Share	0.0313***	0.0322***	0.0138	0.0134	0.0003	0.0012	0.0218	0.0232
	(0.0114)	(0.0115)	(0.0145)	(0.0140)	(0.0150)	(0.0157)	(0.0207)	(0.0207)
Avg. Effect = $\hat{\beta}^* \bar{X}/\bar{Y}$	0.623	0.641	0.029	0.029	0.001	0.003	0.082	0.087
Month FE	Yes		Yes		Yes		Yes	
Year FE	Yes		Yes		Yes		Yes	
Month-Year FE		Yes		Yes		Yes		Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Married Mothers (N)	6883	6883	6883	6883	6883	6883	6883	6883
Observations	103,526	103,526	103,526	103,526	103,526	103,526	103,526	103,526
R-squared	0.5222	0.5223	0.8144	0.8145	0.7669	0.7669	0.7468	0.7469

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

Notes: Regressions include education dummies, age, age squared, marital status dummies, urban/rural status, number of children under 5, number of disabled children, and monthly state unemployment rate. Regressions also include a dummy for seam month. The sample is restricted to married mothers who are not self-employed, with determined hours, and non-negative earnings. Real hourly wage is restricted to working mothers with real wages between $2 and $100. Real monthly earnings are restricted to be $0 or up to $10,000. Part Time = Hours > 20. Full Time = Hours > 40. Simulated % Eligible*Weighted Share is on a 0 to 100 scale. All standard errors are clustered at the state level.

Table 7: Effect of Simulated % Eligible on Married Mother Labor Supply, Longitudinal Panel Weights

VARIABLES	(1) Hours	(2) Hours	(3) Log (Earnings)	(4) Log (Earnings)	(5) Real Monthly Earnings	(6) Real Monthly Earnings	(7) Log(Real Hourly Wage)	(8) Log(Real Hourly Wage)	(9) Real Hourly Wage	(10) Real Hourly Wage
Simulated %Eligible	0.0125	0.0153	0.0045	0.0049	-0.7168	-0.6267	-0.0003	-0.0002	-0.0128	-0.0119
	(0.0348)	(0.0361)	(0.0055)	(0.0058)	(1.9255)	(1.9326)	(0.0011)	(0.0011)	(0.0243)	(0.0241)
Avg. Effect = $\hat{\beta}^* \overline{X}/\overline{Y}$	0.010	0.013	0.017	0.018	-0.007	-0.006	-0.002	-0.001	-0.013	-0.013
Month FE	Yes		Yes		Yes		Yes		Yes	
Year FE	Yes		Yes		Yes		Yes		Yes	
Month-Year FE		Yes		Yes		Yes		Yes		Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Married Mothers (N)	6883	6883	6883	6883	6883	6883	3429	3429	3429	3429
Observations	103,526	103,526	103,526	103,526	103,526	103,526	31,321	31,321	31,321	31,321
R-squared	0.7961	0.7962	0.8110	0.8111	0.8819	0.8820	0.9321	0.9322	0.9438	0.9439

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

Notes: Regressions include education dummies, age, age squared, marital status dummies, urban/rural status, number of children under 5, number of disabled children, and monthly state unemployment rate. Regressions also include a dummy for seam month. The sample is restricted to married mothers who are not self-employed, with determined hours, and non-negative earnings. Real hourly wage is restricted to working mothers with real wages between $2 and $100. Real monthly earnings are restricted to be $0 or up to $10,000. Part Time = Hours > 20. Full Time = Hours > 40. Simulated % Eligible is on a 0 to 100 scale. All standard errors are clustered at the state level.

Table 7 (Continued): Effect of Simulated % Eligible on Married Mother Labor Supply, Longitudinal Panel Weights

VARIABLES	(11) P(School = 1)	(12) P(School = 1)	(13) P(Working = 1)	(14) P(Working = 1)	(15) P(Part Time = 1)	(16) P(Part Time = 1)	(17) P(Full Time = 1)	(18) P(Full Time = 1)
Simulated %Eligible	0.0021**	0.0021**	0.0013	0.0013	0.0004	0.0005	0.0025*	0.0026*
	(0.0009)	(0.0009)	(0.0010)	(0.0009)	(0.0009)	(0.0010)	(0.0013)	(0.0013)
Avg. Effect = $\hat{\beta}^* \bar{X}/\bar{Y}$	0.529	0.529	0.035	0.035	0.013	0.016	0.119	0.124
Month FE	Yes		Yes		Yes		Yes	
Year FE	Yes		Yes		Yes		Yes	
Month-Year FE		Yes		Yes		Yes		Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Married Mothers (N)	6883	6883	6883	6883	6883	6883	6883	6883
Observations	103,526	103,526	103,526	103,526	103,526	103,526	103,526	103,526
R-squared	0.5221	0.5222	0.8144	0.8145	0.7669	0.7669	0.7469	0.7470

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

Notes: Regressions include education dummies, age, age squared, marital status dummies, urban/rural status, number of children under 5, number of disabled children, and monthly state unemployment rate. Regressions also include a dummy for seam month. The sample is restricted to married mothers who are not self-employed, with determined hours, and non-negative earnings. Real hourly wage is restricted to working mothers with real wages between $2 and $100. Real monthly earnings are restricted to be $0 or up to $10,000. Part Time = Hours > 20. Full Time = Hours > 40. Simulated % Eligible is on a 0 to 100 scale. All standard errors are clustered at the state level.

Table 8: Effect of Simulated % Eligible on Single Mother Labor Supply, Wave 1 Person Weights

VARIABLES	(1) Hours	(2) Hours	(3) Log (Earnings)	(4) Log (Earnings)	(5) Real Monthly Earnings	(6) Real Monthly Earnings	(7) Log(Real Hourly Wage)	(8) Log(Real Hourly Wage)	(9) Real Hourly Wage	(10) Real Hourly Wage
Simulated %Eligible*Weighted Share	-0.9082*	-0.8478*	-0.1389	-0.1261	-47.8337*	-44.6110*	-0.0176	-0.0189	-0.1965	-0.2131
	(0.5140)	(0.4992)	(0.0894)	(0.0869)	(25.0856)	(26.3181)	(0.0155)	(0.0162)	(0.1554)	(0.1656)
Avg. Effect = $\hat{\beta}^* \overline{X}/\overline{Y}$	-0.420	-0.392	-0.310	-0.282	-0.341	-0.318	-0.076	-0.081	-0.173	-0.188
Month FE	Yes		Yes		Yes		Yes		Yes	
Year FE	Yes		Yes		Yes		Yes		Yes	
Month-Year FE		Yes		Yes		Yes		Yes		Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Single Mothers (N)	1127	1127	1127	1127	1127	1127	728	728	728	728
Observations	36,429	36,429	36,429	36,429	36,429	36,429	15,789	15,789	15,789	15,789
R-squared	0.6912	0.6915	0.7092	0.7095	0.8477	0.8478	0.8781	0.8785	0.8966	0.8969

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

Notes: Regressions include education dummies, age, age squared, marital status dummies, urban/rural status, number of children under 5, number of disabled children, and monthly state unemployment rate. Regressions also include a dummy for seam month. The sample is restricted to single mothers who are not self-employed, with determined hours, and non-negative earnings. Real hourly wage is restricted to working mothers with real wages between $2 and $100. Real monthly earnings are restricted to be $0 or up to $10,000. Part Time = Hours > 20. Full Time = Hours > 40. Simulated % Eligible*Weighted Share is on a 0 to 100 scale. All standard errors are clustered at the state level.

Table 8 (Continued): Effect of Simulated % Eligible on Single Mother Labor Supply, Wave 1 Person Weights

VARIABLES	(11) P(School = 1)	(12) P(School = 1)	(13) P(Working = 1)	(14) P(Working = 1)	(15) P(Part Time = 1)	(16) P(Part Time = 1)	(17) P(Full Time = 1)	(18) P(Full Time = 1)
Simulated %Eligible*Weighted Share	-0.0058	-0.0064	-0.0097	-0.0077	-0.0231*	-0.0217	-0.0099	-0.0090
	(0.0099)	(0.0102)	(0.0113)	(0.0110)	(0.0135)	(0.0130)	(0.0098)	(0.0094)
Avg. Effect = $\hat{\beta}^* \bar{X}/\bar{Y}$	-0.460	-0.508	-0.148	-0.118	-0.418	-0.393	-0.264	-0.240
Month FE	Yes		Yes		Yes		Yes	
Year FE	Yes		Yes		Yes		Yes	
Month-Year FE		Yes		Yes		Yes		Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Single Mothers (N)	1127	1127	1127	1127	1127	1127	1127	1127
Observations	36,429	36,429	36,429	36,429	36,429	36,429	36,429	36,429
R-squared	0.5161	0.5164	0.7075	0.7077	0.6581	0.6585	0.6615	0.6617

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

Notes: Regressions include education dummies, age, age squared, marital status dummies, urban/rural status, number of children under 5, number of disabled children, and monthly state unemployment rate. Regressions also include a dummy for seam month. The sample is restricted to single mothers who are not self-employed, with determined hours, and non-negative earnings. Real hourly wage is restricted to working mothers with real wages between $2 and $100. Real monthly earnings are restricted to be $0 or up to $10,000. Part Time = Hours > 20. Full Time = Hours > 40. Simulated % Eligible*Weighted Share is on a 0 to 100 scale. All standard errors are clustered at the state level.

Table 9: Effect of Simulated % Eligible on Single Mother Labor Supply, Wave 1 Person Weights

VARIABLES	(1) Hours	(2) Hours	(3) Log (Earnings)	(4) Log (Earnings)	(5) Real Monthly Earnings	(6) Real Monthly Earnings	(7) Log(Real Hourly Wage)	(8) Log(Real Hourly Wage)	(9) Real Hourly Wage	(10) Real Hourly Wage
Simulated %Eligible	-0.1690**	-0.1564*	-0.0282*	-0.0256*	-9.2909**	-8.5903*	-0.0034	-0.0036	-0.0385	-0.0412
	(0.0824)	(0.0807)	(0.0150)	(0.0145)	(4.5777)	(4.7719)	(0.0028)	(0.0030)	(0.0279)	(0.0300)
Avg. Effect = $\hat{\beta}^* \overline{X}/\overline{Y}$	-0.335	-0.310	-0.270	-0.245	-0.284	-0.262	-0.063	-0.066	-0.146	-0.156
Month FE	Yes		Yes		Yes		Yes		Yes	
Year FE	Yes		Yes		Yes		Yes		Yes	
Month-Year FE		Yes		Yes		Yes		Yes		Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Single Mothers (N)	1127	1127	1127	1127	1127	1127	728	728	728	728
Observations	36,429	36,429	36,429	36,429	36,429	36,429	15,789	15,789	15,789	15,789
R-squared	0.6912	0.6915	0.7093	0.7096	0.8477	0.8478	0.8781	0.8785	0.8966	0.8969

Clustered standard errors in parentheses

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

Notes: Regressions include education dummies, age, age squared, marital status dummies, urban/rural status, number of children under 5, number of disabled children, and monthly state unemployment rate. Regressions also include a dummy for seam month. The sample is restricted to single mothers who are not self-employed, with determined hours, and non-negative earnings. Real hourly wage is restricted to working mothers with real wages between $2 and $100. Real monthly earnings are restricted to be $0 or up to $10,000. Part Time = Hours > 20. Full Time = Hours > 40. Simulated % Eligible is on a 0 to 100 scale. All standard errors are clustered at the state level.

Table 9 (Continued): Effect of Simulated % Eligible on Single Mother Labor Supply, Wave 1 Person Weights

VARIABLES	(11) P(School = 1)	(12) P(School = 1)	(13) P(Working = 1)	(14) P(Working = 1)	(15) P(Part Time = 1)	(16) P(Part Time = 1)	(17) P(Full Time = 1)	(18) P(Full Time = 1)
Simulated %Eligible	-0.0017	-0.0018	-0.0019	-0.0015	-0.0043**	-0.0041**	-0.0017	-0.0015
	(0.0016)	(0.0017)	(0.0019)	(0.0018)	(0.0021)	(0.0020)	(0.0016)	(0.0015)
Avg. Effect = $\hat{\beta}^* \bar{X}/\bar{Y}$	-0.578	-0.612	-0.125	-0.098	-0.334	-0.318	-0.194	-0.171
Month FE	Yes		Yes		Yes		Yes	
Year FE	Yes		Yes		Yes		Yes	
Month-Year FE		Yes		Yes		Yes		Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Single Mothers (N)	1127	1127	1127	1127	1127	1127	1127	1127
Observations	36,429	36,429	36,429	36,429	36,429	36,429	36,429	36,429
R-squared	0.5162	0.5165	0.7075	0.7077	0.6581	0.6585	0.6615	0.6617

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

Notes: Regressions include education dummies, age, age squared, marital status dummies, urban/rural status, number of children under 5, number of disabled children, and monthly state unemployment rate. Regressions also include a dummy for seam month. The sample is restricted to single mothers who are not self-employed, with determined hours, and non-negative earnings. Real hourly wage is restricted to working mothers with real wages between $2 and $100. Real monthly earnings are restricted to be $0 or up to $10,000. Part Time = Hours > 20. Full Time = Hours > 40. Simulated % Eligible is on a 0 to 100 scale. All standard errors are clustered at the state level.

CHAPTER 3

Do State Earned Income Tax Credits Increase Program Participation at the Federal Level?[47]

Introduction

The Earned Income Tax Credit (EITC) is a federal program that provides refundable tax credits for working people with low to moderate income. Initially enacted in 1975 to help offset the regressive effects of rising payroll taxes for lower-income working families, the EITC underwent significant federal expansions in the 1980s and 1990s.[48] Its rapid expansion has made it the largest federal cash transfer program in the United States, with 26.7 million families receiving over $65 billion in cash assistance in tax year 2014 (Internal Revenue Service, 2015). The EITC is designed to primarily benefit low-income families with children, with only a small credit available to qualifying workers without children.

Existing research has generally found that the EITC boosts employment and earnings for single mothers (e.g., Eissa & Liebman, 1996; Meyer & Rosenbaum, 2001) and reduces the share of families in poverty (Neumark & Wascher, 2001; Hoynes & Patel, 2014). Other work has presented evidence of positive effects of the EITC on consumption (Goodman-Bacon & McGranahan, 2008), child and maternal health (Hoynes et al., 2012; Evans & Garthwaite, 2014), and on child achievement (Dahl & Lochner, 2012). Nonetheless, not all eligible recipients claim their benefits, with the overall take-up rate

[47] This chapter is co-authored with David Neumark.
[48] More recently, the 2009 American Recovery and Investment Act temporarily expanded the EITC for families with 3+ children and married couples.

estimated to be around 75% (Scholz, 1994; Plueger, 2009).[49] This take-up rate is relatively high compared to other social programs such as food stamps or Temporary Assistance for Needy Families (TANF), but nevertheless, improving program participation can have positive welfare effects for qualified working families (Currie, 2006).

State and local governments should be interested in maximizing participation of their constituents in the federal EITC. First, because the EITC effectively increases incomes of poor and low-income families – especially those with children – increased participation can reduce the burden on state-provided income supports to low-income families and children.[50] Second, state and local economies can also potentially benefit in the short-run from increased federal tax dollars being spent by EITC recipients, even accounting for potentially higher taxes, both because of the increased employment of recipients, and because evidence suggests that EITC eligible households increase consumption spending in the months that they are likely to receive their EITC refund (Barrow & McGranahan, 2000).

Perhaps reflecting these additional benefits, in recent years many states and local governments have implemented or expanded their own, supplemental EITCs. The first state EITC was offered in Rhode Island in 1986, and by 2015, the number of non-federal EITC programs had increased to 26 states, including the District of Columbia (Internal Revenue Service, 2015). While most non-federal EITCs have been implemented at the state level, a small number of EITCs have also been introduced at the more localized city or county level.[51] These state and smaller local EITCs supplement the federal credit and are

[49] Take-up rate here is defined as EITC recipients per eligible filer.
[50] Of course federal taxpayers ultimately have to pay higher taxes if there are more recipients, but state and local policymakers may be unlikely to internalize this cost. Moreover, they may prefer redistributional policies for which they do not have to directly raise taxes.
[51] These local government EITCs have been introduced in Montgomery County, Maryland, New York City, New York, and San Francisco, California.

generally structured as a fraction of the federal credit. Furthermore, most of the supplemental EITCs are refundable.[52]

This state EITC policy variation has been used in prior studies to examine the effect of the EITC on employment (Meyer & Rosenbaum, 2001; Neumark & Wascher, 2011), poverty (Neumark & Wascher, 2001), marriage (Dickert-Conlin & Houser, 2002), and fertility (Baughman & Dickert-Conlin, 2009). These studies do not focus on the effect of state EITCs explicitly, instead using the state EITC variation to strengthen the identification of the overall EITC effect. In this paper, we evaluate a different question – namely, whether these supplemental EITCs encourage federal EITC participation, which could provide another motivation besides the direct effect of state EITCs for state and local governments to offer these programs.

There are several reasons for why state or local EITCs could influence participation in the federal EITC program. Existing studies on the federal EITC have found that informational complexity and low program awareness contribute importantly to low EITC take-up (Chetty et al., 2013; Bhargava & Manoli, 2015; Manoli & Turner, 2016).[53] To receive the federal EITC, eligible workers must file a federal tax return, even if their income is below the federal filing requirement. Because the EITC targets low-income working families, many eligible workers may not be familiar with how to file a tax return, or even know what tax credits are available. To address these issues, the Internal Revenue Service

[52] In 2015, out of the 26 states (including the District of Columbia) that offered an EITC, 22 were either partially or fully refundable.

[53] Other possible explanations for low social program take-up include social stigma or high perceived economic costs of claiming (Bhargava & Manoli, 2015). It is unlikely that social stigma is relevant to the EITC, given that it is claimed through one's tax return, and hence participation is most likely unknown to employers or others. Although through 2010 EITC recipients could choose to get their EITC in each paycheck, nearly all chose to take their payment as a lump sum at the end of the year, which may have been to avoid stigma effects.

(IRS), states, and many local governments have engaged in outreach efforts to promote both state and federal EITCs (Internal Revenue Services, 2016).

For example, when California enacted its own EITC program in 2015, the state's tax board partnered with community-based organizations, non-profits, and other government agencies to raise awareness of both the federal and state credits.[54] Efforts included a direct mailer campaign to California taxpayers with incomes below the state filing requirement, education outreach events, and marketing materials with information about the available credits. To help taxpayers who may not know how to file for the EITC, the state also collaborated with local partners to provide free tax assistance services.[55] States or local governments that successfully promote their own EITCs should increase federal EITC participation, since in order to receive a state or local EITC, qualifying workers must file a state tax return and have already filed a federal tax return and completed the federal EITC application.

In some states, low-income individuals may be required to file a state income tax return when they do not have to file a federal tax return. For instance, a state may have a lower income filing requirement than the federal requirement. For these low-income individuals that already file a state return, they may learn about the federal EITC if the state offers their own supplemental program, either through additional EITC qualifying questions asked on their state tax return, or through increased exposure to marketing materials encouraging them to apply for both credits.

---

[54] See https://www.ftb.ca.gov/individuals/faq/net/900_media.shtml (viewed August 9, 2016).

[55] Similarly, the city of San Francisco's Working Families Credit (WFC) was created in 2004 to help promote participation in the federal EITC. In tax year 2015, the WFC offered up to $250 to qualified low-income families who had never received the federal credit before. The program also engaged in a marketing campaign to expand awareness of the EITC. See http://www.workingfamiliescredit.org/whatiswfc.htm (viewed May 29, 2016).

Finally, because the state (or local) EITC supplement to the federal EITC increases the effective wage an eligible person (most notably, single mothers) can earn, it has an unambiguously positive predicted effect on employment for single taxpayers, accompanied by higher federal EITC participation. Additionally, since EITC eligibility is based on family income, a higher state EITC may create a disincentive to work for married secondary earners, resulting in declining employment, but increased EITC participation.[56]

This paper explores whether state EITCs (and more generous state EITCs) increase participation in the federal EITC. To do this, we exploit variation in state EITC policies across states and over the tax years 1997-2008. During these sample years, there was substantial state-level EITC policy variation, but there were no major changes to the federal EITC structure. Since the federal EITC remains relatively fixed during this period, we are able to use the state EITC policy variation to identify how changes in state EITC generosity can affect federal EITC program participation.

We measure program participation using data on federal EITC recipients and expenditures, per potential filer. These variables capture the extensive (whether or not the filer claims the federal EITC) and intensive (amount of the federal EITC claimed) participation responses. Data on federal EITC recipients and expenditures come from the IRS' Statistics of Income (SOI) annual public-use samples of federal tax returns. These data do not include detailed demographic or employment information, so we also use data on individuals from the Current Population Survey Annual Social and Economic Supplement (CPS ASEC) to construct estimates of potential filers. To combine the two datasets, we aggregate the individual-level data to the state and year level. For each state-year cell, we

[56] Eissa and Hoynes (2004) find that federal EITC expansions led to a decline in labor force participation for married women, and a slight increase in labor force participation for married men.

construct the total number of EITC recipients and expenditures per potentially eligible population for samples of single filers with children, married filers with children, and childless filers.

Overall, similar to the existing literature, we find that state EITCs increase employment for single filers with children, slightly decreases employment for married filers with children, and has no effect on employment for childless filers. These employment responses suggest that individuals adjust their labor supply in response to more generous state EITC credits, which should result in higher federal EITC participation. However, our identification strategy is limited by the lack of detailed demographic information in the SOI tax filer data, and the need to aggregate the SOI and CPS ASEC data when constructing our measures of program participation. We show that aggregating the CPS ASEC data decreases the precision of our employment estimates, but we still find the same qualitative employment effects in the existing literature. However, this highlights how due to the tax filer data limitations, we may not fully capture the employment effects in our EITC participation estimates.

Our estimates on the effects of state EITCs on program participation are qualitatively similar to the theoretical predictions based on the employment responses. We find some evidence that state EITCs increase federal EITC program participation and expenditures for single filers with children. Our results suggest that the federal participation response is stronger in states with larger shares of the population that are likely to be affected by state EITC policies.

Federal and State Earned Income Tax Credits

The federal EITC is a refundable tax credit, administered through the federal tax system. Eligibility is based in part on earned income of a tax-filing unit, and qualifying income must be positive and below the maximum allowable amount. The credit amount an eligible taxpayer receives depends on the taxpayer's positive earned income and the number of EITC qualifying children.

Figure 1 illustrates the federal EITC structure for the year 2008, the last year in our sample period. The credit amount is displayed as a function of earnings for single filers with zero, one, and two or more EITC qualifying children. As shown in Figure 1, the EITC is far more generous for taxpayers with children. In 2008, the maximum credit amount available was $438 for childless taxpayers, $2,917 for EITC recipients with one child, and $4,824 for EITC recipients with two or more children.[57]

The EITC structure is characterized by three main regions. The "phase-in" region is the range of income for which the credit amount increases, equal to earned income times the applicable credit rate. During the sample years 1997-2008, the phase-in federal credit rate was 40% for eligible families with two or more children, 34% for families with one child, and 7.65% for childless taxpayers. Next, the "plateau", or "flat", region is the range of income for which the maximum credit amount is received. Finally, the "phase-out" region is the range of income for which the EITC credit amount declines with each additional dollar of earned income, until no credit is available.

In addition to the federal EITC, 26 states (including the District of Columbia) have enacted their own supplemental EITCs to help low-income working families. Generally, state EITCs are based on federal guidelines for eligibility and are structured as a percentage

[57] More recently, in 2015 the maximum credit available was $503 for childless taxpayers, $3,359 for filers with one child, $4,448 for filers with two children, and $6,242 for filers with three or more children.

of the federal EITC credit.[58] Figure 1 also shows how a 10% state supplemental EITC increases the total credit amount received by eligible taxpayers with two or more children.

There is considerable variation in the adoption of state EITCs during the sample period 1997-2008, both across states, over time, and in supplement generosity. In 1997, only nine states offered an EITC, with supplements ranging from 5% to 50% of the federal credit. By 2008, 23 states had offered an EITC, with supplements ranging from 3.5% to 40% of the federal credit.

Figure 2 displays the average supplement, expressed as a percentage of the federal credit, by year. For each year, the average supplement is displayed for all states, and for states that had a supplement in that year. The graph reflects both the rising number of states adopting the EITC, as well as the variation in generosity of the state EITCs adopted. This is also shown in Figure 3, which displays the average supplement amount by state, for various years during the sample period.

Figure 4 displays both the number of states with EITCs and the number of federal EITC filers per potentially eligible population for the sample years 1997-2008. These time series show suggestive evidence of an increase in state EITCs and corresponding increase in federal EITC filing.

State Earned Income Tax Credits and Theoretical Labor Supply Predictions

It is possible that increased publicity and outreach efforts related to state EITCs can increase federal EITC participation and expenditures. Another key avenue for how a state EITC can affect federal program participation and expenditures is through individual

[58] During the sample period, only two states did not express the state EITC supplement as a simple percentage of the federal EITC. In Minnesota, the state supplement percentage varies with income, so the average supplement amount is used (33%). In Wisconsin, the state supplement percentage depends on the number of children, so we use the supplement for families with two children (14%).

behavioral labor supply responses to the increased credit generosity. Individuals are likely to adjust their hours and earnings to take advantage of the credit.

To evaluate the effect of state EITCs on the federal program, it is important to understand the predicted labor supply responses to a state EITC. Here, we focus on the predicted labor supply effects for individuals originally not working, individuals with pre-EITC incomes within the EITC eligible range, and individuals with pre-EITC incomes just above the end of the EITC phase-out range. We discuss the predicted effects for both single and married taxpayers and then relate the predicted employment responses to the predicted changes in federal EITC participation and expenditures.

The EITC and Labor Supply Model

In a standard labor-leisure choice model, a person's labor supply decision is determined by their utility function and budget constraint. An individual receives utility from consumption of goods (M) and consumption of leisure (L). But, their consumption of goods and leisure is constrained by time and income. Their budget constraint can be written as:

$$M = Y + w(T - L) \quad (1)$$

M is the total amount spent on consumption (with a maximum of $Y + wT$), Y is non-labor income, w is the hourly wage rate, and T is total hours allocated to work (H) or leisure (L), where $T = H + L$.

Figure 5 illustrates an individual's budget constraint ("No EITC") by graphing consumption as a function of leisure hours. As leisure hours increase, hours worked decreases, and earned income decreases, until all time is spent on leisure and $M = Y$.

Figure 5 also illustrates how a federal EITC shifts an individual's budget constraint ("Federal EITC").

State EITCs are based on federal income eligibility requirements, so the budget line shares the same kink points as the budget line with the federal EITC. As shown in Figure 5, the state EITC enhances the federal credit, steepening the budget line in the phase-in and phase-out regions and increasing the maximum credit amount received ("State EITC").

Theoretical Labor Supply Predictions: Not Working Pre-State EITC

Prior to the state EITC, some individuals choose not to work due to a high reservation wage. These individuals may have a relatively higher reservation wage because of tastes for work, high non-labor income, or due to high fixed costs of working (such as child care costs). A federal EITC may not increase their after-tax effective wage enough to exceed their reservation wage and induce labor market entry. However, the additional state EITC supplement may raise their net wage enough to encourage labor market entry (shown in Figure 6). Thus, for single earners initially not working, a state EITC is expected to have an unambiguously positive effect on labor market participation.

For married dual-earner households, the predicted labor supply responses for the secondary earner (generally the wife) can differ from the responses of a single or primary earner. This is because EITC eligibility depends on total family income. If both the primary and secondary earners in a married household are initially not working, a state EITC can draw some of these individuals into the workforce as leisure becomes relatively more costly (a positive substitution effect).

If only the primary earner is working, and the primary earner's income falls in the EITC eligible range, an increase in a state EITC can be viewed as an increase in non-labor

income for the secondary earner. In this case, the effect of a state EITC on the secondary earner's labor force participation is ambiguous. The additional non-labor income effectively raises their reservation wage, creating a disincentive to work. However, the secondary earner will still enter the labor force if the positive substitution effect related to the higher after-tax wage dominates the negative income effect from higher non-labor income.

Theoretical Labor Supply Predictions: Pre-State EITC Income within EITC Eligible Range

For single taxpayers with pre-EITC income that falls within the phase-in, plateau, or phase-out range of the EITC schedule, a state EITC increase should not affect labor market participation, but can affect hours worked. For instance, for individuals with income in the phase-in region of the EITC schedule, there is an ambiguous effect on hours due to a negative income effect and positive substitution effect. If their income is in the plateau region, hours are expected to decrease due to a negative income effect. Finally, if their income is in the phase-out region, hours are also expected to decrease due to negative income and substitution effects.

For married taxpayers with pre-EITC income that falls within the EITC eligible range, the predicted effects on hours are similar to single taxpayers. However, there can also be an effect on labor force participation for the secondary earner. If a family is eligible for the EITC based only on the primary earner's income, an increase in state EITC generosity increases the secondary earner's non-labor income, creating a disincentive to work. In response to this negative income effect, the secondary earner may decrease hours or even leave the labor force.

Theoretical Labor Supply Predictions: Pre-State EITC Income above EITC Phase-Out Range

Finally, for some individuals with pre-EITC income above the phase-out region of the credit, the altered budget set can induce them to reduce their hours so that their earned income falls in the EITC eligible range. Figure 7 illustrates this case. Originally, the individual earns too much to qualify for the credit, but they are able to increase their utility by working less and receiving the EITC. Similar to single taxpayers, married secondary earners also face a disincentive to work. If the family's combined income exceeds the EITC income eligibility requirements, but falls into the EITC income eligibility range without the secondary earner's income, the secondary earner may reduce their hours or leave the labor force.

Predicted Labor Supply Responses and Federal EITC Participation and Expenditures

These labor supply responses to a state EITC can affect federal EITC participation both extensively (whether the tax unit claims the federal EITC or not) and intensively (the amount of the federal EITC claimed). Individuals with pre-state EITC incomes outside of the EITC qualifying range are incentivized by the additional after-tax EITC income to adjust their labor supply in order to qualify for the program, thus increasing program participation and federal EITC expenditures. Individuals with pre-state EITC income within the EITC qualifying range may adjust their hours (or even leave the labor force in the case of married secondary earners) due to incentives created by the kinked budget structure, which can affect federal EITC expenditures.[59]

Overall, for single and married filers with children, state EITCs are generally expected to have a positive effect on federal participation and expenditures. However, the predicted effects can vary depending on marital/filing status and where the individual's or

[59] A summary of the predicated labor supply responses and corresponding predicted federal EITC participation and expenditures responses are displayed in Appendix Table A.1.

family's income falls on the budget constraint. Furthermore, if policymakers are interested in state EITCs as a way to reduce poverty and to boost local economies through an influx of federal dollars, it is important to evaluate to what extent increases in EITC participation may be related to labor supply reductions. For this reason, we examine the employment and federal EITC participation and expenditures responses separately for groups of single and married filers with children.

An additional complication is that the predicted behavioral labor supply responses described above do not account for general equilibrium effects. Some workers may be adversely affected by the increased labor supply of EITC filers with children due to increased competition for jobs. To study the potential disemployment effects, we evaluate the employment and EITC participation responses of childless EITC filers.[60] For this group, the general equilibrium disemployment effects may decrease EITC participation and expenditures.

Data

Ideally, to examine the effect of state EITCs on federal EITC participation and expenditures, we would need data on EITC filers, potentially eligible filers, and their location on the budget constraint. Data on EITC tax filers come from the Statistics of Income (SOI) public use tax files, which are cross-sectional samples of nationally representative US Federal Individual Income Tax returns. The SOI data include information on EITC recipients and the credit amount received, filing/marital status, the number of EITC qualifying children, and state of residence. However, while rich in tax income information, the SOI data cannot be used to locate individuals on the budget

[60] Again, while a tax credit is available for childless filers, the small credit offered is unlikely to induce a significant behavioral labor supply response.

constraint. The SOI data do not include information on employment hours, wages, or useful demographic information such as age, race, sex, or education.

Alternatively, we use demographic and labor force data from the CPS ASEC to determine potentially eligible filers and to help identify where individuals are on the budget constraint. The CPS ASEC is an annual survey of households that provides information related to work, program participation, income, demographics, and more. Individuals are typically surveyed in March and are asked about income and employment in the previous year. We identify potentially eligible filers in the CPS ASEC based on EITC program qualifying rules unrelated to income, to avoid any endogenous income responses.[61]

Specifically, a household or individual was identified as potentially eligible if they had a qualifying child, defined as a child under the age of 19, under the age of 24 and a full-time student, or permanently disabled. The CPS ASEC only includes information on children living at home, but EITC eligibility is also based on qualifying children living at home. Potentially eligible childless filers were identified as household heads between the ages of 25 and 65.

The CPS ASEC data is also used to measure the share of workers likely to be affected by a state EITC. Among working individuals, low-skilled workers are more likely to be on the phase-in region of the EITC budget constraint, relative to high-skilled workers. As a proxy for low-skilled, we use data on individual's education from the CPS ASEC. We define low-skilled as having no more education than a high school degree.

[61] It is important to note that a true estimate of EITC take-up would be based on actual eligibility (EITC filers per eligible filers). However, since we are interested in how state EITCs induce federal EITC participation through employment, this measure would not be appropriate, as both EITC filers and eligibility would respond. Thus, while our potentially eligible measure overestimates the true eligible population, it avoids any endogenous responses to changes in the state EITC.

To combine the individual-level tax filer data with the individual-level data from the CPS ASEC, both datasets are aggregated to the state and year level.[62] Prior to aggregating the SOI data, we restrict the sample to exclude all high-income filers, for which there are no state identifiers due to confidentiality reasons. We exclude filers from Puerto Rico, Guam, the Virgin Islands, or U.S. citizens and military personnel living abroad, since these filers are all assigned the same geographic identifier. Finally, the tax filer sample excludes late filers.

Using the aggregated CPS ASEC and SOI data, we construct estimates of EITC recipients and EITC expenditures per potentially eligible population for single filers with children, married filers with children, and childless filers.[63] We evaluate these groups separately since participation responses may be more meaningful for single filers with children, because state EITCs are predicted to induce labor market entry.

Additionally, the CPS ASEC data are used to construct state-year level estimates of employment and various demographic measures for each group, including the share of the population with low-skill and the share of the population that is female, Hispanic, or black.

These data are combined with data on state unemployment rates, state and federal minimum wages, and data on historical state EITC parameters (our policy variation). The historical EITC parameters are taken from the Center on Budget and Policy Priorities and are expressed as a fraction of the federal credit. Additionally, existing research suggests that minimum wage effects may not be immediately apparent, so we use the average of the

[62] This is similar to the procedure used by Bitler et al. (2015) in their analysis of EITC caseloads and business cycles, except the authors aggregated the data into cells by state, year, marital status, and number of EITC qualifying children.

[63] Single includes individuals who reported their marital status as head of household or widowed. Since tax filers who file as married filing separately cannot claim the EITC, the SOI excludes these filers, and the CPS ASEC excludes individuals who report being married, but spouse absent.

current and lagged year's minimum wage (defined as the higher of the state or federal minimum wage).

The sample period covers the years 1997-2008. These are the years for which we have data on state EITC policies and for which the federal EITC structure remains unchanged.[64] Table 1 displays summary statistics for the distribution of federal EITC tax filers, our measures of federal EITC participation (EITC Filers and Expenditures per Potentially Eligible Population), and our state EITC policy variables. The majority of the EITC recipients are single filers with children. In 2008, 59% of federal EITC filers were single with children, and they received 74% of all federal EITC expenditures. Childless filers only received about 3% of all federal EITC dollars.

Empirical Approach and Specifications

Examining the Effect of State EITCs on Employment

First, to confirm our theoretical labor supply predictions, we attempt to replicate earlier work from Neumark & Wascher (2011) evaluating the effects of state EITCs on employment. If we wanted to precisely estimate the effect of state EITCs on employment, it would be better to use the greater sample variation provided by the individual-level CPS ASEC data. However, our goal is to determine if we can identify the state EITC employment effects, when we impose the SOI data constraints.

Before fully restricting our data to the constraints imposed in the SOI data, we aggregate the CPS ASEC data to cells that vary by state, year, number of kids (0, 1, 2, 3+), and skill-level (low-skilled or not). We then estimate the following difference-in-

[64] Beginning in 2009, the EITC was expanded to include 3+ children.

difference-in-differences specification, which is a more aggregated version of the specification estimated in Neumark & Wascher (2011):

$$Y_{stkl} = \alpha + \beta_1 EITC_{st} + \beta_2 EITC_{st} \cdot Kids_{stkl} + X_{stkl}\pi + \gamma_s + \lambda_t + \varepsilon_{st} \quad (2)$$

Y_{stkl} is the average employment rate for state s in year t for group k (0, 1, 2, 3+ kids) and for skill level l (low-skilled or not). $EITC$ is the state EITC expressed as a fraction of the federal credit, and is equal to zero if the state did not have an EITC. $Kids$ is a dummy variable equal to one if the number of kids cell is greater than zero, and is zero otherwise. X_{stkl} is a matrix of state-year-kids-skill group controls, including the share that is black, share Hispanic, the share of the sample with young children, the share that are high school drop outs, and average age. The specification also controls for group dummy variables for 1, 2, and 3+ kids, and state unemployment rate.

State fixed effects (γ_s) are included to control for unobservable differences across states that may be correlated with EITC adoption. Year fixed effects (λ_t) control for other time-varying factors that are common to all states but may be correlated with state EITC policy changes, such as the national business cycle, or changes to other federal policies. To account for arbitrary patterns of serial correlation within states, and heteroscedasticity across states, standard errors are clustered at the state level. The employment regressions are weighted by the number of observations in each cell.

We analyze specification (2) for a sample of all single women, and a subsample of low-skilled single women. We also restrict the sample to single mothers, replacing the $EITC$ and $Kids$ interaction with an $EITC$ and $Lowskilled$ interaction. We repeat the analysis for a sample of married women.

We then restrict the CPS ASEC data to match the data constraints in the SOI data, aggregating the CPS ASEC data to cells that vary at the state-year level. We estimate the following difference-in-differences specification for the samples of single filers with children, married filers with children, and childless filers.

$$Y_{st} = \alpha + \beta_1 EITC_{st} + \beta_2 EITC_{st} \cdot Lowskilled97_s + X_{st}\pi + \gamma_s + \lambda_t + \varepsilon_{st} \quad (3)$$

These variables are similar to before, only now vary at the state-year level. However, $Lowskilled97$ is the share of low-skilled workers for each sample group in 1997, defined as having an education level no higher than a high school degree. This low-skilled measure is a proxy for the share of the state's population likely to be located near the phase-in region of the EITC budget constraint. The 1997 low-skilled baseline value is used to avoid potentially endogenous responses in state populations, although estimates are also reported for the contemporaneous low-skilled share.[65] We also define the low-skilled share in some specifications to be the share low-skilled with two or more children, to more clearly identify groups that are likely to respond strongly to EITCs.

In the specifications with the EITC and low-skilled share interactions, the variables are demeaned. So, in specification (3), β_1 represents the effect of a state EITC for states with an average low-skilled share in 1997. The predicted effects of state EITCs on employment, federal participation, and expenditures should be stronger in states with larger shares of the population that are potentially affected by a state EITC. This effect should be captured by the positive coefficient β_2.

Federal EITC Participation and Expenditures

[65] Estimates using the state's sample average low-skill share are reported in the Appendix Tables A.2-A.4.

We then evaluate the effect of state EITCs for our main outcome variables. We estimate (3), but replace the employment rate with our corresponding EITC participation measures, EITC filers per potentially eligible population, and EITC expenditures per potentially eligible population. We estimate (3) separately for the sample groups identified by the SOI data: single filers with children, married filers with children, and childless filers. Estimates in our EITC participation regressions are weighted by the sample's population of potentially eligible filers for each state-year cell.

The main identifying assumption is that conditional on state and year effects and economic and demographic controls, the timing of the introduction and expansions in state supplemental EITCs is not correlated with other omitted factors that may affect the EITC filing share.

Results

Examining the Effect of State EITCs on Employment

Table 2 reports the results from specification (2), estimating the effect of state EITCs on the employment rate of single and married women between the ages of 21 and 44 for the years 1997-2008. Column 1 reports the estimates for all single women, column 2 reports the estimates for the subsample of low-skilled single women, and column 3 reports the estimates for the sample of single mothers. The employment and EITC policy data are on a scale of 0 to 1. So, focusing on column 1, a 10% increase in a state EITC supplement is associated with a 1.6 percentage point increase in the employment of single mothers, relative to single women without kids. However, while the magnitude of this estimate is similar to the existing literature, this estimate is statistically insignificant. When we restrict the sample to low-skilled single women, results are similar to column 1. In column

3, for the sample of single mothers, a 10% increase in a state EITC is associated with a 2.5 percentage point increase in employment for low-skilled single mothers, relative to non-low-skilled single mothers (significant at the 10% level).

For married women, the estimates in Table 2 suggests that state EITCs have a negative effect on the employment of married women with children, relative to married women without children. However, the estimates indicate a positive employment effect for low-skilled childless married women, which are mostly offset by the negative employment responses from married mothers. In column 4, a 10% EITC supplement increases the probability of employment by 1.3 percentage points for childless married women, but decreases employment by .6 percentage points for married mothers. In column 5, restricting the sample to low-skilled married women, we find that the EITC increases employment for low-skilled childless married women, but has no effect on the employment of low-skilled married mothers. These estimates overall suggest that the EITC may draw some married childless women into the labor force, but also may incentivize some married mothers to leave the labor force (perhaps to stay home with the children).

The estimates in Table 2 generally replicate the qualitative results of the existing literature, finding a positive employment response for single mothers, and a small negative response for married mothers, although the evidence for married mothers is mixed. Aggregating the data makes our estimates less precise, but the estimates are generally consistent with the literature, especially for single mothers.

Next, we report the estimates for the specifications that aggregate our data to the state-year level (which matches the data constraints imposed by the SOI data). Table 3 reports the estimates related to specification (3), estimating the effect of state EITCs on the

employment rate of single individuals with children. In column 1, we do not control for the share low-skilled. We find a positive and significant coefficient on the state EITC variable, suggesting that a 10% state EITC supplement increase is associated with a 1.6 percentage point increase in the employment of single individuals with children. In column 2, the contemporaneous share low-skilled variable is added, and column 3 includes the interaction between the EITC and contemporaneous share low-skilled variables. The main EITC estimates do not change much, even after including the interaction term. Column 4 is similar to column 3, but redefines the share low-skilled to be the share low-skilled with two or more children, for whom the EITC is more generous, and for whom we expect a stronger employment response. Unexpectedly, the estimated coefficients on the share low-skilled and EITC interaction terms in columns 3 and 4 are negative, but these estimates are very imprecise. However, using a contemporaneous low-skilled share is likely not appropriate, since the share low-skilled population may be responding endogenously to the state EITC policy itself. Instead, we replace the share low-skilled variable with its 1997 baseline value.

Columns 5 and 6 report the estimates using the 1997 baseline values for the share low-skilled and share low-skilled with two or more children. The estimated coefficients are now positive, but are only statistically significant for the share low-skilled with 2+ children in 1997. In column 6, a 1% increase in the state EITC supplement is associated with a .16 percentage point increase in employment for single individuals with children in states with average shares of low-skilled individuals with 2+ children. A 1% increase in the state EITC combined with a 1% increase in the share low-skilled with 2+ children in 1997 is associated with an additional .81 percentage point increase in the employment of single parents. Qualitatively, these estimates suggest that the EITC has a positive effect on the

employment of single individuals, especially for low-skilled single individuals with two or more children.

Table 4 reports the estimates for specification (3), but for the sample of single women with children. The SOI data do not include information about the sex of the filer, so Table 3 is the more appropriate comparison sample for the EITC participation regressions. However, we restrict the sample to single mothers in Table 4, since the existing literature largely examines EITC effects for this group. Focusing on the share low-skilled from the baseline year of 1997, we find that the state EITC has a positive effect on the employment of single mothers in states with average share of low-skilled individuals. Although the interaction term between the EITC and share low-skilled in 1997 is negative, but insignificant, when we interact the EITC variable with the share low-skilled with 2+ kids in 1997, the estimated coefficient is positive. But, these estimates on the low-skilled interactions are more imprecise with the higher level aggregated data.

Tables 5 and 6 report the employment results for the samples of married individuals with children, and married women with children, respectively. For both samples, the estimates suggest possible small disemployment effects of the state EITC on average, but the estimates are generally insignificant. Finally, Table 7 reports the estimated state EITC effects on employment for the sample of childless individuals. These estimates suggest that state EITCs do not have a significant effect on the employment of childless individuals, with the main EITC coefficient estimates being close to zero.

Overall, when we allow for additional sample variation in specification (2) (reported in Table 2), we generally replicate the existing literature's employment effects. State EITCs are associated with a positive employment response from single mothers, and

this employment response is largely coming from low-skilled single mothers. For married individuals, the overall employment response is less clear, but usually slightly negative for married mothers with children. However, the more we aggregate the data, the less precise our estimates get. These results highlight some of the potential limitations of using the aggregated SOI data. The results imply that while our estimates still tend to support the existing EITC employment literature findings, we may not fully capture the employment effects on federal EITC participation when using aggregated data.

Main Results: Examining the Effect of State EITCs on Federal EITC Participation and Expenditures

Tables 8a and 8b report the estimated effect of state EITCs on federal EITC participation for single filers with children, and on federal EITC expenditures for single filers with children, respectively. We focus on the estimates using the share low-skilled in the baseline year 1997. In Table 8a, columns 6 and 7, a 1% increase in a state EITC in states with an average shares of low-skilled single parents (and average shares of low-skilled single parents with 2+ kids) is associated with a .18 percentage point increase in federal EITC filers per potentially eligible population (but this estimate is statistically insignificant). The positive coefficient on the low-skilled and state EITC interaction terms suggest that the state EITC effect on federal EITC participation is estimated to be larger in states that have relatively greater shares of low-skilled individuals. In column 7, a 1% state EITC increase and a 1% increase in the share of low-skilled individuals with 2+ kids is estimated to increase EITC filers per potentially eligible population by 8.5 percentage points. This effect seems large in magnitude, especially since the estimated employment effects are much smaller (.81 percentage points in Table 3, column 6).

Similarly, for the sample of single filers with children, we find a positive effect of state EITCs on federal EITC expenditures (reported in Table 8b). The EITC expenditures are reported in thousands of dollars. So, in column 7, a 1% increase in a state EITC and a 1% increase in the state's share of low-skilled workers with 2+ children is associated with a $23,000 increase in federal expenditures per potentially eligible filers, a very large effect. Even when considering that state EITCs may increase federal program participation in other ways besides employment (such as through better publicity), these estimated effects seem implausibly large. But, although large, the federal participation estimates follow the same qualitative story as the employment estimates, with larger employment responses corresponding with larger federal EITC participation responses.

Table 9a reports the estimates for the effect of state EITCs on federal EITC filers for the sample of married filers with children. In the specifications using the baseline low-skilled shares, we find no significant effects of a state EITC on married federal EITC filers. Previously, we found evidence state EITCs encouraging married parents to the labor force. This does not necessarily mean that we should see an extensive EITC participation effect if the married couple already receive the EITC before the state EITC expansion. Table 9b reports the estimates for the outcome variable federal EITC expenditures per potentially eligible population. In Table 9b, column 6, a 1% state EITC in states with average shares of low-skilled workers decreases federal EITC expenditures by $310 per potentially eligible filers, but the main state EITC effects are not significant across other specifications. While negative, these estimates are not as implausibly large as the estimates for the single filers with children sample.

Finally, we report the estimates of the effect of state EITCs on federal program participation for the sample of childless filers. A state EITC is estimated to have very small positive effects on federal EITC filers and expenditures for the sample of childless filers. We generally found no employment effects for childless filers (Table 7), suggesting that while a state EITC may induce some childless filers to take up the federal credit, these participation responses are not from the childless filers entering the labor force. Nor do we find evidence that suggests disemployment general equilibrium effects are forcing childless individuals to lose their EITC eligibility, since EITC filers per potentially eligible population did not decline. However, this does not necessarily mean that childless individuals do not suffer from general equilibrium effects. It is plausible that the increase in federal EITC participation and expenditures could be due to lower incomes resulting from employers reducing the available hours of childless individuals.

Conclusion

Existing research on the federal EITC has linked the program to many positive labor supply and welfare outcomes for low to moderate income families. At the state and local government level, smaller supplemental EITCs have become increasingly popular. These supplemental EITCs enhance the federal credit by providing additional income support to low-income working families. While both individuals and states can benefit from increased participation in the federal EITC through decreased poverty, economic benefits from increased spending of federal tax dollars, or other mechanisms, it has been previously unclear whether these smaller EITCs affect federal program participation.

In this paper, we explore whether state EITCs boost federal EITC participation, measured by federal EITC filers and federal EITC expenditures, per potential filer. Our

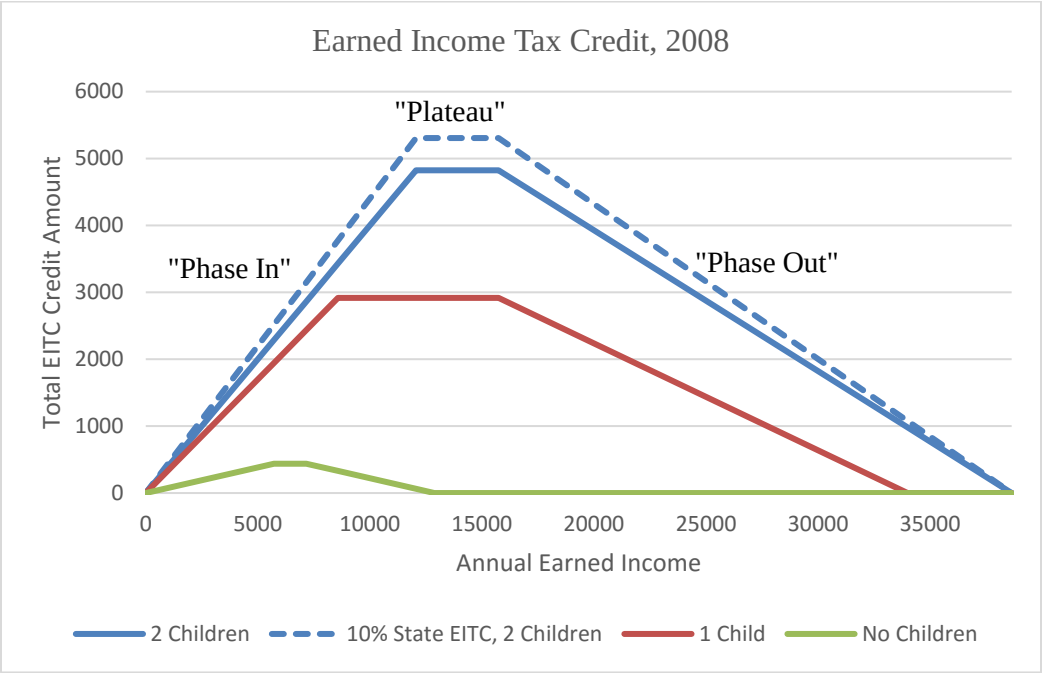
measures of EITC participation require us to use data from two sources. Specifically, we use data on tax filers from the IRS' Statistics of Income and demographic and employment data (used to estimate the population of potentially eligible filers) from the CPS ASEC. To combine these datasets, we aggregate individual-level data to the state-year level.

Using these aggregated data, we re-examine estimates from the existing EITC and employment literature, estimating the effect of state EITCs on employment for single filers with children, married filers with children, and childless filers. Similar to existing research, we generally find that EITCs encourage work for single mothers and discourage work for married mothers, which should lead to increased federal EITC participation for these groups. However, when using the aggregated data, our estimates are less precise. This suggests that due to the data constraints imposed by the tax filer data, our EITC participation estimates may be imprecise.

In our analysis of the effects of state EITCs on federal EITC recipients and expenditures per potential filers, we find that state EITCs increase federal program participation primarily for single individuals with children. Similar to the employment results, we find evidence that the effect of state EITCs depends on the state's population of low-skilled workers, a measure for the share of the population that is likely to be affected by the state EITC. Our estimates imply that the effect of state EITCs on federal program participation is larger in states with greater shares of potentially affected populations. However, our estimates for the sample of single individuals with children are very large. While the aggregated data may not clearly capture the effect of state EITCs on federal program participation, our estimates point to positive increases in participation and expenditures for single filers with children.

Future work will involve examining the interactive effects of state minimum wage policies on federal EITC participation. In a competitive labor market, a higher minimum wage is predicted to decrease employment, making fewer people eligible for the EITC. States may be concerned about this minimum wage effect because of the potential decrease in welfare and decrease in federal tax dollars coming into the state. We will explore how state minimum wages, coupled with an EITC, affects federal EITC expenditures and participation. Finally, we also will explore how differences in state filing requirements may influence federal program participation.

Figure 1: Federal and State EITC Parameters, 2008



Notes: The EITC schedule is based on parameters for single filers from the year 2008.

Figure 2: Average EITC Supplement (Fraction of Federal EITC), 1997-2008

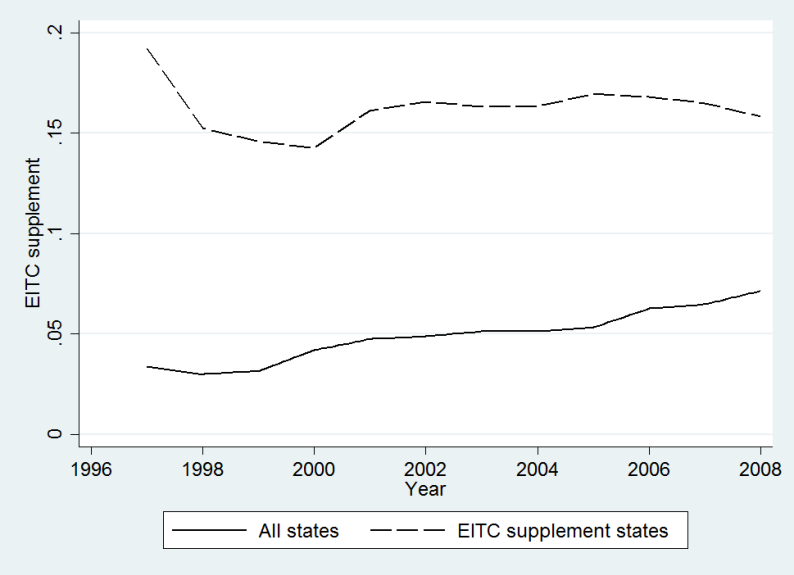
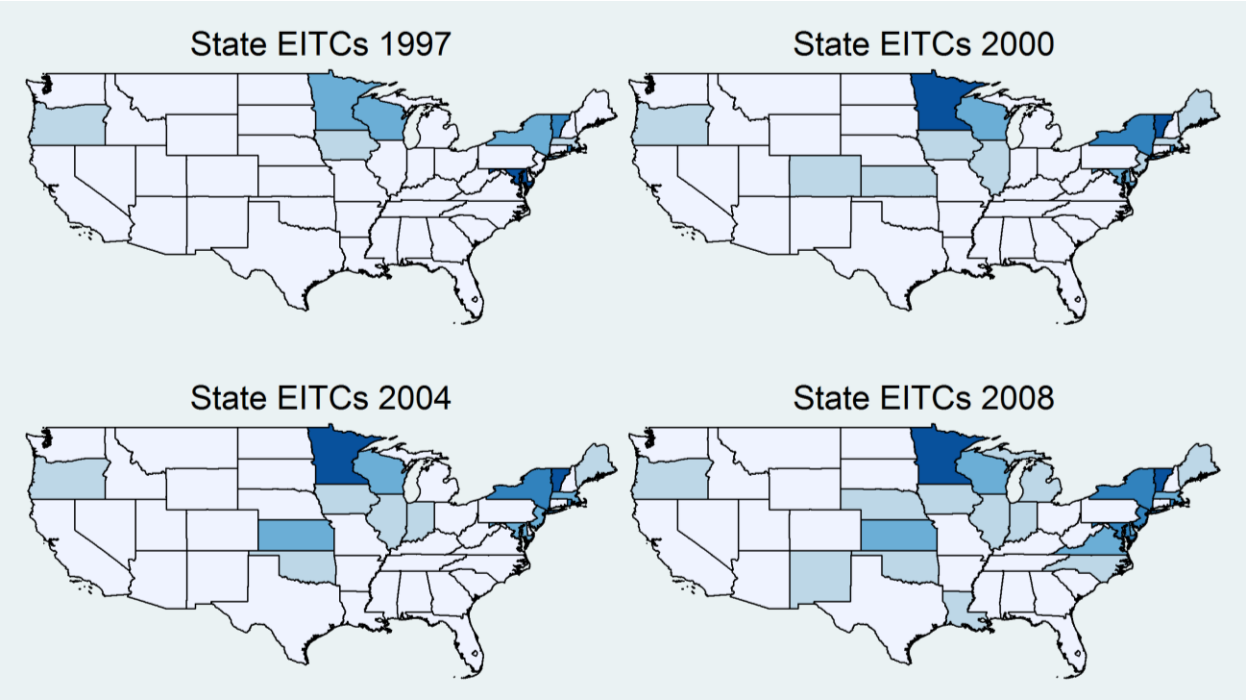


Figure 3: Average EITC Supplement (Fraction of Federal Credit), by State and Year



Legend:

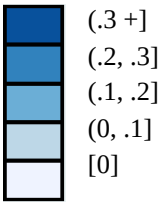


Figure 4: Number of State EITCs and Federal EITC Filers per Potentially Eligible Population, 1997-2008

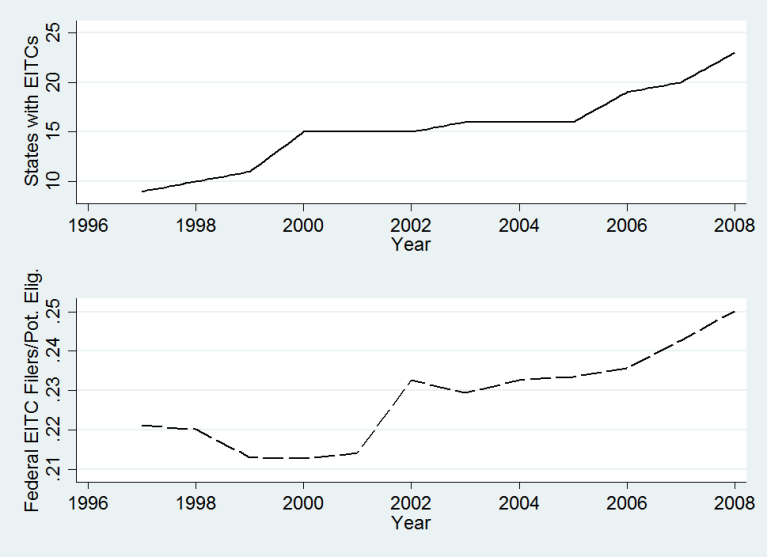


Figure 5: Federal and State EITCs and the Budget Line

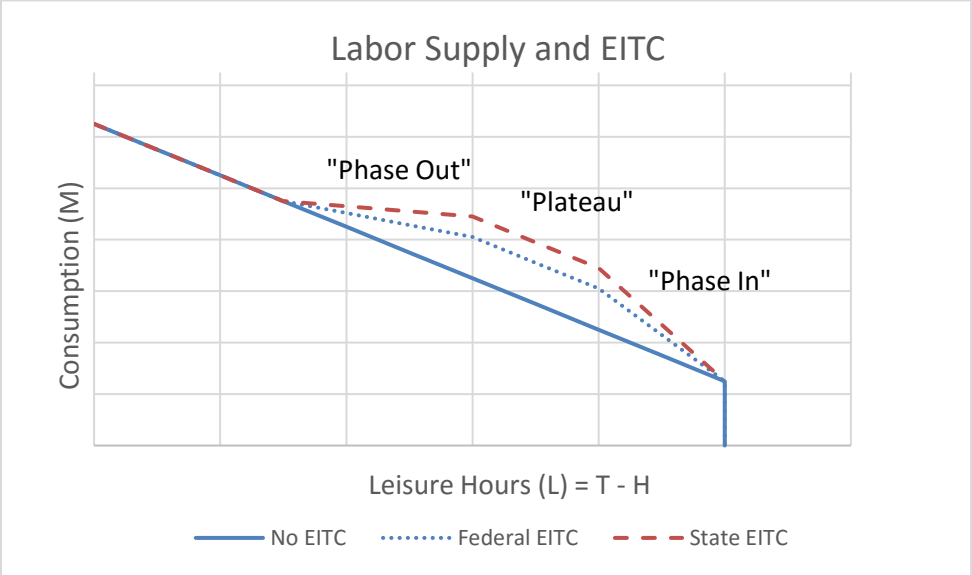


Figure 6: State EITC Induces Labor Market Entry

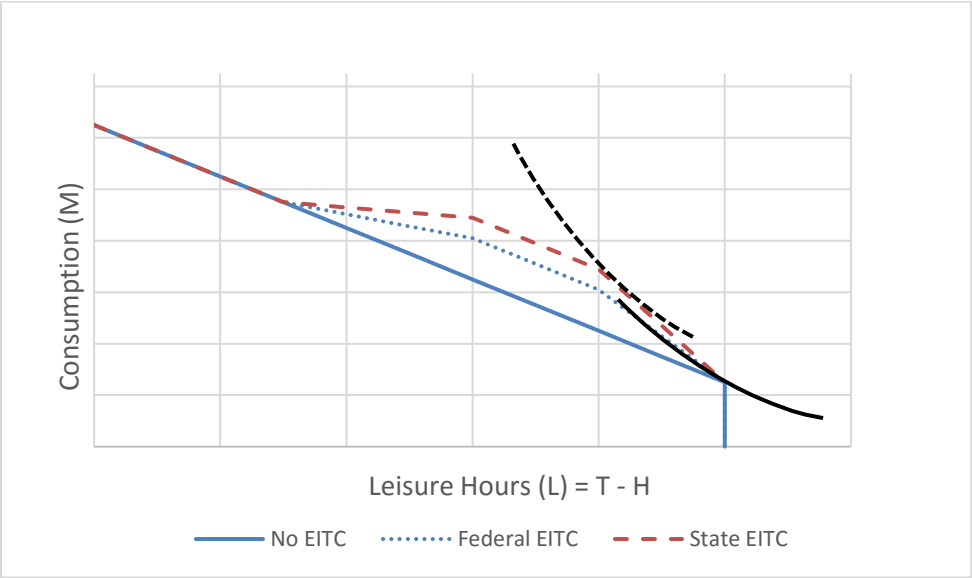


Figure 7: State EITC Decreases Hours Worked

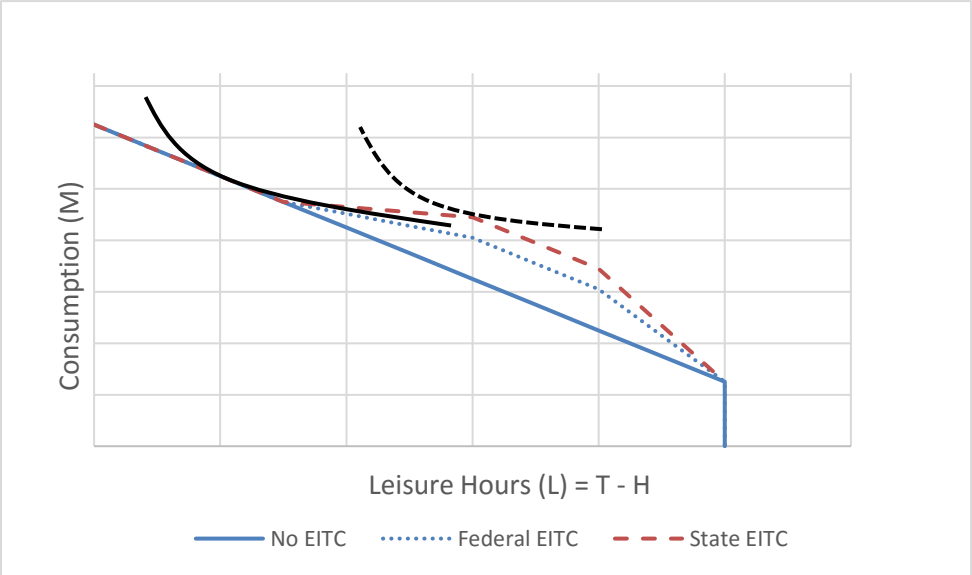


Table 1: Summary Statistics, 1997-2008

Tax Filers, Statistics of Income, 2008	2008
Total EITC Recipients (Millions)	24.4
Total Tax Filers (Millions)	131.4
Total EITC Expenditures (Billions)	$50.50

Share of Federal EITC Recipients, by Group	2008
Single with Children	0.59
Married with Children	0.19
No Children	0.22

Share of Federal EITC Expenditures, by Group	2008
Single with Children	0.74
Married with Children	0.23
No Children	0.03

Federal EITC Filers per Potentially Eligible Population	Mean	Obs
Full Sample	0.219	612
Single with Children	0.918	612
Married with Children	0.154	612
No Children	0.076	612

Federal EITC Expenditures per Potentially Eligible Population		
Full Sample	0.219	612
Single with Children	0.918	612
Married with Children	0.154	612
No Children	0.076	612

State Policy Variables

State EITC % Supplement	Mean	Min	Max
All States	0.048	0	0.5
EITC States	0.164	0.035	0.5

Notes: Data on tax filers and EITC recipients come from the Statistics of Income, tax years 1997-2008. Data on the population of potentially eligible filers come from the CPS ASEC, survey years 1997-2008. The variable "% of Potentially Eligible Population Filing for EITC" is defined as total EITC recipients divided by total potential filers, for each state and year cell. Statistics are weighted to either represent the populaltion of tax filers or by the population of potentially eligible filers for each cell.

Table 2: Estimated EITC Effects on Employment, Single and Married Women, Aged 21-44, 1997-2008

	(1)	(2)	(3)	(4)	(5)	(6)
		Single Women			Married Women	
VARIABLES	All	Low-skilled	Single Mothers	All	Low-skilled	Married Mothers
EITC	0.05	0.08	0.01	0.13*	0.25**	-0.14**
	(0.04)	(0.07)	(0.07)	(0.07)	(0.11)	(0.06)
EITC*Kids	0.16	0.18		-0.19**	-0.25	
	(0.11)	(0.15)		(0.09)	(0.19)	
EITC*Low-skilled			0.25*			0.14
			(0.14)			(0.15)
Low-skilled	-0.17***		-0.17***	-0.24***		-0.24***
	(0.03)		(0.03)	(0.03)		(0.03)
1 Kid	0.01	0.04***		0.03	0.06	
	(0.01)	(0.01)		(0.03)	(0.07)	
2 Kids	0.02	0.03**	0.00	-0.00	0.05	-0.03***
	(0.01)	(0.02)	(0.01)	(0.03)	(0.07)	(0.01)
3+ Kids	-0.04**	-0.02	-0.05***	-0.06*	0.01	-0.09***
	(0.01)	(0.02)	(0.02)	(0.03)	(0.07)	(0.01)
Observations	4,754	2,447	3,530	4,806	2,447	3,582
R-squared	0.67	0.51	0.51	0.75	0.71	0.72

Robust standard errors in parentheses

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

Notes: These regressions use the CPS ASEC data on individuals, aggregated to cells that vary by state, year, kid (0, 1, 2, 3+), and skill level (at least a HS degree). Regressions control for state unemployment rate, state*year*kids*skill demographic measures including percent black, Hispanic, education controls, children under the age of six, and average age. The EITC and share low-skilled variables are demeaned in the specifications with EITC*low skilled interactions. Estimates are weighted by the number of observations in each cell and standard errors are clustered at the state level.

Table 3: Estimated EITC Effects on Employment, Single Individuals with Children, Aged 21-44, 1997-2008

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
					Share low-skilled in 1997	
EITC	0.16***	0.15***	0.15***	0.14**	0.16***	0.16***
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Share Low-skilled		-0.09	-0.09			
		(0.05)	(0.06)			
EITC*Share low-skilled			-0.19			
			(0.60)			
Share low-skilled with 2+ children				-0.34		
				(0.39)		
EITC*Share low-skilled with 2+ children				-0.17		
				(0.11)		
EITC*Share low-skilled in 1997					0.32	
					(0.99)	
EITC*Share low-skilled with 2+ children in 1997						0.81**
						(0.38)
Observations	612	612	612	612	612	612
R-squared	0.67	0.67	0.67	0.67	0.67	0.67

Robust standard errors in parentheses

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

Notes: Regressions control for state unemployment rate, state minimum wage, and state demographic measures including percent black, Hispanic, female, and single. Regressions also control for average age, the share of the state's population with 2+ kids, and the share of the state's population with a child under the age of 6. State and year fixed effects are also included. The EITC and share low-skilled variables are demeaned in the specifications with EITC*low skilled interactions (columns 3-6). Estimates are weighted by the number of observations and standard errors are clustered at the state level.

Table 4: Estimated EITC Effects on Employment, Single Women with Children, Aged 21-44, 1997-2008

VARIABLES	(1)	(2)	(3)	(4)	(5) Share low-skilled in 1997	(6) Share low-skilled in 1997
EITC	0.18**	0.18**	0.17**	0.14**	0.17**	0.18**
	(0.08)	(0.08)	(0.07)	(0.05)	(0.07)	(0.08)
Share Low-skilled		-0.07	-0.07			
		(0.05)	(0.05)			
EITC*Share low-skilled			-0.14			
			(0.67)			
Share low-skilled with 2+ children				-0.72		
				(0.55)		
EITC*Share low-skilled with 2+ children				-0.11		
				(0.09)		
EITC*Share low-skilled in 1997					-0.95	
					(1.25)	
EITC*Share low-skilled with 2+ children in 1997						0.66
						(0.63)
Observations	612	612	612	612	612	612
R-squared	0.64	0.64	0.64	0.64	0.64	0.64

Robust standard errors in parentheses

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

Notes: Regressions control for state unemployment rate, state minimum wage, and state demographic measures including percent black, Hispanic, female, and single. Regressions also control for average age, the share of the state's population with 2+ kids, and the share of the state's population with a child under the age of 6. State and year fixed effects are also included. The EITC and share low-skilled variables are demeaned in the specifications with EITC*low skilled interactions (columns 3-6). Estimates are weighted by the number of observations and standard errors are clustered at the state level.

Table 5: Estimated EITC Effects on Employment, Married Individuals with Children, Aged 21-44, 1997-2008

VARIABLES	(1)	(2)	(3)	(4)	(5) Share low-skilled in 1997	(6) Share low-skilled in 1997
EITC	-0.06*	-0.06**	-0.10***	-0.08***	-0.06	-0.08
	(0.03)	(0.03)	(0.03)	(0.03)	(0.05)	(0.06)
Share Low-skilled		-0.03	-0.03			
		(0.03)	(0.03)			
EITC*Share low-skilled			-0.41**			
			(0.20)			
Share low-skilled with 2+ children				-0.30		
				(0.29)		
EITC*Share low-skilled with 2+ children				-0.03		
				(0.04)		
EITC*Share low-skilled in 1997					0.07	
					(0.49)	
EITC*Share low-skilled with 2+ children in 1997						-0.39
						(0.63)
Observations	612	612	612	612	612	612
R-squared	0.86	0.86	0.86	0.86	0.86	0.86

Robust standard errors in parentheses

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

Notes: Regressions control for state unemployment rate, state minimum wage, and state demographic measures including percent black, Hispanic, female, and single. Regressions also control for average age, the share of the state's population with 2+ kids, and the share of the state's population with a child under the age of 6. State and year fixed effects are also included. The EITC and share low-skilled variables are demeaned in the specifications with EITC*low skilled interactions (columns 3-6). Estimates are weighted by the number of observations and standard errors are clustered at the state level.

Table 6: Estimated EITC Effects on Employment, Married Women with Children, Aged 21-44, 1997-2008

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
						Share low-skilled in 1997
EITC	-0.08	-0.08	-0.14**	-0.11**	-0.00	-0.08
	(0.06)	(0.06)	(0.06)	(0.05)	(0.08)	(0.10)
Share Low-skilled		-0.08	-0.08*			
		(0.05)	(0.05)			
EITC*Share low-skilled			-0.53			
			(0.36)			
Share low-skilled with 2+ children				-0.46		
				(0.43)		
EITC*Share low-skilled with 2+ children				-0.07		
				(0.06)		
EITC*Share low-skilled in 1997					1.47	
					(1.12)	
EITC*Share low-skilled with 2+ children in 1997						-0.04
						(1.50)
Observations	612	612	612	612	612	612
R-squared	0.86	0.86	0.86	0.86	0.86	0.86

Robust standard errors in parentheses

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

Notes: Regressions control for state unemployment rate, state minimum wage, and state demographic measures including percent black, Hispanic, female, and single. Regressions also control for average age, the share of the state's population with 2+ kids, and the share of the state's population with a child under the age of 6. State and year fixed effects are also included. The EITC and share low-skilled variables are demeaned in the specifications with EITC*low skilled interactions (columns 3-6). Estimates are weighted by the number of observations and standard errors are clustered at the state level.

Table 7: Estimated EITC Effects on Employment, Childless Individuals, Aged 21-34, 1997-2008

	(1)	(2)	(3)	(4)
VARIABLES				
EITC	0.01	0.01	0.01	-0.03
	(0.04)	(0.05)	(0.05)	(0.06)
Share Low-skilled		-0.06	-0.06	
		(0.04)	(0.04)	
EITC*Share low-skilled			-0.06	
			(0.22)	
EITC*Share low-skilled in 1997				-0.67
				(0.66)
Observations	612	612	612	612
R-squared	0.76	0.76	0.76	0.76

Robust standard errors in parentheses

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

Notes: Regressions control for state unemployment rate, state minimum wage, and state demographic measures including percent black, Hispanic, female, average age, and single. State and year fixed effects are also included. The EITC and share low-skilled variables are demeaned in the specifications with EITC*low skilled interactions (columns 3-4). Estimates are weighted by the number of observations and standard errors are clustered at the state level.

Table 8a: Estimated State EITC Effects on Federal EITC Participation, Single Filers with Children, 1997-2008

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	Y = EITC Filers/Potentially Eligible Population						
EITC	-0.02	-0.02	0.09	-0.03	-0.02	0.18	0.18
	(0.21)	(0.21)	(0.19)	(0.20)	(0.20)	(0.25)	(0.20)
Share Low-skilled		0.02	0.02				
		(0.24)	(0.24)				
EITC*Share low-skilled			3.40				
			(2.42)				
Share low-skilled with 2+ children				0.73*	0.73*		
				(0.38)	(0.38)		
EITC*Share low-skilled with 2+ children					0.03		
					(1.81)		
EITC*Share low-skilled in 1997						8.50	
						(6.97)	
EITC*Share low-skilled with 2+ children in 1997							8.49**
							(3.73)
State MW	-0.34	-0.34	-0.33	-0.35*	-0.35*	-0.34*	-0.35*
	(0.20)	(0.20)	(0.20)	(0.20)	(0.20)	(0.20)	(0.20)
Observations	612	612	612	612	612	612	612
R-squared	0.80	0.80	0.80	0.80	0.80	0.80	0.80

Robust standard errors in parentheses

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

Notes: Regressions control for annual state average unemployment rate, group demographic measures including percent black, Hispanic, female, and average age. Regressions also control for the share of the group's state population with 2+ children, as well as state and year fixed effects. The EITC and share low-skilled variables are demeaned in the specifications with EITC*low skilled interactions. Estimates are weighted by the number of potential filers and standard errors are clustered at the state level.

Table 8b: Estimated State EITC Effects on Federal EITC Expenditures, Single Filers with Children, 1997-2008

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	Y = EITC Expenditures/Potentially Eligible Population						
EITC	0.12	0.12	0.37	0.10	0.11	0.92	0.66
	(0.55)	(0.55)	(0.52)	(0.52)	(0.53)	(0.74)	(0.60)
Share Low-skilled		0.23	0.22				
		(0.67)	(0.65)				
EITC*Share low-skilled			7.99				
			(6.76)				
Share low-skilled with 2+ children				2.34**	2.34**		
				(1.12)	(1.13)		
EITC*Share low-skilled with 2+ children					0.42		
					(5.07)		
EITC*Share low-skilled in 1997						34.27	
						(20.98)	
EITC*Share low-skilled with 2+ children in 1997							23.07**
							(10.80)
State MW	-1.15*	-1.16*	-1.15*	-1.18*	-1.19*	-1.16*	-1.17*
	(0.63)	(0.63)	(0.63)	(0.62)	(0.63)	(0.62)	(0.62)
Observations	612	612	612	612	612	612	612
R-squared	0.82	0.82	0.82	0.82	0.82	0.82	0.82

Robust standard errors in parentheses

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

Notes: EITC expenditures are in real 2008 thousands of dollars. Regressions control for annual state average unemployment rate, group demographic measures including percent black, Hispanic, female, and average age. Regressions also control for the share of the group's state population with 2+ children, as well as state and year fixed effects. The EITC and share low-skilled variables are demeaned in the specifications with EITC*low skilled interactions. Estimates are weighted by the number of potential filers and standard errors are clustered at the state level.

Table 9a: Estimated State EITC Effects on Federal EITC Filers, Married Filers with Children, 1997-2008

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	Y = EITC Filers/Potentially Eligible Population						
EITC	-0.05	-0.05	-0.05	-0.06	-0.07	-0.08	-0.03
	(0.04)	(0.04)	(0.05)	(0.06)	(0.06)	(0.06)	(0.06)
Share Low-skilled		0.02	0.02				
		(0.07)	(0.07)				
EITC*Share low-skilled			0.05				
			(0.37)				
Share low-skilled with 2+ children				0.06	0.06		
				(0.08)	(0.09)		
EITC*Share low-skilled with 2+ children					-0.02		
					(0.52)		
EITC*Share low-skilled in 1997						-0.39	
						(0.61)	
EITC*Share low-skilled with 2+ children in 1997							0.40
							(0.65)
State MW	0.04	0.05	0.05	0.04	0.04	0.04	0.04
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Observations	612	612	612	612	612	612	612
R-squared	0.85	0.85	0.85	0.85	0.85	0.85	0.85

Robust standard errors in parentheses

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

Notes: Regressions control for annual state average unemployment rate, group demographic measures including percent black, Hispanic, female, and average age. Regressions also control for the share of the group's state population with 2+ children, as well as state and year fixed effects. The EITC and share low-skilled variables are demeaned in the specifications with EITC*low skilled interactions. Estimates are weighted by the number of potential filers and standard errors are clustered at the state level.

Table 9b: Estimated State EITC Effects on Federal EITC Expenditures, Married Filers with Children, 1997-2008

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	Y = EITC Expenditures/Potentially Eligible Population						
EITC	-0.18*	-0.18	-0.20	-0.18	-0.23	-0.31*	-0.22
	(0.11)	(0.11)	(0.14)	(0.11)	(0.15)	(0.16)	(0.19)
Share Low-skilled		0.06	0.06				
		(0.19)	(0.19)				
EITC*Share low-skilled			-0.19				
			(1.15)				
Share low-skilled with 2+ children				0.14	0.13		
				(0.23)	(0.23)		
EITC*Share low-skilled with 2+ children					-0.63		
					(1.54)		
EITC*Share low-skilled in 1997						-2.22	
						(1.82)	
EITC*Share low-skilled with 2+ children in 1997							-0.70
							(1.98)
State MW	0.07	0.07	0.07	0.07	0.07	0.07	0.07
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
Observations	612	612	612	612	612	612	612
R-squared	0.84	0.84	0.84	0.84	0.84	0.84	0.84

Robust standard errors in parentheses

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

Notes: EITC expenditures are in real 2008 thousands of dollars. Regressions control for annual state average unemployment rate, group demographic measures including percent black, Hispanic, female, and average age. Regressions also control for the share of the group's state population with 2+ children, as well as state and year fixed effects. The EITC and share low-skilled variables are demeaned in the specifications with EITC*low skilled interactions. Estimates are weighted by the number of potential filers and standard errors are clustered at the state level.

Table 10: Estimated State EITC Effects on Federal EITC Participation and Expenditures, Childless Filers, 1997-2008

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Y = EITC Filers/Potentially Eligible Population				Y = Federal EITC Expenditures/Potentially Eligible Population			
EITC	0.02	0.02	0.02	0.04*	0.01*	0.01*	0.01	0.01**
	(0.01)	(0.01)	(0.02)	(0.02)	(0.00)	(0.00)	(0.00)	(0.01)
Share Low-skilled		0.02	0.02			0.00	0.00	
		(0.03)	(0.03)			(0.01)	(0.01)	
EITC*Share low-skilled			0.04				-0.07	
			(0.16)				(0.06)	
EITC*Share low-skilled in 1997				0.54				0.13
				(0.43)				(0.10)
State MW	0.01	0.01	0.01	0.01	0.00	0.00	0.00	0.00
	(0.02)	(0.02)	(0.02)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	612	612	612	612	612	612	612	612
R-squared	0.66	0.66	0.66	0.66	0.61	0.61	0.61	0.61

Robust standard errors in parentheses

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

Notes: EITC expenditures are in real 2008 thousands of dollars. Regressions control for annual state average unemployment rate, group demographic measures including percent black, Hispanic, female, and average age. Regressions also control for state and year fixed effects. The EITC and share low-skilled variables are demeaned in the specifications with EITC*low skilled interactions. Estimates are weighted by the number of potential filers and standard errors are clustered at the state level.

REFERENCES

Ahn, H. (2012). Child care subsidy, child care costs, and employment of low-income single mothers. *Children and Youth Services Review, 34*, 379-387.

Anderson, P., & Levine, P. (2000). Child care and mothers' employment decisions. In R. M. Blank & D. Card (Eds.), Finding jobs: Work and welfare reform. New York, NY: Russell Sage Foundation.

Assistant Secretary for Planning and Evaluation, U.S. Department of Health and Human Services. (2012). Estimates of Child Care Eligibility and Receipt for Fiscal Year 2009. Washington, DC.

Auriemma, F., Cooper, B., & Smith, S. (1992). *Graying teachers: A report on state pension systems and school district early retirement incentives*. Eugene, OR: ERIC Clearinghouse on Educational Management.

Averett, S., Peters, E., & Waldman, D. (1997). Tax credits, labor supply, and child care. Review of Economics and Statistics, 79, 125–135.

Barrow, L. and McGranahan, L. (2000). The Effects of the Earned Income Tax Credit on the Seasonality of Household Expenditures. *National Tax Journal*, 53(4): 1211-1244.

Baughman, R. and Dickert-Conlin, S. (2009). The Earned Income Tax Credit and Fertility. *Journal of Population Economics*, 22(3): 537-63.

Berger, M., & Black, D. (1992). Child care subsidies, quality of care, and the labor supply of low-income, single mothers. The Review of Economics and Statistics, 74, 635–642.

Besharov, Morrow, Shi (2006). Child care data in the survey of income and program participation, inaccuracies and corrections

Bitler, M., Hoynes, H., and Kuka, E. (2015). Do In-Work Tax Credits Serve as a Safety Net? (*Forthcoming) Journal of Human Resources.*

Bhargava, S. and Manoli, D. (2015). Psychological Frictions and the Incomplete Take-Up of Social Benefits: Evidence from an IRS Field Experiment. *American Economic Review*, 105(11): 3489-3529.

Blank, R. (2002). Evaluating Welfare Reform in the United States. *Journal of Economic Literature*, 40(4): 1105-1166.

Blau, D. (2000). Child care subsidy programs. Working Paper 7806. Cambridge, MA: National Bureau of Economic Research.

Blau, D., & Robins, P. (1991). Child care and the labor supply of young mothers over time. Demography, 28, 333–351.

Blau, D., & Tekin, E. (2007). The determinants and consequences of child care subsidies for single mothers in the USA. Journal of Population Economics, 20, 719–741.

Boyd, D., Grossman, P., Lankford, H., Loeb, S., & Wyckoff, J. (2009). Who leaves? Teacher attrition and student achievement. CALDER Working Paper No. 23.

Brown, K. (2012). The link between pensions and retirement behavior: Lessons from California teachers. *Journal of Public Economics*, 98, 1-14.

Bureau of Labor Statistics, U.S. Department of Labor, *The Editor's Desk*, Changes in men's and women's labor force participation rates on the Internet at http://www.bls.gov/opub/ted/2007/jan/wk2/art03.htm

California State Teachers' Retirement System. (2012a). *CalSTRS' comprehensive annual financial report for the fiscal year ended June 30, 2012.* California State Teachers' Retirement System, Sacramento, CA.

California State Teachers' Retirement System. (2012b). *Overview of the California State Teachers' Retirement System and related issues.* California State Teachers' Retirement System, Sacramento, CA.

Card, D. & Hyslop, D. (2005). Estimating the effects of a time-limited earnings subsidy for welfare-leavers. Econometrica, 73(6). 1723-1770.

Chetty, R., Friedman, J., & Rockoff, J. (2011). The long-term impacts of teachers: Teacher value-added and student outcomes in adulthood. NBER Working Paper No. 17699. Cambridge, MA: National Bureau of Economic Research.

Chetty, R., Friedman, J. and Saez, E. (2013). Using Differences in Knowledge Across Neighborhoods to Uncover the Impacts of the EITC on Earnings. *American Economic Review,* 103(7): 2683-2721.

Connelly, R. (1992). The effect of child care costs on married women's labor force participation. The Review of Economics and Statistics, 74, 83–90.

Costrell, R. & Podgursky, M. (2007). Efficiency and equity in the time pattern of teacher pension benefits. An analysis of four state systems. CALDER Working Paper No. 6.

Costrell, R. & Podgursky, M. (2009). Peaks, cliffs and valleys: The peculiar incentives in teacher retirement systems and their consequences for school staffing. *Education Finance and Policy*, 4, 175-211.

Currie, J. (2006). The Take-up of Social Benefits. *Poverty, The Distribution of Income, and Public Policy*, edited by A. Auerbach, D., Card., and Quigley, J., pp. 80-148, New York: Russel Sage.

Cutler, D. & Gruber, J. (1996). Does public insurance crowd out private insurance? The Quarterly Journal of Economics, 111(2), 391-430.

Dahl, G., and Lochner, L. (2012). The Impact of Family Income on Child Achievement: Evidence from the Earned Income Tax Credit. *American Economic Review,* 102(5): 1927-56.

Darling-Hammond, L. (2000). Teacher quality and student achievement: A review of state policy evidence. *Education Policy Analysis Archives,* 8(1), 1-44.

Dowd, T., and Horowitz, J. (2011) Income Mobility and the Earned Income Tax Credit: Short-Term Safety Net or Long-Term Income Support. *Public Finance Review*, 39(5): 619-652.

Dickert-Conlin, S., and Houser, S. (2002). EITC and Marriage. *National Tax Journal*, 55(1): 25-40.

Ehlers, R., & Estrada, J. Legislative Analyst Office, (2012). *Year-three survey: Update on school district finance in California*. Retrieved from website: www.lao.ca.gov

Eissa, N. (1995). Taxation and labor supply of married women: The tax reform act of 1986 as a natural experiment. NBER Working Paper No. 5023.

Eissa, N. and Hoynes, H. (2004). Taxes and the Labor Market Participation of Married Couples: The Earned Income Tax Credit. *Journal of Public Economics,* 88(9-10): 1931-58.

Eissa, N. and Liebman, J. (1996). Labor Supply Response to the Earned Income Tax Credit. *The Quarterly Journal of Economics*, 111 (2): 605-637.

Estrada, J. Legislative Analyst Office, (2012). *A review of the teacher layoff process in California*. Retrieved from website: www.lao.ca.gov

Evans, W. and Garthwaite, C. (2014). Giving Mom a Break: The Impact of Higher EITC Payments on Maternal Health. *American Economic Journal: Economic Policy*, 6(2): 258-90.

Fitzpatrick, M., & Lovenheim, M. (2014). Early retirement incentives and student achievement. *American Economic Journal: Economic Policy,* 6(3), 120-154.

Friedberg, L. & Turner, S. (2011). Pensions and public school teacher retirement: An analysis using national teacher data. TIAA-CREF Institute Research Dialogue, No. 99 (January).

Furgeson, J., Strauss, R., & Vogt, W. (2006). The effects of defined benefit pension incentives and working conditions on teacher retirement decisions. *Education Finance and Policy 5*(4), 463-491.

Giannarelli, Linda, Minton, S., Durham, C., and U.S. Department of Health and Human Services Administration for Children and Families (2013). Child Care and Development Fund (CCDF) Policies Database, 2012. Office of Planning, Research and Evaluation.

Goodman-Bacon, A. and McGranahan, L. (2008). How Do EITC Recipients Spend Their Refunds? *Economic Perspectives* 32(2), 17-32.

Grogger, J. (2003). The Effects of Time Limits, the EITC, and Other Policy Changes on Welfare Use, Work and Income Among Female-Headed Families. *The Review of Economics and Statistics*, 85(2): 394-408.

Guyton, J., Manoli, M., Schafer, B., and Sebastiani, M. (2016). Reminders and Recidivism: Evidence from Tax Filing & EITC Participation among Low-Income Nonfilers. NBER Working Paper No. 21904. Cambridge, MA: National Bureau of Economic Research.

Ha, Y. (2009). Stability of child-care subsidy use and earnings of low-income families. *Social Service Review, 83*(4), 495-525.

Ha, Y. & Meyer, D. (2010). Child care subsidy patterns: Are exits related to economic setbacks or economic successes? *Children and Youth Services Review, 32*, 346-355.

Han, W., & Waldfogel, J. (2001). Child care and women's employment. A comparison of single and married mothers with pre-school age children. Social Science Quarterly, 82, 552–568.

Hanushek, E. (1971). Teacher characteristics and gains in student achievement: Estimation using micro data. *The American Economic Review*, 61(2), 280-288.

Harris, D., & Sass, T. (2011). Teacher training, teacher quality and student achievement. *Journal of Public Economics*, 95, 798-812.

Herbst, Chris M. (2008). Who are the eligible non-recipients of child care subsidies? Children and Youth Services Review, 30(9), 1037-1054. doi: http://dx.doi.org/10.1016/j.childyouth.2008.01.003

Herbst, C. (2010). The labor supply effects of child care costs and wages in the presence of subsidies and the earned income tax credit. Rev. Econ Household (2010) 8:199-230.

Herbst, C., & Tekin, E. (2011). Do child care subsidies influence single mothers' decisions to invest in human capital? Economics of Education Review, 30, 901-912.

Herbst, C., & Tekin, E. (2012). The geographic accessibility of child care subsidies and evidence on the impact of subsidy receipt on childhood obesity. *Journal of Urban Economics, 71,* 37-52.

Hirasuma, D., and Stinson, T. (2006). Earned Income Tax Credit Utilization by Welfare Recipients: A Case Study of Minnesota's Earned Income Program. *Journal of Policy Analysis and Management*, 26(1): 125-148.

Hotz, J. and Scholz, K. (2003). The Earned Income Tax Credit. *Means-Tested Transfer Programs in the United States.*

Hoynes, H., Miller, D., and Simon, D. (2015). Income, the Earned Income Tax Credit, and Infant Health. *American Economic Journal: Economic Policy*, 7(1): 172-211.

Hoynes, H., and Patel, A. (2015). Effective Policy for Reducing Inequality? The Earned Income Tax Credit and the Distribution of Income. NBER Working Paper No. 21340. Cambridge, MA: National Bureau of Economic Research.

Internal Revenue Service. "State and Local Government EITC Programs." (2016, March 22). Retrieved from https://www.eitc.irs.gov/Tax-Preparer-Toolkit/stateandlocalEITC

Johnson, G., & Gaetino, J. (1982). Teacher attitudes toward early retirement incentive plans. *Journal of Education Finance, 7*(3), 243-261.

Kimmel, J. (1997). Child care costs as a barrier to employment for single and married mothers. The Review of Economics and Statistics

Koedel, C., Podgursky, M., & Shi, S. (2013). Teacher pension systems, the composition of the teaching workforce, and teacher quality. *Journal of Policy Analysis and Management,* 32(3), 574-596.

Kowalski, T., & Sweetland, S. (2005). Retire-rehire policy in state pension programs for school administrators. *Planning and Changing,* 36(1 & 2), 3-22.

Lankford, H., Loeb, S., & Wyckoff, J. (2002). Teacher sorting and the plight of urban schools: A descriptive analysis. *Educational Evaluation and Policy Analysis*, 24(1), 37-62.

Loeb, S., Darling-Hammond, L., & Luczak, J. (2005). How teaching conditions predict teacher turnover in California schools. *Peabody Journal of Education*, *80*(3), 44-70.

Laughlin, Lynda. 2013. *Who's Minding the Kids? Child Care Arrangements: Spring 2011.* Current Population Reports, P70-135. U.S. Census Bureau, Washington, DC.

Manoli, D. and Turner, N. (2016). Nudges and Learning: Evidence from Informational Interventions for Low-Income Taxpayers. NBER Working Paper No. 20718. Cambridge, MA: National Bureau of Economic Research.

Meyer, B. and Rosenbaum, D. (2001). Welfare, the Earned Income Tax Credit, and the Labor Supply of Single Mothers. *Quarterly Journal of Economics* 116(3): 1063-1114.

Meyers, M., Heintze, T., & Wolf, D. (2002). Child care subsidies and the employment of welfare recipients. *Demography, 39*(1), 165-179.

Meyers, Marcia K., Peck, Laura P., Davis, Elizabeth E., Collins, Ann, Kreader, J. Lee, Georges, Annie, Olson, Jerry A. (2002). The dynamics of child care subsidy use: a collaborative study of five states (pp. 1-60): National Center for Children in Poverty.

National Center for Education Statistics, U.S. Department of Education, Schools and Staffing Survey (SASS), "Public School Teacher Data File," 2011-12.

Neumark, D. and Wascher, W. (2001). Using the EITC to Help Poor Families: New Evidence and a Comparison with the Minimum Wage. *National Tax Journal*, 54(2):281- 317.

Neumark, D. and Wascher, W. (2011). Does a Higher Minimum Wage Enhance the Effectiveness of the Earned Income Tax Credit? *Industrial and Labor Relations Review* 64(4):712- 746.

Plueger, D. (2009). *Earned Income Tax Credit Participation Rate for Tax Year 2005.* Internal Revenue Service.

Ribar, D. (1992). Child care and the labor supply of married women: Reduced form evidence. The Journal of Human Resources, 27, 134–165.

Ribar, D. (1995). A structural model of child care and the labor supply of married women. JOLE, 13:3, 558-97

Rivkin, S., Hanushek, E., & Kain, J. (2005). Teachers, schools, and academic achievement. *Econometrica*, 73(2), 417-458.

Rockoff, J. (2004). The impact of individual teachers on student achievement: Evidence from panel data. *The American Economic Review*, 94(2), 247-252.

Ronfeldt, M., Loeb, S., & Wyckoff, J. (2013). How teacher turnover harms student achievement. *American Educational Research Journal*, 50(1), 4-36.

Scholz, J. (1994). The Earned Income Tax Credit: Participation, Compliance, and Antipoverty Effectiveness. *National Tax Journal* 47(1): 63-87.

State Controller's Office, Division of Audits. (2002-2012). *California state teachers' retirement system cost analysis report: Early retirement incentive*

Tarter, S. & McCarthy, M. (1989). Early retirement incentive programs for teachers. *Journal of Education Finance,* 15(2), 119-133.

Tekin, E. (2005). Child care subsidy receipt, employment, and child care choices of single mothers. Economic Letters, 89, 1–6.

United States Census Bureau American FactFinder. "B09001: Population Under 18 Years in Households by Household Type and Age of Children in Households." *2005 American Community Survey*. U.S. Census Bureau's American Community Survey Office, 2005. Web. 2 December 2014. http://factfinder2.census.gov

U.S. Department of Health and Human Services Administration for Children and Families, 2004. Child Care and Development Fund Expenditure Data. Office of Child Care. Available at:

http://www.acf.hhs.gov/programs/occ/resource/expenditures-2004

U.S. Department of Health and Human Services Administration for Children and Families, 2004. Child Care and Development Fund Statistics. Office of Child Care. Available at:

http://www.acf.hhs.gov/programs/occ/resource/ccdf-statistics

U.S. Department of Health and Human Services Administration for Children and Families, 2012. Child Care and Development Fund Statistics. Office of Child Care. Available at: http://www.acf.hhs.gov/programs/occ/resource/ccdf-statistics

Weber, R., Grobe, D., & Davis, E. (2014). Does policy matter? The effect of increasing child care subsidy policy generosity on program outcomes. *Children and Youth Services Review,44*, 135-144.

Yelowitz, A. (1995). The Medicaid notch, labor supply, and welfare participation: Evidence from eligibility expansions. *The Quarterly Journal of Economics, 111*(4), 909-939.

CHAPTER 1 APPENDIX

Figure A.1: Share of Students Taking CST Algebra 1 and CST Geometry

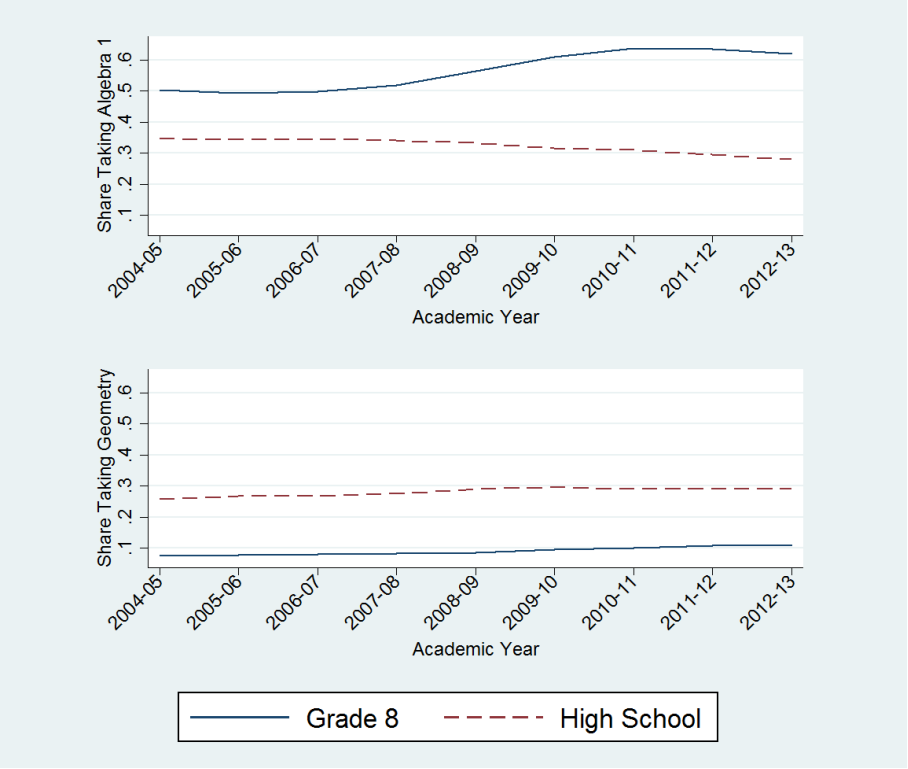


Figure A.2: Event Study: Total Number of FTE Pupil Services and FTE Administrators

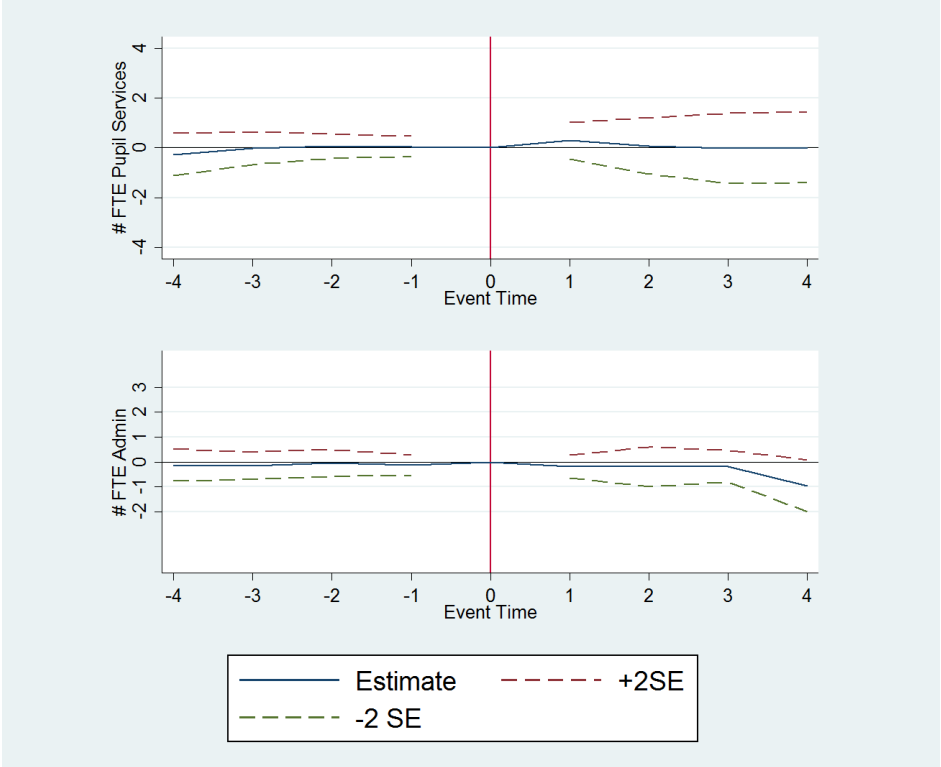


Table A.1: Summary Statistics, by Year

Variable	Full Sample Mean	Full Sample Std. Dev.	Offered ERI Mean	Offered ERI Std. Dev.	Never Offered ERI Mean	Never Offered ERI Std. Dev.
Academic Year 2004-05						
School-Level						
% FRPM	56.27	30.52	57.00	31.19	52.42	26.41
% ELL	30.09	22.40	30.03	22.45	30.39	22.17
% Black	11.08	13.87	11.26	14.31	10.16	11.21
% Hispanic	48.19	28.77	50.01	29.17	38.66	24.47
% Asian	10.36	13.95	8.26	10.57	21.39	22.04
Pupil-FTE Teacher Ratio	21.96	3.25	22.00	3.18	21.74	3.59
Enrollment/100	10.71	8.48	10.97	8.58	9.30	7.83
# FTE Teachers						
30+ Years Exp	3.81	4.26	3.94	4.36	3.15	3.64
25-29 Years Exp	3.35	3.69	3.52	3.78	2.47	3.07
20-24 Years Exp	3.63	3.58	3.76	3.61	2.96	3.34
15-19 Years Exp	5.17	4.26	5.33	4.28	4.32	4.05
10-14 Years Exp	6.87	5.75	7.14	5.83	5.41	5.03
6-9 Years Exp	10.02	7.36	10.32	7.45	8.44	6.68
<5 Years Exp	13.95	12.65	14.10	12.93	13.20	11.04
1 Year Exp	2.85	3.68	2.86	3.72	2.83	3.48
Average Experience	12.49	3.36	12.60	3.34	11.92	3.38
Academic Year 2008-09						
School-Level						
% FRPM	60.32	29.92	61.00	30.38	56.46	26.78
% ELL	28.98	20.20	28.53	19.81	31.55	22.10
% Black	9.75	12.21	9.86	12.54	9.14	10.06
% Hispanic	51.58	29.15	53.33	29.43	41.57	25.28
% Asian	10.13	14.10	8.18	10.70	21.33	23.12
Pupil-FTE Teacher Ratio	22.04	3.77	22.04	3.85	21.99	3.28
Enrollment/100	10.02	7.92	10.15	7.92	9.31	7.87
# FTE Teachers, School Level						
30+ Years Exp	3.28	3.95	3.38	3.98	2.68	3.73
25-29 Years Exp	2.57	2.71	2.56	2.62	2.62	3.16
20-24 Years Exp	3.98	3.50	4.02	3.43	3.73	3.86
15-19 Years Exp	5.27	4.33	5.33	4.34	4.91	4.22
10-14 Years Exp	9.48	6.75	9.60	6.78	8.84	6.54
6-9 Years Exp	8.29	6.99	8.45	7.17	7.35	5.76
<5 Years Exp	11.04	11.92	11.17	12.22	10.32	9.98
1 Year Exp	1.82	2.72	1.84	2.79	1.73	2.22
Average Experience	13.01	3.40	13.05	3.45	12.75	3.02
Total Districts	54		46		8	

Table A.2: Effect of ERIs on Pupil-FTE Teacher Ratios

VARIABLES	(1) Pupil-FTE Teacher Ratio	(2) Pupil-FTE Teacher Ratio	(3) Pupil-FTE Teacher Ratio	(4) Pupil-FTE Teacher Ratio	(5) Pupil-FTE Teacher Ratio	(6) Pupil-FTE Teacher Ratio	(7) Pupil-FTE Teacher Ratio	(8) Pupil-FTE Teacher Ratio
lag(First ERI)	0.360	0.383	0.488	0.420	0.194	0.230	0.323	0.272
	(0.385)	(0.361)	(0.363)	(0.348)	(0.360)	(0.332)	(0.333)	(0.320)
lag(Second ERI)			1.363**	1.248**			1.386**	1.305***
			(0.547)	(0.473)			(0.557)	(0.478)
lag(Third ERI +)			0.050	-0.448			0.044	-0.449
			(0.559)	(0.477)			(0.565)	(0.485)
Mean of Dependent Variable	22.75	22.75	22.75	22.75	22.75	22.75	22.75	22.75
Enrollment Controls	Y	Y	Y	Y				
Weighted by school enrollment		Y		Y		Y		Y
School FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Observations	24,476	24,476	24,476	24,476	24,476	24,476	24,476	24,476
R-squared	0.600	0.584	0.605	0.587	0.587	0.566	0.592	0.570

Clustered standard errors in parentheses

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

Notes: All regressions control for % eligible for free or reduced price lunch, % English language learner, % black, % Hispanic, and % Asian. Columns 1-4 also control for enrollment and enrollment squared. All controls vary at the school level. Standard errors clustered at the district level.

Table A.3: Effect of Non-Cumulative ERIs on Total # of Teachers with Varying Levels of Experience

VARIABLES	(1) 30+ Years Exp	(2) 25-29 Years Exp	(3) 20-24 Years Exp	(4) 15-19 Years Exp	(5) 10-14 Years Exp	(6) 6-9 Years Exp	(7) <5 Years Exp	(8) 1 Year Exp	(9) 2 Years Exp
lag(First ERI)	-0.370***	-0.121*	-0.245***	-0.037	-0.236	0.116	0.285	0.263*	-0.102
	(0.079)	(0.068)	(0.081)	(0.164)	(0.194)	(0.259)	(0.312)	(0.154)	(0.114)
Mean of Dependent Variable	3.19	2.90	4.01	5.90	8.93	8.42	9.75	1.87	1.90
School FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	24,494	24,494	24,494	24,494	24,494	24,494	24,461	24,461	24,494
R-squared	0.804	0.741	0.743	0.740	0.776	0.786	0.853	0.555	0.592

Clustered standard errors in parentheses

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

Notes: All regressions control for % eligible for free or reduced price lunch, % English language learner, % black, % Hispanic, % Asian, enrollment, and enrollment squared. All controls vary at the school level. Standard errors clustered at the district level.

Table A.4: Effect of Experience Distribution on CST English-Language Arts Test Scores

VARIABLES	(1) All Grades	(2) Elementary	(3) Middle	(4) High School
Share 30+ Years Experience	0.320***	0.206**	0.382**	0.490**
	(0.095)	(0.096)	(0.159)	(0.236)
Share 25-29 Years Experience	0.028	0.012	-0.202	0.147
	(0.105)	(0.115)	(0.184)	(0.231)
Share 20-24 Years Experience	0.082	0.005	0.040	0.468**
	(0.087)	(0.088)	(0.169)	(0.197)
Share 15-19 Years Experience	0.168***	0.091*	0.219*	0.429***
	(0.040)	(0.046)	(0.123)	(0.149)
Share 6-9 Years Experience	0.066	0.090	0.053	0.048
	(0.058)	(0.058)	(0.101)	(0.115)
Share <5 Years Experience	-0.056	-0.052	-0.020	-0.066
	(0.054)	(0.049)	(0.098)	(0.101)
School-by-grade FE	Y	Y	Y	Y
Grade-by-year FE	Y	Y	Y	Y
Observations	91,993	72,335	9,471	10,187
R-squared	0.940	0.920	0.962	0.959

Clustered standard errors in parentheses

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

Notes: All regressions control for % eligible for free or reduced price lunch, % ELL, % black, % Hispanic, % Asian, enrollment, and enrollment squared. All controls vary at the school-grade-year level, except for % free or reduced price lunch, which varies at the school-year level. Regressions weighted by school-grade enrollment. Standard errors clustered at the district level.

Table A.5: Effect of Experience Distribution on CST Mathematics Test Scores

VARIABLES	(1) All Grades	(2) Elementary	(3) Middle	(4) High School
Share 30+ Years Experience	0.453**	0.156	0.341	0.919*
	(0.193)	(0.128)	(0.344)	(0.470)
Share 25-29 Years Experience	0.012	-0.121	-0.475	0.461
	(0.144)	(0.142)	(0.290)	(0.454)
Share 20-24 Years Experience	-0.002	-0.176*	-0.159	0.579
	(0.115)	(0.100)	(0.230)	(0.359)
Share 15-19 Years Experience	0.206**	-0.004	0.330*	0.569*
	(0.082)	(0.066)	(0.191)	(0.284)
Share 6-9 Years Experience	0.158*	0.155*	0.063	0.301
	(0.083)	(0.092)	(0.192)	(0.233)
Share <5 Years Experience	-0.135	-0.023	-0.294	-0.048
	(0.097)	(0.093)	(0.183)	(0.238)
School-by-grade FE	Y	Y	Y	Y
Grade-by-year FE	Y	Y	Y	Y
Observations	104,106	72,335	13,222	18,549
R-squared	0.800	0.857	0.788	0.765

Clustered standard errors in parentheses

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

Notes: All regressions control for % eligible for free or reduced price lunch, % ELL, % black, % Hispanic, % Asian, enrollment, and enrollment squared. All controls vary at the school-grade-year level, except for % free or reduced price lunch, which varies at the school-year level. Regressions weighted by school-grade enrollment. Standard errors clustered at the district level.

Table A.6: Effect of Experience Distribution on CST Science, CST History-Social Science, and CST U.S. History Test Scores

VARIABLES	(1) All Grades CST: Science	(2) Grade 5 CST: Science	(3) Grade 8 CST: Science	(4) Grade 10 CST: Life Science	(5) All Grades CST: History	(6) Grade 8 CST: History	(7) Grade 11 CST: U.S. History
Share 30+ Years Experience	0.048	-0.127	0.400	-0.058	0.353*	0.339	0.278
	(0.138)	(0.114)	(0.244)	(0.431)	(0.205)	(0.209)	(0.323)
Share 25-29 Years Experience	-0.264*	-0.182	-0.496*	-0.318	-0.143	-0.374*	0.095
	(0.154)	(0.150)	(0.261)	(0.399)	(0.213)	(0.222)	(0.320)
Share 20-24 Years Experience	-0.052	-0.020	-0.307	0.219	0.060	-0.122	0.391
	(0.114)	(0.093)	(0.239)	(0.324)	(0.193)	(0.208)	(0.338)
Share 15-19 Years Experience	0.044	0.053	-0.043	0.127	0.359***	0.370**	0.367*
	(0.074)	(0.066)	(0.162)	(0.237)	(0.124)	(0.153)	(0.201)
Share 6-9 Years Experience	0.103	0.053	0.087	0.218	0.216*	0.274**	0.088
	(0.080)	(0.096)	(0.140)	(0.183)	(0.113)	(0.127)	(0.166)
Share <5 Years Experience	-0.147*	-0.165**	-0.095	-0.177	-0.115	0.025	-0.342***
	(0.074)	(0.081)	(0.131)	(0.174)	(0.118)	(0.141)	(0.127)
School-by-grade FE	Y	Y	Y	Y	Y	Y	Y
Grade-by-year FE	Y	Y	Y	Y	Y	Y	Y
Observations	22,792	15,519	4,223	3,050	8,061	4,693	3,368
R-squared	0.926	0.905	0.931	0.945	0.939	0.943	0.935

Clustered standard errors in parentheses

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

Notes: All regressions control for % eligible for free or reduced price lunch, % ELL, % black, % Hispanic, % Asian, enrollment, and enrollment squared. All controls vary at the school-grade-year level, except for % free or reduced price lunch, which varies at the school-year level. Regressions weighted by school-grade enrollment. Standard errors clustered at the district level.

CHAPTER 2 APPENDIX A

Figure A.1A: Hours

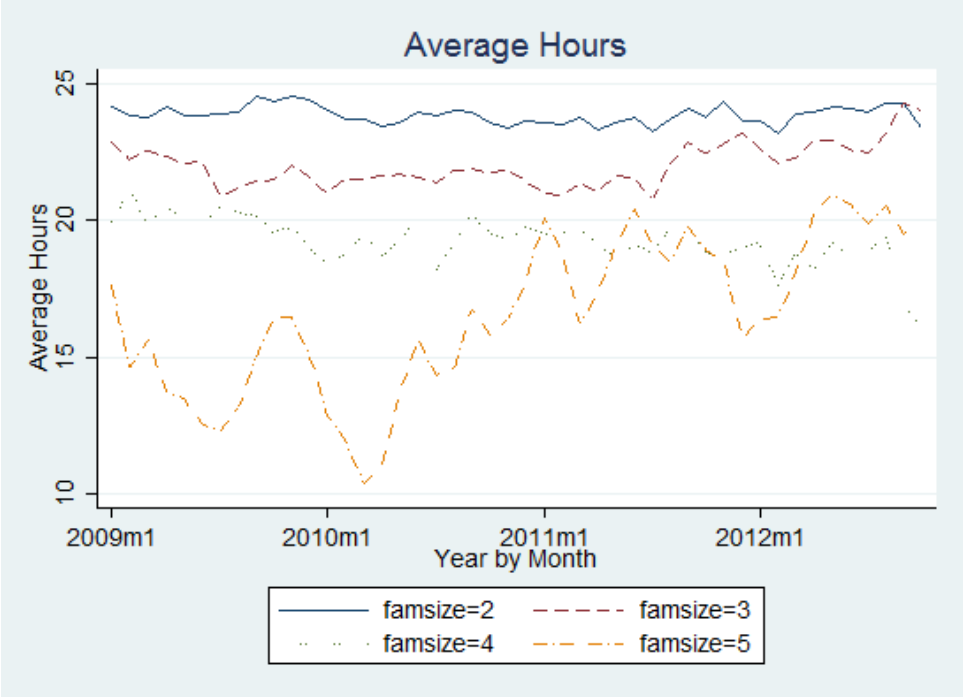


Figure A.1B: Real Earnings

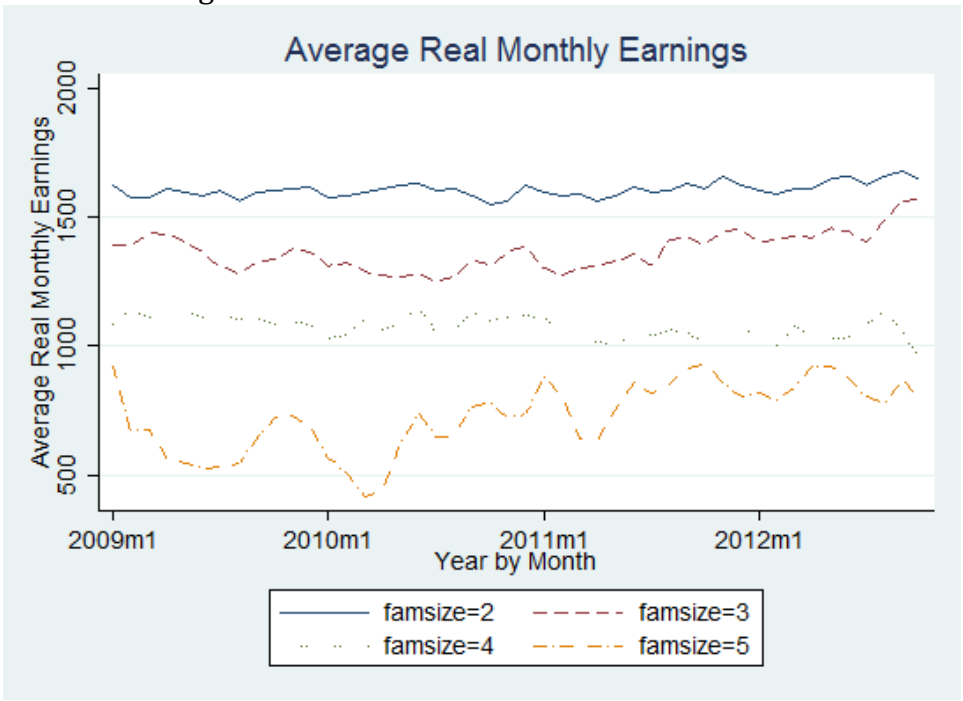


Figure A.1C: Real Wages

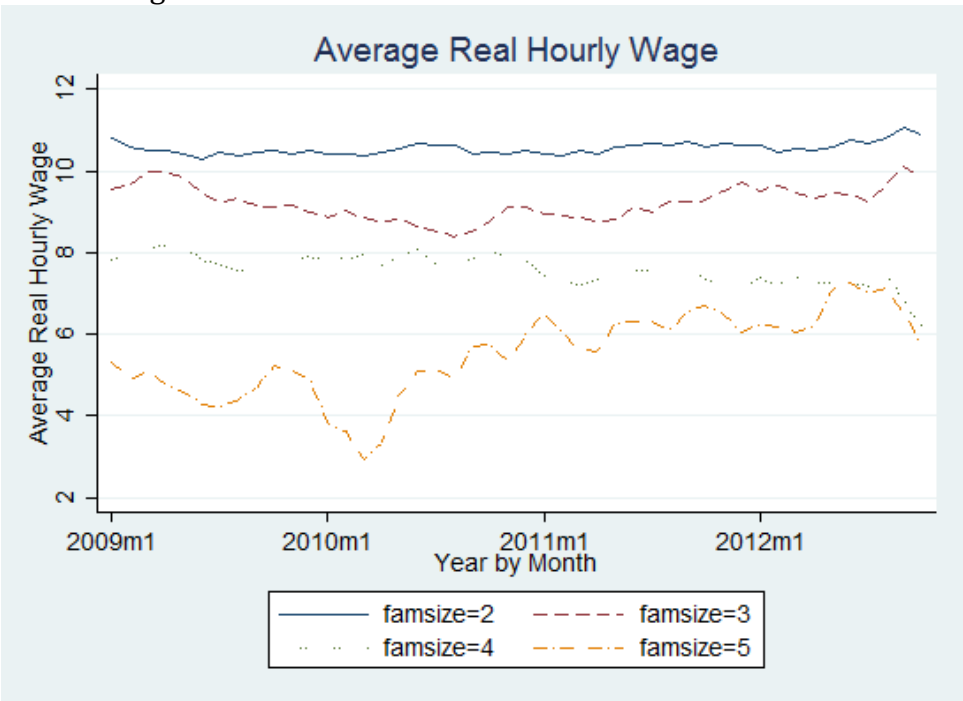


Figure A.1D: Share of Single Mothers in School

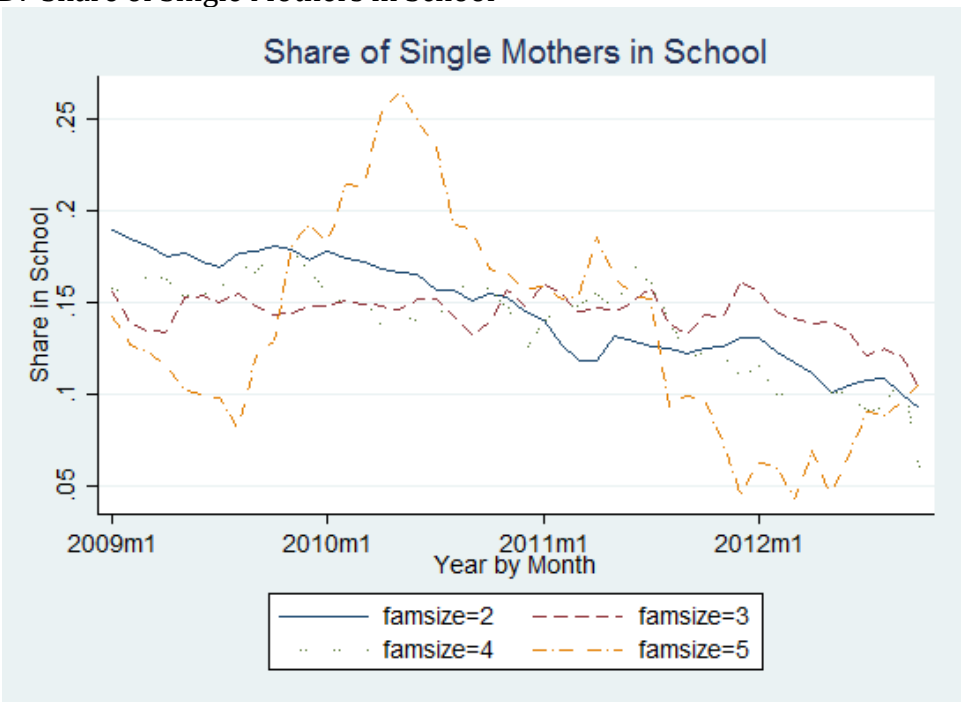


Figure A.1E: Share of Single Mothers Working

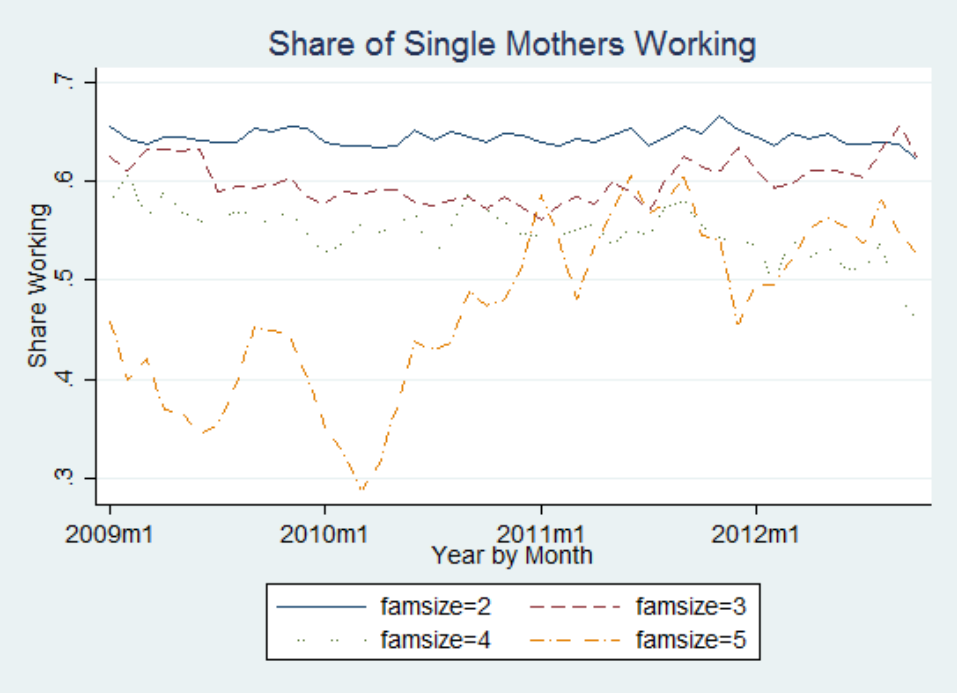


Figure A.1F: Share of Single Mothers Working Part Time

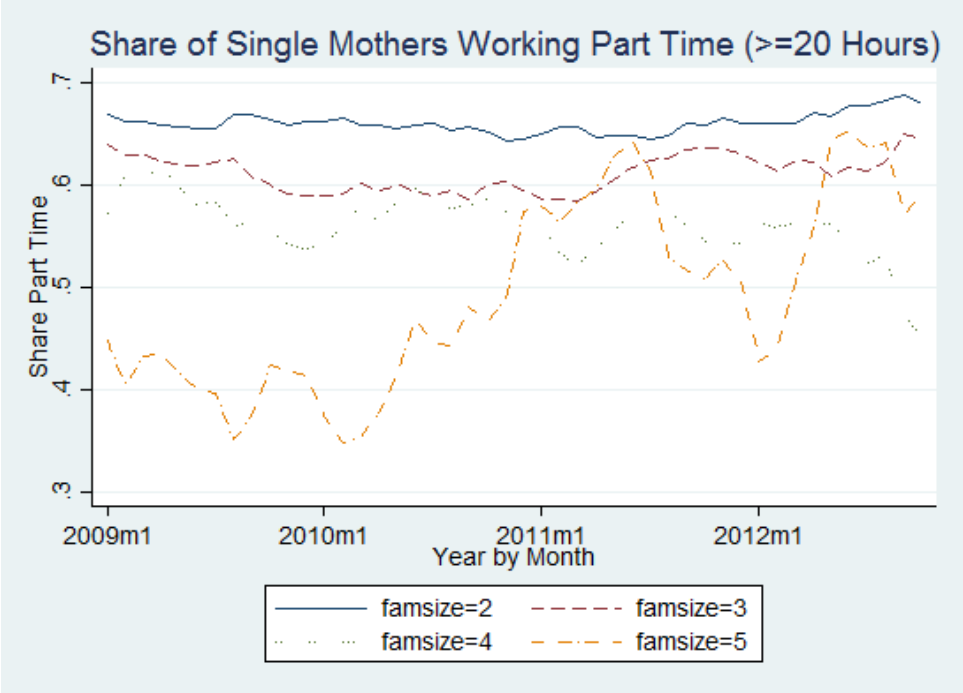


Figure A.1G: Share of Single Mothers Working Full Time

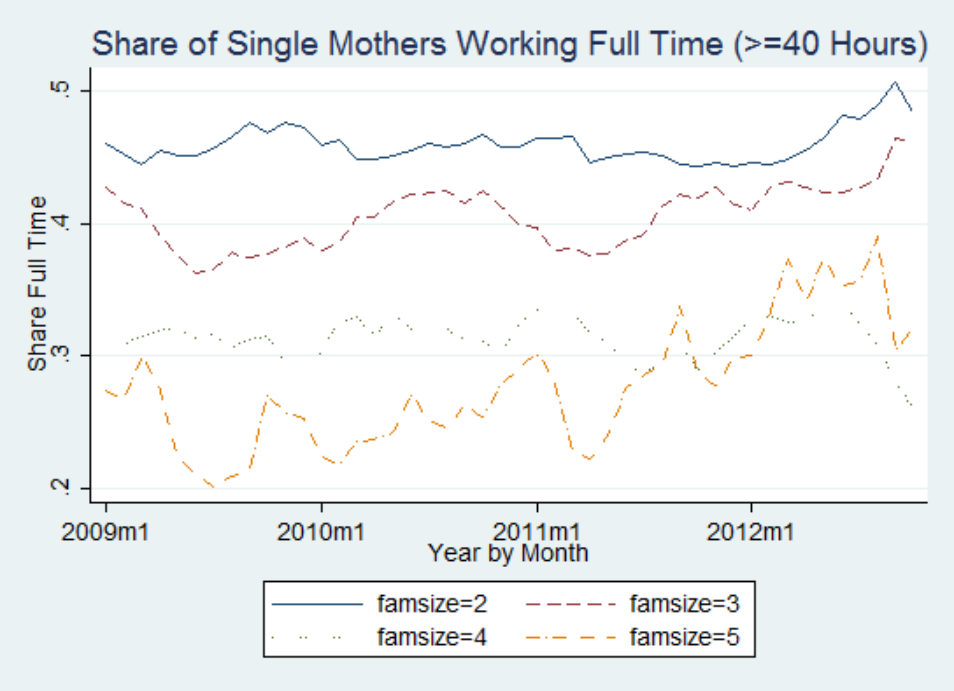


Figure A.2: Longitudinal Panel Weights by Year

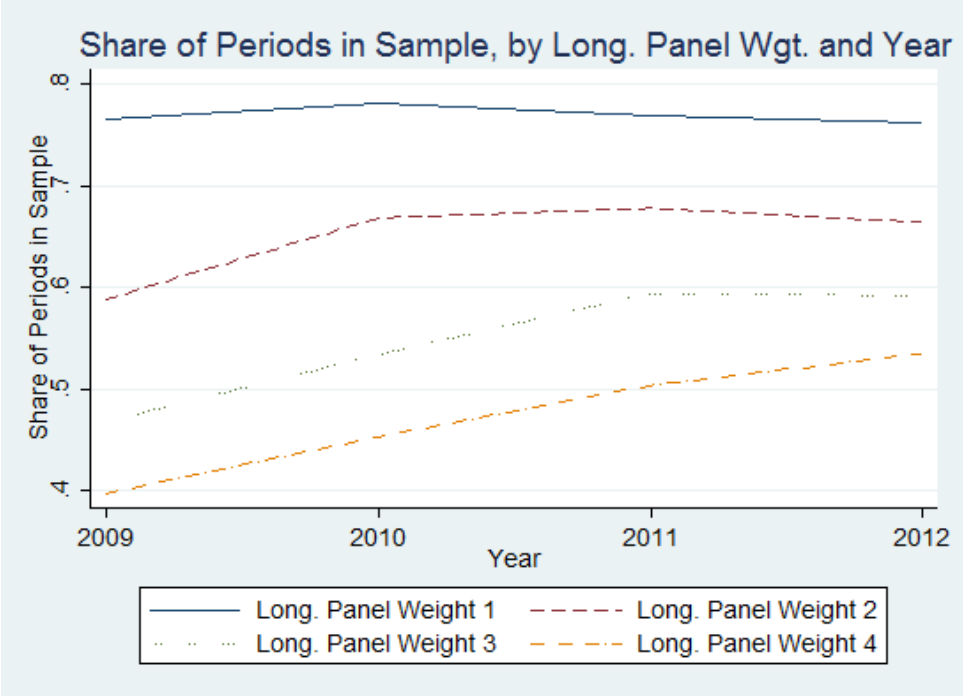


Table A1: Effect of Simulated % Eligible on Single Mother Probability of Working in Past Month

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Longitudinal Panel Weights				Wave 1 Person Weights			
VARIABLES	P(Working = 1)	P(Working = 1)	P(Working = 1)	P(Working = 1)	P(Working = 1)	P(Working = 1)	P(Working = 1)	P(Working = 1)
Y = Simulated %Eligible*Weighted Share	-0.0249**	-0.0242**			-0.0184	-0.0167		
	(0.0117)	(0.0117)			(0.0123)	(0.0120)		
Y = Simulated %Eligible			-0.0049**	-0.0047**			-0.0038*	-0.0034*
			(0.0021)	(0.0022)			(0.0020)	(0.0020)
$Avg.Effect = \hat{\beta} * \bar{X}/\bar{Y}$	-0.417	-0.405	-0.352	-0.337	-0.308	-0.280	-0.273	-0.244
Month FE	Yes		Yes		Yes		Yes	
Year FE	Yes		Yes		Yes		Yes	
Month-Year FE		Yes		Yes		Yes		Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Single Mothers (N)	3190	3190	3190	3190	1127	1127	1127	1127
Observations	33,748	33,748	33,748	33,748	36,429	36,429	36,429	36,429
R-squared	0.6645	0.6651	0.6645	0.6651	0.6718	0.6721	0.6719	0.6722

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

Notes: "Working" is redefined as having worked at least one week in the past month. Regressions include education dummies, age, age squared, marital status dummies, urban/rural status, number of children under 5, number of disabled children, and monthly state unemployment rate. Regressions also include a dummy for seam month. The sample is restricted to single mothers who are not self-employed, with determined hours, and non-negative earnings. Real hourly wage is restricted to working mothers with real hourly wages between $2 and $100. Real monthly earnings are restricted to be $0 or up to $10,000. Part Time = Hours > 20. Full Time = Hours > 40. Simulated % Eligible is on a 0 to 100 scale. All standard errors are clustered at the state level.

Table A.2: Dependent Variable (Y): Actual % Eligible

VARIABLES	(1) Entire Sample	(2)	(3)	(4)	(5)	(6)
Simulated % Eligible	0.5436***	0.7137***	0.7124***	0.8031***	0.8106***	0.9100***
	(0.0431)	(0.0342)	(0.0347)	(0.0323)	(0.0316)	(0.0452)
Sample Restrictions						
States with any cells containing >= 50 observations		Yes				
States with at least 25% cells containing >= 50 observations			Yes			
States with at least 50% cells containing >= 50 observations				Yes		
States with at least 75% cells containing >= 50 observations					Yes	
States with all cells containing >= 50 observations						Yes
# of States	51	21	19	14	11	2
Observations	2,296	669	663	571	485	92
F-statistic	159.04	435.87	421.38	620.07	659.88	404.99
R-squared	0.0648	0.3952	0.3893	0.5215	0.5774	0.8182

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

Note: Total month-year cells from January 2009-October 2012 = 46.

Table A.3: Effect of Simulated % Eligible*Weighted Share on Probability of Ever Attriting from Sample

VARIABLES	(1)	(2) Controls	(3) Controls	(4) Baseline Controls	(5) Baseline Controls
Simulated % Eligible*Weighted Share	0.0033 (0.0021)	0.0143 (0.0112)	0.0148 (0.0117)	0.0141 (0.0112)	0.0146 (0.0117)
State FE		Yes	Yes	Yes	Yes
Month FE		Yes		Yes	
Year FE		Yes		Yes	
Month-Year FE			Yes		Yes
Single Mothers	3190	3190	3190	3190	3190
Observations	74,194	74,194	74,194	74,194	74,194
R-squared	0.0011	0.0657	0.0660	0.0629	0.0633

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

Notes: Dependent Variable (Attrit) = 1 if single mother ever attrits from the sample. Column (1) regresses Attrit on Simulated % Eligible. Columns (2)-(5) includes controls for education dummies, age, age squared, race dummies, urban/rural status, number of kids under the age of 5, number of disabled kids, state unemployment rate, and a dummy for seam month. Columns (4) and (5) use control variables for first period reported (baseline). Simulated % Eligible*Weighted Share is on a 0 to 100 scale. Regressions use wave 1 person weights and standard errors clustered at the state level.

Table A.4: Effect of Simulated % Eligible on Probability of Ever Attriting from Sample

VARIABLES	(1)	(2) Controls	(3) Controls	(4) Baseline Controls	(5) Baseline Controls
Simulated % Eligible	0.0047**	0.0033	0.0034	0.0033	0.0034
	(0.0018)	(0.0021)	(0.0022)	(0.0021)	(0.0022)
State FE		Yes	Yes	Yes	Yes
Month FE		Yes		Yes	
Year FE		Yes		Yes	
Month-Year FE			Yes		Yes
Single Mothers	3190	3190	3190	3190	3190
Observations	74,194	74,194	74,194	74,194	74,194
R-squared	0.0041	0.0658	0.0661	0.0630	0.0633

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

Notes: Dependent Variable (Attrit) = 1 if single mother ever attrits from the sample. Column (1) regresses Attrit on Simulated % Eligible*Weighted Share. Columns (2)-(5) includes controls for education dummies, age, age squared, race dummies, urban/rural status, number of kids under the age of 5, number of disabled kids, state unemployment rate, and a dummy for seam month. Columns (4) and (5) use control variables for first period reported (baseline). Simulated % Eligible is on a 0 to 100 scale. Regressions use wave 1 person weights and standard errors clustered at the state level.

Table A.5: Effect of Simulated % Eligible on Labor Supply, Longitudinal Panel Weights

VARIABLES	(1) Hours	(2) Hours	(3) Log(Monthly Earnings)	(4) Log(Monthly Earnings)	(5) Real Monthly Earnings	(6) Real Monthly Earnings	(7) Log(Real Hourly Wage)	(8) Log(Real Hourly Wage)	(9) Real Hourly Wage	(10) Real Hourly Wage
Simulated %Eligible*Weighted Share	-0.8602*	-0.8591**	-0.2082***	-0.2059***	-55.0890**	-55.5129***	0.0059	0.0057	0.0473	0.0455
	(0.4464)	(0.4033)	(0.0742)	(0.0700)	(20.9318)	(18.5115)	(0.0085)	(0.0082)	(0.1112)	(0.1062)
Month FE	Yes		Yes		Yes		Yes		Yes	
Year FE	Yes		Yes		Yes		Yes		Yes	
Month-Year FE		Yes		Yes		Yes		Yes		Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Single Mothers (N)	2684	2684	2684	2684	2684	2684	1658	1658	1658	1658
Observations	33,029	33,029	33,029	33,029	33,029	33,029	13,341	13,341	13,341	13,341
R-squared	0.7458	0.7462	0.7537	0.7541	0.8849	0.8851	0.9245	0.9249	0.9399	0.9402

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

Notes: Regressions include education dummies, age, age squared, marital status dummies, urban/rural status, number of children under 5, number of disabled children, and monthly state unemployment rate. Regressions also include a dummy for seam month. The sample is restricted to single mothers who are not self-employed, with determined hours, and non-negative earnings. Real hourly wage is restricted to working mothers with real wages between $2 and $100. Real monthly earnings are restricted to be $0 or up to $10,000. Part Time = Hours > 20. Full Time = Hours > 40. Simulated % Eligible*Weighted Share is on a 0 to 100 scale. All standard errors are clustered at the state level.

Table A.5 (Continued): Effect of Simulated % Eligible on Single Mother Labor Supply, Longitudinal Panel Weights

VARIABLES	(11) P(School = 1)	(12) P(School = 1)	(13) P(Working = 1)	(14) P(Working = 1)	(15) P(Part Time = 1)	(16) P(Part Time = 1)	(17) P(Full Time = 1)	(18) P(Full Time = 1)
Simulated %Eligible*Weighted Share	-0.0118	-0.0116	-0.0173**	-0.0161**	-0.0256**	-0.0253***	-0.0029	-0.0036
	(0.0144)	(0.0134)	(0.0085)	(0.0080)	(0.0105)	(0.0092)	(0.0088)	(0.0080)
Month FE	Yes		Yes		Yes		Yes	
Year FE	Yes		Yes		Yes		Yes	
Month-Year FE		Yes		Yes		Yes		Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Single Mothers (N)	2684	2684	2684	2684	2684	2684	2684	2684
Observations	33,029	33,029	33,029	33,029	33,029	33,029	33,029	33,029
R-squared	0.4987	0.4990	0.7300	0.7304	0.7189	0.7194	0.7076	0.7079

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

Notes: Regressions include education dummies, age, age squared, marital status dummies, urban/rural status, number of children under 5, number of disabled children, and monthly state unemployment rate. Regressions also include a dummy for seam month. The sample is restricted to single mothers who are not self-employed, with determined hours, and non-negative earnings. Real hourly wage is restricted to working mothers with real wages between $2 and $100. Real monthly earnings are restricted to be $0 or up to $10,000. Part Time = Hours > 20. Full Time = Hours > 40. Simulated % Eligible*Weighted Share is on a 0 to 100 scale. All standard errors are clustered at the state level.. All standard errors are clustered at the state level.

Table A.6: Effect of Simulated % Eligible on Single Mother Labor Supply, Longitudinal Panel Weights

VARIABLES	(1) Hours	(2) Hours	(3) Log(Monthly Earnings)	(4) Log(Monthly Earnings)	(5) Real Monthly Earnings	(6) Real Monthly Earnings	(7) Log(Real Hourly Wage)	(8) Log(Real Hourly Wage)	(9) Real Hourly Wage	(10) Real Hourly Wage
Simulated %Eligible	-0.1354*	-0.1330**	-0.0366***	-0.0357***	-10.3415***	-10.3242***	0.0007	0.0007	0.0059	0.0056
	(0.0702)	(0.0639)	(0.0133)	(0.0130)	(3.8279)	(3.4550)	(0.0018)	(0.0018)	(0.0232)	(0.0226)
Month FE	Yes		Yes		Yes		Yes		Yes	
Year FE	Yes		Yes		Yes		Yes		Yes	
Month-Year FE		Yes		Yes		Yes		Yes		Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Single Mothers (N)	2684	2684	2684	2684	2684	2684	1658	1658	1658	1658
Observations	33,029	33,029	33,029	33,029	33,029	33,029	13,341	13,341	13,341	13,341
R-squared	0.7457	0.7461	0.7536	0.7540	0.8849	0.8851	0.9245	0.9249	0.9399	0.9402

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

Notes: Regressions include education dummies, age, age squared, marital status dummies, urban/rural status, number of children under 5, number of disabled children, and monthly state unemployment rate. Regressions also include a dummy for seam month. The sample is restricted to single mothers who are not self-employed, with determined hours, and non-negative earnings. Real hourly wage is restricted to working mothers with real wages between $2 and $100. Real monthly earnings are restricted to be $0 or up to $10,000. Part Time = Hours > 20. Full Time = Hours > 40. Simulated % Eligible is on a 0 to 100 scale. All standard errors are clustered at the state level.

Table A.6 (Continued): Effect of Simulated % Eligible on Single Mother Labor Supply, Longitudinal Panel Weights

VARIABLES	(11) P(School = 1)	(12) P(School = 1)	(13) P(Working = 1)	(14) P(Working = 1)	(15) P(Part Time = 1)	(16) P(Part Time = 1)	(17) P(Full Time = 1)	(18) P(Full Time = 1)
Simulated %Eligible	-0.0032	-0.0032	-0.0030**	-0.0027**	-0.0044**	-0.0043***	-0.0003	-0.0004
	(0.0022)	(0.0020)	(0.0014)	(0.0013)	(0.0017)	(0.0015)	(0.0014)	(0.0013)
Month FE	Yes		Yes		Yes		Yes	
Year FE	Yes		Yes		Yes		Yes	
Month-Year FE		Yes		Yes		Yes		Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Single Mothers (N)	2684	2684	2684	2684	2684	2684	2684	2684
Observations	33,029	33,029	33,029	33,029	33,029	33,029	33,029	33,029
R-squared	0.4989	0.4992	0.7299	0.7304	0.7188	0.7193	0.7076	0.7079

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

Notes: Regressions include education dummies, age, age squared, marital status dummies, urban/rural status, number of children under 5, number of disabled children, and monthly state unemployment rate. Regressions also include a dummy for seam month. The sample is restricted to single mothers who are not self-employed, with determined hours, and non-negative earnings. Real hourly wage is restricted to working mothers with real wages between $2 and $100. Real monthly earnings are restricted to be $0 or up to $10,000. Part Time = Hours > 20. Full Time = Hours > 40. Simulated % Eligible is on a 0 to 100 scale. All standard errors are clustered at the state level.

Table A.7: Effect of Simulated % Eligible on Single Mother Labor Supply, Wave 1 Person Weights

VARIABLES	(1) Hours	(2) Hours	(3) Log(Monthly Earnings)	(4) Log(Monthly Earnings)	(5) Real Monthly Earnings	(6) Real Monthly Earnings	(7) Log(Real Hourly Wage)	(8) Log(Real Hourly Wage)	(9) Real Hourly Wage	(10) Real Hourly Wage
Simulated %Eligible*Weighted Share	-0.6776	-0.6209	-0.1740**	-0.1627**	-51.0374***	-48.6578***	-0.0035	-0.0034	-0.0806	-0.0799
	(0.4050)	(0.3745)	(0.0705)	(0.0656)	(17.7304)	(15.4422)	(0.0062)	(0.0062)	(0.0749)	(0.0740)
Month FE	Yes		Yes		Yes		Yes		Yes	
Year FE	Yes		Yes		Yes		Yes		Yes	
Month-Year FE		Yes		Yes		Yes		Yes		Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Single Mothers (N)	970	970	970	970	970	970	627	627	627	627
Observations	35,138	35,138	35,138	35,138	35,138	35,138	14,200	14,200	14,200	14,200
R-squared	0.7574	0.7577	0.7644	0.7647	0.8881	0.8882	0.9292	0.9297	0.9455	0.9458

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

Notes: Regressions include education dummies, age, age squared, marital status dummies, urban/rural status, number of children under 5, number of disabled children, and monthly state unemployment rate. Regressions also include a dummy for seam month. The sample is restricted to single mothers who are not self-employed, with determined hours, and non-negative earnings. Real hourly wage is restricted to working mothers with real wages between $2 and $100. Real monthly earnings are restricted to be $0 or up to $10,000. Part Time = Hours > 20. Full Time = Hours > 40. Simulated % Eligible*Weighted Share is on a 0 to 100 scale. All standard errors are clustered at the state level.

Table A.7 (Continued): Effect of Simulated % Eligible on Single Mother Labor Supply, Wave 1 Person Weights

VARIABLES	(11) P(School = 1)	(12) P(School = 1)	(13) P(Working = 1)	(14) P(Working = 1)	(15) P(Part Time = 1)	(16) P(Part Time = 1)	(17) P(Full Time = 1)	(18) P(Full Time = 1)
Simulated %Eligible*Weighted Share	-0.0046	-0.0048	-0.0134	-0.0118	-0.0207*	-0.0191*	-0.0009	-0.0010
	(0.0117)	(0.0111)	(0.0080)	(0.0078)	(0.0104)	(0.0096)	(0.0077)	(0.0074)
Month FE	Yes		Yes		Yes		Yes	
Year FE	Yes		Yes		Yes		Yes	
Month-Year FE		Yes		Yes		Yes		Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Single Mothers (N)	970	970	970	970	970	970	970	970
Observations	35,138	35,138	35,138	35,138	35,138	35,138	35,138	35,138
R-squared	0.5019	0.5022	0.7412	0.7415	0.7271	0.7274	0.7161	0.7163

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

Notes: Regressions include education dummies, age, age squared, marital status dummies, urban/rural status, number of children under 5, number of disabled children, and monthly state unemployment rate. Regressions also include a dummy for seam month. The sample is restricted to single mothers who are not self-employed, with determined hours, and non-negative earnings. Real hourly wage is restricted to working mothers with real wages between $2 and $100. Real monthly earnings are restricted to be $0 or up to $10,000. Part Time = Hours > 20. Full Time = Hours > 40. Simulated % Eligible*Weighted Share is on a 0 to 100 scale. All standard errors are clustered at the state level.

Table A.8: Effect of Simulated % Eligible on Single Mother Labor Supply, Wave 1 Person Weights

VARIABLES	(1) Hours	(2) Hours	(3) Log(Monthly Earnings)	(4) Log(Monthly Earnings)	(5) Real Monthly Earnings	(6) Real Monthly Earnings	(7) Log(Real Hourly Wage)	(8) Log(Real Hourly Wage)	(9) Real Hourly Wage	(10) Real Hourly Wage
Simulated %Eligible	-0.1059*	-0.0936	-0.0303**	-0.0278**	-9.8863***	-9.3617***	-0.0010	-0.0010	-0.0174	-0.0172
	(0.0623)	(0.0571)	(0.0117)	(0.0109)	(3.4315)	(3.0385)	(0.0012)	(0.0012)	(0.0139)	(0.0142)
Month FE	Yes		Yes		Yes		Yes		Yes	
Year FE	Yes		Yes		Yes		Yes		Yes	
Month-Year FE		Yes		Yes		Yes		Yes		Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Single Mothers (N)	970	970	970	970	970	970	627	627	627	627
Observations	35,138	35,138	35,138	35,138	35,138	35,138	14,200	14,200	14,200	14,200
R-squared	0.7573	0.7576	0.7644	0.7646	0.8881	0.8882	0.9292	0.9297	0.9456	0.9458

Robust standard errors in parentheses

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

Notes: Regressions include education dummies, age, age squared, marital status dummies, urban/rural status, number of children under 5, number of disabled children, and monthly state unemployment rate. Regressions also include a dummy for seam month. The sample is restricted to single mothers who are not self-employed, with determined hours, and non-negative earnings. Real hourly wage is restricted to working mothers with real wages between $2 and $100. Real monthly earnings are restricted to be $0 or up to $10,000. Part Time = Hours > 20. Full Time = Hours > 40. Simulated % Eligible is on a 0 to 100 scale. All standard errors are clustered at the state level.

Table A.8 (Continued): Effect of Simulated % Eligible on Single Mother Labor Supply, Wave 1 Person Weights

VARIABLES	(11) P(School = 1)	(12) P(School = 1)	(13) P(Working = 1)	(14) P(Working = 1)	(15) P(Part Time = 1)	(16) P(Part Time = 1)	(17) P(Full Time = 1)	(18) P(Full Time = 1)
Simulated %Eligible	-0.0018	-0.0019	-0.0022	-0.0018	-0.0036**	-0.0033**	0.0001	0.0001
	(0.0017)	(0.0016)	(0.0013)	(0.0013)	(0.0016)	(0.0015)	(0.0012)	(0.0011)
Month FE	Yes		Yes		Yes		Yes	
Year FE	Yes		Yes		Yes		Yes	
Month-Year FE		Yes		Yes		Yes		Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Single Mothers (N)	970	970	970	970	970	970	970	970
Observations	35,138	35,138	35,138	35,138	35,138	35,138	35,138	35,138
R-squared	0.5020	0.5023	0.7411	0.7414	0.7270	0.7274	0.7161	0.7163

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

Notes: Regressions include education dummies, age, age squared, marital status dummies, urban/rural status, number of children under 5, number of disabled children, and monthly state unemployment rate. Regressions also include a dummy for seam month. The sample is restricted to single mothers who are not self-employed, with determined hours, and non-negative earnings. Real hourly wage is restricted to working mothers with real wages between $2 and $100. Real monthly earnings are restricted to be $0 or up to $10,000. Part Time = Hours > 20. Full Time = Hours > 40. Simulated % Eligible is on a 0 to 100 scale. All standard errors are clustered at the state level.

CHAPTER 2 APPENDIX B

Creating Simulated Eligibility Measure

To create a simulated measure of potential eligibility for child care subsidies, I first take a nationally representative sample of single or married mothers from the 2004 SIPP. I then apply each of the 50 states and the District of Columbia's policy rules for eligibility for each month and year to determine a total number of eligible mothers from my sample. To determine eligibility, I use data from the CCDF policies database on acceptable work activities, income deductions and disregards, family size definitions, and maximum income thresholds.

Eligibility determination based on CCDF policy rules is a 4-step procedure:

1. Does the single/married mother have an eligible child?
    - If the parent has a child under the age of 13, proceed to the next step. (Note: The maximum age is 12 for Oregon 2009-2012 and Wyoming 2012)
    - If the parent does not have a child under the age of 13, they are not eligible.
2. Is the parent working or participating in an acceptable work-related activity?
    - This varies from state-to-state, but acceptable work-related activities include going to school (either for a high school diploma or post-secondary education), job search, or job training.
    - If the parent is participating in a state-approved work activity, proceed to the next step.

3. Calculate income. I take family earned income for the month and apply relevant deductions and disregards.
    - Deductions – Includes Temporary Assistance for Needy Families (TANF), Social Security Income (SSI), Supplemental Security Disability Income (SSDI), General Assistance, Food Stamps, Child Support, etc.
    - Disregards – Any income disregarded
4. Compare income to maximum income eligibility threshold. This varies based on family size.
    - If income $<$ maximum income threshold, the parent is eligible for a subsidy
    - For Minnesota, the maximum income threshold depends on TANF status

After conducting the procedure above, I have a count of potentially eligible single or married mothers in my 2004 SIPP sample for each state and month-year period. I construct my simulated % eligible variable for single mothers by dividing the total potentially eligible single mothers by the count of single mothers in my sample (weighted using probability weights).[66]

$$Simulated\ \%\ Eligible_{st} = \frac{Total\ Potentially\ Eligible}{Total\ Single\ Mothers} * 100$$

I have to make some assumptions to construct my simulated eligibility measure. I assume that the parent is looking for full-time care. If a state had multiple policies enacted within the same month (due to a change in policy), I use the policy in effect at the end of

[66] The measure for married mothers divides total potentially eligible married mothers by the total number of married mothers in the sample.

the month. Since I do not have policy data for families bigger than 10 individuals, I apply rules for families of size 10 if the family is larger than 10.

Occasionally, state policy rules vary at the sub-state region. If they vary at the sub-state region, I apply the policy for the state's most populous region. For work activities and income definitions, I apply the rules for New York counties outside of New York City, and for Texas' Gulf Coast Region. For maximum income thresholds, I apply the rules for "Group 3" in Virginia and assume they are not receiving transitional child care.

For income definitions, I assume that TANF, SSI, and SSDI are fully counted. Some states only partially count these variables (only one to seven states), but the policies database does not explain how they are counted.

Finally, I am missing some data. I am missing data on work requirements for 4 states (Maine, January 2009-September 2009; South Carolina, January 2009-July 2009; Vermont, January 2009; and Wyoming, January 2009-September 2009). I am missing data on income definitions for 5 states (District of Columbia, January 2009-August 2009; Maine, January 2009-September 2009; South Carolina, January 2009-July 2009; Vermont, January 2009; and Wyoming, January 2009-September 2009).

CHAPTER 3 APPENDIX

Table A.1: Summary of the Predicted Effects of a State EITC Increase

Group		Predicted Labor Supply Response	Predicted Federal EITC Participation Response	Predicted Federal EITC Expenditures Response
Single Filers with Children	Not Working Pre-EITC	Increase in LFP	Increase	Increase
	Pre-EITC Income Within EITC Eligibility Range	Ambiguous effect on hours in the phase-in region. Decrease in hours in the plateau and phase-out regions.	No change (individual already receives the EITC)	Ambiguous effect on federal expenditures in the phase-in region. No change in plateau region (maximum credit received). Increase in expenditures in phase-out region.
	Pre-EITC Income Above EITC Phase-Out Range	Decrease in hours	Increase	Increase
Married Filers with Children (Secondary Earner Responses)*	Not Working Pre-EITC	1. Increase in LFP if primary and secondary earners both not working pre-EITC. 2. Ambiguous effect on LFP for the secondary earner if only the primary worker is working and the family already receives the EITC	1. Increase 2. No change (family already receives the federal EITC)	1. Increase 2. Ambiguous
	Pre-EITC Family Income within EITC Eligibility Range	Ambiguous effect on hours and/or LFP in the phase-in region, decrease in hours and/or LFP in the plateau and phase-out regions.	No change (family already receives the EITC)	Same as single filers with children.
	Pre-EITC Family Income Above EITC Phase-Out Range	Decrease in hours and/or LFP.	Increase	Increase

*The predicted effects for primary earners in married households are the same as single filers.

Table A.2: Estimated EITC Effects on Employment, Average Low-skilled Share, 1997-2008

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Single Individuals with Children		Single Women with Children		Married Individuals with Children		Married Women with Children	
EITC	0.10	0.13*	0.11	0.14*	-0.08	-0.08	-0.05	-0.02
	(0.06)	(0.06)	(0.07)	(0.07)	(0.06)	(0.08)	(0.09)	(0.12)
EITC*Average low-skilled share	-3.07		-3.37*		-0.29		0.43	
	(1.87)		(1.79)		(0.61)		(1.21)	
EITC*Average low-skilled with 2+ children share		-1.20		-1.37		-0.31		0.95
		(1.21)		(1.48)		(0.97)		(1.68)
State FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Observations	612	612	612	612	612	612	612	612
R-squared	0.67	0.67	0.64	0.64	0.86	0.86	0.86	0.86

Robust standard errors in parentheses

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

Notes: Regressions control for state unemployment rate, state minimum wage, and state demographic measures including percent black, Hispanic, female, and single. Regressions also control for average age, the share of the state's population with 2+ kids, and the share of the state's population with a child under the age of 6. The EITC and share low-skilled variables are demeaned in the specifications with EITC*low skilled interactions (columns 3-8). Estimates are weighted by the number of observations and standard errors are clustered at the state level.

Table A.3: Estimated State EITC Effects on Federal EITC Filers Per Potential Filer, 1997-2008

VARIABLES	(1)	(2)	(3)	(4)	(5)
	Single with Children		Married with Children		Childless
EITC	0.15	0.11	-0.08	-0.07	0.04*
	(0.25)	(0.25)	(0.08)	(0.08)	(0.02)
EITC*Average low-skilled share	7.26		-0.41		0.47
	(5.00)		(0.79)		(0.35)
EITC*Average low-skilled with 2+ children share		5.89		-0.23	
		(4.47)		(1.04)	
State MW	-0.33	-0.34*	0.05*	0.05*	0.01
	(0.20)	(0.20)	(0.03)	(0.03)	(0.02)
State FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Observations	612	612	612	612	612
R-squared	0.80	0.80	0.85	0.85	0.66

Robust standard errors in parentheses

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

Notes: Regressions control for annual state average unemployment rate, state demographic measures including percent black, Hispanic, female, and average age. Regressions also control for the share of the state's population with 2+ children. The EITC and share low-skilled variables are demeaned in the specifications with EITC*low skilled interactions. Estimates are weighted by the number of potential filers and standard errors are clustered at the state level.

Table A.4: Estimated State EITC Effects on Federal EITC Expenditures Per Potential Filer, 1997-2008

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Single with Children		Married with Children		Childless
EITC	0.68	0.69	-0.40*	-0.38	0.01**
	(0.72)	(0.71)	(0.23)	(0.25)	(0.01)
EITC*Average low-skilled share	24.40		-3.08		0.11
	(15.81)		(2.64)		(0.07)
EITC*Average low-skilled with 2+ children share		26.92*		-3.47	
		(13.73)		(3.69)	
State MW	-1.13*	-1.17*	0.07	0.07	0.00
	(0.63)	(0.63)	(0.07)	(0.07)	(0.00)
State FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Observations	612	612	612	612	612
R-squared	0.82	0.82	0.84	0.84	0.61

Robust standard errors in parentheses

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

Notes: EITC Expenditures are in real 2008 thousands of dollars. Regressions control for annual state average unemployment rate, state demographic measures including percent black, Hispanic, female, and average age. Regressions also control for the share of the state's population with 2+ children. The EITC and share low-skilled variables are demeaned in the specifications with EITC*low skilled interactions. Estimates are weighted by the number of potential filers and standard errors are clustered at the state level.