2017

ESSAYS ON INTERGENERATIONAL DEPENDENCY AND WELFARE REFORM

Robert Paul Hartley
University of Kentucky, robertpaul.hartley@uky.edu
Author ORCID Identifier:
https://orcid.org/0000-0002-5640-9169
Digital Object Identifier: https://doi.org/10.13023/ETD.2017.286

STUDENT AGREEMENT:

I represent that my thesis or dissertation and abstract are my original work. Proper attribution has been given to all outside sources. I understand that I am solely responsible for obtaining any needed copyright permissions. I have obtained needed written permission statement(s) from the owner(s) of each third-party copyrighted matter to be included in my work, allowing electronic distribution (if such use is not permitted by the fair use doctrine) which will be submitted to UKnowledge as Additional File.

I hereby grant to The University of Kentucky and its agents the irrevocable, non-exclusive, and royalty-free license to archive and make accessible my work in whole or in part in all forms of media, now or hereafter known. I agree that the document mentioned above may be made available immediately for worldwide access unless an embargo applies.

I retain all other ownership rights to the copyright of my work. I also retain the right to use in future works (such as articles or books) all or part of my work. I understand that I am free to register the copyright to my work.

REVIEW, APPROVAL AND ACCEPTANCE

The document mentioned above has been reviewed and accepted by the student's advisor, on behalf of the advisory committee, and by the Director of Graduate Studies (DGS), on behalf of the program; we verify that this is the final, approved version of the student's thesis including all changes required by the advisory committee. The undersigned agree to abide by the statements above.

Robert Paul Hartley, Student

Dr. James P. Ziliak, Major Professor

Dr. Jenny Minier, Director of Graduate Studies

ESSAYS ON INTERGENERATIONAL DEPENDENCY AND WELFARE REFORM

DISSERTATION

A dissertation submitted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in the Gatton College of Business and Economics at the University of Kentucky

By

Robert Paul Hartley

Lexington, KY

Co-Directors: Dr. James P. Ziliak, Professor of Economics

and Dr. Carlos Lamarche, Professor of Economics

Lexington, KY

ABSTRACT OF DISSERTATION

ESSAYS ON INTERGENERATIONAL DEPENDENCY
AND WELFARE REFORM

This dissertation consists of three essays related to the effects of welfare reform on the intergenerational transmission of welfare participation as well as effects on labor supply and childcare arrangements. States implemented welfare reform at different times from 1992 to 1996, and these policies notably introduced work requirements and other restrictions intended to limit dependency of needy families. One mechanism reforms were intended to address was childhood exposure to a "culture" of ongoing welfare receipt. In Essay 1, I estimate the effect of reform on the transmission of welfare participation for 2961 mother-daughter pairs in the Panel Study of Income Dynamics (PSID) over the period 1968-2013. I find that a mother's welfare participation increased her daughter's odds of participation as an adult by roughly 30 percentage points, but that welfare reform attenuated this transmission by at least 50 percent, or at least 30 percent over the baseline odds of participation. While I find comparable-sized transmission patterns in daughters' adult use of the broader safety net and other outcomes such as educational attainment and income, there is no diminution of transmission after welfare reform. In Essay 2, I estimate behavioral labor supply responses to reforms using experimental data from Connecticut's Jobs First welfare waiver program in 1996. Recent studies have used a distributional analysis of Jobs First suggesting evidence that some individuals reduce hours in order to opt into welfare, an example of behavioral-induced participation. However, estimates obtained by a semi-parametric panel quantile estimator allowing women to vary arbitrarily in preferences and welfare participation costs indicate no evidence of behavioral-induced participation. These findings show that a welfare program imposes an estimated cost up to 10 percent of quarterly earnings, and these costs can be heterogeneous throughout the conditional earnings distribution. Lastly, in Essay 3, I return to PSID data to examine the relationship between welfare spending on childcare assistance and the care arrangements chosen by low-income families. Experimental evidence has shown that formal child care can result in long-term socioeconomic gains for disadvantaged children, and work requirements after welfare reform have necessitated increased demand for child care among single mothers. I find that an increase of a thousand dollars in state-level childcare assistance per child in poverty increases the probability of formal care among low-earnings single-mother families by about 27 to 30 percentage points. When public assistance makes child care more affordable, families within the target population reveal a higher preference for formal care relative to informal, which may be related to perceived quality improvements for child enrichment and development.

KEYWORDS: Welfare reform; Intergenerational persistence; Program participation; Heterogeneous treatment effects; Labor supply; Childhood development

Robert Paul Hartley

July 11, 2017

Date

ESSAYS ON INTERGENERATIONAL DEPENDENCY
AND WELFARE REFORM

By

Robert Paul Hartley

Dr. James P. Ziliak

Co-Director of Dissertation

Dr. Carlos Lamarche

Co-Director of Dissertation

Dr. Jenny Minier

Director of Graduate Studies

July 11, 2017

Date

In loving memory of Virginia and Mary Alice

ACKNOWLEDGMENTS

Throughout my doctoral studies and dissertation research, I have benefited greatly from the collegiality and collaboration within the Department of Economics along with support from the Graduate School. Additionally, I received a formative investment in my dissertation through the Summer School on Socioeconomic Opportunity at the University of Chicago. I am especially thankful for the generous and invaluable mentoring by Jim Ziliak and Carlos Lamarche, including my time working in the Center for Poverty Research. Moreover, I am grateful for the support of Chris Bollinger and Genia Toma on my dissertation committee, as well as the service of Derek Young as outside examiner. I am lucky to have great people in my life who have believed in me, including the support of Gail Hoyt, Klara Sabirianova Peter, Chris Rollston, and Joel Sokol, as well as the inspiration of former teachers Rick Harper, Lou Tiller, and John Sharpe. Finally, my greatest admiration and appreciation belong to my partner Danielle along with our encouraging, loving children Elijah, Nora, and John.

TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

INTRODUCTION

A fundamental question for social welfare policy is whether assistance is helping or hurting, and by how much? The classical conjecture is that cash assistance discourages labor supply, which in turn makes it harder for low-income families to exit poverty and dependence. In the case of able-bodied adults with dependents, welfare dependence may be defined as an increasing propensity to participate in assistance programs given prior participation. A compounding policy issue for welfare dependence, however, is the second-generation effect for dependents who are exposed to poverty and welfare during childhood. If a child grows up in a family that receives welfare, what effect does this have on that child's socioeconomic outcomes later in adulthood?

There are competing stories in public debates on the nature of welfare dependence. Generally, the safety net is intended to support families who experience some negative income shock, which may lead to either temporary or persistent states of need. Many spells of welfare participation are relatively short, yet historically, periods of rising caseloads have prompted reform policies concerned with childhood exposure to a "culture of welfare" in the home. At issue is whether a typical welfare case is characterized as a kind of short-run social insurance that helps families smooth consumption and make long-run child-development investments, or whether welfare use creates a poverty and/or welfare trap such that dependence increases for both generations.

The following essays are concerned with behavioral responses to welfare participation and implications for the transmission of poverty and dependence in the welfare reform era since the 1990s. For social assistance programs to be effectively designed to alleviate poverty and promote sustainability, it is important to understand how policies change family incentives for short-run labor supply decisions as well as long-run human capital investment decisions as a two-generation problem. The theme for this dissertation relates to the outcomes for low-income, single-mother families relative to policies for welfare and work requirements. Evidence concerning welfare dependence is incomplete, partially due to the difficulty of analyzing family choices when ability, preferences, and constraints are not easily observed. However, the policy implications of properly identifying program effects can be long lasting if our public response to poverty now is causally related to levels of poverty and dependence in the future.

In Essay 1, I estimate the effect of welfare reform on the intergenerational transmission of welfare participation among mothers and daughters. Essentially, this transmission is defined by the effect that childhood exposure to a mother's welfare participation has on a daughter's subsequent participation decision as an adult. One of the primary goals of reform was to break the link of welfare dependence, yet there has been no study of whether intergenerational dependence has changed after the reforms of the 1990s. Restrictive reform measures such as work requirements, time limits, and sanctions have greatly decreased welfare caseloads and thus reduced childhood exposure. Now, over 20 years after the major reform period, children during that period can be observed as adults making their own welfare participation decisions.

Using the Panel Study of Income Dynamics (PSID) over the survey period 1968-2013, I find that a mother's welfare participation increases the odds that her daughter participates as an adult by about 30 percentage points. Across a range of models, welfare reform reduces this transmission by at least 50 percent, or adjusting for lower participation after reform, by at least 30 percent over the baseline odds of participation. When considering the daughter's participation in the wider safety net, as opposed to cash assistance, welfare reform has no effect in diminishing participation. Similarly, other economic outcomes for the daughter—such as educational attainment or family income—are negatively related with childhood welfare exposure but undiminished by reforms. These results address potential threats to identification including nonrandom selection into welfare, misclassification error, life-cycle age effects, and cross-state mobility. While welfare reforms were partially effective at reducing dependence on cash assistance, dependence on welfare more broadly has been unchanged and daughters appear to be no better off as adults. Further, some amount of learning or reduction of stigma may be beneficial given that the take-up rate for welfare remains low among eligible mothers. More research is warranted on optimal transfer program design given knowledge spillovers across generations.

In Essay 2, I examine the behavioral response to a work incentive introduced by welfare reform, the earnings disregard, which may have the unintended consequence of causing individuals to reduce their labor supply to be income-eligible. This short-run form of dependence would be considered an example of behavioral-induced participation. Before reform, additional earnings corresponded to equal reductions in cash assistance, while after reform, some states chose to disregard up to 100 percent of earnings in determining the benefit amount. In such cases, a rational individual may attempt to constrain total earnings to some point just below the eligibility cutoff. If high earnings disregards induced more welfare participation, the effects of reform may decrease labor supply for women near the federal poverty line while increasing labor supply for individuals at lower levels of earnings.

Based on experimental data from Connecticut's Jobs First welfare waiver program in 1996, I find no evidence of behavioral-induced participation. In a nonparametric comparison of the welfare recipients and applicants randomly assigned to treatment groups, the reform led to -200 dollars of earnings per quarter for women at the 90th quantile of earnings in the sample. However, women in the treatment group may face welfare participation costs to labor supply compared to women in the control group who are no longer income-eligible to participate and have no comparable transaction costs on their time. Using a semiparametric panel quantile estimator that allows for women to vary arbitrarily in preferences and costs of welfare participation, I find a positive treatment effect of 300 dollars at the upper distribution of earnings. This finding suggests that welfare participation can cost up to 10 percent of quarterly earnings, which corresponds to estimates in the literature. Further, subgroup analysis by participant type with respect to new applicants versus continuing recipients reveals that the most relevant unobserved costs of participation may be related to labor-force informational costs or persistent stigma for longer-term welfare recipients.

Lastly, in Essay 3, I explore the effect of childcare assistance on formal care arrangements among low-income single mothers in the period after welfare reform. Given the work requirements on welfare participation after reform, mothers must choose between informal child care such as a grandmother or neighbor, or more formal arrangements such as center-based child care. Research has shown that disadvantaged children experience better educational outcomes given formal child care, possibly owing to a comparatively rich learning environment conditional on the family's lower socioeconomic status. Child-care assistance funding ramped up in the early 1990s, and by 1996, states were granted a much larger degree of discretion in allocating federal block grant funds toward child care. For instance, after reforms some states began to allocate more welfare funds toward childcare assistance than toward cash assistance.

State-level childcare assistance expenditures after reform may be characterized by two stylized facts: first, spending per child in poverty varies widely by state and over time within states; and, second, childcare subsidy take up is very low on average, implying many welfare-eligible families choose informal care. Using PSID data on family childcare choices along with state expenditure and policy measures from various government sources, I find that an additional thousand dollars of childcare assistance per child in poverty raises the probability a family uses formal child care by around 27 to 30 percentage points. Since experimental evidence suggests positive effects of child care for low-income children, I plan to use childcare assistance policy in future work as a first stage predictor of care arrangement in order to test the effects of formal child care on educational outcomes using observational data that is nationally-representative.

1 WELFARE REFORM AND THE INTERGENERATIONAL TRANSMISSION OF DEPENDENCE

1.1 Introduction

A fundamental goal of the landmark 1996 welfare reform in the United States was to eliminate the dependence of needy families on government assistance. This was premised in part on the belief that dependence is passed down from parent to child through knowledge and values, creating a "culture of welfare" across generations (DeParle, 2004; Haskins, 2007; Murray, 1984). While this belief was bolstered by an empirical consensus documenting a positive intergenerational correlation of welfare use, the literature is much less settled on whether the relationship is causal (Borjas and Sueyoshi, 1997; Dahl, Kostøl, and Mogstad, 2014; Duncan, Hill, and Hoffman, 1988; Gottschalk, 1990, 1992, 1996; Levine and Zimmerman, 1996; McLanahan, 1988; Page, 2004; Pepper, 2000; Solon et al., 1988). Instead, the parent-child link in welfare participation could simply be a spurious by-product of incomes that are correlated across generations. That is, low economic mobility across generations means that children of parents with low incomes likely have low incomes themselves in adulthood, and both generations participate in means-tested programs solely because of their shared poverty status and not welfare exposure per se. If true, then one should not expect generational welfare participation to fall after reform unless poverty among the young declined. Scores of papers have been written evaluating welfare reform (see surveys in Blank, 2002; Grogger and Karoly, 2005; Moffitt, 2003; Ziliak, 2016), but to date there has not been research on whether it achieved a key aim of ending generational welfare dependence.

In this essay, I estimate the effect of welfare reform on the intergenerational transmission of welfare participation. In addition, because the goal of welfare reform was to reduce dependency more broadly, I also estimate whether reform changed the relationship between parental welfare use and other adult economic outcomes of the child including human capital attainment, employment, and poverty status. The empirical framework I use builds on a canonical Becker-Tomes (1979) transmission model with a difference-in-difference-type specification whereby the economic outcome of the child during adulthood is regressed on the prior welfare participation of the parent, a variable reflecting the implementation of welfare reform in the parent's state, and the interaction of the welfare-reform variable with parent's participation. The identification strategy exploits the quasi-experimental variation provided by the 1990s reforms to the Aid to Families with Dependent Children (AFDC) program in the United States. AFDC was established during the Great Depression and was the main cash transfer program for families with dependent children. Conditional on low income and assets, along with the presence of children under age 18, eligibility for assistance was an entitlement. Starting in 1992, states began implementing substantive changes to their AFDC programs with waivers from federal rules, and by 1996, 43 states had implemented some form of waiver affecting program features such as new work requirements, time limits on length of receipt, and caps on benefit generosity. These waivers culminated with passage of the Personal Responsibility and Work Opportunity Reconciliation Act of 1996, which replaced AFDC with the non-entitlement block grant program Temporary Assistance for Needy Families (TANF).

Even though welfare reform provides exogenous variation in access to program benefits across welfare eras, identifying whether there is a causal pathway from parent to child in welfare use *within* periods is complicated by four—potentially reinforcing—forms of bias. First, selection bias in welfare participation across generations can arise through possible unobserved correlations in labor market productivity between the parent and child, perhaps because of latent shared cognitive or noncognitive skills, or shared tastes for welfare relative to work (Gottschalk, 1992, 1996; Pepper, 2000; Solon et al., 1988). The second threat to identification comes from potential misclassification bias in survey responses (Bollinger and David, 1997, 2001; Hausman, Abrevaya, and Scott-Morton, 1998; Kreider et al., 2012; Meyer and Mittag, 2014). In transfer programs, this nonclassical measurement error mostly comes in the form of "false negatives" when the respondent states they did not participate in a program when in fact they did. Meyer, Mok, and Sullivan (2015a,b) document a trend increase in misreporting across all major household surveys in the U.S., including the PSID. Third, so-called life-cycle bias and the 'window problem' may affect intergenerational estimates of economic status because researchers generally only observe snapshots of a parent and child and not their full life cycles (Haider and Solon, 2006; Nybom and Stuhler, 2016; Page, 2004; Wolfe et al., 1996). In the welfare context, this form of bias may exacerbate or attenuate intergenerational transmission estimates depending on whether the window of parent-child observations is dominated by families in the midst of long-term welfare spells. Fourth, there could be bias in the transmission estimates if the child moves across states as an endogenous response to the generosity of the state's welfare system (Gelbach, 2004; Kennan and Walker, 2010; Levine and Zimmerman, 1999; McKinnish, 2007).

To estimate the model, I assemble a long panel of mother-daughter pairs over the survey period 1968-2013 in the Panel Study of Income Dynamics (PSID). I focus on mother-daughter pairs because over 90 percent of AFDC cases were headed by a single mother, and there has been a large secular increase since the 1960s in the fraction of first births to unmarried women in the U.S. from fewer than 1 in 10 to over 4 in 10 such that more than one third of U.S. children were exposed to welfare by age 10 (Cancian and Reed, 2009; Levine and Zimmerman, 2005). I address potential endogenous selection into welfare by instrumenting for mother's welfare use. Because selection is likely to be time-varying, I instrument mother's welfare participation with the state maximum AFDC/TANF benefit guarantee and the maximum federal and state Earned Income Tax Credit (EITC) when daughters are ages 12 to 18. These instruments are constructed during a daughter's critical ages of exposure to her mother's potential welfare, which is generally well before she faces a participation decision as an adult. The mother's welfare participation decision is assumed to respond positively to greater state-level AFDC/TANF benefit standards, whereas EITC benefits may offer a substitute for AFDC/TANF assistance. Fundamentally, these aggregated measures of state-level policies identify the portion of a mother's participation decision that are related to her welfare status separately from conditions related to her poverty status, and consequently, her daughter's future poverty status.

Next, I address the implications of misclassified welfare participation, which may occur in both the dependent variable for daughters as well as the independent variable for mothers. Instruments for mother's participation will partially address misclassification in the right-hand-side variable, and I use a relatively long time history to determine whether the mother ever participated on welfare in the past, which also should attenuate measurement error compared to a contemporaneous measure. I address misclassification bias in the dependent variable by parametric methods using "extra-sample" information based on PSID reporting rates estimated in Meyer et al. (2015b).

I attempt to mitigate the influence of the life-cycle window problem by using the relatively long time series for each mother-daughter pair now available in the PSID. To be included in the estimations sample, mother and daughter must live together at least 5 years during the critical exposure period of ages 12-18, and to observe the daughter for at least five years after she forms her own family unit. On average, I observe mothers and daughters co-residing for 14 years, and daughters for nearly 25 years as head of their own family, and thus I observe the full welfare life cycle for many mother-daughter pairs. As a sensitivity check, I also estimate a variant of the model with the Lee and Solon (2009) age-adjustment in order to re-center the data at a common point in the mothers' and daughters' life cycles. Lastly, for the issue of cross-state mobility, I examine the sensitivity of estimates to possible endogenous migration by examining various subsamples of non-movers.

The estimates show that there is strong evidence for a causal transmission of AFDC/TANF participation from mother to daughter, and it is economically sizable, on the order of 30 percentage points. However, welfare reform significantly attenuated the level of transmission pathway by at least 50 percent, or at least 30 percent over the baseline probability. Moreover, I find that childhood exposure to welfare substantively increases the use of the wider safety net, the odds of nonemployment, and the odds of family earnings at poverty or near poverty levels. Yet in these cases, welfare reform did not affect the transmission path, leaving daughters no better off in broader economic status. Estimates of the reform effect are robust across a variety of specifications, including the length of mother-daughter observation window, the age of welfare exposure by the daughter when living at home, the length of time the daughter is exposed to welfare, life-cycle age adjustments, and misclassification error.

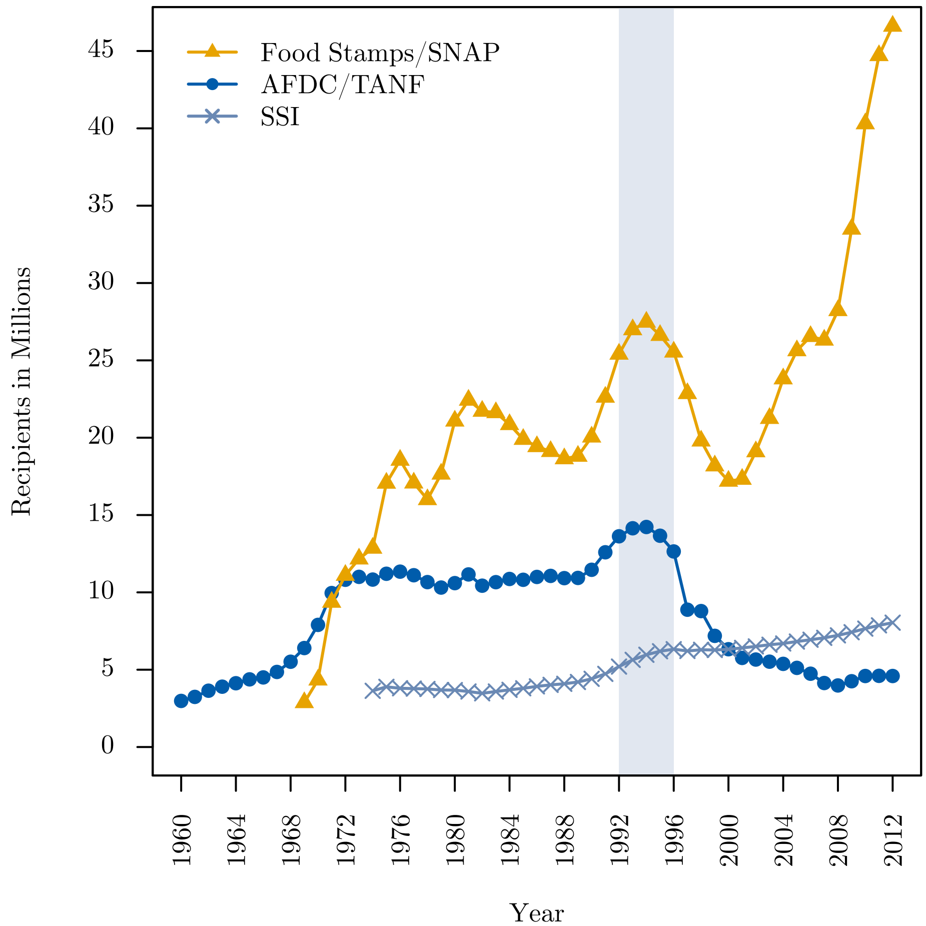
1.2 Welfare Reform and Intergenerational Transmission

"Welfare" in the U.S. through the 1980s was largely defined by the AFDC program, which was established as part of the Social Security Act of 1935 to assist low-income families with children under age 18. Initially, assistance was restricted to the children of destitute widows and widowers, and then later was expanded to cover the guardian of the child, and eventually a second parent if present in the household. In well over 90 percent of the cases, the family was headed by a single mother. Eligibility for assistance (conditional on the presence of a dependent child under age 18) was determined by an income test, a liquid asset test, and a vehicle asset test. The federal government set rules on what counted as income or an asset, and also established limits on the dollar value of those resources. States did have authority to set maximum benefit levels (which increase with family size) and need standards used in assigning income eligibility. The program was an entitlement funded by a federal-state matching grant based on state per-capita income, with the federal government picking up over 60 percent of expenditure on average (Ziliak, 2016).

Beginning in the 1960s, states could apply for waivers from federal rules to experiment with program features, but with few exceptions, they did not utilize this flexibility, and when they did, it was typically for small pilot programs. This changed in the last half of the President George H.W. Bush administration when several states filed waiver applications, and then accelerated under President Clinton, who had pledged to "end welfare as we know it" as part of his 1992 campaign. By 1996, 43 states had waivers approved by the Department of Health and Human Services (Grogger and Karoly, 2005). The waivers were far reaching, and included both strengthening and expanding of pre-existing policies (e.g. work requirements and sanctions on benefits for failing to work or participate in a training program introduced as part of the Family Support Act of 1988), as well as new policies aimed at family responsibility (e.g. caps on the generosity of benefits by family size and time limits on benefit receipt). Some of the new policies actually expanded eligibility, such as higher asset limits and earnings disregards for benefit determination, but the majority were designed to restrict program access. Time-limit waivers in particular were introduced to break long-term spells on AFDC, and in turn to reduce exposure of children to parental use of welfare.

The state-level waivers were codified into federal law with passage of the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) in August of 1996. PRWORA replaced AFDC with a new program called Temporary Assistance for Needy Families (TANF), which is not an entitlement. The new law established federal maximum guidelines regarding funding, work requirements, and time limits, but otherwise devolved much more program design authority to the states. For example, the federal lifetime time limit for benefits for an adult is five years, but nearly half the states opted to impose shorter limits. Nineteen states now require some form of mandatory job search at the point of benefit application, and in fourteen of those states the sanction for non-

Figure 1.1. Trends in AFDC/TANF, Food Stamps/SNAP, and SSI Recipients



Notes: Authors' tabulations of data collected from the U.S. Department of Health and Human Services, U.S. Department of Agriculture, and Social Security Administration. The major waiver period of welfare reform is indicated by the shaded region. Abbreviations: Aid to Families with Dependent Children/Temporary Assistance for Needy Families (AFDC/TANF), Supplemental Nutrition Assistance Program (SNAP), and Supplemental Security Income (SSI).

compliance is to deny the application. Seventeen states have opted to impose a family cap on benefit generosity, and thirty-two states introduced "diversion payments" that steer eligible applicants away from the official caseload and toward a lump-sum payment, typically valued at three months of the maximum benefit for a given family size (Ziliak, 2016).

Figure 1.1 depicts trends in the number of persons on AFDC/TANF, spanning the AFDC era (1960-1991), the major waiver period (1992-1996 shaded in gray), and the

TANF era (from 1997 onward). Participation accelerated throughout the 1960s from about 3 million persons in 1960 to 10 million a decade later. The level of recipients remained fairly constant for nearly two decades, and then increased by approximately 30 percent from 1989 to 1994. By 2012, however, the number of recipients had plummeted 67 percent to levels roughly the same as five decades earlier. Numerous studies demonstrated that while the economy accounted for more of the decline in welfare in the mid 1990s, welfare waivers also reduced participation, especially in those states adopting more stringent responsibility and time limit policies (Blank, 2001; Council of Economic Advisers, 1997; Grogger, 2003; Ziliak et al., 2000). For those few studies that examined caseload decline after passage of PRWORA, greater weight was given to policy reforms in accounting for the decline in participation compared to the waiver era, though the macroeconomy was still the driving force (Grogger and Karoly, 2005). The declining participation stemmed more from reduced entry onto welfare than from increased exits (Frogner, Moffitt, and Ribar, 2009; Grogger, Haider, and Klerman, 2003; Haider and Klerman, 2005).

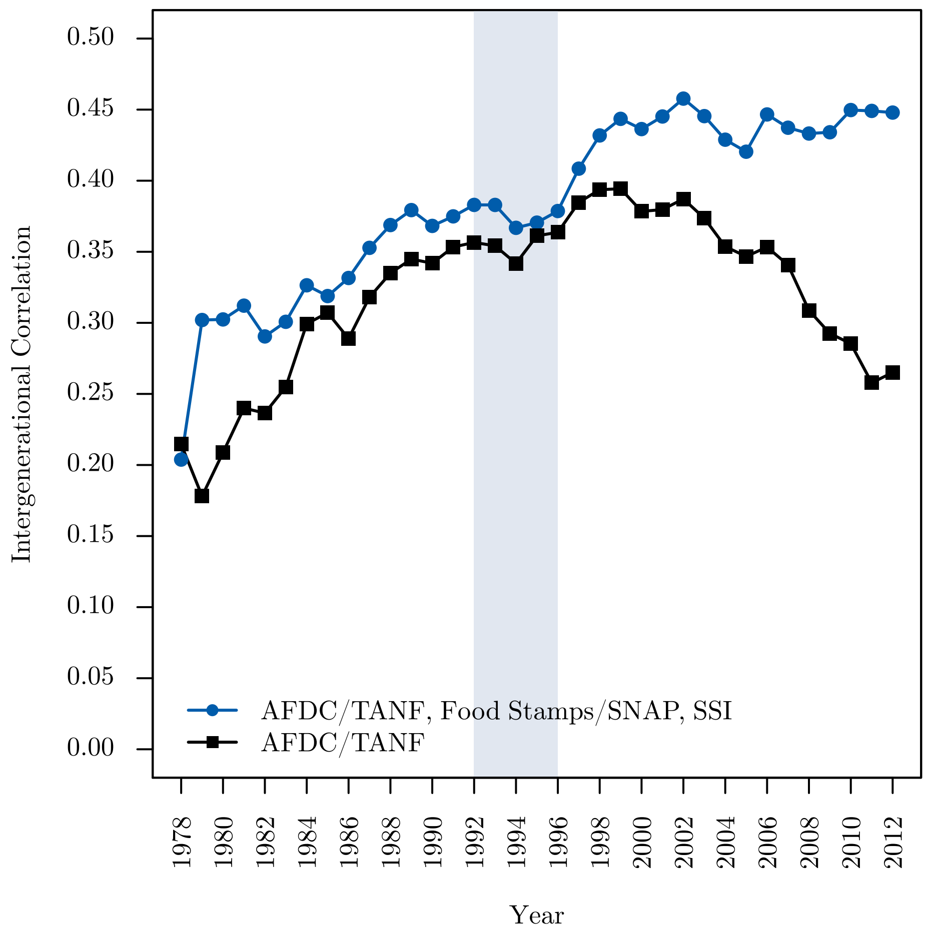
Families that received AFDC were categorically eligible for food assistance from the Food Stamp Program, which started in 1964 but took nearly a decade to roll out nationwide (and was renamed Supplemental Nutrition Assistance Program (SNAP) in 2008). Receipt of AFDC was not necessary for eligibility for food stamps, but it was sufficient, and typically about 80 to 90 percent of AFDC recipients took up both (Committee on Ways and Means, U.S. House of Representatives, 1994). This categorical eligibility remained after the introduction of TANF. While any given individual on AFDC could not simultaneously receive assistance from the disability program Supplemental Security Income (SSI), which began in 1972, it was possible for families to combine benefits with some on AFDC and some on SSI (and still also qualify for food stamps). These provisions remain after welfare reform.

Figure 1.1 also presents trends in the number of recipients on food stamps and SSI. There was a marked drop in food stamp participation in the immediate aftermath of welfare reform, followed by a huge expansion in the subsequent decade. These swings have been attributed to changes in the macroeconomy, welfare and food stamp policies, and program take-up rates among those eligible (Ganong and Liebman, 2013; Ziliak, 2015). There has also been growth in SSI, especially after 1990 when the Supreme Court's *Zebley Decision* expanded eligibility for children (Kubik, 1999), and again after welfare reform where there is some evidence that states systematically facilitated the applications of former AFDC recipients for SSI program benefits (Schmidt and Sevak, 2004). The implication is that even if welfare reform succeeded in breaking the generational cycle on AFDC/TANF, it is not clear *a priori* that it reduced dependence more broadly when additional safety net programs are considered.

As motivating evidence for the role of welfare reform on the intergenerational transmission of dependence, Figure 1.2 presents the correlation between mother's and daughter's welfare participation for rolling cohorts of daughters over time based on the PSID. No attempt is made here to separate out cause and effect, only correlations over time in order to illustrate the trend and to anchor these estimates to those in the prior literature as summarized in Page (2004).[1] Figure 1.2 shows that the intergenerational correlation

[1]Specifically, across rolling cohorts of mother-daughter pairs in each year I estimate $W_{it}^{d} = \alpha_t + \delta_t W_{it}^{m} + \epsilon_{it}^{d}$ where W_{it}^{d} and W_{it}^{m} are the daughter's and mother's welfare indicators, respectively, δ_t is the year-specific intergenerational correlation in welfare use, and ϵ_{it}^{d} is the error term. In order to make these estimates comparable to Page (2004), I use daughter's PSID core longitudinal weights at age 25 in estimation, and I temporarily define the sample and measures of welfare participation for the purposes

Figure 1.2. Trends in the Intergenerational Correlation of Welfare Participation



Notes: The dependent variable is an indicator for whether a daughter ever participated in AFDC/TANF (or AFDC/TANF, SSI, or Food Stamps) in any year after forming her own family through age 27. The independent variable is an indicator for whether the mother ever participated in AFDC/TANF when the child is observed living at home. These trends reflect rolling cohort groups of daughters aged 27-42 in each year. The major waiver period of welfare reform is indicated by the shaded region. Abbreviations: Aid to Families with Dependent Children/Temporary Assistance for Needy Families (AFDC/TANF), Supplemental Nutrition Assistance Program (SNAP), and Supplemental Security Income (SSI).

in welfare increased throughout the two decades leading up to the passage of welfare reform, and did not peak until 1998 when the correlation of 0.40 was more than double that of the late 1970s. The correlation between mothers' and daughters' AFDC/TANF use then fell precipitously afterwards to levels comparable to those in the early 1980s. However, expanding the definition of daughter's welfare to include food stamps or SSI (mother's welfare remains defined by AFDC/TANF use), then there is a very different pattern. The intergenerational correlation is relatively constant after welfare reform. The descriptive evidence thus points to the possibility that welfare reform succeeded in reducing the transmission of AFDC/TANF use across generations, but dependence more broadly defined has not changed.

To identify the intergenerational dependence parameter, one naturally has to separate the poverty trap from the welfare trap. The correlations presented in Figure 1.2 can simply reflect persistence in poverty status, and thus, the evidence does not imply that welfare generated dependence on government assistance transmitted from mother to daughter. The literature, however, has elaborated on potential mechanisms beyond the poverty mechanism (see, for example, Antel, 1992; Duncan et al., 1988; Durlauf and Shaorshadze, 2014; Moffitt, 1983). First, a mother's participation might lower her daughter's stigma associated with welfare as well as other costs of participation. A child on welfare can observe and learn how the program 'works', while her mother does not incorporate potential future costs on her daughter in her utility-maximizing behavior. Secondly, contrasting the idea that welfare offers mothers additional resources in times of

of Figure 1.2. For each year t, the sample consists of daughters ages 27-42 years old who are the heads of their family unit and the dependent variable is an indicator for any welfare use by the daughter between ages 14 and 27. The independent variable is an indicator for mother's welfare use prior to the daughter's matriculation to family headship.

need, participation in government assistance affects job market opportunities for mothers, and consequently, can increase dependence for daughters through several factors such as labor force attachment and social capital, for example. Essentially, the reform targeted these plausible intertemporal mechanisms. Therefore, a framework for identifying the intergenerational transmission of dependence needs to move beyond the correlations presented in Figure 1.2 by considering that the reform could affect daughters' participation decisions, both directly on AFDC/TANF and more broadly on other programs and adult economic outcomes. Further details on identification are discussed in the next section.

1.3 Estimating Intergenerational Transmission Pre- and Post-Reform

Contemporary empirical studies on intergenerational socioeconomic outcomes trace their intellectual foundation to the work of Becker and Tomes (1979, 1986), who provide a structural framework of dynastic family decision making. The corresponding canonical statistical model involves regressing the outcome of interest of the child on the corresponding outcome of the parent, whether it is earnings, education, health, income, wealth, or in this case, welfare participation (see surveys in Black and Devereux, 2011; Solon, 1999). The *prima facie* evidence in Figure 1.2 suggests a structural break in (AFDC) welfare participation starting during the reform era. Introducing welfare reform implies a straightforward modification to the canonical model of the intergenerational transmission of welfare before and after reform as

$$W_{ist}^{d} = \alpha + \beta' \mathbf{x}_{ist}^{d} + \delta W_{is,\forall j<t}^{m} + \gamma R_{st}^{m} + \theta R_{st}^{m} W_{is,\forall j<t}^{m} + \mu_{s}^{d} + \rho_{t}^{d} + v_{ist}^{d}, \tag{1.1}$$

where W_{ist}^{d} is an indicator variable that takes a value of 1 if the daughter (d) in family i residing in state s at time period t participates in welfare and 0 otherwise; $W_{is,\forall j<t}^{m}$ takes a value of 1 if the mother (m) ever participates in welfare in any prior period $j = 1, \ldots, t-1$

and 0 otherwise; $\mathbf{x}_{ist}^d$ is a vector of observed demographic characteristics of the daughter; R_{st}^m is an indicator variable that takes a value of 1 when the state of residence of the mother implements welfare reform and 0 otherwise; and, v_{ist}^d is the unobserved error term.[2] The state effect μ_s^d controls for permanent differences in states such as natural endowments that affect economic opportunities, while the time effect ρ_t^d controls for macroeconomic and policy changes affecting all daughters the same in a given year.

In equation (1.1), once the mother participates, the $W_{is,\forall j<t}^m$ variable remains "on" for each subsequent observation. The use of ever on welfare for the mother instead of contemporaneous participation serves two purposes: first, it implies that once the mother participates in welfare it cannot be "unlearned" by the daughter; and second, the ever-on measure captures a longer window and thus attenuates potential measurement error. The baseline models define welfare of the daughter and mother as participation in AFDC/TANF, but I also explore heterogeneity in the transmission mechanisms by age of the daughter when exposed to the mother's welfare use, the length of exposure to the mother's welfare use, by race of the family, and by stringency of the state's welfare reforms. In addition, to examine whether welfare reform altered the relationship between mother's welfare use and other adult economic outcomes of the daughter, I also estimate models where the dependent variable is replaced with an indicator for broader safety net participation on AFDC/TANF, food stamps/SNAP, or SSI, as well as indicators for low educational attainment, nonemployment, and poverty and near poverty status.[3]

[2]While the notation implies that the daughter and mother share the same state s, this constraint is non-binding in practice where welfare reform implementation and state-level instruments correspond to the mother's state of residence. I test the robustness of the estimates to possible cross-state mobility below.

[3]The prior literature generally only provided estimates of AFDC with General Assistance (for example, Gottschalk, 1996), or of combined AFDC/GA/Food Stamps/SSI in main results with some discussion of estimates restricted to AFDC/GA (for example, Page, 2004; Solon et al., 1988).

In equation (1.1), δ is the intergenerational correlation of welfare participation, and $\delta + \theta$ is the correlation after welfare reform. This specification is akin to a difference-in-difference model whereby I exploit the quasi-experimental variation induced by the fact that different states adopted welfare reform at different times starting in the early 1990s.[4] That is, the indicator R^m_{st} "turns on" when the state s implements a waiver and remains on thereafter. By adopting this functional form, I implicitly assume that the TANF program implemented after PRWORA is a continuation of the reforms begun during the waiver period for those states that were early adopters of reform. This has been a standard assumption in the welfare reform literature, though in some cases researchers allow a trend break between the waiver era and TANF era (Blank, 2002). If welfare reform succeeded in reducing the transmission across generations, then one would expect that $\theta < 0$.

A ubiquitous challenge across the intergenerational transmission literature has been establishing a causal pathway from parent to child, that is, separating out the poverty trap from the welfare trap, because the conditional mean assumption for consistency of least squares that $\mathbb{E}\left[v^d_{ist} \middle| W^m_{is,\forall j<t}, \bullet\right] = 0$ is generally violated. While the state and year effects are likely to control for some forms of endogeneity, it is still possible that the remaining time-varying error term v^d_{ist} can be correlated with mother's welfare use by endogenous selection and measurement error. Below, I offer a detailed discussion of each of these threats to identification, and how I address them. I also investigate other possible identification issues (i.e., life-cycle factors and geographic mobility) later in Section 1.5.3.

[4]Ziliak et al. (2000) show that a state's decision to apply for an AFDC waiver was not an endogenous response to caseload size, which supports the use of the waiver reform period as identifying variation for welfare participation.

1.3.1 Selection Bias

The conditional mean independence assumption for consistent causal estimates of the intergenerational parameters δ and θ will break down if there are unobserved characteristics common to the mother and daughter that affect the decision to participate. That is, if equation (1.1) is backdated by a generation, say $-t$, and write a model of the mother's participation as a function of her demographics ($\mathbf{x}^m_{is,-t}$) and the welfare choice of her mother (i.e. the daughter's grandmother, $W^g_{is,\forall k<-t}$), then shared tastes for work and welfare within families would imply that

$$\mathbb{E}\left[v^d_{ist}v^m_{is,-t}\middle|\mathbf{x}^d_{ist}, W^m_{is,\forall j<t}, R^m_{st}, \mathbf{x}^m_{is,-t}, W^g_{is,\forall k<-t}\right] \neq 0.$$

The quasi-experimental design of using cross-state variation over time in adoption of welfare reform allows us to separate the pre- and post-reform eras, but *within* the AFDC and TANF eras there still remains a possible convolution of state dependence (welfare trap) and unobserved heterogeneity (poverty trap).

There have been several efforts over the years to control for endogenous selection in intergenerational welfare participation. In an early study, Solon et al. (1988) used pairs of sisters in order to control for shared family background (i.e. family fixed effects) in identifying the effect of parental welfare participation. Antel (1992) adopted Heckman's (1978) dummy endogenous variable model within the context of a two-limit tobit specification. He included exclusion restrictions in the mother's reduced form equation such as the state's AFDC benefit guarantee and local labor market conditions as proxied by net migration flows. In lieu of exclusion restrictions, Gottschalk (1996) addressed unobserved heterogeneity by modeling the event histories of daughter's and mother's welfare usage in order to identify causal effects relative to a mother's past participation. Levine and

Zimmerman (1996) used mother's background as additional control variates, as well as state (e.g. welfare generosity) and local (e.g. county unemployment rate) variables as instruments for mother's welfare participation. Dahl et al. (2014), who examined disability insurance in Norway, used the random assignment of appellate-court judges as an instrumental variable to identify parent's disability participation on child's disability insurance claims. Pepper (2000) eschewed point identification methods of the latter authors in favor of nonparametric bounding techniques to control for selection as proposed by Manski (1995). Antel, Gottschalk, Pepper, and Dahl, et al. all conclude that parent's participation in welfare is causal for the child and not spurious, while Solon, et al. and Levine and Zimmerman provide evidence more in favor of spurious poverty traps.

My approach to address possible endogenous selection within welfare regimes is to extend the prior point identification literature by exploiting the comparatively long time histories now available in the PSID and estimate equation (1.1) via instrumental variables. Specifically, I instrument for mother's previous welfare participation using the policy parameters defined by the state maximum AFDC/TANF benefit guarantee and the combined Federal and state maximum EITC. Each of these instruments vary across states, time, and family size—the maximum AFDC/TANF guarantee is set by state legislatures, while the maximum Federal EITC is set by the U.S. Congress to vary by the number of qualifying children in the family and the state portion is set by state legislatures as a fixed percentage of the Federal credit. Both of the variables speak to the prospect of the welfare trap, but in opposite directions. A higher maximum AFDC/TANF benefit guarantee means that all else equal welfare is more attractive to the mother, while a higher maximum EITC means that work is more attractive than welfare since EITC eligibility is work conditioned. To ensure that the policy instruments are most salient to the mother's welfare choice, I restrict the time period of the instruments by aggregating over values that are applicable to the mother when her daughter is in the critical exposure ages of 12-18 years old and not an adult living independently. Note that because the models are estimated with state and time effects, these instruments are demeaned variables by state and year, and therefore, they exploit exogenous transitory policy changes at the state level during a daughter's childhood. These welfare policies while the daughter is young should have no effect on her subsequent welfare decisions in adulthood except via the welfare choice of her mother (Antel, 1992; Levine and Zimmerman, 1996; Moffitt, 1992).

Four measures of welfare generosity are used for constructing instruments: the average and maximum of the state-specific AFDC/TANF benefit standard for families of 2, 3, or 4 or more persons, and the average and maximum of the combined Federal and state EITC maximum credit amounts for 0, 1, or 2 or more dependents. The EITC benefit is defined as $EITC_{it} \cdot (1 + p_{ist})$, where $EITC_{it}$ is the Federal credit that varies by the number of qualifying children and year and p_{ist} is the fraction of the Federal EITC that a state refunds on the state return. The Federal EITC was begun in 1975, and expanded in 1986, 1991, 1993, and 2009, while states began introducing the refundable state EITC in the late 1980s. By the mid 2000s, nearly half the states had a separate EITC, providing cross-state and family-size variation over time in the instrument. In equation (1.1) both mother's welfare participation and its interaction with welfare reform are treated as endogenous, and thus the full set of instruments enter directly and interacted with the welfare reform indicator. Tests for both the first-stage strength and the validity of overidentifying restrictions are included in the results section. I also test the robustness to additional policy and economic instruments.

1.3.2 Misclassification Bias

Misreporting of welfare is present both at the extensive participation margin and the intensive dollar margin, it pervades all social surveys, and has gotten worse over time (Meyer et al., 2015a,b). In the case of welfare participation, misreports can be in the form of “false negatives”—the respondent states they do not receive assistance when in fact they do—and “false positives”—the respondent states they receive assistance when in fact they do not. Based on validation studies of the Food Stamp Program and TANF, most misclassifications are false negatives (Bollinger and David, 1997, 2001; Meyer, Goerge, and Mittag, 2014; Meyer and Mittag, 2014, 2015).[5] The reasons for the increase in misreporting are generally unknown, but this trend may in part be a result of the increasing importance of in-kind transfers in the TANF program, which are generally more difficult for the respondent to place a monetary value.

Remedies for classification bias are not straightforward in the context of dichotomous variables. A standard approach for continuous variables in the intergenerational income literature with classical measurement error is to take 3- or 5-year averages of parent's (and possibly child's) income (Mazumder, 2005; Solon, 1992, 1999). While such averages are likely to improve things in dichotomous participation models, this is not ensured as the errors have been found to vary systematically with characteristics and are nonclassical. Some have proposed parametric or semiparametric adjustments to the likelihood func-

[5]When false positives do occur, the issue is often misreporting the correct source of actual transfer income or mistaking the timing of receipt, thus aggregate measures of welfare participation over time or across survey questions should diminish the relevance of this error type given the independent variable definition.

tion to incorporate misclassification (Bollinger and David, 1997, 2001, 2005; Hausman et al., 1998; Meyer and Mittag, 2014), while others have proposed partial-identification nonparametric bounding techniques (Black, Berger, and Scott, 2000; Bollinger, 1996; Kreider et al., 2012; Kreider, Pepper, and Roy, 2016; Molinari, 2008). These solutions have been proposed for cross-sectional data either for measurement error in the dichotomous dependent variable, or the independent variable, though the setting for this study has potentially mismeasured dichotomous variables on both the left- and right-hand sides of the equation.

I consider several potential remedies for misclassification bias. First, evidence in Bollinger and David (2005) showed that respondents have a latent propensity to report or not report, and that cooperation increases with length of panel participation. Since the data follow mothers for at least 14 years on average and daughters for 25 years, correct reporting should be more prevalent than in a sample with short observation windows. Second, for right-hand-side mismeasurement of mother's participation, again recall that I measure if the mother *ever* participates, which is likely to be less noisy than contemporaneous participation.[6] Moreover, the instrumental variables discussed in the prior section on selection bias are also likely to improve matters for misreports of mother's participation. Third, for left-hand-side classification error, I consider parametric bias-corrections along the lines proposed in Bollinger and David (1997, 2001) and Hausman et al. (1998). Specifically, I follow Hausman et al. (1998) and assume that misreporting is independent of model covariates and constant across individuals, which implies that the partial effect of mother's participation on daughter's participation in equation (1.1) from observed data

[6] For further support that the mother's indicator for any prior welfare participation is measured more accurately, Appendix A demonstrates how the probability of ever misreporting tends to zero as the number of mother observations increases.

is proportional to the true partial effects,

$$\Pr\left(W_{ist}^{d}=1\middle|W_{is,\forall j<t}^{m}=1,\bullet\right)-\Pr\left(W_{ist}^{d}=1\middle|W_{is,\forall j<t}^{m}=0,\bullet\right)=$$
$$\left(1-\tau_{0t}-\tau_{1t}\right)\left(\delta+\theta R_{st}^{m}\right), \quad (1.2)$$

where $\bullet$ represents other controls, τ_{0t} is the false positive reporting rate at time t, and τ_{1t} is the false negative reporting rate at time t. To implement this correction, I set the false positive rate to 0, and for the linear probability models rescale all the right-hand-side variables in equation (1.1) by $(1-\hat{\tau}_{1t})$, which is based on estimates of AFDC/TANF reporting rates in the PSID by Meyer et al. (2015b) as depicted in Appendix Table A.1. Refer to Appendix A for additional details on the two-stage approach used to estimate the parameter of interest in equation (1.2).

A convenient aspect of the proposed methodology is that it allows us to estimate models with endogenous variables using instrumental variables. This is an important innovation because, as discussed in the previous section, selection bias due to correlation of unobservables is likely to create biased estimates of the effect of welfare reform on the transmission parameter.

1.4 Data

The data come from the Panel Study of Income Dynamics (PSID), which was begun in 1968 as a survey of 4,800 American families. The survey has followed the children and grandchildren of original sample parents as they split off to form their own households so that today there are over 10,000 PSID families and 24,000 individuals. As the longest continuously running longitudinal survey, the PSID is ideally suited for the study of intergenerational transmission, and has been found to be robust over time to changes in sample composition (Fitzgerald, 2011; Fitzgerald, Gottschalk, and Moffitt, 1998). The original sample drew about 60 percent of the families from the nationally representative Survey Research Center (SRC) subsample, and the other 40 percent from an oversample of low-income and minority families as part of the Survey of Economic Opportunity (SEO) subsample. I focus on linked mother-daughter pairs over the entire life of the PSID survey years from 1968-2013, and in order to ensure adequate sample sizes I include observations from both the SRC and SEO subsamples.

The oversample of low-income families in the PSID allows for more precise estimation of welfare participation, yet this unrepresentative sample will yield biased causal estimates if, after conditioning on control variables, the selection probability remains endogenous to daughter's welfare participation, or if there exist heterogeneous transmission effects relative to the oversampled population (see Solon, Haider, and Wooldridge, 2015).[7] Some examples in the literature have addressed endogenous sampling directly by controlling on observed characteristics (Corcoran et al., 1992; Pepper, 2000), or by restricting the estimation sample to the SRC only (Lee and Solon, 2009; Moffitt and Gottschalk, 2002). Other examples have used weights for estimators that are based on frequency counts (Page, 2004; Solon et al., 1988), as a sensitivity check (Solon, 1992), or in the main estimation Hoynes and Schanzenbach (2012). A primary concern for estimates in this study is the potential heterogeneity of welfare participation transmission by race coupled with overrepresented low-income, minority families, and my model maintains a fairly parsimonious structure that may not adequately account for this source of bias. Therefore, in all of the estimation results, I provide weighted estimates and also

[7]See PSID documentation for background on survey selection procedures and sample weight construction. For detailed issues relate to the Survey of Economic Opportunity, see Brown (1996).

demonstrate that the results are robust to the use of weights or restriction to the SRC subsample.

The baseline sample consists of mother-daughter pairs that are observed for at least five years while the daughter is living in the same household during the critical exposure period spanning the ages of 12-18, and that the daughter is observed at least five years as the head of her own family unit. Selecting adolescence and teenage years as the observation window for childhood exposure pervades the welfare transmission literature (Duncan and Yeung, 1995; Gottschalk, 1996; Page, 2004; Pepper, 2000; Solon et al., 1988). Part of this stems from data needs; that is, requiring the observation of early childhood as well as enough years in adulthood would impose greater demands on the data in terms of length of time in the panel, and in turn, end up with fewer mother-daughter observations. The other reason for focusing on adolescent and teenage years is that cognitive, emotional, and physiological development are sufficiently advanced for the potential of "welfare learning" from the parent. However, it remains an open question in the literature which stage of childhood development is most important for the potential of welfare learning. Research shows that economic deprivation in early childhood has more deleterious effects in terms of achievement and health in early adulthood than does similar deprivation during adolescence (Duncan and Brooks-Gunn, 2000; Duncan et al., 1998; Elango et al., 2016; Ziol-Guest et al., 2012). But this research has not separated out the independent role of welfare in this process. As such, I follow convention and focus on the five years observed during the ages 12-18 as a key period of welfare exposure for the baseline models, and then explore how the estimates change as the age and length of exposure changes.

Table 1.1. Descriptive Statistics

A. Daughter's Characteristics as an Adult	Before Reform	After Reform	Pooled
Currently Receiving Welfare?			
AFDC/TANF (%)	0.080	0.025	0.044
	(0.271)	(0.157)	(0.206)
AFDC/TANF, SNAP, SSI (%)	0.132	0.112	0.119
	(0.338)	(0.315)	(0.323)
Years Before/After Welfare Reform (%)	0.348	0.652	
	(0.476)	(0.476)	
Age	28.245	38.666	35.041
	(5.572)	(9.009)	(9.400)
Number of Children	1.249	1.186	1.208
	(1.169)	(1.273)	(1.238)
Race:			
Black (%)	0.161	0.170	0.167
	(0.368)	(0.375)	(0.373)
White (%)	0.812	0.805	0.807
	(0.391)	(0.396)	(0.394)
Other (%)	0.027	0.025	0.026
	(0.162)	(0.157)	(0.159)
Resides in Same State as Birth (%)	0.759	0.703	0.723
	(0.428)	(0.457)	(0.448)
B. Mother's Characteristics	Before Reform	After Reform	Pooled
Any Previous Welfare?			
AFDC/TANF (%)	0.269	0.066	0.271
	(0.444)	(0.248)	(0.444)
AFDC/TANF, SNAP, SSI (%)	0.428	0.190	0.433
	(0.495)	(0.392)	(0.496)
Years Before/After Welfare Reform (%)	0.858	0.142	
	(0.158)	(0.158)	
Age	42.472	59.357	61.429
	(8.841)	(10.512)	(11.425)
Policy Measures when Daughter Aged 12-18 (in thousands of 2012 dollars):			
AFDC/TANF Benefit Standard, Average	0.736	0.393	0.724
	(0.334)	(0.213)	(0.336)
AFDC/TANF Benefit Standard, Maximum	0.913	0.476	0.904
	(0.363)	(0.226)	(0.365)
EITC Federal/State Credit, Average	0.801	3.223	0.876
	(0.726)	(1.417)	(0.878)
EITC Federal/State Credit, Maximum	1.208	3.873	1.318
	(0.886)	(1.405)	(1.085)
Mother-Child Family Unit Observations			14.212
Daughter-as-Adult Observations			25.085
Total Observations	25331	30737	56068

Notes: Sample averages are weighted by the daughter's PSID core longitudinal weights for both daughters' and mothers' statistics. Further, the pooled statistics for mothers are not a simple weighted average of before/after reform. Mothers' statistics before/after reform reflect her observed history during potential welfare participation years, 1967-2007, and the pooled statistics correspond to the daughter's current observation year in the estimation sample. Abbreviations: Food Stamps/Supplemental Nutrition Assistance Program (SNAP), and Supplemental Security Income (SSI).

A daughter is considered an adult at first childbirth or when establishing a new family unit if she is at least age 14, though she may continue to live at home as a subfamily. This yields a baseline sample of 2,961 mother-daughter pairs spanning 56,067 observation years of the daughter as an adult. Table 1.1 contains the key variables from the baseline sample used in estimation of equation (1.1), separated into the pre- and post-welfare reform eras, and weighted by the daughter's core longitudinal weight. While 4.4 percent of daughters receive AFDC/TANF as an adult in the sample period, the odds of participation are nearly 70 percent lower after welfare reform, falling from 8 percent to 2.5 percent.[8] On the other hand, there is much more stability over time in participation in any of the three programs, with 13.2 percent receiving AFDC/TANF, food stamps/SNAP, or SSI before reform and 11.2 percent afterwards. Almost all of the additional uptake in welfare use is from food stamps/SNAP. The bottom panel of Table 1.1 shows that about 27 percent of mothers were ever on AFDC/TANF prior to welfare reform, and 6.6 percent were ever on during the period after reform, while those figures jump to 43 and 19 percent, respectively, if the mother ever received AFDC/TANF, food stamps/SNAP, or SSI. Note that it is possible for the mother to first participate on welfare after the daughter forms her own family unit. For AFDC/TANF participation, this can occur only if the mother has children (or dependents) under age 18 remaining in the household other than the focal daughter. Learning thus can occur from direct exposure while the daughter resides

[8] The PSID asks about AFDC/TANF receipt of the family head, spouse, and other family members, as well as an "other welfare" category (not including SSI, food stamps, workers' compensation, housing, Social Security). This other welfare category can contain assistance from various public sources including General Assistance. If the percent of daughters participating in AFDC/TANF are adjusted for misclassification (by inflating sample statistics by the reporting rates shown in Appendix Table A.1), then the baseline participation over the sample period would be 7.8 percent of daughters, which then falls to an adjusted 5.6 percent after welfare reform.

in the household with her mother, or from indirect "word of mouth" once the daughter forms her own family unit. This mechanism is discussed in the results section below.

The other focal regressor in equation (1.1) is the indicator for welfare reform. As discussed previously, states began reforming AFDC in earnest starting in 1992, four years prior to passage of PRWORA. States had to submit requests for waivers from Federal rules to the U.S. Department of Health and Human Services, e.g., to introduce a time limit on benefits or to expand asset limits for eligibility. If the waiver was approved, then there was generally a lag between the time of approval and when the policy was implemented. Indeed, some approved waivers never were implemented (Grogger and Karoly, 2005). I thus use the implementation date of the waiver as the date when reform is first in place, and the variable remains on for each year thereafter. For those states that did not implement waivers I use the implementation date of their TANF program. While the major AFDC waiver implementation period is defined as 1992-1996, the earliest major waivers were officially implemented in Michigan and New Jersey as of October 1992, and the latest implementation of TANF was in New York as of November 1997. In the data, the implementation of welfare reform is denoted by the earliest year in which at least 3 quarters of the year are observed after reform (either by waiver or TANF), implying that the reform variable spans 1993-1998.[9] As seen in Table 1.1, about 65 percent of daughter-year observations occur after welfare reform, while for mothers it is only around 14 percent.

Table 1.1 also contains demographic characteristics of the daughter and mother, as well as the main instrumental variables. Daughters are 28 years old on average before reform and 39 after reform, while mothers are 42 and 59 years old, respectively, highlighting

[9]For specific dates of welfare reform waiver approval and implementation, see Crouse (1999).

the long observation windows I observe families compared to prior research. For the estimation sample, approximately 72 percent of daughters reside in their state of birth during adulthood.[10] The nominal values of the maximum guarantees and credit are converted to real 2012 dollars using the personal consumption expenditure deflator.[11] The average real maximum AFDC/TANF benefit standard facing mothers was $724 over the entire sample period, but fell nearly in half in the post reform era which reflects the fact that most states have left the nominal guarantees unchanged for decades (Ziliak, 2007). On the other hand, the real value of the EITC facing mothers in the welfare/no-welfare decision increased by a factor of three, highlighting the push to a work-based safety net in recent decades.

1.5 Estimates of Welfare Reform on Intergenerational Transmission

In presenting the empirical results, I first focus on the baseline linear probability model correcting for nonrandom selection and misclassification error, and then expand the outcomes to include participation in additional transfer programs as well as human capital and employment. Having established that welfare reform only affected the intergenerational transmission of AFDC/TANF and not additional outcomes, I then return to the AFDC/TANF model to assess the robustness of the findings to life-cycle bias and cross-state mobility. All models control for time-varying demographic controls of the daughter (a quadratic in her age and indicators for the number of children in her home) as well as dummy variables for state of residence and year. The standard errors are robust to heteroscedasticity and clustered at the state level given the focus on state welfare reforms.

[10]Also, statistics not shown in Table 1.1 indicate that 63 percent of daughters live in the same state as their mothers, while 57 percent never change states during the entire observation period.

[11]Source: U.S. Bureau of Economic Analysis, 2016, Personal Consumption Expenditures Excluding Food and Energy, Chain-Type Price Index [series: DPCCRG3A086NBEA], retrieved from FRED, Federal Reserve Bank of St. Louis.

1.5.1 Baseline Estimates

The first four columns of Table 1.2 contain the baseline estimates of the parameters of interest in equation (1.1), with and without instrumental variables and corrections for misclassification of the dependent variable. The IV estimate of the effect of mother's AFDC participation prior to welfare reform in column (2) is 0.281 (s.e. = 0.056), meaning that if the daughter's mother ever participated in AFDC then the daughter is 28 percentage points more likely to participate as an adult. This estimate, which corrects for correlated unobservables between mother and daughter and possible measurement error in mother's survey reports, is economically large and nearly double the OLS estimate in column (1), but is within the range of estimates among studies from that era surveyed in Page (2004).[12] That correlation falls 70 percent after welfare reform to 0.084 ($= 0.281 - 0.197$). Because the underlying probability of being on welfare fell by a similar proportion as seen in Table 1.1, if considering percent changes in transmission as a fraction of the baseline probability, then the effect of welfare reform in column (1) would be a 48 percent reduction ($= 1 - ((0.281 - 0.197)/0.025)/(0.281/0.044))$. The p-value of these changes is less than 0.005. This suggests that two-thirds of the post-reform reduction in the probability of AFDC/TANF participation came about from reduced transmission from mother to child. Note that the after-welfare reform variable has a positive effect on daughter's participation, suggesting that in the absence of welfare reform the trend increase in intergenerational transmission would have continued.

[12]Note that this estimate is lower than a simple average of the trend estimates in Figure 1.2 because the samples differ. Figure 1.2 depicts whether the daughter is ever on welfare before age 27, while the sample used in estimating equation (1.1) is for any contemporaneous welfare use after forming a family unit, regardless of daughter age. Table 1.2 also includes state and year effects as well as daughter control variables, while the figure shows unconditional correlations.

Table 1.2. Intergenerational Transmission of Mother's AFDC/TANF Participation

Daughter's Outcome:	AFDC/TANF				AFDC/TANF, SNAP, SSI			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mother's Participation	0.146	0.281	0.236	0.428	0.226	0.279	0.294	0.349
	(0.014)	(0.056)	(0.022)	(0.093)	(0.019)	(0.070)	(0.024)	(0.091)
After Welfare Reform	0.036	0.072	0.047	0.087	0.003	−0.016	−0.011	−0.049
	(0.007)	(0.022)	(0.014)	(0.033)	(0.013)	(0.031)	(0.020)	(0.043)
Mother's Participation ×	−0.101	−0.197	−0.134	−0.234	−0.044	0.055	−0.020	0.159
After Welfare Reform	(0.015)	(0.050)	(0.030)	(0.081)	(0.021)	(0.080)	(0.030)	(0.109)
Instrumental Variables	No	Yes	No	Yes	No	Yes	No	Yes
Misclassification Correction	No	No	Yes	Yes	No	No	Yes	Yes
Weak IV Test Statistic		23.092		21.083		23.092		21.739
p-value		0.002		0.004		0.002		0.003
Hansen J Statistic		2.370		2.069		9.970		9.792
p-value		0.883		0.913		0.126		0.134
Percent Change in Levels	-70%	-70%	-57%	-55%	-19%	20%	-7%	46%
p-value	0.000	0.000	0.000	0.000	0.025	0.552	0.495	0.268
Percent Change over Baseline	-47%	-48%	-40%	-37%	-14%	27%	-6%	47%
p-value	0.000	0.004	0.006	0.031	0.118	0.439	0.548	0.259
Number of Daughters	2961	2961	2961	2961	2961	2961	2961	2961
Observations	56068	56068	56068	56068	56068	56068	56068	56068

Notes: Robust standard errors with state clustering are shown in parentheses. All specifications control for state and year effects in addition to time-varying controls for daughter's age, age squared, and indicators for number of children equal to 1, 2, 3, or at least 4. Instrumental variables include average and maximum measures of the mother's AFDC/TANF benefit standard and federal/state EITC maximum credit by family size, which are defined over the daughter's critical exposure ages 12-18, and interactions of each with an indicator for welfare reform. The weak IV test statistic is a Kleibergen-Paap (2006) rank statistic. The misclassification correction uses reporting rates in the PSID to address potential misreporting for the daughter's welfare participation (see Appendix A for details). Daughters' PSID core longitudinal weights are used in estimation. Abbreviations: Food Stamps/Supplemental Nutrition Assistance Program (SNAP), and Supplemental Security Income (SSI).

While the baseline estimates intrinsically address misclassification of the mother's welfare participation by design (longer panels of nonattriters, instrumental variables, and ever on welfare instead of contemporaneous), they do not directly address the possibility of a binary mismeasured dependent variable. Columns (3) and (4) in Table 1.2 show the baseline estimates with misclassification bias corrections. As expected, the estimates are larger than those with no correction in columns (1) and (2), and indeed the corrected estimates without instruments in column (3) are on par with the uncorrected IV estimates in column (2). The IV estimates in column (4) suggest that the transmission from mother to daughter is stronger in the pre-reform AFDC period after adjusting for misclassification, but the post reform reduction is still a large and statistically significant 55 percent, or 37 percent over the baseline odds of participation. Note that the bias-corrected IV estimates are likely to be upper-bounds because the estimates of reporting rates from Meyer et al. (2015b) come from annual cross sections of the PSID but the estimation sample consists of a long panel of stayers who tend to be more accurate in reporting (Bollinger and David, 2005).

A standard concern with IV estimates is the quality and exogeneity of the instruments. In Appendix Table B.1 I present the first stage estimates of the effect of the instruments on the mother's participation decision in the pre-reform period (recall that the model also instruments the interaction between mother's welfare and her state-by-year welfare reform indicator), and in the middle of Table 1.2 I present standard tests of instrument strength and exogeneity. The null hypothesis of weak instruments is strongly rejected using the Kleibergen-Paap rank test, while the null of valid overidentifying restrictions from the Hansen J-test is not, suggesting the IV estimates are consistent.

In Appendix Tables B2-B7 I subject the baseline IV estimates to a number of specification checks. In Table B.2 I consider several additional state-by-year instruments, including the overall application denial rate in AFDC/TANF, the application denial rate for procedural reasons, the rate at which hearing requests are disposed in favor of the claimant, and the state unemployment rate. The first three of these are indicators for how administratively stringent the states application procedures are and are potentially strong instruments for separating the welfare and poverty trap arguments. I do not include the overall application denial rate in the baseline Table 1.2 estimates because the denial rate includes not only exogenous procedural denials but also legitimate denials based on failing income and asset tests, while the other two are not included because I was unable to construct a full state-by-year time series over the 45 years of the sample (note the loss of over 23,000 observations). Although prior research has demonstrated the strong role the macroeconomy plays in determining participation in AFDC/TANF, it also is a key determinant of the cyclicality of poverty rates and thus may not be as effective in separating out the poverty trap from the welfare trap and thus I do not include it in Table 1.2. Regardless, across the 6 columns in Table B.2 I get nearly identical transmission effects both before and after welfare reform as in Table 1.2. Likewise, the results are little changed when I add controls for mother's background like education and income (Table B.3), when I do not weight the estimates or when I drop the SEO oversample of the poor (Table B.4), when I limit attention to eldest daughters only (Table B.5), and when I restrict the sample to those mothers at greatest risk of welfare participation, i.e. low education or ever in poverty or near poverty (Table B.6).[13]

In all variants of equation (1.1) estimated in Table 1.2, I find that the OLS estimate of mother's participation is smaller than the IV estimate, a result that is consistent with other papers in the literature (see, for example, Dahl et al., 2014). Generally, the OLS can be different from the IV estimate for, at least, three reasons: selection bias, heterogeneous effects, and measurement error. In this setting, it is difficult *a priori* to predict the sign of the bias of OLS. For instance, it may be natural to expect upward-biased OLS estimates under the assumption that unobservables are positively correlated

[13]Regarding controls for mother's income and education, Levine and Zimmerman (1996) note that these variables could be endogenous to the daughter's welfare choice for the same reasons that the mother's welfare participation is likely to be endogenous. The unweighted estimates are larger in magnitude due to the oversample of the poor, suggesting that weights are needed as the weighted estimates are more comparable to the SRC subsample estimates.

over generations. However, the effects could be heterogeneous, too. The estimation sample includes a subpopulation of mothers who are not likely to be affected by the instruments because their family income is above the poverty line over the entire period of analysis. Appendix Figure B.1 shows, as expected, that mothers exposed to higher ADFC/TANF benefits were more likely to participate on welfare, with the exception of mothers whose average family income is more than twice the poverty line. When I consider a subsample of mothers with less than 9 years of education in Table B.6, I find smaller IV estimates of welfare transmission compared to the corresponding OLS estimates in Table 1.2.[14] These results and the instrument-induced probabilities shown in Figure B.1 suggest that the difference between IV and OLS estimates can be attributed to heterogeneous effects.

The last initial check is in Table B.7. As a falsification exercise, I investigate whether mother's future welfare use in any year from $t+5$ to $t+11$ correlates with daughter's welfare use at time t. The OLS estimates suggest that among mothers who previously participated on welfare, future participation significantly increases the likelihood of daughter's participation by 25 percentage points (column 3). This point estimate is naturally biased and a probable explanation is failure of controlling for lack of economic opportunities which creates dependence between mother's and daughter's unobservables in this specification. On the other hand, using the same set of instruments as in Table 1.2, I find an estimate that it is not statistically significantly different from zero. The results for the broader safety net suggest similar conclusions. Overall, these results offer suggestive em-

[14]In the case of the broader safety net, the targeted-population IV estimate in Table B.6 column (5) is smaller than the corresponding full-population OLS estimate in Table 1.2 column (5).

pirical evidence that the IV approach seems to attenuate, and possibly eliminate, biases in the estimation of the impact of the reform.

1.5.2 Participation in the Wider Safety Net and Economic Outcomes

Even if welfare reform reduced the causal transmission of AFDC/TANF participation, a relevant policy question is the extent to which welfare participation defined more generally is transmitted across generations. In columns (5)-(8) of Table 1.2, I examine what effects mother's AFDC/TANF participation and welfare reform had on the daughter's decision to participate in AFDC/TANF, food stamps/SNAP, or SSI. The specifications exactly parallel those in columns (1)-(4) and with the same controls for daughter's characteristics and state and year fixed effects. The estimates in columns (5)-(8) show that the magnitude of intergenerational transmission is very similar prior to welfare reform—mother's use of AFDC/TANF increased the odds of the daughter using welfare, food, or disability assistance in adulthood by 25-35 percentage points. But this is where the similarity with columns (1)-(4) end as I find no evidence that this transmission channel was changed after welfare reform.[15] In results not tabulated I obtain a similar result if I also define mother's participation as welfare, food, or disability assistance.

In addition to reducing welfare participation, the architects of welfare reform aimed to improve the long-term economic outcomes of children. In Table 1.3 I present least squares and instrumental variables estimates of equation (1.1) where I alternately replace the dependent variable of daughter's welfare participation with indicators equal to 1 for (i) whether her educational attainment is less than or equal to a high school diploma (ii) years of no earnings, (iii) years with earnings less than the poverty line, and (iv)

[15] For misclassification-corrected estimates in Table 1.2 columns (7) and (8), the reporting rate $(1 - \hat{\tau}_{1t})$ used in estimation is the maximum reporting rate for AFDC/TANF and food stamps/SNAP shown in Appendix Table A.1.

Table 1.3. Mother's AFDC/TANF Participation Effect on Daughter's Human Capital and Labor Market Outcomes

Daughter's Outcome:	High School Education or Less		No Earnings		Earnings Below 100% Poverty		Earnings Below 200% Poverty	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mother's Participation	0.251	0.698	0.134	0.181	0.252	0.365	0.313	0.488
	(0.049)	(0.326)	(0.019)	(0.057)	(0.023)	(0.076)	(0.023)	(0.094)
After Welfare Reform	0.026	0.120	0.014	0.022	0.012	0.012	0.014	0.006
	(0.044)	(0.093)	(0.012)	(0.023)	(0.020)	(0.032)	(0.021)	(0.041)
Mother's Participation ×	0.005	−0.167	−0.023	−0.037	−0.049	−0.002	−0.041	0.063
After Welfare Reform	(0.064)	(0.274)	(0.017)	(0.056)	(0.022)	(0.076)	(0.032)	(0.109)
Instrumental Variables	No	Yes	No	Yes	No	Yes	No	Yes
Weak IV Test Statistic		22.238		23.191		23.191		23.191
p-value		0.002		0.002		0.002		0.002
Hansen J Statistic		2.430		7.978		6.586		7.137
p-value		0.876		0.240		0.361		0.308
Percent Change in Levels	2%	-24%	-17%	-20%	-19%	-1%	-13%	13%
p-value	0.938	0.416	0.117	0.472	0.015	0.978	0.173	0.586
Percent Change over Baseline	11%	-18%	-25%	-28%	-19%	0%	-9%	18%
p-value	0.705	0.584	0.009	0.264	0.019	0.991	0.358	0.471
Number of Daughters	2873	2873	2961	2961	2961	2961	2961	2961
Observations	2873	2873	55906	55906	55906	55906	55906	55906

Notes: Robust standard errors with state clustering are shown in parentheses. All specifications control for state and year effects in addition to time-varying controls for daughter's age, age squared, and indicators for number of children equal to 1, 2, 3, or at least 4. Instrumental variables include average and maximum measures of the mother's AFDC/TANF benefit standard and federal/state EITC maximum credit by family size, which are defined over the daughter's critical exposure ages 12-18, and interactions of each with an indicator for welfare reform. The weak IV test statistic is a Kleibergen-Paap (2006) rank statistic. Daughters' PSID core longitudinal weights are used in estimation.

years with earnings less than twice the poverty line. I present IV estimates because of possible shared unobservables that spill over from mother to daughter into wider economic domains. Here I find a consistent pattern that daughters exposed to welfare are at risk of worse economic outcomes in adulthood. The IV estimates suggest they are 18 percentage points more likely to have episodes of nonemployment compared to daughters not exposed, 37 percentage points more likely to have incomes under poverty in a given year, 49 points more likely to have episodes of near poverty, and 70 percentage points more likely to have lower human capital attainment. The findings in Table 1.3 indicate that the 1996 reform to welfare did not substantively alter these risks for daughters.

Because the evidence thus far points to a reduced transmission in AFDC/TANF participation across generations after welfare reform, but not on wider use of the safety net or risk of worse economic outcomes in adulthood, in the remaining sections I focus on potential mechanisms of the AFDC/TANF transmission pathway. The next section explores how the IV estimates (with and without misclassification corrections) vary once I adjust the length of observation window for mother and daughter living together during potential years of welfare exposure, which may be critical years susceptible to life-cycle bias. It also investigates the sensitivity of the results to daughters' geographic movements that may be an endogenous response to the welfare climate in the state.

1.5.3 Sensitivity Analysis for Life-Cycle Bias and Geographic Mobility

A data constraint facing most intergenerational research is that full life cycles of daughters and mothers are generally not available. This leads to two related forms of bias, potentially reinforcing. One form of bias results from the fact that mothers and daughters are typically observed at different points of their life cycles. In the intergenerational income mobility literature, this has come to be known as life-cycle bias (Grawe, 2006; Haider and Solon, 2006; Jenkins, 1987; Lee and Solon, 2009; Nybom and Stuhler, 2016). The issue with income is that daughters tend to be observed when young and incomes low (but rising), and mothers at middle age when incomes are high (and stable or perhaps falling). This systematic deviation of current income from lifetime income is a form of nonclassical measurement error and tends to attenuate the intergenerational correlation of incomes. In the welfare context, participation tends to be high when young, both because incomes are low and odds of the presence of young children high, and participation is low when older (for the opposite reason of the young), again leading to attenuation in the intergenerational correlation.

A related measurement issue, frequently referred to as the "window problem" in the welfare literature (Gottschalk, 1992, 1996; Page, 2004; Wolfe et al., 1996), occurs when the length of observation is too short for either, or perhaps both, generations. The window problem is a form of measurement error in the sense that limited observations of an individual's welfare participation is an underreporting issue when complete histories are not available. Short windows could lead to underestimation of parameters if true participation is omitted, yet it could also lead to overestimation if long-term spells are overrepresented in the short window and long-term exposure matters more for transmitting dependency.

My primary solution to the life-cycle bias and window problem is to utilize the much longer time series now available in the PSID compared to prior studies. For each mother-daughter pair, I observe the daughter as head/spouse of her own family unit for 25 years on average and for as long as 38 years. In addition, I observe the mother and daughter co-residing for 14 years on average with at least 5 years during the daughter's ages 12-18 when the potential for welfare learning is heightened. Thus, the data come much closer to covering the entire life cycle of welfare participation. As a first check, in Appendix Table B.8 I examine the window problem by extending the minimum requirement that the pairs be observed for at least ten and fifteen years, respectively. In those cases, the reduction in the level of mother's transmission after welfare reform ranges between 56 percent to 77 percent, while the reduction in terms of baseline probability of participation

Table 1.4. Intergenerational Transmission of AFDC/TANF Participation with Lee-Solon-type (2009) Life-Cycle Adjustments

	(1)	(2)	(3)	(4)
Mother's Participation	0.114	0.254	0.223	0.443
	(0.012)	(0.042)	(0.020)	(0.081)
After Welfare Reform	0.024	0.062	0.032	0.065
	(0.008)	(0.017)	(0.015)	(0.038)
Mother's Participation ×	−0.066	−0.140	−0.108	−0.192
After Welfare Reform	(0.015)	(0.043)	(0.035)	(0.100)
Instrumental Variables	No	Yes	No	Yes
Misclassification Correction	No	No	Yes	Yes
Weak IV Test Statistic		31.231		36.102
p-value		0.455		0.242
Hansen J Statistic		30.746		31.094
p-value		0.428		0.411
Percent Change in Levels	-58%	-55%	-49%	-44%
p-value	0.000	0.000	0.000	0.010
Percent Change over Baseline	-26%	-21%	-29%	-22%
p-value	0.151	0.343	0.134	0.351
Number of Daughters	2961	2961	2961	2961
Observations	56068	56068	56068	56068

Notes: Robust standard errors with state clustering are shown in parentheses. All specifications control for state and year effects in addition to time-varying controls for daughter's age, age squared, and indicators for number of children equal to 1, 2, 3, or at least 4. Additional controls for Lee-Solon-type age adjustments include a quartic on mother's mean age during prior years of potential welfare participation, a quartic on daughter's current age detrended by 25, and mother's participation indicator interacted with the quartic on daughter's detrended age. Instrumental variables include average and maximum measures of the mother's AFDC/TANF benefit standard and federal/state EITC maximum credit by family size, which are defined over the daughter's critical exposure ages 12-18, and interactions of each with an indicator for welfare reform as well as interactions with a quartic in daughter's detrended age. The weak IV test statistic is a Kleibergen-Paap (2006) rank statistic. The misclassification correction uses reporting rates in the PSID to address potential misreporting for the daughter's welfare participation (see Appendix A for details). Daughters' PSID core longitudinal weights are used in estimation.

ranges between 43 percent and 60 percent, both of which are comparable to the estimates reported in Table 1.2.

I next present estimates in Table 1.4 that implement a life-cycle age adjustment proposed by Lee and Solon (2009) in the context of income mobility. Specifically, I augment the model with a quartic in the average age of the mother during prior (to time t) periods of potential welfare participation, a quartic in the detrended daughter's current age, and the interactions between the quartic in daughter's detrended age and mother's participation as well as the indicator for mother's participation after welfare reform. Note that as before the interactions with mother's welfare participation are endogenous in this setting, and therefore, in the IV models of columns (2) and (4), the instrument set includes a detrended quartic in daughter's age times the average of mother's AFDC/TANF benefit standard and federal/state EITC by family size when the daughter was living with the mother and she was between 12 and 18 years old, and these instruments are also interacted with reform. Because fertility rates among low-income women peak in their mid 20s (Lopoo, 2007), I detrend around daughter's age of 25. Comparing the OLS estimates in column (1) of Tables 1.2 and 1.4, it is clear that the age adjustments do not influence the results qualitatively, and with only small quantitative differences in the pre-reform era and slightly larger attenuation in the post-reform era (in absolute value).[16]

Up to this point, the models have allowed for the possibility that daughters reside in a different state than their mothers and/or have moved to another state during adulthood. If such movements are an endogenous response to the welfare climate in the state, then this could lead to biased estimates of welfare reform and the transmission across generations. The power and exogeneity of the instrumental variables hinge on the degree to which welfare policies determine participation, and on the extent to which families have no control over welfare policy, especially via endogenous migration. The evidence on whether there is endogenous internal migration in response to welfare generosity in the U.S. is mixed (Gelbach, 2004; Kennan and Walker, 2010; Levine and Zimmerman, 1999; McKinnish, 2007), yet when effects are found, they are very small in magnitude. Also,

[16]While the IV estimates of percent reductions after reform are smaller, the Lee-Solon age adjustment introduces multiple endogenous variable interactions leading to lower instrument efficiency in estimation. Overall, the evidence is suggestive that a long panel is adequate to account for potential life-cycle bias in intergenerational transmission of welfare.

Table 1.5. IV Estimates of the Intergenerational Transmission of AFDC/TANF Participation by Daughter's Geographic Mobility Status

Daughter's State of Residence:	Same as Birth		Same as Mother		Never Moves	
	(1)	(2)	(3)	(4)	(5)	(6)
Mother's Participation	0.331	0.527	0.379	0.534	0.414	0.644
	(0.081)	(0.140)	(0.082)	(0.133)	(0.105)	(0.168)
After Welfare Reform	0.084	0.103	0.079	0.086	0.107	0.132
	(0.030)	(0.050)	(0.019)	(0.034)	(0.046)	(0.070)
Mother's Participation ×	−0.235	−0.273	−0.265	−0.279	−0.297	−0.348
After Welfare Reform	(0.075)	(0.125)	(0.070)	(0.116)	(0.104)	(0.160)
Misclassification Correction	No	Yes	No	Yes	No	Yes
Weak IV Test Statistic	18.419	17.718	18.119	16.813	13.906	13.735
p-value	0.010	0.013	0.011	0.019	0.053	0.056
Hansen J Statistic	3.924	3.427	3.834	3.432	3.279	3.570
p-value	0.687	0.754	0.699	0.753	0.773	0.735
Percent Change in Levels	-71%	-52%	-70%	-52%	-72%	-54%
p-value	0.000	0.001	0.000	0.000	0.000	0.000
Percent Change over Baseline	-49%	-33%	-48%	-36%	-52%	-38%
p-value	0.034	0.124	0.001	0.042	0.010	0.051
Number of Daughters	2618	2618	2757	2757	1961	1961
Observations	44122	44122	36823	36823	36404	36404

Notes: Robust standard errors with state clustering are shown in parentheses. All specifications control for state and year effects in addition to time-varying controls for daughter's age, age squared, and indicators for number of children equal to 1, 2, 3, or at least 4. Instrumental variables include average and maximum measures of the mother's AFDC/TANF benefit standard and federal/state EITC maximum credit by family size, which are defined over the daughter's critical exposure ages 12-18, and interactions of each with an indicator for welfare reform. The weak IV test statistic is a Kleibergen-Paap (2006) rank statistic. The misclassification correction uses reporting rates in the PSID to address potential misreporting for the daughter's welfare participation (see Appendix A for details). Daughters' PSID core longitudinal weights are used in estimation.

Ziliak et al. (2000) show that states' decisions to adopt waivers were not an endogenous response to the growing welfare caseload in the early 1990s. Both of these suggest that state-level welfare policies like the maximum guarantee are exogenous to an individual's welfare choice.

As a test on the baseline sample, I consider three alternatives to the IV models in Table 1.2: restricting the sample of daughters to those who reside in the same state as their birth state, restricting the sample of daughters to those residing in the same state as their mothers, and restricting the sample of daughters to those who never move during their observed lifetime. Table 1.5 shows that both the direct effect of mothers' participation and the interaction with welfare reform are larger in absolute value in Table 1.5 compared to estimates in Table 1.2, yet the changes are relatively proportional such that both the percent reduction in levels and percent-over-baseline reduction of transmission after welfare reform are roughly the same. The magnitudes of estimates in Table 1.5 get successively larger in absolute value as I tighten the geographic link between mother and daughter, and are suggestive that the mobility of daughters across state lines can "undo" some of the intergenerational transmission of welfare, although the differences from the baseline estimates are modest.

1.6 Heterogeneity of Policy Effects

I next investigate timing of transmission by age and duration of exposure, and heterogeneity by race and welfare reform aggressiveness.

1.6.1 Timing of Welfare Transmission Effects

In the first set of results, I examine how the base-case IV estimates with and without misclassification corrections in Table 1.2 change if I restrict the daughter's potential welfare exposure to only periods of co-residence. Recall that in Table 1.2, the daughter could be exposed to her mother's welfare use at any time in the life cycle provided it was prior to the current period t, including those periods when the daughter no longer lived at home but had younger siblings at home that make her mother welfare-eligible. In the first two columns of Table 1.6, the pre-reform transmission effect is little changed relative to the baseline in Table 1.2, and again, the post-reform interaction changes proportionally. This implies that welfare reform had the same percent reduction of welfare transmission among those daughters exposed only during co-residence.

Table 1.6. Intergenerational Transmission of AFDC/TANF Participation by Exposure Mechanism via "Word of Mouth"

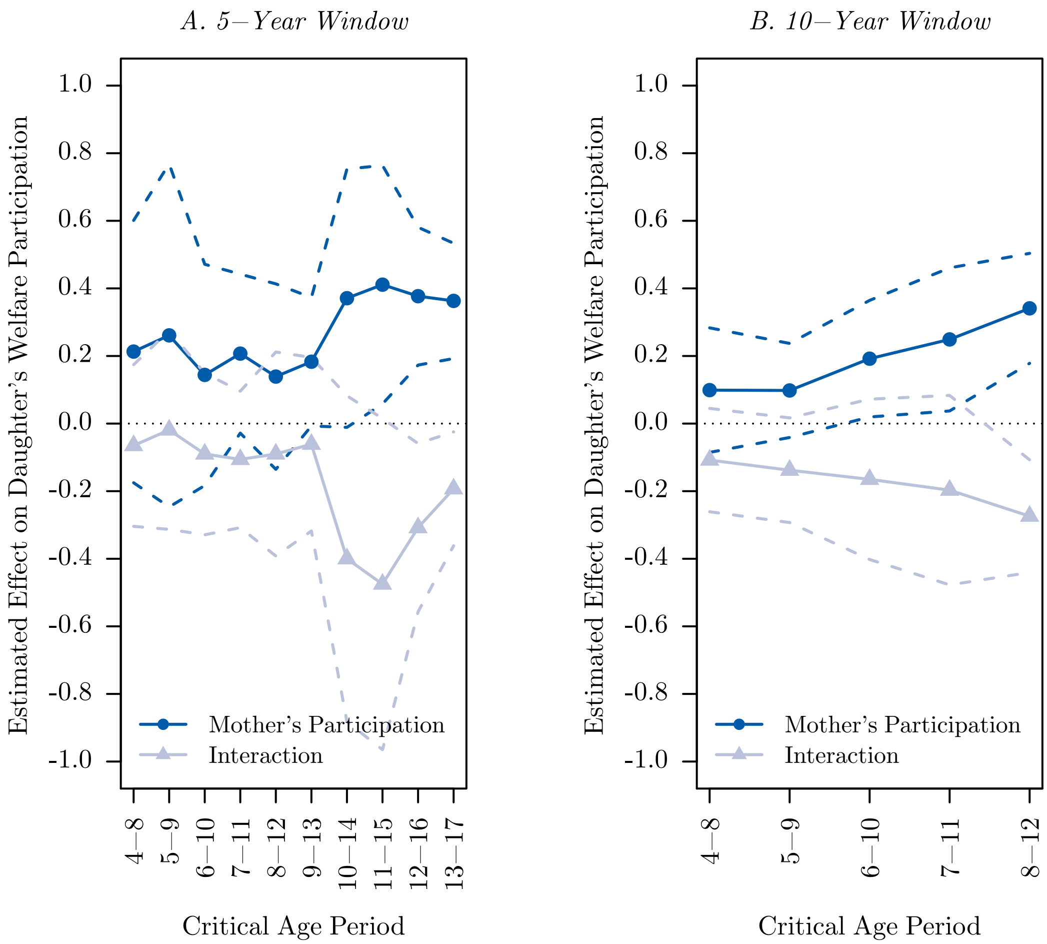
	Exposure During Co-Residence Only		Any Prior Exposure with Daughter Fixed Effects and "Word-of-Mouth" Learning	
	(1)	(2)	(3)	(4)
Mother's Participation	0.272	0.413	0.079	0.218
	(0.058)	(0.070)	(0.023)	(0.032)
After Welfare Reform	0.058	0.070	0.052	0.073
	(0.018)	(0.029)	(0.011)	(0.020)
Mother's Participation ×	−0.188	−0.213	−0.128	−0.175
After Welfare Reform	(0.058)	(0.070)	(0.019)	(0.034)
Daughter Fixed Effects	No	No	Yes	Yes
Instrumental Variables	Yes	Yes	No	No
Misclassification Correction	No	Yes	No	Yes
Weak IV Test Statistic	17.399	16.969		
p-value	0.015	0.018		
Hansen J Statistic	6.285	6.123		
p-value	0.392	0.410		
Percent Change in Levels	-69%	-52%	-100%	-81%
p-value	0.000	0.006	0.000	0.000
Percent Change over Baseline	-46%	-33%	-100%	-72%
p-value	0.062	0.206	0.010	0.003
Number of Daughters	2961	2961	2961	2961
Observations	56068	56068	56068	56068

Notes: Robust standard errors with state clustering are shown in parentheses. All specifications control for state and year effects in addition to time-varying controls for daughter's age, age squared, and indicators for number of children equal to 1, 2, 3, or at least 4. Instrumental variables include average and maximum measures of the mother's AFDC/TANF benefit standard and federal/state EITC maximum credit by family size, and interactions of each with an indicator for welfare reform. Given the independent variable definition in columns (1) and (2), the instruments are defined over the years of mother-daughter co-residence only (elsewhere, instruments are defined over the critical exposure years when the daughter is aged 12-18). The weak IV test statistic is a Kleibergen-Paap (2006) rank statistic. The misclassification correction uses reporting rates in the PSID to address potential misreporting for the daughter's welfare participation (see Appendix A for details). Daughters' PSID core longitudinal weights are used in estimation.

In the baseline models, the estimation sample requires mothers and daughters to co-reside at least five years during the ages of 12-18. As discussed in the data section, this age range was selected in part from convention in the literature, but there is little prior evidence on whether "age of exposure" mattered for welfare learning. In Figure 1.3, I present new empirical evidence of age at critical exposure windows by using rolling five-year and ten-year windows from age 4 through age 17. The figure presents IV estimates of the pre-welfare reform effect of mothers' AFDC participation and the interaction between mother's participation and reform, along with 95-percent pointwise confidence intervals. Figure 1.3 shows that the magnitude of the direct effect of the mother's participation increases as the age of first exposure increases, suggesting that the learning effect is stronger during adolescence and teen years relative to early childhood. The definition of a critical exposure period matters more for shorter windows given that larger windows are more likely to include some critical learning period.

As a further exploration of age of exposure, columns (3)-(4) in Table 1.6 present panel-data fixed-effects estimates of the welfare transmission with and without misclassification corrections. Specifically, I admit error components into the model consisting of latent person-specific heterogeneity as $v_{ist}^d = \lambda_i^d + u_{ist}^d$, where λ_i^d is a time-invariant daughter fixed effect and u_{ist}^d is an error term. I assume that the daughter fixed effect contains a component common to the daughter and the mother from shared family heritage and experiences (including health status, attitudes), as well as that which is daughter-specific such as school quality and neighborhood. Identification of the direct, pre-reform effect of mother's participation is subtler in the fixed-effects specification. Namely, transmission can only occur via "word-of-mouth" from mother to daughter after the daughter has left

Figure 1.3. Critical Exposure Period for AFDC/TANF Transmission Through Age 17



Notes: The dependent variable is daughter's current AFDC/TANF status, and the independent variables include any previous AFDC/TANF participation for the mother, an indicator for after welfare reform, an interaction term for mother's participation after welfare reform, state and year effects, daughter time-varying controls, and instrumental variables including the average and maximum of mother's AFDC/TANF benefit standard and federal/state EITC maximum benefit by family size during the daughter's critical age period, and interactions of each with an indicator for welfare reform.

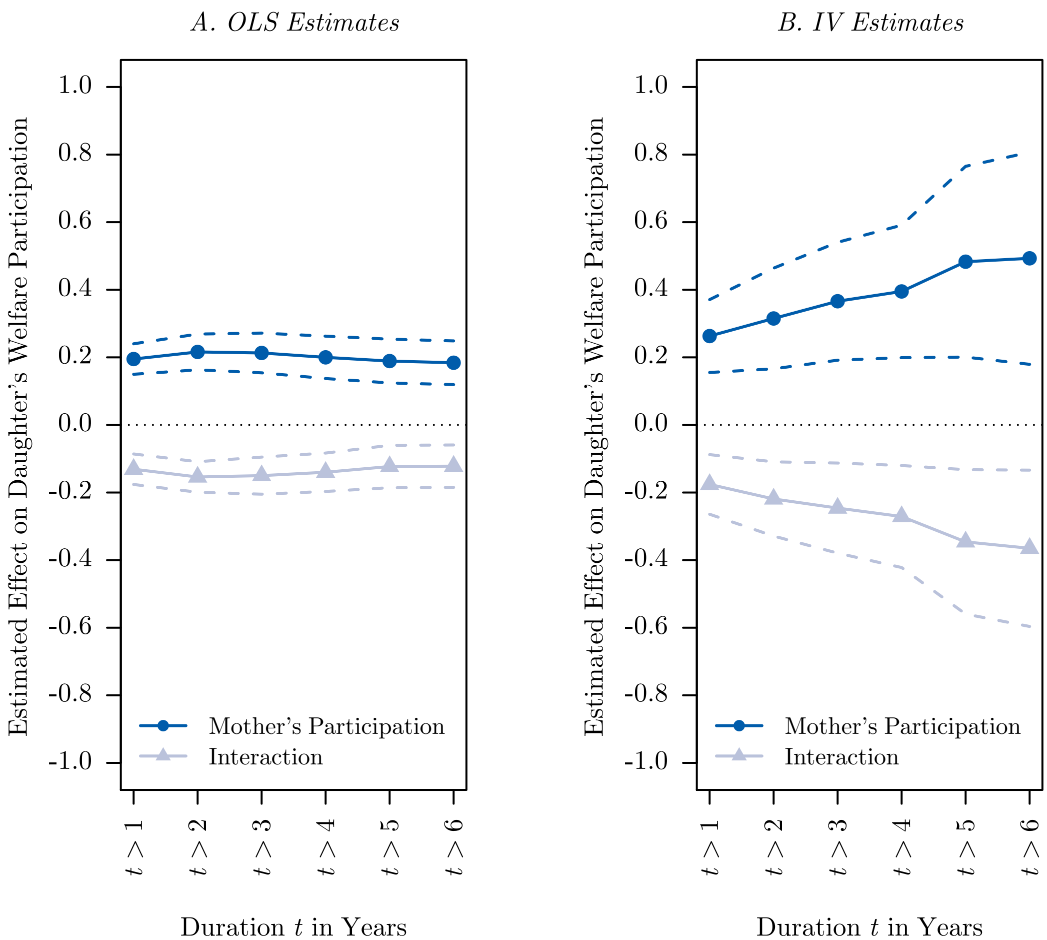
home to form her own family unit. This follows from my definition of mother's prior welfare use that once the variable "turns on" it remains on for the duration that they remain in the sample. If the mother joins welfare while the daughter co-resides then I cannot separate this from the fixed effect; however, if she joins after the daughter leaves because of younger children present, then verbal transmission of the program can still occur and identify the parameters of interest.

The direct effect of mother's transmission in column (3) of Table 1.6 is almost half the size of the estimate from column (1) of Table 1.2, suggesting that a sizable fraction of the transmission that is passed from parent to child occurs after the daughter leaves home. In fact, the total effect after welfare reform is negative $(0.079 - 0.128)$, suggesting that welfare reform shut down this transmission channel. However, fixed-effects methods exacerbate attenuation bias, so it is natural to find estimates lower in absolute value.[17] Once I make time-varying corrections for misclassification in column (4), the mother's direct effect only drops about one tenth from the estimate in column (3) of Table 1.2, though the percent change after reform is larger and I find that the level and percentage of the word-of-mouth transmission channel declines significantly.

A daughter's exposure to welfare and her resulting propensity for dependence will likely vary as a function of her mother's duration of participation, or otherwise stated, her intensity of treatment exposure. Gottschalk and Moffitt (1994) propose measuring welfare dependence as the total time on welfare or the total percent of income from transfers, and Pepper (2000) models daughters' welfare outcomes depending on categorical definitions of mother's duration in years. In order to allow the mother's effect to vary by duration,

[17] For measurement error in a dichotomous independent variable in a panel setting, see Freeman (1984); the case for errors in continuous variables in panels is addressed by Griliches and Hausman (1986).

Figure 1.4. AFDC/TANF Transmission Effects by Duration of Mother's Longest Spell on Welfare



Notes: The dependent variable is daughter's current AFDC/TANF participation status, and the independent variables include an indicator for whether the mother's maximum welfare spell duration is greater than $t' = \{1, 2, \ldots, 6\}$ (see x-axis), an indicator for after welfare reform, an interaction term for mother's longest spell duration indicator and after welfare reform, state and year effects, and daughter time-varying controls for her age, age squared, and indicators for number of children 1, 2, 3, or 4 or more. Instrumental variables for the mother's participation in Panel B include average and maximum measures of the mother's AFDC/TANF benefit standard and federal/state EITC maximum credit by family size and interactions of each with an indicator for welfare reform. Dashed lines represent 95-percent pointwise confidence intervals with state-level clustering.

I successively redefine mothers' welfare participation as at least 1 year, at least 2 years, and so on, until at least 6 years and re-estimate the model with each specification.

Figure 1.4 shows the effects of mother's welfare participation differentiated by short- and long-term welfare dependence on the same dependent variable described above, that is, a daughter's extensive-margin decision to participate in a given year. While the OLS estimates suggest that the transmission effect is constant regardless of length of exposure, the IV estimates in Panel B indicate that the level of transmission effect of long-term mother's participation on welfare is larger than the effect of short-term participation. However, because the post-reform coefficient is getting larger in absolute value as the length of exposure increases, the percent reduction in transmission after welfare reform is fairly stable post reform.

1.6.2 Welfare Transmission Effects by Race and Strictness of Reform

There is a vast literature on the socioeconomic differences between blacks and whites (see, for example, Donohue and Heckman, 1991; Duncan and Hoffman, 1990; Smith and Welch, 1989), but with the notable exceptions of Gottschalk (1996) and Pepper (2000), whether or not there are racial differences in the transmission of intergenerational welfare has received less attention compared to other outcomes. The issue is salient in part because the risk of out-of-wedlock births is at least two times higher among blacks than whites, as is the risk of poverty in childhood.

The first two columns of Table 1.7 present OLS and IV estimates for the transmission of AFDC/TANF from mother to daughter separated by blacks and whites. Specifically, I include an indicator variable for whether the daughter is black, and I interact that with both mother's participation and welfare reform (and interact all instrumental variables

Table 1.7. Heterogeneous Intergenerational Transmission of AFDC/TANF Participation

Transmission Effects by:	*A. Race*		*B. Reform Aggressiveness*	
	(1)	(2)	(3)	(4)
	Black		Aggressive States	
Mother's Participation	0.166	0.442	0.139	0.184
	(0.027)	(0.173)	(0.016)	(0.040)
Mother's Participation ×	−0.101	−0.233	−0.099	−0.133
After Welfare Reform	(0.032)	(0.185)	(0.022)	(0.040)
	White		Non-Aggressive States	
Mother's Participation	0.068	0.146	0.148	0.305
	(0.013)	(0.073)	(0.018)	(0.074)
Mother's Participation ×	−0.057	−0.125	−0.102	−0.228
After Welfare Reform	(0.014)	(0.071)	(0.018)	(0.065)
Instrumental Variables	No	Yes	No	Yes
Weak IV Test Statistic		25.131		26.612
p-value		0.022		0.014
Hansen J Statistic		8.813		9.567
p-value		0.719		0.654
	Black		Aggressive States	
Percent Change in Levels	-61%	-53%	-71%	-72%
p-value	0.000	0.041	0.000	0.000
Percent Change over Baseline	-31%	-17%	-50%	-51%
p-value	0.193	0.703	0.087	0.035
	White		Non-Aggressive States	
Percent Change in Levels	-84%	-85%	-69%	-75%
p-value	0.000	0.002	0.000	0.000
Percent Change over Baseline	-72%	-74%	-46%	-56%
p-value	0.001	0.115	0.002	0.001
Number of Daughters	2848	2848	2961	2961
Observations	54956	54956	56068	56068

Notes: Robust standard errors with state clustering are shown in parentheses. All specifications control for state and year effects in addition to time-varying controls for daughter's age, age squared, and indicators for number of children equal to 1, 2, 3, or at least 4. Instrumental variables include average and maximum measures of the mother's AFDC/TANF benefit standard and federal/state EITC maximum credit by family size, which are defined over the daughter's critical exposure ages 12-18, interactions with an indicator for welfare reform, and interactions of each with an indicator for daughter's race is black in columns (1)-(2) or state's reform is stringent in columns (3)-(4). The weak IV test statistic is a Kleibergen-Paap (2006) rank statistic. Daughters' PSID core longitudinal weights are used in estimation.

with the indicator for daughter's race). As before, all models control for state and year effects, a quadratic in daughter's age, and indicators for the number of children in the daughter's family. The first two columns in the upper panel of Table 1.7 suggest that the pre-reform effect of welfare transmission was much stronger among blacks than whites (in column (2), 0.442 compared to 0.146). However, while the transmission channel was substantively reduced among both blacks and whites after welfare reform, the percent change is much larger among whites.

States differed dramatically in the degree of aggressiveness in implementation of welfare reform, both in the waiver era and after TANF. While there is no agreed upon measure of strictness in the literature, I follow Grogger and Karoly (2005, Table 4.2) and define strict states as those whereby all main studies surveyed agree that the sanctions policy adopted by the state during 1992-1996 was strict (there were 13 states that met this criteria). Ziliak (2007) examined five different categories of welfare reform aggressiveness and concluded that the latter measure was the best proxy for strict policy reforms. I then include this measure of welfare reform stringency in a triple-difference framework to test whether there were differences in intergenerational transmission in those states that adopted more-strict reforms compared to states with less-strict reforms.

The last two columns of Table 1.7 report estimates corresponding to the effects of interest for the triple-difference model based on state reform aggressiveness. Across both specifications, the transmission mechanisms between mother and daughter before welfare reform were qualitatively smaller in aggressive states than in non-aggressive states. This suggests that there was some permanent difference among residents in states adopting strict reforms versus less strict reforms (even after controlling for state fixed effects).

However, after reform, this difference was attenuated, resulting in very similar percent reductions in both the levels and probability of participation, suggesting some degree of convergence in welfare climates across states after welfare reform.

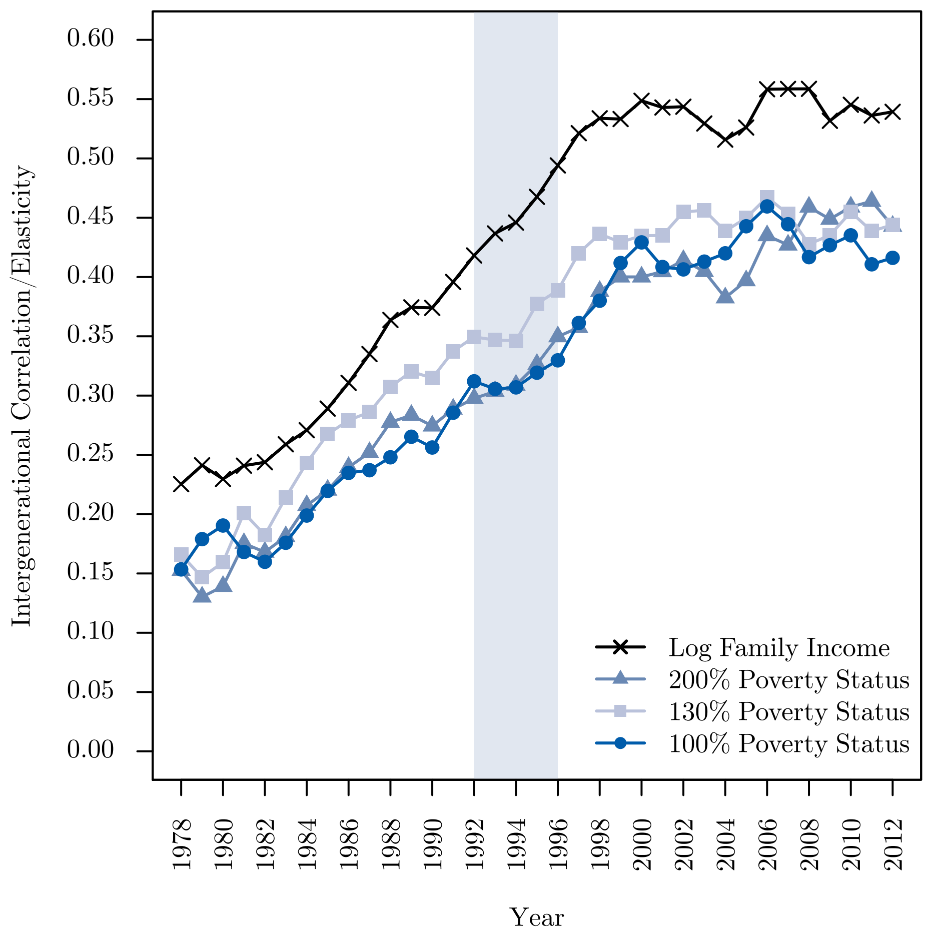
1.7 Discussion and Conclusion

A focal aim of policymakers with the 1990s welfare reform was to end dependence on welfare, and based on the metric of the intergenerational transmission between mother and daughter, the evidence presented here suggests partial success toward meeting that goal. Viewed narrowly from the lens of participation in the AFDC/TANF program, I find strong evidence that the level of transmission from mother to daughter was reduced by at least 50 percent, and by at least 30 percent over the baseline odds of participation. These results are robust across a variety of specifications that address major threats to identification including selection bias, misclassification bias, life-cycle bias, and geographic mobility. Despite the statistical challenges faced in this work, one consistent interpretation of these results implies that when the AFDC/TANF use fell precipitously after 1996, the reform had a differential impact among adult daughters who were exposed to welfare in their childhood and those who were not. The change of at least 30 percentage points over the odds of participation suggests that between one-half and two-thirds of the caseload decline comes from reduced transmission.

Beyond participation in AFDC/TANF, however, the 1996 welfare reform did not alter the generational economic bonds between mother and daughter. These findings suggest that welfare reform did not change the transmission of participation in the wider safety net including food and disability assistance, nor did it alter the ties between mothers welfare use and daughters later life outcomes of human capital or labor market success. This finding is consistent with the previous welfare reform research on mothers' outcomes—the reforms explained some of the decline in AFDC/TANF participation but had no substantive effects on work, earnings, marriage, health, or wealth (Blank, 2002; Moffitt, 2003; Ziliak, 2016). That research also found no substantive changes on the well-being of children, although the evidence in that domain is more limited. The results of this study expand upon the previous null effects of welfare reform on the wider domain of intragenerational economic outcomes to the intergenerational context.

At first blush this lack of effect on economic success seems surprising given the scale and scope of the reform. However, this becomes more clear when examining how states chose to allocate their block grants. Prior to reform states spent around \$0.75 of every \$1 of benefit in the form of cash assistance, whereas today only about \$0.20 goes toward cash, and another \$0.20 toward child care. Moreover, there is great variation across states, ranging from less than \$0.15 on cash assistance and child care in Arizona to nearly \$0.70 in Pennsylvania. The remaining funds are known as "non-assistance" and states have great leeway in how those funds get allocated, ranging from marriage preparation programs to middle class tax cuts (Bitler and Hoynes, 2016). That is, the program is substantially less target-efficient and does not entail much investment in long-term economic self-sufficiency. A potential consequence is the stagnating mobility of daughters. I explore this possibility in Figure 1.5 where I present descriptive trends in intergenerational correlations between mothers and daughters akin to Figure 1.2, but now for four measures of economic status: (1) poverty status defined as an income-to-needs ratio less than 1, where needs is defined by the U.S. Census Bureau poverty line that varies by family size; (2) poverty status defined as an income-to-needs ratio less than 1.3 (the cutoff for food stamps); (3) poverty

Figure 1.5. Trends in the Intergenerational Transmission of Poverty Status and Family Income



Notes: The intergenerational transmission for poverty status represents linear probability model estimates based on indicators for whether an individual's mean family income is equal to or below 100, 130, or 200% of the mean federal poverty threshold observed during adulthood through age 27, and the intergenerational elasticity of family income is based on a log-log model of a daughter's average income through age 27 and the average of all of her mother's prior family income. The major waiver period of welfare reform is indicated by the shaded region.

status defined as an income-to-needs ratio less than 2; and, (4) log family income.[18] In the two decades from the late 1970s to 2000, the income mobility of daughters declined (i.e. the correlation was increasing). And while immobility has not deteriorated further in the past decade, the income correlations suggest daughters had continued economic need for assistance from the wider safety net.

I conclude by noting that implicit in most discussion surrounding welfare reform is that the transmission of welfare reliance from parent to child is inherently a bad outcome. It is not obvious, however, what is the socially efficient intergenerational correlation of welfare outcomes. For example, a correlation of zero—perfect mobility with respect to welfare use—would imply that accumulating "family capital" (wealth, culture, information, and skills) does nothing to ensure the self-sufficiency of future generations. In some cases, though, there may be positive attributes to intergenerational transmission of welfare knowledge if take-up rates are low and learning the welfare system helps needy recipients (Currie, 2006). Indeed, in the few years after welfare reform, take-up rates of food stamps among those eligible fell about 20 percentage points to just over 50 percent, mainly because potential recipients were not aware of their eligibility in a post-reform environment that discouraged welfare more generally (Ganong and Liebman, 2013; Ziliak, 2015). The policy response by USDA was to grant more authority to states to design their programs to improve take up. Presumably, among those 50 percent who continued participation, some retained eligibility was because of shared information from parent to child. This suggests a need for future theoretical and empirical research on optimal transfer program design that incorporates knowledge spillovers across generations.

[18] Income-to-needs ratios are constructed as the mean income to mean poverty threshold for a daughter's adult life through age 27, and for the mother's years while the daughter lives at home.

2 BEHAVIORAL RESPONSES AND WELFARE REFORM: EVIDENCE FROM A RANDOMIZED EXPERIMENT

2.1 Introduction

Distributional effects of policies are increasingly the causal effect of interest among social scientists. For example, policy evaluation for experimental reforms may depend on considerations of target efficiency, such as whether program features induce behavioral responses where individuals reduce labor supply in order to become income-eligible for participation. Questions of equity and efficiency were prominent in the public debate leading up to the largest policy reforms in U.S. welfare history during the 1990s. By 1995, at least 40 states had implemented a policy waiver from the federal rules under Aid to Families with Dependent Children (AFDC) in order to experiment with work incentives and restrictions. Connecticut was a prime example for policy evaluation in that its reforms were a combination of the most generous earnings disregards (earnings not used in benefit reductions) and the most strict time limits. Beginning in 1996, Connecticut implemented a welfare waiver program called Jobs First, under which women could increase earnings up to the federal poverty line without any reduction in benefits. Welfare recipients and applicants were randomized into either Jobs First or AFDC, making the identification of quantile treatment effects (QTEs) possible by experimental design and under selection on observables. This leads to the key contribution of this study, which is to investigate empirically whether unobserved heterogeneity, possibly associated with welfare participation costs, is relevant for considerations of target efficiency.

The behavioral effects of welfare reform on low-income single mothers, especially regarding intensive margin responses on labor hours supplied, is an area of great interest in policy discussions and within the research literature. For the historical context and details of welfare policy reform from AFDC to the creation of Temporary Assistance for Needy Families (TANF), see Moffitt (2003) and Ziliak (2016). For a review of welfare reform effects across 15 European countries, see Immervoll et al. (2007). Bargain and Orsini (2006), Brewer et al. (2006), and González (2008) investigate other welfare reform responses in Europe. Beffy et al. (2016) demonstrate the importance of restrictions on offered hours for low-income mothers in the United Kingdom. Mogstad and Ponzato (2012) compare work hours for married and single mothers after welfare reform in Norway. Similar work is beginning to look at behavioral responses to transfer programs in developing economies where the informal labor market provides an alternative margin of response (Banerjee et al., 2017; Bergolo and Cruces, 2016).

It has been recognized in the literature that AFDC-assigned women leave welfare at different rates over time than those assigned to Jobs First since these programs have different features including earnings disregards and time limits (Bitler, Gelbach, and Hoynes, 2006). It is also known that welfare imposes costs on participants including transactions costs and stigma (Blank, Card, and Robins, 2000; Blank and Ruggles, 1996; Currie, 2006; Moffitt, 1983). It is natural then to expect that these factors vary by treatment status after the random assignment, leading to a framework where AFDC-assigned women who exited welfare have no participation costs and Jobs First-assigned women who did not exit welfare have non-zero participation costs. In order to address the plausible cost differentials by treatment status, I use a semiparametric quantile estimator for a sparse econometric model and revisit the distributional analysis of the Jobs First welfare reform experiment.[1] Keys to the identification and estimation of QTEs are the use of experimental data and an assumption of zero participation costs for women who exited welfare at conditionally high earnings.

Consistent with the literature, I find a treatment effect estimate at the 0.90 quantile of -200 dollars of quarterly earnings which suggests that women reduced hours to become income-eligible for participation. In contrast, once I allow for costs of welfare participation to be different by individuals and treatment status (e.g., zero participation costs for AFDC-assigned women), I find evidence suggesting a positive treatment effect of 300 dollars. I propose and perform a test that indicates that these QTEs are significantly different at standard levels. At other quantiles, however, there is no statistically significant difference between estimates. To put the size of estimated treatment effect differences in context, Moffitt (1983) estimates that AFDC participation imposes a cost of about 4 hours of labor supply per week, which corresponds to around 520 dollars per quarter at an hourly wage of 10 dollars (the 90th percentile wage at quarter 1).[2] I interpret this evidence as indicative that women make choices regarding work and welfare based on their opportunity cost of time, which is increasing in earnings and depends on family

[1]This approach relates to recent penalized estimators for sparse models proposed in the literature (see, for example, Belloni and Chernozhukov, 2011; Belloni et al., 2017; Imai and Ratkovic, 2013; for panel data, see also Harding and Lamarche, 2017). For theory and application of distributional analysis for welfare programs and treatment effects, see Heckman, Smith, and Clements (1997), Bollinger, Gonzalez, and Ziliak (2009) and Blundell, MaCurdy, and Meghir (2007), among others. Burtless and Hausman (1978) and Moffitt (2002) investigate the importance of individual parameters in the specification of econometric models.

[2]Meyer and Rosenbaum (2001) incorporate variable stigma/transaction costs in a structural model with panel data and estimate that the monetized cost of working while on welfare is approximately $643 per quarter.

structure, preferences, and program features given treatment status. Further, I expand the empirical analysis by estimating QTEs for continuing welfare recipients and new applicants given possible differences in participation costs. Long-term welfare participation, associated with ongoing recipients, may imply higher informational costs to labor supply due to limited labor market experience or the effects of persistent stigma. I find evidence that controlling for latent individual heterogeneity affects behavioral-induced participation more for those with less labor market experience and longer, more frequent welfare spells compared to new applicants.

Heterogeneous impacts of the Jobs First experiment have already received notable attention. Bitler et al. (2006) illustrate the importance of estimating the distributional effects of a welfare reform experiment by using a nonparametric estimator for the difference between the treatment and control distributions at given quantiles. According to their estimates, the reform had no impact at the lower tail of the conditional distribution of earnings, it increased the conditional median of earnings, and it reduced the upper tail of the earnings distribution. While the mean treatment effect provides an uninformative summary of opposing effects, treatment effects exhibit significant differences across quantiles. More recently, Kline and Tartari (2016) estimate bounds for individual behavioral responses based on revealed preference assumptions. For women who would earn above the federal poverty line under AFDC, the authors estimate that at least 20 percent would reduce earnings in order to participate under Jobs First. Both of these studies find evidence that women in the upper earnings distribution reduce labor supply in order to receive welfare transfers, an example of behavioral-induced participation (Ashenfelter, 1983). Consistent with these studies, I employ experimental data for Connecticut's Jobs First waiver program and estimate distributional effects. I distinguish my approach from the previous literature, however, by incorporating the panel nature of the Jobs First experiment and allowing a behavioral component of program participation. My results compare directly to the nonparametric quantile treatment effect as constructed in Bitler et al. (2006), and I also discuss this design-based approach in comparison to the methodology and findings of Kline and Tartari (2016). The policy implications regarding behavioral-induced participation as described in the literature do not generalize to all individuals on welfare near the eligibility threshold, especially regarding continuing recipients.

This essay is organized as follows. Section 2.2 provides a simple framework for motivating the economic model and making testable predictions for heterogeneous labor supply responses to treatment. Section 2.3 introduces an econometric model consistent with the framework developed in Section 2.2 and then proposes a new approach for estimating QTEs in a regression setting. While Section 2.4 discusses the data, Section 2.5 presents the empirical analysis including regression results and inference on estimated quantile treatment effects. Section 2.6 offers a discussion on estimation issues and interpretation of results. Section 2.7 concludes.

2.2 Economic Implications of Jobs First and AFDC programs

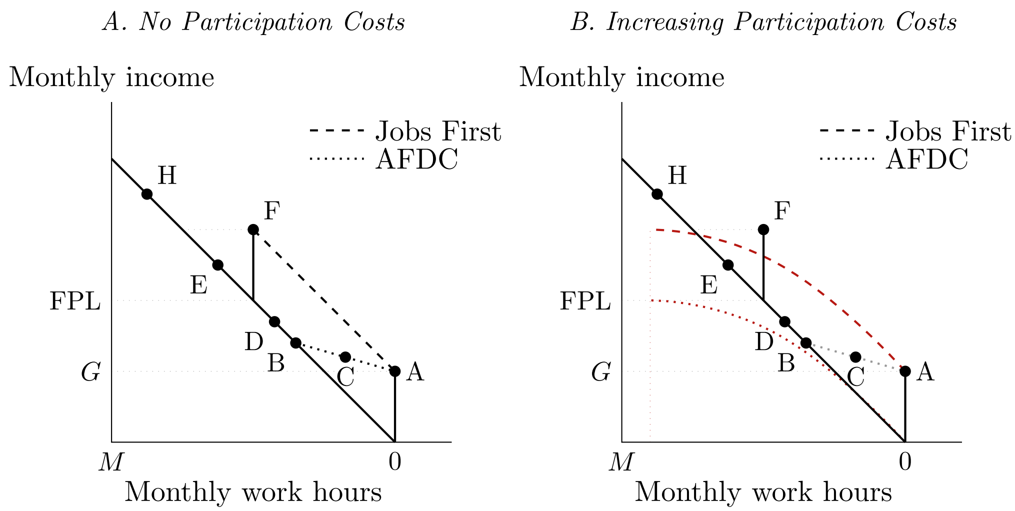
Connecticut implemented Jobs First as a welfare reform waiver program beginning in 1996. Approximately half of the cash welfare participants were assigned to Jobs First and the other half to AFDC. The key feature of this waiver program is a 100-percent earnings disregard up to the federal poverty line, which leads to an implicit marginal tax of zero percent. In contrast, AFDC disregarded \$120 of monthly earnings for the first year in the program and $90 after the first year. The statutory marginal tax rate on earnings under AFDC is 100 percent such that each additional dollar earned reduces transfers by one dollar. This dramatic policy change with respect to earnings creates a strong work incentive for many welfare participants, but it also creates a work disincentive around a significant notch at the federal poverty line above which Jobs First participants become income-ineligible for transfers.

While earnings disregards provide a strong motivation for predicting labor supply, the policy reform included other features that may be salient for behavioral responses. For instance, earnings were also fully disregarded in Food Stamp benefit determination for the treatment group. The experimental programs differed along other policy dimensions such us work requirements, sanctions, and time limits. While Jobs First has a strict 21-month time limit, AFDC has no time limits. Additionally, Jobs First participants were eligible for more generous transition benefits for child care and Medicaid after exiting welfare.[3] The program features of Jobs First demonstrate a range of policies implemented at the state level after the transition from AFDC to Temporary Assistance for Needy Families (TANF).

Bitler et al. (2006) note that static labor supply theory motivates the prediction of heterogeneous treatment effects across the distribution of total income (earnings plus cash welfare and Food Stamp benefits). Panel A in Figure 2.1 reproduces their stylized budget constraint with monthly income on the vertical axis and monthly work hours from 0 to M on the horizontal axis. The federal poverty line is indicated by FPL, and the welfare benefit guarantee amount by G. The Jobs First budget constraint with 100-

[3] See Bloom et al. (2002) for a detailed description of differences between Jobs First and AFDC programs.

Figure 2.1. Stylized Budget Constraints under Jobs First and AFDC



Notes: Panel A is reproduced from Bitler et al. (2006) Figure 1. Panel B shows a hypothetical constraint for Jobs First given nonlinear costs of welfare program participation.

percent earnings disregards is shown by segment AF, and the AFDC constraint is shown by segment AB.[4] Most women in the Jobs First study would begin at a location near point A, so the figure represents points where women might locate over time under the AFDC policy in order to predict how their behavior would change under Jobs First. In panel A, women who would locate at points like D and E under AFDC might be induced to participate in welfare under Jobs First. For instance, at point D, a woman under AFDC would become mechanically eligible for welfare under Jobs First and therefore might be induced to participate by income effect if leisure and consumption were normal goods. A woman located at point E under AFDC might be behaviorally induced to participate in Jobs First if the utility gain from participation compensates the reduced earnings necessary to become eligible below the federal poverty line.[5] If women in Jobs

[4]Whereas the AFDC statutory implicit tax rate is 100 percent, the effective tax rate would be somewhat lower in practice, as shown for segment AB.

[5]Ashenfelter (1983) emphasizes the distinction between behavioral components of participation, where an individual changes labor supply behavior in order to meet program eligibility, and mechanical com-

First are able to increase utility by reducing hours, this would imply a *negative* treatment effect at the upper tail of the earnings distribution.

The stylized budget constraint in Figure 2.1 illustrates points to which women may potentially relocate over time according to Bitler et al. (2006, pg. 994–995). Thus, treatment effect predictions presume that women in both experimental groups will likely increase earnings and relocate along their respective budget constraints. In order to observe behavioral-induced participation, the mechanism must either be reduced exits or reentry after exit. For both groups, there will be a natural rate of exit from welfare that may depend on program features. For instance, low-income women assigned to AFDC can locate at points like D or E after random assignment, and consequently, one can expect a relative increase in participation due to Jobs First assignment. Therefore, policy impact estimates should take into account changes in work and participation decisions related to exposure to treatment over time as well as possible nonlinear changes in participation costs as hours increase. Becker (1965) motivates the relevance for one's cost of time with respect to labor market activities and household production. A woman's implicit time costs of working while also meeting welfare program requirements will depend on her characteristics such as family structure, neighborhood, preferences, or social capital.[6]

Suppose that women experience individual-specific costs of welfare participation that are increasing in hours of labor supply. Those with higher earnings have a higher opportunity cost of time, and thus transaction costs associated with welfare participation may

ponents of participation, where changes in program rules make participation preferable given newly available choices in an individual's budget constraint.

[6] Given that the Jobs First and AFDC programs differ by other features besides earnings disregards and time limits, there might be other behavioral responses that could differ by treatment status over time.

be more relevant for women who are still eligible for welfare while working more hours.[7] Therefore, panel B suggests some hypothetical cost for participation that is increasing with labor hours such that a woman would have to work more hours to reach the same net earnings.[8] Note that women assigned to AFDC would not face such participation costs at higher earnings since participation eligibility is phased out at lower income levels. In this case, Jobs First would not be interpreted as a pure income shift at points like D and E, and thus the labor supply predictions become ambiguous depending on an individual's preferences and participation costs.

2.3 A Model and the Proposed Methodology

2.3.1 Economic Model

To start analyzing the economic implications of the Jobs First program, I turn to the standard potential outcome approach to causal inference. A response variable or potential outcome has two values for a low-income woman i at quarter t, $(Y_{0,it}, Y_{1,it})$, one of which is observed and is labeled Y_{it}. The observed outcome depends upon the random treatment assignment, D_{it}, which can take $\{0, 1\}$ values indicating AFDC or Jobs First status, respectively. I then write $Y_{it} = D_{it}Y_{1,it} + (1 - D_{it})Y_{0,it}$ and assume that D_{it} is independent of the potential outcome.

Let $Q_Y(\tau)$ denote the τ-th quantile of the distribution of Y. The outcome variable Y is continuous and the treatment status D is independent of a p-dimensional vector of observed covariates, $\mathbf{x}$, as well as unobserved covariates, by the experimental design of

[7] Also, Gottschalk (2005) shows that the preferences of low-income women may change with exposure to work and welfare participation, which implies that program features may influence women differently regarding the disutility of work or welfare.

[8] Moffitt (1983) illustrates stigma costs through lower levels of utility, though he notes that the utility model is "closely analogous to one in which costs of participation are monetized and included in the budget constraint." In Figure 2.1 panel B, the dashed line indicates the implicit budget constraint (with monetized participation costs) for Jobs First. A dotted guideline is shown as parallel to net income by a height equal to the guarantee amount, G, and it continues up to an intersection with the FPL.

the program. The parameter of interest is the quantile treatment effect (QTE) defined as

$$\Delta(\tau) = Q_Y(\tau|D=1) - Q_Y(\tau|D=0), \tag{2.1}$$

representing the change in earnings resulting from treatment at a given quantile. It is known that the QTE can also be obtained as a parameter of interest in a quantile regression model. It is immediately apparent that Y_{it} can be written as $Y_{it} = Y_{0,it} + \Delta D_{it}$ where $\Delta = Y_{1,it} - Y_{0,it}$. Therefore, $\Delta(\tau)$ can be obtained from the quantile model associated with the following equation: $Y_{it} = \Delta D_{it} + u_{it}$, where the error term is $u_{it} = Y_{0,it}$.[9]

As an extension to the previous model, consider now that the treatment is subject-specific satisfying $\Delta_i + Y_{0,it} = Y_{1,it}$. This can be motivated by individual costs associated with welfare participation. Without loss of generality, let $\Delta_i = v_i + \Delta$. It follows then that the treatment effect can be estimated using $Y_{it} = Y_{0,it} + v_i D_{it} + \Delta D_{it}$, or, if the treatment indicator is considered time-invariant given assignment at $t = 0$, then $Y_{it} = Y_{0,it} + \alpha_i + \Delta D_{i0}$, where unobserved individual heterogeneity is represented by $\alpha_i = v_i D_{i0}$. A distinctive feature of this extension is that it leads to a *sparse* model where α_i is equal to zero if $D_{i0} = 0$ and $\alpha_i = v_i$ if $D_{i0} = 1$, which leads to the following QTE parameter:

$$\tilde{\Delta}(\tau) := \Delta(\tau) + \alpha(\tau) = Q_Y(\tau|D=1,\alpha) - Q_Y(\tau|D=0). \tag{2.2}$$

The QTE parameter in equation (2.2), $\tilde{\Delta}(\tau)$, is identical to that in (2.1), $\Delta(\tau)$, in two cases. First, participation costs are not different by treatment status, implying that $Q_Y(\tau|D=1,\alpha) - Q_Y(\tau|D=0,\alpha) = \Delta(\tau)$. Note however that this case is not

[9] Naturally, the model includes an intercept, but it is omitted here to simplify the presentation of the model and parameter of interest.

consistent with the empirical observation that women assigned to AFDC leave welfare at different rates over time than those assigned to Jobs First. Moreover, $\tilde{\Delta}(\tau) = \Delta(\tau)$ if Y_1 is independent of v. However, the assumption $Q_Y(\tau|D=1,\alpha) = Q_Y(\tau|D=1,v) = Q_Y(\tau|D=1)$ contradicts the mechanism discussed in Figure 2.1 where participation costs v_i are not independent of $Y_{1,it}$.

At the upper quantiles of earnings, the model has a natural interpretation. Women assigned to AFDC would not face participation costs at higher earnings (since they are not likely to participate), while women assigned to Jobs First do face participation costs. Returning to Figure 2.1, a woman i that would locate, for example, at point E (above the poverty line) would only reduce hours if the decreased earnings plus her individual cost of welfare participation, $\alpha_i = v_i$, are compensated by the increased transfers under Jobs First. Therefore, there is a range of earnings toward the upper conditional quantiles where welfare participation costs are relevant to evaluate behavioral-induced effects.

To evaluate the role of participation costs, I briefly focus on two particular quantile functions and discuss their interpretation within the literature.

Example 1 *Consider the following simple model for earnings similar to those estimated in the literature:* $Y_{it} = \beta + \Delta D_{i0} + \alpha_i + (1 + D_{i0}\gamma_0 + \alpha_i\gamma_i)u_{it}$, *where* γ_0 *is a scale parameter,* γ_i *is a function of the transfer benefit, and the error term is distributed as* F *with location zero and unit variance. The corresponding quantile function is* $Q_{Y_{it}}(\tau|D_{i0},\alpha_i) = \beta(\tau) + \Delta(\tau)D_{i0} + \alpha_i(\tau)$, *where* $\beta(\tau) = \beta + F_u^{-1}(\tau)$, $\Delta(\tau) = \Delta + \gamma_0 F_u^{-1}(\tau)$ *and* $\alpha_i(\tau) = \alpha_i\left(1 + \gamma_i F_u^{-1}(\tau)\right) = v_i D_{i0}\left(1 + \gamma_i F_u^{-1}(\tau)\right)$. *If* $D_{i0} = 0$, *then* $\alpha_i(\tau) = 0$, *representing the case of no participation costs. If* $D_{i0} = 1$, *then* $\alpha_i(\tau) = v_i(1 + \gamma_i(\tau))$, *which is expected* *to be increasing on τ. In this model, v_i might be interpreted as 'flat' stigma and $\gamma_i(\tau)$ as 'variable' stigma (Moffitt, 1983).*[10]

Example 2 *Consider the model presented in Example 1 where participation affects hours offered, and consequently it affects earnings. For simplicity, assume that scale parameters (γ_0, γ_i) are zero for all mothers. Then, $Y_{it} = \beta + \Delta D_{i0} + \alpha_i R_i + u_{it}$, where R_i is an indicator variable for whether woman i participates on welfare. Note that the associated conditional quantile function includes an additional variable R_i and omission of this variable in a quantile model leads to inconsistent results since participation at t might not be independent of D_{i0}. Joint decisions for welfare participation and labor supply are discussed extensively in the literature (see, for example, Moffitt, 1983, 2002, among others).*

This analysis leads to simple tests that can be performed using regression analysis as follows. I first estimate the QTE in equation (2.1) and then compare with the QTE in equation (2.2). The difference between parameters is:

$$C(\tau) := \tilde{\Delta}(\tau) - \Delta(\tau) \geq 0, \tag{2.3}$$

and it represents the relative cost of welfare participation at different quantiles of earnings. At low conditional quantiles of earnings, one could expect $C(\tau) = 0$ if α is zero or the cost of participation does not vary by treatment status, and at high conditional quantiles, one could expect $C(\tau) > 0$ if Jobs First-assigned women incur participation costs

[10] *Moffitt (1983) proposes an economic model where 'flat' stigma is defined as the cost related to any welfare participation, and 'variable' stigma is a cost proportional to the size of the benefit. Although the benefit size is constant for Jobs First participants, variable stigma may be considered the cost proportional to the benefit-to-income ratio, which is decreasing over the earnings distribution. While Moffitt found no evidence of variable stigma, he provided mean estimates that were pre-welfare reform under AFDC such that any variable costs of participation at higher incomes post-welfare reform are unknown, such as the case of Jobs First.*

to labor supply but AFDC-assigned women are not eligible for welfare, and consequently have zero cost of participation. Additionally, I estimate the QTE in equation (2.1) and then compare with the QTE of a model that incorporates an indicator variable for participation. In the next section, I discuss estimation approaches for the QTE parameter and then turn to investigating and testing the previous hypotheses in Sections 2.5.3 and 2.5.5.

2.3.2 Background

Although the QTE in equation (2.1) can be estimated using different approaches (see, for example, Koenker, 2005), previous analyses of welfare reforms have been concerned with potential selection into treatment. For consistent estimation of the parameter of interest, $\Delta(\tau)$, the identification restriction used in the literature is known as selection on observables (Firpo, 2007; Heckman et al., 1998; Rubin, 1977). Thus, given a set of covariates, it is typically assumed that women are randomly assigned to Jobs First or AFDC. This identification condition gives rise to different estimation strategies.

A nonparametric approach, denoted here as NP, to estimating the QTE for individuals $i = 1, \ldots, N$ pooled over quarters $t = 1, \ldots, T$ is given by

$$\hat{\Delta}(\tau) = \inf\left\{y : \hat{F}_{Y_{it}}\left(y|D_{i0}=1\right) \geq \tau\right\} - \inf\left\{y : \hat{F}_{Y_{it}}\left(y|D_{i0}=0\right) \geq \tau\right\},$$
$$\text{for } \hat{F}_{Y_{it}}\left(y|D_{i0}=d\right) = \left[\frac{1}{NT}\sum_{i=1}^{N}\sum_{t=1}^{T}\hat{w}_i\left(\mathbf{x}_i\right)\cdot 1\left(D_{i0}=d\right)\cdot 1\left(y_{it}\leq y\right)\right], \quad (2.4)$$

where $d = \{0, 1\}$ and the empirical inverse-propensity weight is $\hat{w}_i(\mathbf{x}_i) = D_{i0}/\hat{p}_i(\mathbf{x_i}) + (1 - D_{i0})/(1 - \hat{p}_i(\mathbf{x}_i))$. This approach was used by Bitler et al. (2006). The variable $\hat{p}_i(\mathbf{x}_i)$ is the estimated propensity score obtained from a logit regression of an individual's propensity to be treated conditional on observed characteristics $\mathbf{x}_i$. Also considering selection on observables, Firpo (2007) proposes an estimation method that is a weighted version of the classical quantile regression estimator for cross-sectional data (Koenker and Bassett (1978)).[11] The method is semiparametric in the sense that no parametric assumption is made on the joint distribution of the observed variables. The quantile regression estimator (QR) for the QTE in equation (2.1) can be obtained by solving:

$$\min_{(\beta_0,\Delta)\in\mathbf{\Theta}} \sum_{i=1}^{N}\sum_{t=1}^{T} \hat{w}_i\left(\mathbf{x}_i\right)\cdot\rho_\tau\left(Y_{it}-\beta_0-\Delta D_{i0}\right), \tag{2.5}$$

where $\rho_\tau(u) = u\left(\tau - I\left(u \leq 0\right)\right)$ is the standard piecewise-linear quantile check function. I consider two variations of the minimizer of equation (2.5): a weighted estimator defined as above and an unweighted estimator with $\hat{w}_i(\mathbf{x}_i) = 1$ for AFDC and Jobs First participants.

2.3.3 A Penalized Semiparametric Approach

Identification of the time-invariant treatment effect is based on the sparse nature of the model. Let N_0 denote the number of women assigned to the control group, and N_1 the number assigned to treatment, with $N_0 + N_1 = N$. Thus, the model can be augmented by $\alpha = (\alpha_0', \alpha_1')'$, where α_0 is an N_0-dimensional sparse vector for the set of individuals in AFDC, $i \in \mathcal{D}_0 = \{i : D_{i0} = 0\}$, and α_1 is an N_1-dimensional vector of individual effects for the set of individuals in Jobs First, $i \in \mathcal{D}_1 = \{i : D_{i0} = 1\}$. This model can be estimated by an extension of existing penalized quantile estimators with individual effects since the conditions for consistent estimation are satisfied by the experimental design of the program (Assumption 2, Lamarche (2010)). I augment the QR optimization problem defined in equation (2.5) with individual effects and introduce an ℓ_1 penalty function of

[11] Alternative program evaluation methodologies have been recently proposed by Cattaneo (2010) and Słoczyński and Wooldridge (2017). The treatment in this case, however, is not multivalued and I employ inverse-propensity score weighting as in Bitler et al. (2006) for comparability of results.

the following form:

$$Pen(\alpha) = \sum_{i=1}^{N} |\alpha_i| = \sum_{i=1}^{N} (D_{i0}|\alpha_{i,1}| + (1 - D_{i0})|\alpha_{i,0}|) = \sum_{j \in \mathcal{D}_0} |\alpha_j| + \sum_{k \in \mathcal{D}_1} |\alpha_k|. \tag{2.6}$$

Recall that the framework discussed before leads to a model with $N_1 < N$ individual effects possibly having non-zero costs of welfare participation. Following the economic intuition in Sections 2.2 and 2.3, individual effects enter into the equation via a sparse relationship to treatment, that is $\alpha_i = v_i$ for women in Jobs First and $\alpha_i = 0$ for those in AFDC. In order to incorporate this restriction in a penalized setting, I allow the Tikhonov regularization parameter, or tuning parameter, to be defined by treatment status, $\{\lambda_0, \lambda_1\}$. The regularization parameter shrinks the influence of individual effects toward zero as λ_d increases, so imposing the condition $\lambda_0 \gg \lambda_1$ would imply that participation costs are smaller in the control group than in the treatment group.[12] (The selection of the tuning parameters for the methods presented in this section is discussed in Appendix D.1.) Therefore, the restricted panel quantile regression estimator (R-PQR) at a given τ can be estimated by solving

$$\min_{(\beta_0, \Delta, \alpha') \in \mathbf{\Gamma}} \sum_{i=1}^{N} \sum_{t=1}^{T} \hat{w}_i(\mathbf{x}_i) \cdot \rho_\tau \left(Y_{it} - \beta_0 - \Delta D_{i0} - \alpha_i \right) + \lambda_0 \sum_{j \in \mathcal{D}_0} |\alpha_j| + \lambda_1 \sum_{k \in \mathcal{D}_1} |\alpha_k| \, . \tag{2.7}$$

The restricted estimator can be easily adapted to the simultaneous estimation of J conditional quantiles as in Koenker (2004), and it differs from existing penalized estimators for panel quantiles (for example, Harding and Lamarche, 2017) by controlling for selection on observables. Moreover, the proposed semiparametric estimator in equation (2.7) relaxes the identification conditions in the literature by partially allowing for selection on time-variant unobservables when $\lambda_0 = \lambda_1 = c$.

[12] Imai and Ratkovic (2013) use separate constraints in a least absolute shrinkage and selection operator (LASSO) framework in order to select pre-randomization variables that are causally related to heterogeneous treatment responses, though their study does not address quantiles nor the panel dimension.

More importantly, the restricted estimator offers a direct test of the economic model. It is designed to allow for program-specific costs of participation that are more relevant to Jobs First-assigned women than to AFDC-assigned women at conditionally higher earnings.

In theory, AFDC-assigned women at the upper quantiles of monthly income do not have costs of participating since they are out of welfare, while Jobs First-assigned women may have costs of participating since they are still welfare-eligible. Although in practice AFDC-assigned women were in general out of welfare several quarters after the reform, some of them were still in the program. Moreover, stigma effects can persist over time. Therefore, it is important to consider the case of $\lambda = \lambda_0 = \lambda_1$, which does not strictly impose the framework presented in Section 2.2, however it does shrink the smallest individual effects, presumably the $\hat{\alpha}_{i_0}$'s, to zero.

Let the unrestricted panel quantile regression (PQR) estimator of the QTE at a given τ be defined as

$$\min_{(\beta_0,\Delta,\alpha')\in\mathbf{\Gamma}} \sum_{i=1}^{N}\sum_{t=1}^{T} \hat{w}_i(\mathbf{x}_i)\cdot\rho_\tau\left(Y_{it}-\beta_0-\Delta D_{i0}-\alpha_i\right)+\lambda\sum_{i=1}^{N}\left|\alpha_i\right|, \tag{2.8}$$

where $\lambda \in \mathbb{R}_+$ is a tuning parameter. Notice that for identification of the QTE, the tuning parameter cannot be equal to zero. But for small values of λ, the QTE should be interpreted as an estimator from a model with individual effects. As the value of λ increases, the individual effects go to zero such that PQR estimates converge to QR estimates. If the restricted and unrestricted estimators give similar results, then the evidence supports a differential role of individual effects by treatment status, which corresponds to the suggested extensions to the economic framework in Section 2.2 and modeling assumptions above.

The economic model is nested such that comparisons for each of these estimators correspond directly to testable assumptions proposed earlier in this section. That is, if individual effects have no differential role in labor supply by treatment status, then the methodology for estimating the QTE parameter would be inconsequential whether using equations (2.5), (2.7), or (2.8).[13] Evidence for the proposed method is given in Section 2.5, and more details on the role of penalized individual effects and tuning parameter selection are given in Sections 2.6.2 and D.1.

2.4 Data

Experimental data for Connecticut's Jobs First waiver program were obtained from MDRC (formerly Manpower Demonstration Research Corporation). The observations represent 4803 women, current welfare recipients or new applicants, who were randomly assigned into either the AFDC control group ($N_0 = 2407$) or the Jobs First treatment group ($N_1 = 2396$). Along with a range of time-invariant demographics, the data include quarterly measures of earnings rounded to the nearest hundred dollars, and AFDC and Food Stamps benefits rounded to the nearest fifty. Income measures are observed for quarters -8 to -1 before random assignment, quarter 0 at the time of random assignment into treatment (D_{i0}), and quarters 1 to 16 where the Jobs First time limit binds by quarter 7.[14]

[13]Estimates using RPQR are equivalent to those of PQR in either case (i) $\lambda_0 = \lambda_1 = \lambda$, or (ii) the penalized individual effects for the treatment and control groups are no different when differentially determined by the data given $\lambda = \lambda^*$ compared to when estimated given $\lambda_0 \gg \lambda_1$. Further, RPQR estimates are equivalent to those of QR if (i) $\lambda_0 = \lambda_1 = \lambda \gg 0$ given that all individual effects are penalized to zero as $\lambda \to \infty$, or (ii) if there are no differential individual effects by treatment status for any value of λ.

[14]Earnings data are missing for 30 women in quarter 16; AFDC and Food Stamps data are missing for 3175 women in pre-treatment quarter -8. See Bloom et al. (2002) for a background of the Jobs First program including, for example, experimental design, program implementation, and initial outcomes.

Table 2.1. Descriptive Statistics by Treatment Status

	Levels		Differences	
Variables	Jobs First	AFDC	Unadjusted	Adjusted
Newhaven County (urban)	0.753	0.757	−0.004	−0.000
	(0.431)	(0.429)	(0.012)	(0.012)
Never married	0.624	0.631	−0.007	−0.000
	(0.484)	(0.483)	(0.014)	(0.016)
HS dropout	0.331	0.313	0.018	−0.000
	(0.471)	(0.464)	(0.013)	(0.013)
More than two children	0.227	0.206	0.021∗	−0.000
	(0.419)	(0.405)	(0.012)	(0.012)
Mother younger than 25	0.289	0.297	−0.007	−0.000
	(0.454)	(0.457)	(0.013)	(0.014)
Mother older than 34	0.301	0.286	0.015	0.000
	(0.459)	(0.452)	(0.013)	(0.014)
Recipient (stock) sample	0.624	0.593	0.031∗	−0.001
	(0.484)	(0.491)	(0.014)	(0.014)
Currently working $\geq$ 30 hours	0.276	0.313	−0.037	−0.033
	(0.447)	(0.464)	(0.029)	(0.027)
Hourly wage	6.583	6.808	−0.225	−0.164
	(2.234)	(2.592)	(0.155)	(0.153)
Public or subsidized housing	0.356	0.346	0.010	0.003
	(0.479)	(0.476)	(0.014)	(0.014)
Ever on AFDC as a child	0.248	0.258	−0.010	−0.010
	(0.432)	(0.438)	(0.013)	(0.013)
Ever received AFDC at prior quarter 7	0.548	0.528	0.020	−0.000
	(0.498)	(0.499)	(0.014)	(0.014)
Length in months of 1st AFDC spell	17.622	14.221	3.402∗	3.115∗
	(9.910)	(10.654)	(0.344)	(0.380)
Number of AFDC spells	1.173	1.217	−0.044∗	−0.046∗
	(0.583)	(0.685)	(0.018)	(0.018)
Long-term recipient (> 2 years)	0.569	0.554	0.015	−0.005
	(0.495)	(0.497)	(0.014)	(0.013)
		Pre-Treatment Quarters		
Average quarterly earnings	678.908	785.895	−106.988∗	−0.887
	(1303.749)	(1544.720)	(41.240)	(108.313)
Average quarterly cash welfare	890.818	835.112	55.706∗	−0.833
	(806.032)	(784.845)	(22.958)	(23.029)
Fraction of quarters with earnings	0.322	0.351	−0.029∗	0.000
	(0.363)	(0.372)	(0.011)	(0.011)
Fraction of quarters with cash welfare	0.573	0.544	0.029∗	−0.001
	(0.452)	(0.450)	(0.013)	(0.013)
		Experimental Quarters 1-7		
Average quarterly earnings	1173.187	1139.047	34.141	81.931
	(1501.393)	(1739.033)	(46.875)	(123.695)
Average quarterly cash welfare	1083.255	889.050	194.205∗	167.264∗
	(620.003)	(639.856)	(18.180)	(20.162)
Fraction of quarters with earnings	0.514	0.450	0.064∗	0.077∗
	(0.394)	(0.398)	(0.011)	(0.012)
Fraction of quarters with cash welfare	0.746	0.662	0.084∗	0.071∗
	(0.345)	(0.380)	(0.010)	(0.011)

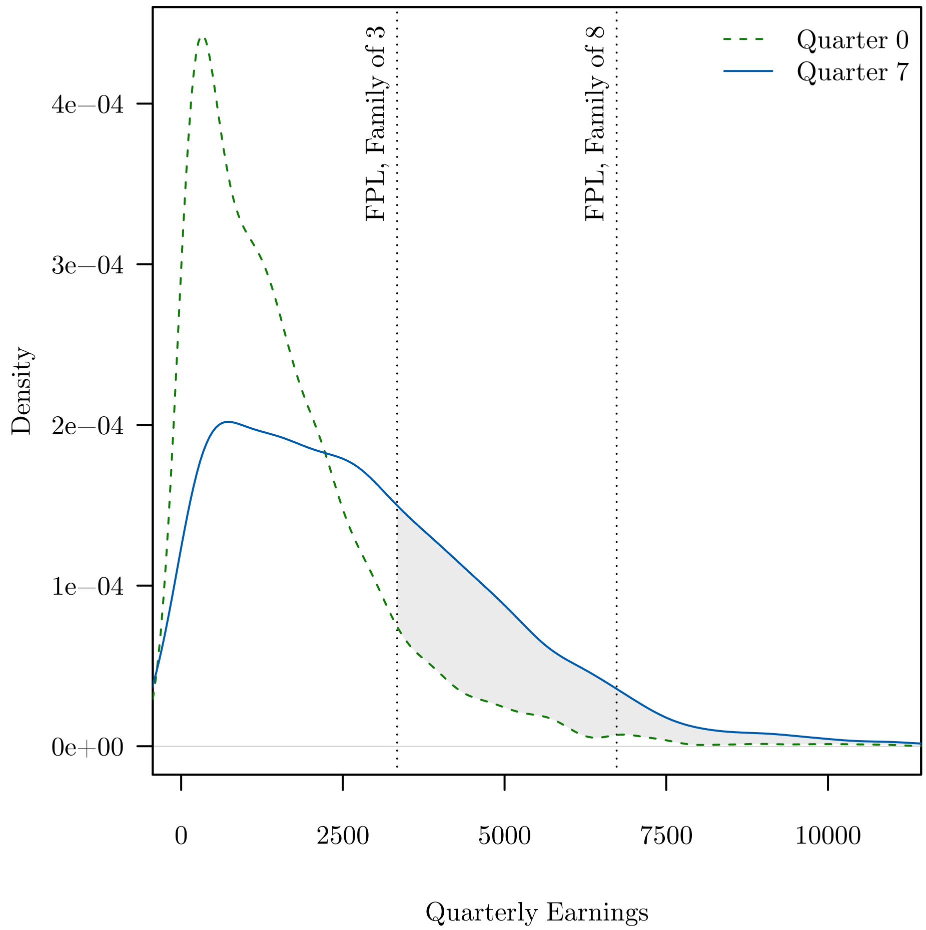
Notes: Standard deviations are shown in parentheses, and * denotes statistically significant differences at the 10-percent level.

Although Jobs First was a randomized experiment, a simple inspection of basic statistics reveals the presence of statistically significant differences by treatment status (Table 2.1). For instance, women in the Jobs First group have on average less quarterly earnings and more cash welfare. This however has been well documented in the literature. To address sample selection, Bitler et al. construct inverse-propensity weights by estimating the probability of treatment conditional on 60 variables including quarterly pre-treatment earnings and transfers as well as indicators for individual characteristics and family structure at the time of random assignment.[15] After estimating each individual's propensity to be treated, $\hat{p}_i$, the inverse-propensity weight is constructed as $\hat{w}_i = D_{i0}/\hat{p}_i + (1 - D_{i0})/(1 - \hat{p}_i)$ where, as before, D_{i0} indicates treatment status. For consistent comparison of results, I employ the same weights in the estimators defined in equations (2.4), (2.5), (2.7) and (2.8).

The descriptive statistics in Table 2.1 highlight the role of sample correction as well as some general outcomes of the treatment. First, note that randomization works well overall based on the unadjusted differences shown in the third column. The differences that remain statistically significant, though, may be important for estimating treatment effects on earnings. In addition to Jobs First participants having less earnings and more cash transfers on average during pre-treatment quarters, they also tend to have larger families and are more likely to be continuing recipients instead of new applicants. Another distinction between Jobs First and AFDC participants is that the Jobs First participants have longer first spells on welfare yet slightly fewer spells, on average. The fourth column

[15] The covariates are quarterly levels of pre-treatment earnings, cash transfers, and Food Stamps; quarterly indicators for any pre-treatment earnings, cash transfers, and Food Stamps; indicators for new applicant status at randomization, any employment in the year before randomization, any cash transfers in the year before randomization; indicators for black, Hispanic, white, never married, married/living apart, age less than 25, age 25-34, no high school degree or GED, high school degree or GED, more than two children; and, indicators for any missing data for education, children, and marital status.

Figure 2.2. Empirical Distributions of Earnings at Quarters 0 and 7



Notes: FPL denotes the federal poverty line as of 1997, the midpoint between random assignment and quarter 7.

in Table 2.1 shows sample differences adjusted by inverse-propensity weighting. For the variables adequately controlled for in the first-stage propensity estimation, sample-corrected characteristics are well balanced: only very small and statistically insignificant differences remain in the adjusted variables for the pre-treatment period. However, for the exceptions of number and duration of previous spells, there are still statistically significant differences at the 10-percent level.

Of the 4803 total participants, 2923 women are continuing welfare recipients compared to 1880 who are new applicants. When the Jobs First experiment began in 1996, the entire state of Connecticut transitioned to Jobs First except for the two experimental counties, Newhaven and Manchester. Therefore, any new applicants would have different selection into welfare than continuing recipients. The same descriptive statistics discussed above are shown by participant type for recipients and applicants in Appendix Table C.1. As expected, applicants are different from recipients by every dimension shown in the table. In summary, applicants are less urban, more educated, less reliant on transfers, and they work more hours at higher wages. In particular, only about 20 percent of applicants had ever received AFDC around 2 years prior to random assignment compared to just over 75 percent of recipients, and the probability of being a long-term welfare recipient is about 43 percentage points lower for the applicant group on average. Applicants also earn more and receive less transfers than recipients both before and after random assignment. While applicants and recipients may experience some similar costs of welfare participation in terms of hassle, the ongoing recipients group may face additional costs related to persistent labor force detachment.

Lastly, Figure 2.2 shows the empirical distribution of earnings for working women at random assignment (quarter 0) and at the Jobs First time limit (quarter 7). The figure also shows the federal poverty line (FPL), which varies by family size. In the Jobs First data, the modal family size is approximately 3 members with the maximum size around 8.[16] The figure highlights how the probability that families earn more than the federal poverty line changes over time, which is shown by the area shaded in gray. Referring

[16]Administrative data on family size is available for only 225 individuals in both quarter 0 and quarter 7. Otherwise, family structure is identified by the variable *kidcount*, which is top-coded at 3.

to Figure 2.1, the likelihood of locating around points E or H (locations at or above the federal poverty line) is small at the time of random assignment, but some participants will relocate to those higher earnings locations by quarter 7. The increase in individuals relocating near the poverty line, as shown in Figure 2.2, motivates the presumption that treatment effects at upper quantiles are related to potential behavioral responses around the eligibility notch.

2.5 Empirical Analysis

In this section, I employ the proposed estimation methods to investigate whether there is evidence that suggests that individuals reduce hours in order to opt into welfare, the behavioral-induced participation hypothesis. I compare findings with results obtained by employing alternative estimation methods. Finally, I formulate a series of tests to examine whether the new results offered in this study are significantly different than existing results. Standard errors for all empirical results are constructed based on a block bootstrap method for comparability across estimators; for details, see Appendix Section D.2.

2.5.1 Pooled Data Results

As a baseline estimate of the QTE, I present results based on the non-parametric approach given by equation (2.4) and the semiparametric approach introduced in equation (2.5). I restrict attention to earnings in the first 7 quarters after the reform is introduced in order to focus on behavioral responses in the upper tail of the earnings distribution before the Jobs First time limit becomes binding. If behavioral-induced participation is expected, it would be most evident before time limits apply to women assigned to Jobs First.

Estimates are also provided for total income (earnings, cash welfare, and Food Stamps) for quarters 8-16, which represent a long-run outcome.

Table 2.2 presents results for the QTE parameter given by the non-parametric estimator where estimates obtained with inverse-propensity weighting are shown in column (1) and estimates without weights in column (2). I reproduce all of the NP estimates exactly with only slight variations in the confidence intervals based on different random samples used for the 1000 bootstrap replications.[17] Given the random design of Jobs First, which remains a model program for welfare reform evaluation, one might expect the unweighted and weighted QTE results to be similar. In fact, there is no qualitative difference and only small quantitative differences in the point estimates presented in columns (1) and (2). For the results shown, the only difference from weighting the NP estimates is at the 0.75 quantile for total income in quarters 8-16: 300 in column (1) and 250 in column (2), though this difference is not statistically significant at conventional levels.[18]

Consistent with the predictions of the framework described in Figure 2.1 (panel A), the table shows that the reform had no impact at the lower tail of the conditional earnings distribution and it increased earnings at the 0.50 and 0.75 quantiles. At the upper tail, NP estimates indicate that the reform reduces earnings by 200 dollars, suggesting that some women reduced hours in order to opt for welfare.

Under the assumption that the treatment effect is linear and treatment status is randomly assigned, the nonparametric estimator for the QTE and the quantile regression

[17] In order to reproduce the confidence intervals reported in Bitler et al. (2006), it is necessary to use only bootstrap samples that are sufficiently balanced given that their software, which is available through the AER website, weights the empirical cumulative distributions of each treatment group across the full sample. This procedure causes the inference on the nonparametric approach to appear artificially more precise than otherwise with respect to the semiparametric quantile regression methods.

[18] Comparing weighted and unweighted QTEs for several quantiles between 0.05 and 0.95 (in results not shown here but available upon request), I find that there is no qualitative difference by weighting, and little quantitative difference.

Table 2.2. Quantile Treatment Effects on the Distributions of Earnings and Total Income

	NP		QR		R-PQR		PQR	
τ	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Earnings, Quarters 1-7				
0.10	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
0.25	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
0.50	100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00
	(31.92)	(30.92)	(32.88)	(28.30)	(52.64)	(52.64)	(52.66)	(52.66)
0.75	300.00	300.00	300.00	100.00	500.00	600.00	400.00	500.00
	(94.59)	(129.66)	(93.61)	(100.58)	(122.58)	(108.90)	(123.67)	(115.44)
0.90	-200.00	-200.00	-200.00	-300.00	300.00	400.00	300.00	200.00
	(117.68)	(217.92)	(119.89)	(128.17)	(110.08)	(120.12)	(110.49)	(107.01)
				Total Income, Quarters 8-16				
0.10	0.00	0.00	0.00	0.00	-600.00	-650.00	-350.00	-400.00
	(0.00)	(0.00)	(0.00)	(0.00)	(91.55)	(89.92)	(101.78)	(89.66)
0.25	-150.00	-150.00	-150.00	-150.00	-50.00	0.00	-50.00	-150.00
	(109.66)	(123.39)	(108.96)	(118.75)	(60.79)	(74.86)	(60.57)	(61.89)
0.50	50.00	50.00	50.00	50.00	50.00	50.00	50.00	50.00
	(64.62)	(74.46)	(64.08)	(67.64)	(75.60)	(75.60)	(75.61)	(75.62)
0.75	300.00	250.00	300.00	200.00	227.45	129.16	228.00	250.00
	(90.67)	(125.68)	(90.68)	(100.02)	(81.92)	(75.26)	(80.88)	(81.20)
0.90	0.00	0.00	0.00	0.00	474.17	573.99	300.00	328.00
	(118.30)	(213.37)	(118.33)	(120.37)	(99.22)	(99.06)	(87.10)	(87.35)
IPW	Yes	No	Yes	No	Yes	No	Yes	No

Notes: NP denotes the non-parametric quantile estimator, QR denotes the semiparametric quantile regression estimator, R-PQR denotes the restricted panel quantile regression estimator, and PQR denotes the unrestricted panel quantile regression estimator. Bootstrap standard errors are shown in parentheses based on 1000 replications. IPW denotes inverse-propensity weighting.

estimator for a model that conditions on the treatment indicator variable are expected to yield similar results (Koenker, 2005). The weighted and unweighted QR results are shown in columns (3) and (4) of Table 2.2. Although Table 2.2 shows some differences between NP and the QR estimates in column (4), a closer examination of the estimated effects across the 0.05 quantile through the 0.95 quantile reveals that the QTE estimates obtained using QR are similar to the NP estimates.[19] Therefore, the NP results appear to

[19] In estimates not shown here, the NP and QR are statistically significantly different (at the 10-percent level) at 6 quantiles in the interval $\{0.05, 0.06, \ldots, 0.95\}$, which includes the 0.75 quantile shown in Table

be robust to the use of weights and an alternative parametric specification for estimating the QTE. It is important to emphasize that this additional empirical evidence continues to indicate that there is substantial heterogeneity predicted by labor supply theory and low-income women can increase income by reducing hours and claiming welfare, which is consistent so far with the behavioral-induced participation hypothesis.

2.5.2 Panel Data Results

In the last columns of Table 2.2, I present panel quantile regression estimates for both the restricted and unrestricted cases. For the restricted estimator, I let $\lambda_1 = 0.01$ for Jobs First participants and $\lambda_0 = 1$ for AFDC.[20] The weighted R-PQR estimates are shown in column (5) and unweighted estimates in column (6). Despite controlling for women's heterogeneity by treatment status, the R-PQR estimator delivers results that are similar to those of NP and QR at the center of the distribution. In contrast, I observe large differences at the upper quantiles of the conditional distribution of earnings. For the unrestricted case, weighted and unweighted PQR estimates are shown in columns (7) and (8), respectively. In these cases, $\hat{\lambda}$ is estimated to be approximately 0.718 for earnings and 0.673 for total income.[21] I note that the R-PQR and PQR estimates are qualitatively similar. Restricting the degree of shrinkage for individual effects by treatment status imposes no difference at the median, though the unrestricted estimates are somewhat smaller at upper quantiles. Unrestricted penalized estimates, therefore, offer a more conservative contrast to pooled estimates. However, large differences between pooled and

2.2 but not the 0.90 quantile. For weighted estimates, the NP and QR are statistically significantly different at 7 quantiles.

[20] The values of λ here are chosen to illustrate the economic model. See Appendix Section D.1 in Appendix D for details on tuning parameter selection.

[21] See Appendix D for tuning parameter estimation details, and Section 2.6.26.2 for robustness evidence for the tuning parameter selection.

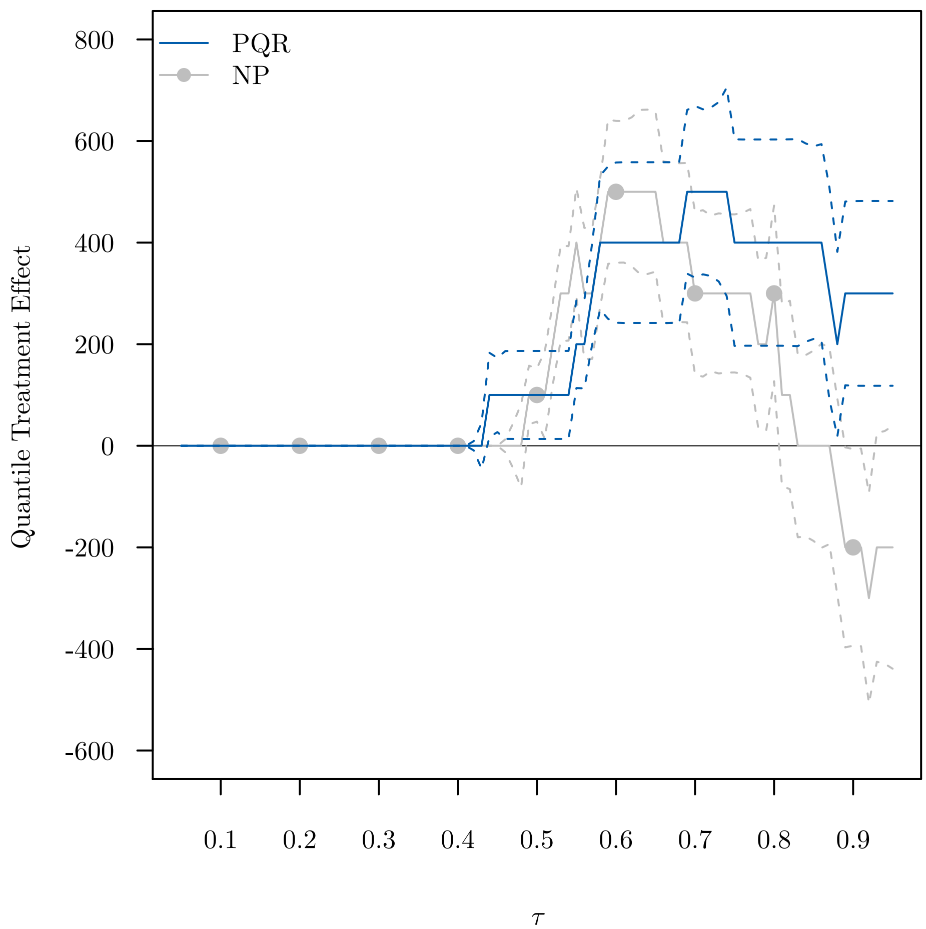
panel results in the upper earnings distribution are robust to modeling assumptions on differential shrinkage for individual effects by treatment status. Thus, given the more conservative, yet similar results under weaker assumptions, the unrestricted PQR estimates are preferred as the main results for comparison to the pooled results.

Figure 2.3 compares weighted estimates by NP and PQR for quantiles in the interval from 0.05 to 0.95. The series of estimates appear to be equivalent through the 0.65 quantile after which they tend to diverge at upper quantiles. The reduction of earnings in the upper tail of the distribution was predicted as a natural consequence of behavioral-induced participation attributed to a reduction of exits from welfare. However, when I control for latent individual heterogeneity, the negative treatment effect disappears. Looking at the 0.90 quantile of earnings, for example, there is a weighted NP estimate of -200 dollars compared to a PQR estimate of 300 dollars. As opposed to seeing a negative effect in the upper tail of the earnings distribution, the estimated treatment effect continues to be positive and statistically significantly different from zero. Although it is naturally challenging to explain the mechanism behind these differences from the reduced form coefficients, the evidence is consistent with the framework developed in the previous sections, which points to the fact that welfare participation costs can have a differential effect at the upper tail of the conditional distribution of earnings.

2.5.3 Welfare Participation Effect on Labor Supply

The individual effect at the 0.90 quantile, $\alpha_i(0.90)$, is intended to capture individual-specific sources of variability, or unobserved heterogeneity that was not adequately controlled for by other covariates. If these latent factors do not affect earnings or are independent of the treatment variable D_{i0}, the proposed panel approach is expected to produce

Figure 2.3. Quantile Treatment Effects on the Distribution of Earnings, Quarters 1-7



Notes: PQR denotes panel quantile regression estimates and NP denotes non-parametric quantile estimates. The dashed lines represent 90-percent confidence intervals obtained by 1000 bootstrap replications.

similar findings to other methods. This is not what is evident in Figure 2.3. I interpret the differences between nonparametric estimates and semiparametric panel estimates as suggesting that participation costs of welfare affects Jobs First and AFDC participants differentially at the upper tail. This is consistent with the economic implications discussed in Section 2.2 since it is expected that high earners who were assigned to AFDC do not participate on welfare and high-earners who were selected to Jobs First do participate. Moreover, it is natural to assume that these women make choices regarding work and welfare based on their opportunity cost of time that depends on family structure, preferences, and program features given treatment status. If there is a nonlinear cost of participation across the distribution of earnings, then controlling for program participation in the pooled model may offer a simple check for the interpretation of labor supply differences that are explained by latent characteristics related to program features.

The experimental data for Connecticut's Jobs First waiver program allow us to run a simple, yet important, robustness check. The data have information on whether the individual was receiving cash welfare or Food Stamps at each quarter.[22] Then, if there are latent costs in terms of participation, one can introduce an indicator variable for welfare participation in order to capture the potential omitted variable in the cross-sectional quantile model. I expect, however, small differences in the panel results (PQR) since the method is designed to account for these sources of variability and participation is roughly constant over time.

Let $R_{it} = 1$ if individual i receives either cash welfare or Food Stamps in quarter t, and $R_{it} = 0$ otherwise. In results shown in Figure 2.4, I estimate the QTE by quantile

[22]For Jobs First, 67.6 percent of women's participation status do not change over quarters 1-7, and for AFDC women, 63.1 percent do not change. The standard deviation for an indicator of participation over this time period is 0.176 in Jobs First and 0.193 in AFDC.

regression by solving

$$\min_{(\beta',\Delta)\in\mathbf{\Theta}} \sum_{i=1}^{N}\sum_{t=1}^{T} \hat{w}_i\left(\mathbf{x}_i\right)\cdot\rho_\tau\left(Y_{it}-\beta_0-\beta_1 R_{it}-\Delta D_{i0}\right),$$

where $\beta=(\beta_0,\beta_1)'$ and by panel quantile regression by solving

$$\min_{(\beta',\Delta,\alpha')\in\mathbf{\Gamma}} \sum_{i=1}^{N}\sum_{t=1}^{T} \hat{w}_i(\mathbf{x}_i)\cdot\rho_\tau\left(Y_{it}-\beta_0-\beta_1 R_{it}-\Delta D_{i0}-\alpha_i\right)+\hat{\lambda}\sum_{i=1}^{N}\left|\alpha_i\right|,$$

where $w_i(\mathbf{x}_i)$ and λ are estimated as before. Recall that the cross-sectional methods QR and NP offer, as expected, similar point estimates (Table 2.2). Moreover, I found that while the NP (and thus QR) point estimate is equal to -200 dollars at the 0.90 quantile of earnings, PQR suggests a positive treatment effect of 300 dollars (Figure 2.3). It is very interesting to see now that the cross-sectional estimates and panel estimates are roughly equivalent when controlling for participation as in Figure 2.7, suggesting that QR, NP and PQR do not offer significantly different results.[23] Also, as expected, the PQR results are robust to the inclusion of a woman's welfare participation status.

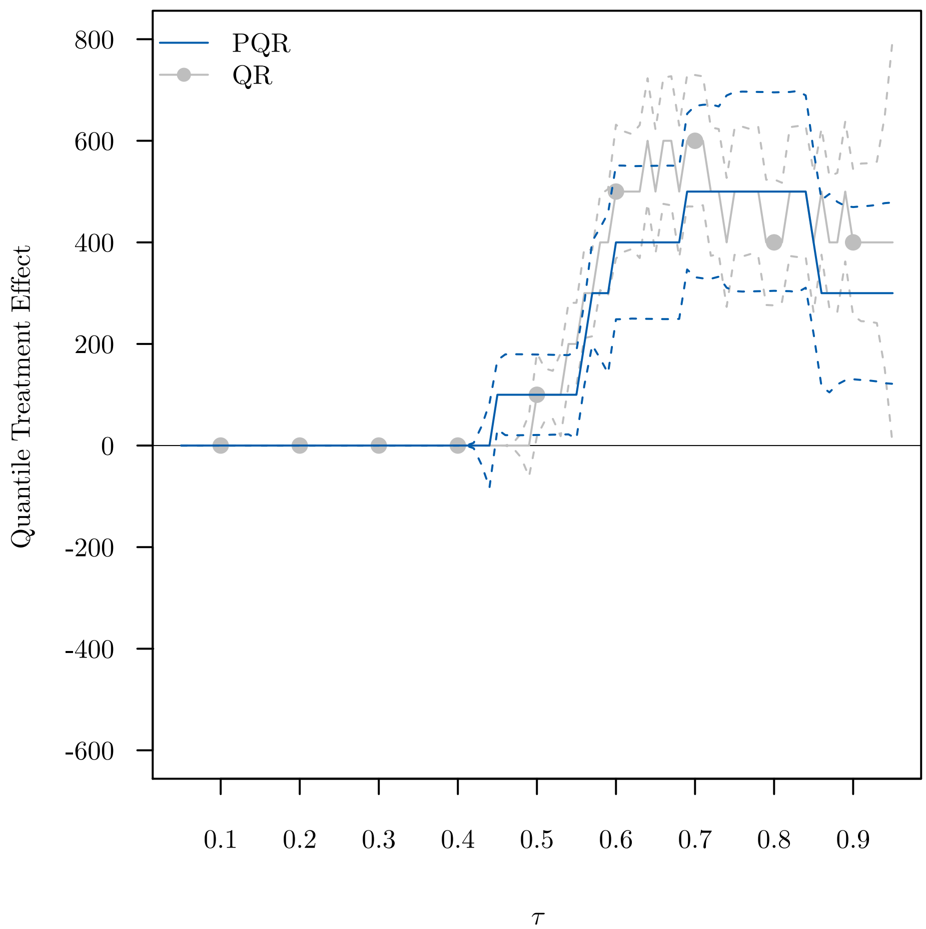
2.5.4 Quantile Treatment Effects by Participant Type

The evidence so far suggests that individuals may have differential participation costs by treatment status and that these unobserved costs are increasing in work hours. However, approximately three-fifths of the experimental sample are continuing welfare recipients, whereas the remaining two-fifths of the sample are new applicants. While both participant types would face welfare participation costs such as paperwork and standing in lines, longer-term costs may differ between applicants and recipients.[24] For instance, long-term recipients may have higher informational costs of managing work, child care, and

[23]The findings of this robustness check are not sensitive to the definition of R_{it}. Results are qualitatively similar for participation defined by cash transfers only, or by Food Stamps only.

[24]Blank and Ruggles (1996) differentiate between participation decisions for women who are persistently eligible versus those who may just qualify for eligibility for a short time.

Figure 2.4. Quantile Treatment Effects on the Distribution of Earnings Conditional on Welfare Participation, Quarters 1-7



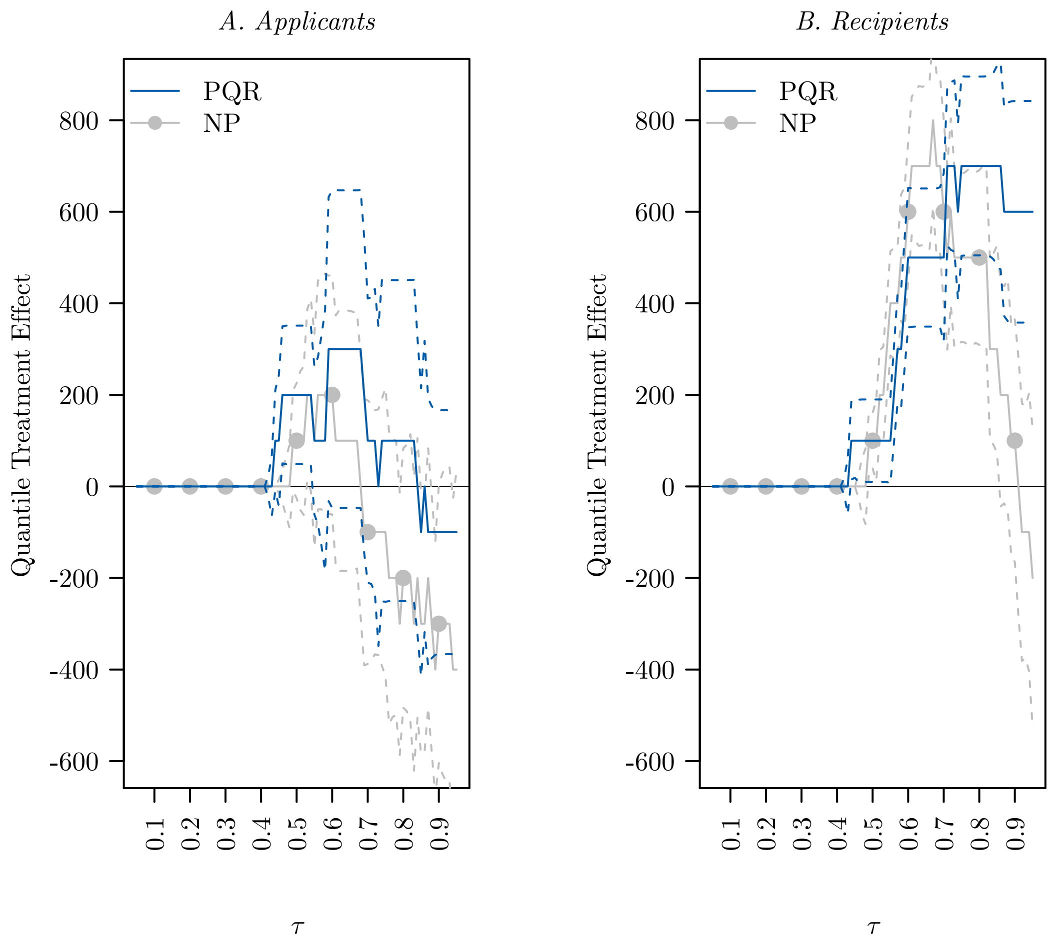
Notes: PQR denotes panel quantile regression estimates and QR denotes quantile regression. The dashed lines represent 90-percent confidence intervals obtained by 1000 bootstrap replications.

welfare participation because of limited labor market experience. Also, persistent stigma related to long-term welfare participation may affect individuals' beliefs about market productivity.

As noted above, descriptive statistics shown in Appendix Table C.1 demonstrate significant differences between samples by participant type. Ongoing recipients are characterized by longer and more frequent welfare spells, as well as higher dependence on public housing and less experience in the labor market. Recipients have less labor force attachment and thus may be affected differentially by informational costs or stigma. Newer applicants, however, are more likely to have higher education and be working more hours at higher wages. The costs of participation for new applicants may be more transitory in nature given that individuals with temporary shocks and better earnings potential may select into welfare under Jobs First based on the generous disregards near the federal poverty line. If there is evidence supporting the behavioral-induced participation hypothesis, it should be related to behavioral responses among applicants as opposed to recipients.

Figure 2.5 shows the QTE by participant type. In panel A, there is still weak evidence of a negative treatment effect in the upper quantiles for pooled estimates, but the panel estimates are statistically no different from zero throughout nearly the entire distribution of earnings (except at the median). Just as before, there is no difference based on individual effects through the middle of the distribution, and now there is only a small and statistically insignificant difference at the 0.90 quantile. The implication is that participation costs might not play as important of a role for the applicant group as they do for the full sample. Panel B, however, exhibits large differences in treatment effects

Figure 2.5. Quantile Treatment Effects on the Distribution of Earnings by Participant Type, Quarters 1-7



Notes: PQR denotes panel quantile regression estimates and NP denotes non-parametric quantile estimates. The dashed lines represent 90-percent confidence intervals obtained by 1000 bootstrap replications.

at the upper quantiles of the distribution of earnings for recipients, though there is no longer a negative treatment effect for the pooled estimates at the 0.90 quantile. On the other hand, PQR estimates at the upper tail continue to be positive and significant. This evidence suggests that controlling for individual costs of participation matters less for applicants with treatment affects attenuated toward zero and matters more for recipients where the treatment effect is increasing and positive at the upper conditional quantiles.

2.5.5 Characterizing Participation Costs using Experimental Data

The experimental research design allows us to test whether relative participation costs are heterogeneous across the conditional distribution of earnings. A test on the difference between NP and QR is interpreted as a test on the adequacy of the linear parametrization of the model. More importantly, a test on the difference between NP and PQR can be interpreted within the framework discussed in Sections 2.2 and 2.3. If participation costs are zero or do not depend on treatment status, then the parameter $C(\tau)$ in equation (2.3) is zero. The framework suggests that $C(0.5) \approx 0$, while $C(0.9) > 0$.

Table 2.3 shows estimator differences at the 0.50 and 0.90 quantiles, with the associated p-values of Hausman-type test statistics as described in Section D.3 (Appendix B). I also show the percentage difference in terms of quarterly earnings and an estimated cost in terms of number of hours per week. As expected, there are no significant differences between the weighted estimates for NP and QR, as shown in column (1). When regarding weighted estimates by NP and PQR, shown in column (2), I find that there is no statistically significant difference at the 0.50 quantile, and a significant absolute difference of 500 dollars of quarterly earnings at the 0.90 quantile. Columns (3) and (4) show absolute differences between weighted NP and PQR estimates by participant type for applicants

Table 2.3. Estimator Differences for Quantile Treatment Effects
on the Distribution of Earnings, Quarters 1-7

		Full Sample		Applicants	Recipients
		QR−NP	PQR−NP	PQR−NP	PQR−NP
Quantile τ	Statistic	(1)	(2)	(3)	(4)
0.50	Difference	0.00	0.00	100.00	0.00
	p-value	[1.00]	[1.00]	[0.15]	[1.00]
	Percentage Difference	0%	0%	33%	0%
	Hours per week ($6 wage)	0.00	0.00	1.28	0.00
0.90	Difference	0.00	500.00	200.00	500.00
	p-value	[1.00]	[0.00]	[0.34]	[0.01]
	Percentage Difference	0%	10%	4%	11%
	Hours per week ($10 wage)	0.00	3.83	1.53	3.83

Notes: NP denotes the non-parametric quantile estimator, QR denotes quantile regression, and PQR denotes panel quantile regression. The p-values shown in brackets are based on 1000 bootstrap replications.

and recipients, respectively. For applicants, I fail to reject the null of exogeneity at standard levels, yet for recipients I reject it at the 0.90 quantile at the 5-percent level with differences similar in magnitude to the full sample estimates. These findings suggest that Jobs First imposes an estimated cost of 10% of quarterly earnings, and it is larger for recipients than for applicants at high conditional quantiles.[25] In terms of labor supply, the cost of participating in welfare under Jobs First is equivalent to 3.8 less hours per week at the 0.90 quantile of earnings, which is slightly smaller than the estimated AFDC participation cost of about 4 hours per week found by Moffitt (1983, pg. 331).

2.6 Discussion

Although QTE estimates for Jobs First are robust to selection on observables, the panel estimates indicate that the QTE is influenced by unobserved characteristics differentially at the upper conditional quantiles of the earnings distribution. As a possible explanation, I have suggested that women experience nonlinear costs of welfare participation that are

[25]Although the percentage difference for applicants at median earnings is 33%, the difference is statistically insignificant and the magnitude of the percentage is driven by a small denominator of median quarterly earnings.

increasing with hours worked, which may only be relevant to Jobs First participants who are still eligible at higher earnings. Referring back to the stylized budget constraints in Figure 2.1, a woman's unobserved characteristics might be interpreted as capturing different costs and benefits for welfare participation when labor supply is high. *Ex ante*, labor supply predictions for high earners may be ambiguous conditional on costs of participation given that welfare participants are increasing hours and earnings over time (as documented in Figure 2.2) while also being exposed to treatment over time. Based on the QTE estimate differences shown in the previous section, individual heterogeneity plays a prominent role in behavioral responses to Jobs First.

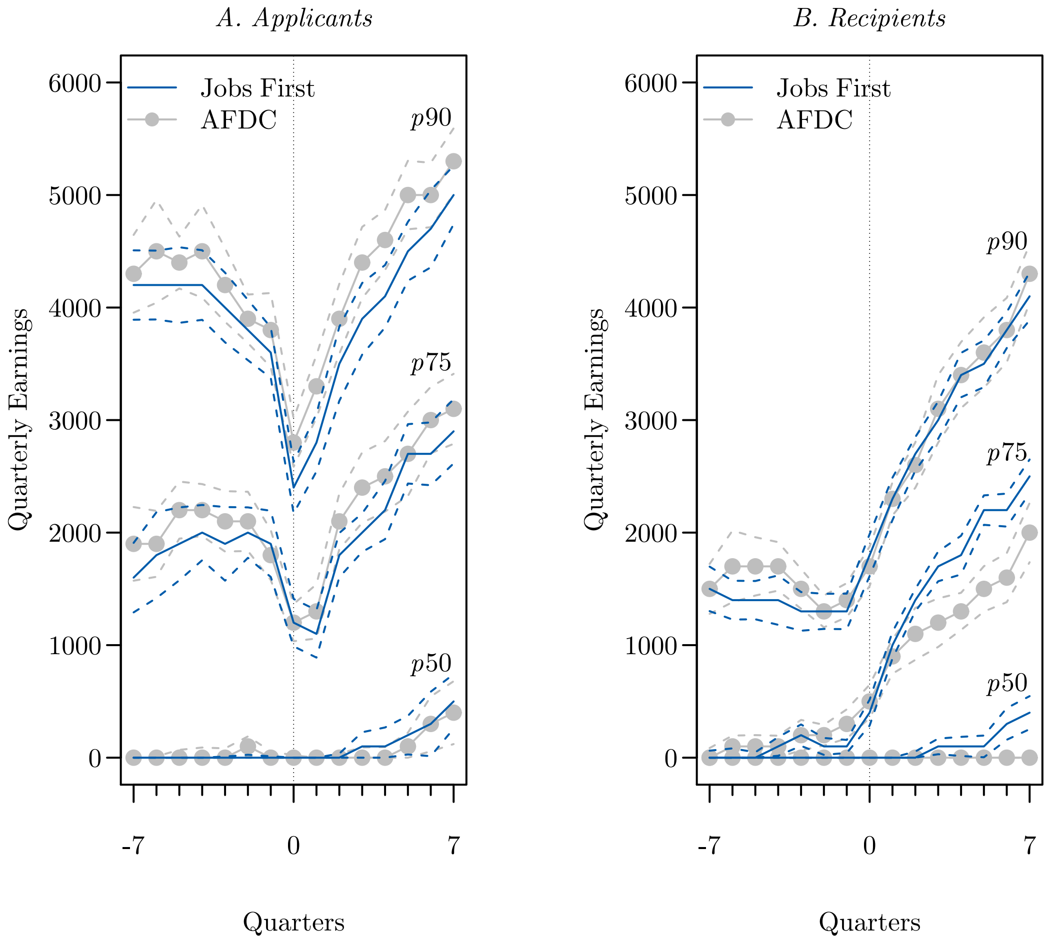
In what follows, this section explores the plausibility of the panel interpretation along with alternative explanations for finding different results only in the upper tail of the earnings distribution.

2.6.1 The Pre-Treatment Period and Randomization

An alternative explanation for finding differences when controlling for individual effects is that randomization may have successfully balanced experimental samples based on observed characteristics though not for unobserved characteristics. To investigate this possibility, it may be helpful to consider earnings trajectories by treatment status before and after randomization, as shown in Figure 2.6.[26] Pre-treatment earnings are mostly censored at the median where as many as 70 percent of the full sample had no earnings 2 years before random assignment. For applicants shown in panel A, the 75th and 90th percentiles of pre-treatment earnings exhibit a pronounced dip, which is perhaps not

[26] In the full sample, there are statistically significant differences in earnings by treatment group one year before random assignment, which suggests that differences in pre-treatment shocks may be related to subsequent earnings processes. Meghir and Pistaferri (2004) discuss the importance of unobserved heterogeneity for the variance of earnings related to transitory and permanent income shocks.

Figure 2.6. Earnings Trajectories by Participant Type
and Treatment Group, Quarters -7 to 7



Notes: The 50th, 75th, and 95th percentiles of earnings are shown for Jobs First and AFDC before and after random assignment, which is indicated by the dotted line at quarter 0. The dashed lines represent 90-percent confidence intervals obtained by 1000 bootstrap replications.

surprising in light of the work of Ashenfelter (1978, 1983). However, for recipients shown in panel B, pre-treatment earnings are relatively flat with no evidence that might suggest induced participation.

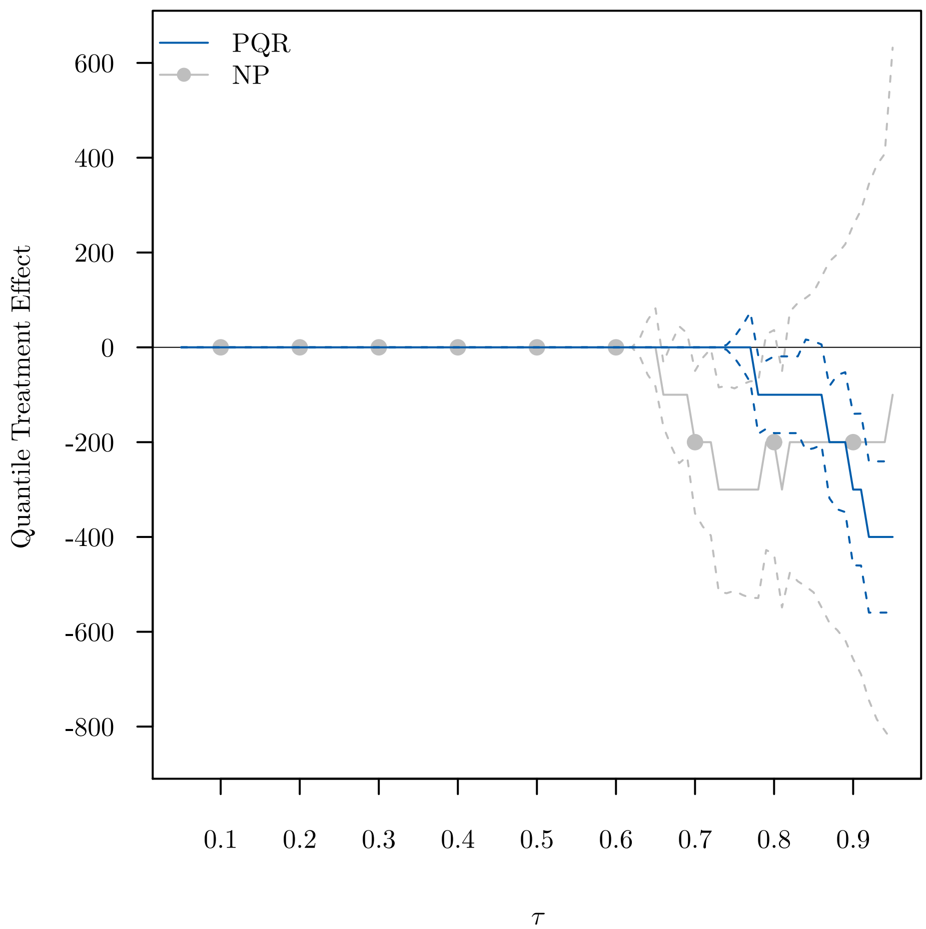
If there are important pre-treatment differences in experimental groups, then estimates of the effect of Jobs First on earnings before random assignment may be revealing. Naturally, one would expect zero treatment effect before randomization since the significant earnings disregard had not been implemented yet. Figure 2.7 shows the estimates for the parameter of interest obtained using NP and PQR for pre-treatment earnings, quarters -8 to -1.[27] Through the middle of the pre-treatment earnings distribution, the estimated effects are zero due to the large number of censored observations for earnings before random assignment, thus no information is conveyed about randomization except at the upper quantiles. At the 0.75 quantile of pre-treatment earnings, estimates by the NP estimator and PQR estimator differ by 300 [0.02] while the estimates at the 0.90 quantile only differ by 100 [0.71], with p-values shown in brackets. These results suggest that NP and PQR estimates are similar in the pre-treatment period and the direction of effects do not diverge as shown before in Figure 2.3.

2.6.2 On the Plausibility of the Sparse Model

If participation costs, or any other source of latent heterogeneity, are negligible at the upper tail of the earnings distribution, the nonparametric estimator and semiparametric estimator are not expected to produce different results. On the other hand, if costs associated with welfare participation are important in the economic model, one would expect different results because the identifying assumption on observables does not hold against the data, or alternatively, there are non-zero latent costs associated with participation for women who did not exit welfare in the period after the reform. The proposed methodology offers the possibility of investigating these conditions by changing the parameter λ. As the regularization parameter increases, the estimated individual effects $\hat{\alpha}_i(\tau, \lambda)$'s tend to zero, and thus, the PQR estimator converges to QR and NP. At the same time, unobserved heterogeneity should not affect the QTE estimates for small values of λ by

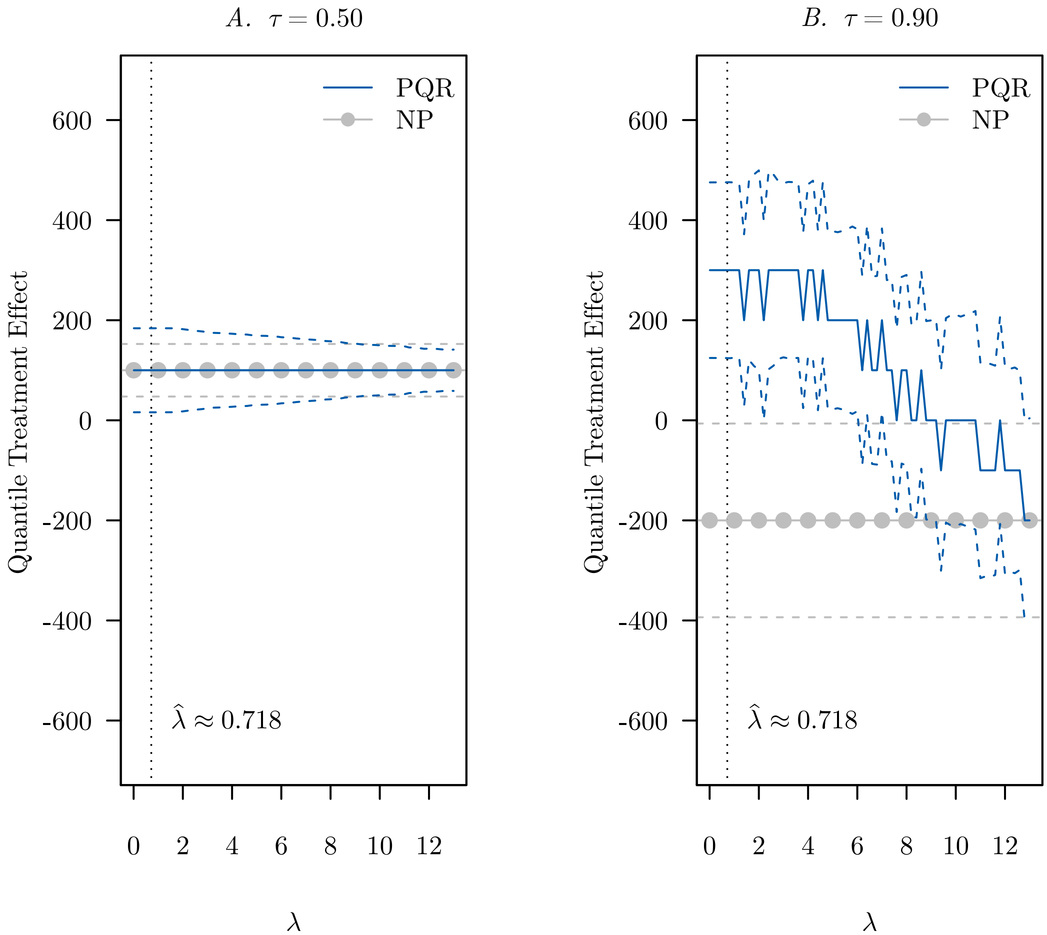
[27] The pre-treatment PQR results are based on an estimate of λ equal to 0.716 and pre-treatment earnings are not weighted since predetermined earnings and transfer data are not available.

Figure 2.7. Quantile Treatment Effects on the Distribution of Earnings, Quarters -8 to -1 (Pre-Reform)



Notes: PQR denotes panel quantile regression estimates and NP denotes non-parametric quantile estimates. The dashed lines represent 90-percent confidence intervals obtained by 1000 bootstrap replications.

Figure 2.8. Quantile Treatment Effects on the Distribution of Earnings as a Function of λ at the 0.50 and 0.90 Quantiles, Quarters 1-7



Notes: PQR denotes panel quantile regression estimates and NP denotes non-parametric quantile estimates. The dashed lines represent 90-percent confidence intervals obtained by 1000 bootstrap replications.

the experimental research design of the program. Recall that costs of participation are assumed to be nonlinear and should be more pronounced at the upper tail of the earnings distribution. Then, one could expect (i) no differences between the PQR and NP estimates at the 0.50 quantile for all values of λ, and (ii) significant differences between PQR and NP at the upper tail for those values of λ that are not sufficiently large enough to shrink the individual effect for all i to zero.

Figure 2.8 shows QTE results for the 0.50 quantile and 0.90 quantile as a function of λ over the interval $(0, 13]$. It also shows the estimated value of λ which is equal to 0.718 for earnings in quarters 1-7. While panel A shows results for the median, panel B shows results for the 0.90 quantile. As expected, panel A demonstrates the case where PQR and NP give similar results. There are no differences between nonparametric estimates and semiparametric panel estimates for all λ, even for $\lambda \to 0$ consistent with a "fixed" effects version of the estimator. On the other hand, panel B shows that PQR estimates at the 0.90 quantile can differ substantially from NP estimates. A value of λ less than 4 is consistent with the previous findings since PQR is roughly constant around 300 dollars.

2.6.3 Censored Earnings

When estimating labor supply responses to welfare reform, the considerable amount of censoring at zero earnings may be concerning. To characterize the extent of censoring in the Jobs First data, 70 percent of women had zero earnings two years before random assignment and the average number of censored observations is just above 50 percent for quarters 1-7. Censoring therefore might be expected to affect the shape of the QTE estimates over quantiles τ. I briefly investigated the robustness of the findings to addressing censored observations for earnings and found that censoring is not likely to be driving the empirical results. Censored quantile regression following the methods proposed by Powell (1986) and Fitzenberger (1997), as well as Chernozhukov and Hong (2002), produce similar estimates as QR at the 0.50 quantile and exactly the same estimates at the 0.75 and 0.90 quantiles of the conditional distribution of earnings in quarters 1-7. I argue that the similarity of these results is partially explained by the random assignment of the treatment variable leading to a similar proportion of censored observations by treatment status (49 percent among Jobs First-assigned women and 55 percent among AFDC-assigned women).

2.6.4 Policy Relevance of Participation Costs

Despite the importance of potential work disincentives of welfare generosity, there has been little empirical evidence illustrating behavioral-induced participation where individuals reduce labor supply to gain welfare eligibility. Jobs First has become a primary case study based on the prominent work of Bitler et al. (2006) and Kline and Tartari (2016). The panel estimates of quantile treatment effects shown here should be seen as complementary, though with the important exception of highlighting the role of participation costs. Bitler et al. demonstrated the significance of quantile treatment effects for policy impact analysis, particularly for welfare reform. This analysis extends their work directly by allowing the costs and benefits of a given program to vary by individual throughout the earnings distribution. Further, I demonstrate that different participant types may incur different costs relative to program exposure as in the case of ongoing recipients compared to new applicants.

Using a structural bounds approach, Kline and Tartari (2016) estimate that the intensive margin effect is bounded by $\{0.28, 1.00\}$ with a 95-percent confidence interval of $[0.20, 1.00]$, which implies that at least 20 percent of women who would have earned above the poverty line are behaviorally induced to reduce hours in order to opt into Jobs First. While Kline and Tartari's model accounts for population heterogeneity by introducing primitives drawn independently from a parametric distribution, the design-based approach in this study can be seen as allowing for earnings to be unconditionally serially dependent. Although our findings are similar in spirit, the evidence presented here suggests that behavioral-induced participation does not generalize to all low-income mothers on welfare near the eligibility threshold, especially regarding continuing recipients, and that a possible explanation of the difference among long- and short-term recipients is participation costs.

2.7 Conclusion

Behavioral responses to welfare policy, particularly concerning induced participation, are still relevant to public debates for potential reforms regarding TANF and other means-tested transfer programs. It is typically expected however that randomization provides the basis for anticipating that observables and unobservables are equally balanced by treatment status which applies to a range of policy interventions. Motivated by the work of Moffitt (1983) and Blank et al. (2000), this essay points out the importance of addressing unobserved heterogeneity in the estimation of QTEs using experimental data. I proposed a semi-parametric panel quantile estimator for a model that allows women to vary arbitrarily in preferences and costs of participating in welfare programs. Using data from a welfare reform experiment, I find no evidence of reduced earnings from behavioral-induced participation once I control for unobservables possibly capturing participation costs. The evidence suggests that welfare programs impose different participation costs by treatment status, and these costs can be heterogeneous throughout the conditional earnings distribution.

The literature using observational data had recognized and addressed some of these issues. Moffitt (1983) established the importance of stigma effects, or costs of welfare participation, on labor supply using data from the Panel Study of Income Dynamics. He finds that AFDC participation implies a fixed cost of about four hours of reduced labor supply per week. While Moffitt finds no significant variable stigma, the results are mean estimates under AFDC program rules where eligibility phases out at lower earnings than in the Jobs First context. Regarding other studies related to behavioral-induced participation, the literature has shown little evidence that women reduce hours to opt into welfare given constraints on labor supply adjustments and the lack of bunching near budget constraint notches where welfare phases out (see, for example, Saez, 2010). The literature on distributional effects of welfare reform has mainly abstracted away from the panel nature of policy reform in terms of short-run and long-run effects, which suggests a need to model how low-income single mothers' preferences, incentives and participation costs respond to program features and changes in labor supply over time.

3 THE CHILD CARE SAFETY NET: EVALUATING THE IMPACT OF ASSISTANCE, NONASSISTANCE, AND POLICY AFTER WELFARE REFORM

3.1 Introduction

Childhood poverty and the transmission of dependence are primary concerns for means-tested transfer programs in the United States. Given that welfare reform during the 1990s dramatically changed the mechanisms of government assistance, it is important to reexamine the long-term effects of childhood welfare exposure. In particular, after the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) of 1996, the entitlement cash assistance program Aid to Families with Dependent Children (AFDC) was replaced by the state block grant program Temporary Assistance for Needy Families (TANF), a work-conditioned program with less emphasis on cash assistance and more emphasis on in-kind supports, or "nonassistance" transfers. Although the literature has extensively noted the declines of welfare caseloads and cash assistance following reform, the share of nonassistance transfers has increased such that total real expenditure has remained relatively stable for TANF as a major transfer program. However, the impact of nonassistance TANF participation is unknown. Notably, the largest TANF expenditure category after cash assistance is directed toward child care, which represents a potentially consequential child development input considering work requirements for single mothers. This study provides an empirical analysis of the role of TANF childcare expenditures and policies—which vary widely by states over time—on center-based childcare utilization and children's educational outcomes.

Children living in structural poverty face significantly worse outcomes for health, education, and earnings, so the extent to which means-tested transfer programs affect child development matters for cycles of poverty and future program participation. The importance of child development is highlighted in a recent study of extreme poverty in the United States: Shaefer and Edin (2013) estimate that in 2011, about 4.18 million children live on $2 or less per day based on cash income, 3.55 million accounting for cash welfare transfers, and 1.17 million accounting for cash transfers, post-tax transfers, and in-kind food and housing assistance.[1] Recent studies have emphasized the critical role of early childhood interventions on human capital accumulation (see surveys in Elango et al., 2016; Heckman and Mosso, 2014), as well as the role of child care for low-income single mothers (e.g., Bernal and Keane, 2011; Del Boca, Flinn, and Wiswall, 2014). To the extent that TANF childcare transfers confer a positive option value to single mothers, state expenditure allocation decisions and child-related policies may increase access to higher quality child care. For example, a mother may choose to substitute away from informal care, such as a friend or grandmother, to center-based child care with a potentially more enriching learning environment.

The major legislative precursor to PRWORA was the Family Support Act of 1988, which introduced the Job Opportunities and Basic Skills (JOBS) program to encourage work for AFDC participants along with guaranteed childcare assistance. As part of the new legislation introduced by PRWORA, the Child Care and Development Block

[1]Shaefer and Edin (2013) use the Survey of Income and Program Participation. Post-tax and in-kind transfers for this study include the Earned Income Tax Credit, Food Stamps, and housing subsidies.

Grant Amendments of 1996 consolidated AFDC child care along with other assistance programs into the Child Care and Development Fund (CCDF). However, TANF would continue to directly fund childcare subsidization as well as transferring funds to the state CCDF program. Child care under TANF ceased to be guaranteed, though total spending increased, and states gained a large degree of discretion in how to allocate funds and set policies. As of fiscal year 2010, about 30 percent of all TANF expenditure in the U.S. went to basic cash assistance, while about 15 percent went to childcare-related expenses. Further, TANF childcare spending varies widely by state, for example, Wisconsin allocated almost half of all spending on child care in 2010, while Colorado allocated less than 2 percent of all spending.[2] Depending on the state, TANF may be the largest funding source for childcare transfers; for example, Illinois provides about 54 percent of all childcare expenditure with TANF-allocated funds over the years 1997-2014. With more state discretion through the TANF federal block grant, the concept of "welfare" has been implemented in vastly different ways (for examples, see Hahn, Golden, and Stanczyk, 2012), which prompts the question of which state policies are helping children become more self-sufficient in the next generation.

Using state-level expenditure data and policy parameters along with individual-level data from the Panel Study of Income Dynamics (PSID), this analysis estimates the effects of welfare spending on center-based childcare utilization among low-income families. I find that an additional thousand dollars of childcare assistance spending per child in poverty increases a low-income single-mother family's likelihood of choosing formal child care by about 27 to 30 percentage points. To the extent that TANF assistance helps

[2]Other TANF expenditure categories include transportation, work-related expenses, individual development accounts, refundable tax credits, prevention of out-of-wedlock pregnancies, and diversion payments.

participants to expand their choice set for childcare providers and potentially increase quality of care, one might expect that child development may benefit along both cognitive and non-cognitive dimensions in those states that prioritize block grant funds towards child care.

3.2 Welfare Reform and Child Care Policy

The historical antecedent for public childcare assistance is the Lanham Act of 1940 (see Herbst, 2017b), which was motivated by surges of women going to work during World War II. While some women remained in the labor force after the war, it was not until the 1960s that public assistance for child care became a means-tested program.[3] As part of President Johnson's "War on Poverty", the Economic Opportunity Act of 1964 established Head Start, which was primarily targeted at children below poverty.[4] Also during the 1960s, in response to rising welfare rolls, the AFDC program introduced its first reform intended to encourage work and assist in childcare expenses, though the size of these programs were quite small relative to total AFDC expenditure. Further, women with young children were still not as prominent in the labor force at this time, though this begins to change by the late 1970s as seen in Figure 3.1.

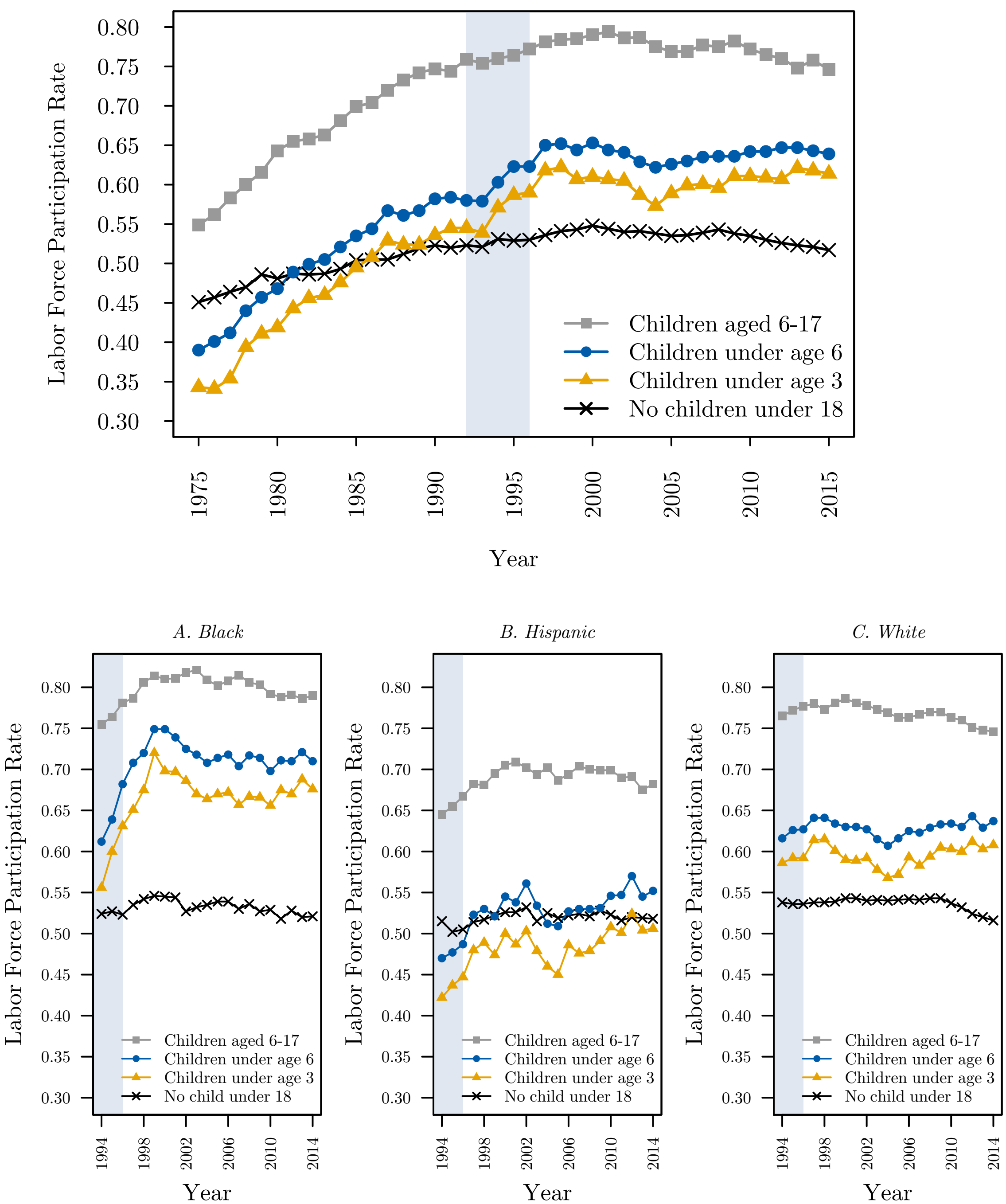
3.2.1 Childcare Assistance through 1996

After experimenting with "workfare" programs in the early part of the decade, the Social Security Act of 1967 introduced the Work Incentive Program (WIN) that promoted job

[3] According to Michel (1999), President Elect Kennedy is quoted at the onset of the 1960s as saying, "I believe we must take further steps to encourage day care programs that will protect our children and provide them with a basis for a full life in later years."

[4] Head Start was originally required to serve at least 90 percent of child recipients at or below poverty and 10 percent with some disability. Early Head Start, a program extension aimed at children 0 to 5 years old, began in 1995. For research on the impact of Head Start, see Bitler, Hoynes, and Domina (2014); Chang et al. (2007); Deming (2009); Garces, Thomas, and Currie (2002); Gelber and Isen (2013); Kline and Walters (2016); Ludwig and Miller (2007); Walters (2015).

Figure 3.1. Women's Labor Force Participation
by Presence and Age of Own Children, 1975-2015



Notes: "Own" children include children through birth, marriage, or adoption, and exclude other related and unrelated children. The major waiver period of welfare reform is indicated by the shaded region. *Source:* "Women in the Labor Force: A Databook", U.S. Bureau of Labor Statistics, Report 1065, Table 7. Employment status of women, by presence and age of children, March 1975-March 2015. U.S. Bureau of Labor Statistics, Current Population Survey, 19752015 Annual Social and Economic Supplement. Marital and Family Labor Force Statistics, U.S. Bureau of Labor Statistics, Series FMUP1392239, 1392263, 1392275, 1392289. Children are "own" children and include sons, daughters, step-children, or adopted children. Not included are nieces, nephews, grandchildren, and other related and unrelated children.

training and search while providing supporting services such as childcare assistance.[5] As of 1969, the WIN program spent about \$77.5 million (in 2014 dollars) on childcare assistance for around 55,000 children per month (compared to Head Start spending about \$1.6 billion and serving around 663,600 children on average). AFDC spending on childcare would remain almost negligibly small until 1976, which saw the introduction of Title XX of the Social Security Act, which later became the Social Services Block Grant (SSBG).[6] By 1978, Title XX was allocating about \$587 million towards childcare assistance for almost 600,000 children per month, and approximately half of the children served belonged to AFDC-participating families. In the same year, for comparison, Head Start spent about \$1.8 billion to serve close to 400,000 children.

The welfare reform period of the 1990s was *again* motivated by public debate about the nature of welfare dependence and the value of work (for historical, sociopolitical context, see DeParle, 2004; Noble, 1997). As a harbinger to more widespread reforms, the Reagan administration introduced the Family Support Act of 1988, which emphasized work requirements in an effort to reduce long-term welfare participation for families with children.[7] This act created Job Opportunities and Basic Skills (JOBS) program within AFDC along with new childcare programs for AFDC recipients (AFDC Child Care), which was guaranteed for JOBS participants, and for AFDC program leavers (Transitional

[5]Before WIN, select states implemented work-support programs for AFDC recipients: the Community Work and Training Program from 1962-1967, and the Work Experience and Training Program created by the Economic Opportunity Act.

[6]Title XX funds were directed to support work-related services to low-income families, including AFDC recipients, and operated as a matching grant to states up to some federally mandated ceiling. The block grant was established under the Omnibus Budget Reconciliation Act of 1981.

[7]Prior state experiments with workfare and child care include: Massachusetts 's Employment and Training Choices program, California's Greater Avenues to Independence (GAIN) program, Project Chance in Illinois, Michigan's Opportunities and Skills Training program, and the Realizing Economic Achievement (REACH) program in New Jersey.

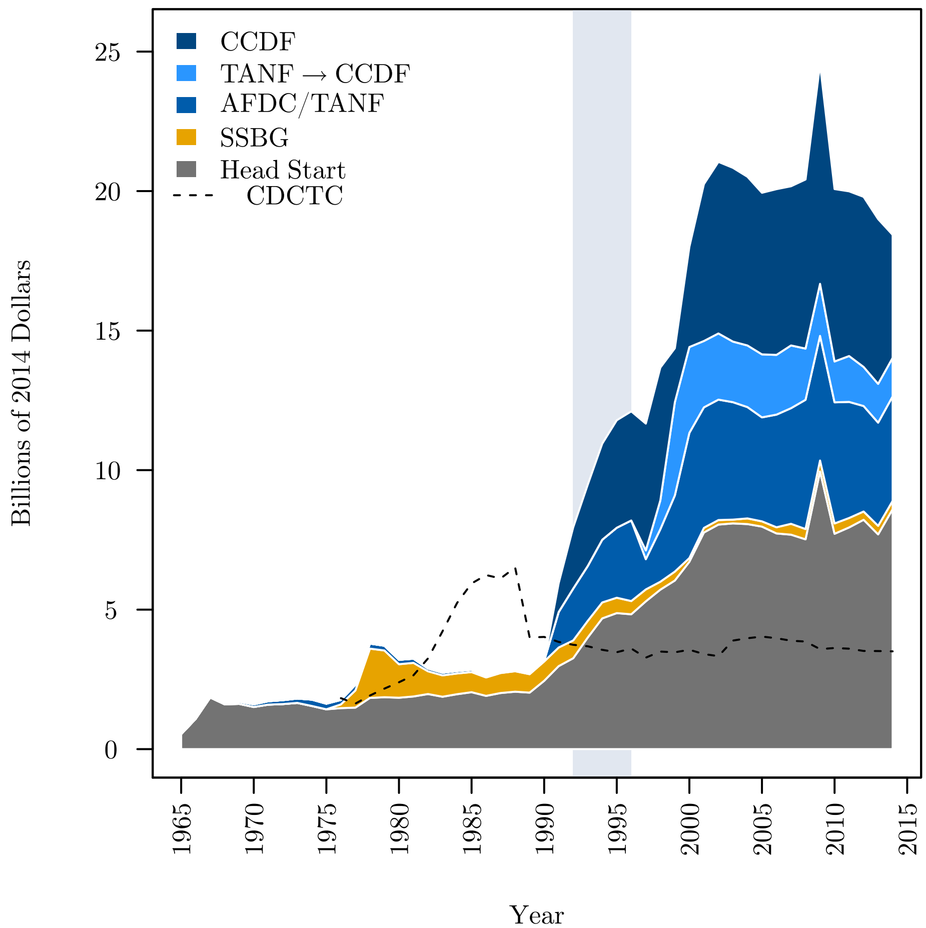
Child Care). The JOBS program was to be implemented in every state by October 1990.[8] In November of 1990, the Omnibus Budget Reconciliation Act established the Child Care and Development Block Grant—the predecessor for CCDF—as well as the "At-Risk" Child Care program for families who are nearly eligible for AFDC. All AFDC recipients not otherwise exempt by law are required to participate in JOBS. As states experimented with AFDC program rules, namely making welfare conditional on recipients working, the accessibility of suitable child care was a prerequisite before cases could be considered noncompliant and subject to sanctions. Therefore, AFDC spending on child care instantly shot upward.Figure 3.2 shows the U.S. expenditure trends for the major childcare and early childhood education programs from 1965 to 2014, which features a stark rise in spending as of 1991. (Note that the expenditures in Figure 3.2 are stacked cumulatively, except for the Child and Dependent Care Tax Credit, which is nonrefundable and less relevant for low-income families, shown separately as a dashed line.) By 1996, AFDC spending on child care reached about $2.9 billion serving over 140,000 children through the JOBS program and 210,000 children whose families were non-JOBS cases.

3.2.2 Childcare Assistance after the Welfare Reform Act of 1996

As introduced above, the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA)—sometimes referred to as the Welfare Reform Act—was a significant turning point for the government role in childcare subsidization. The Child Care and Development Block Grant was consolidated along with a myriad of smaller federal childcare programs into one primary assistance program, the Child Care Development Fund. At the same time, the match-grant AFDC program was replaced by the block-grant TANF

[8] According to an official report by the Government Accountability Office, only about 11 percent of AFDC recipients participated. See other GAO reports for related implementation issues.

Figure 3.2. Childcare Expenditures of Major Assistance Programs, 1965-2014



Notes: CCDF expenditures are shown separately from those funds transferred from TANF to CCDF. CDCTC expenditures are shown separately given that this nonrefundable credit is less applicable to low-income families. Abbreviations: Child Care and Development Fund (CCDF); Title XX/Social Services Block Grant (SSBG); Child and Dependent Care Tax Credit (CDCTC). The major waiver period of welfare reform is indicated by the shaded region, 1992-1996. *Source:* U.S. Department of Health and Human Services; Background Material and Data on Programs within the Jurisdiction of the Committee on Ways and Means; and, various other government publications. (See Data Appendix for details.)

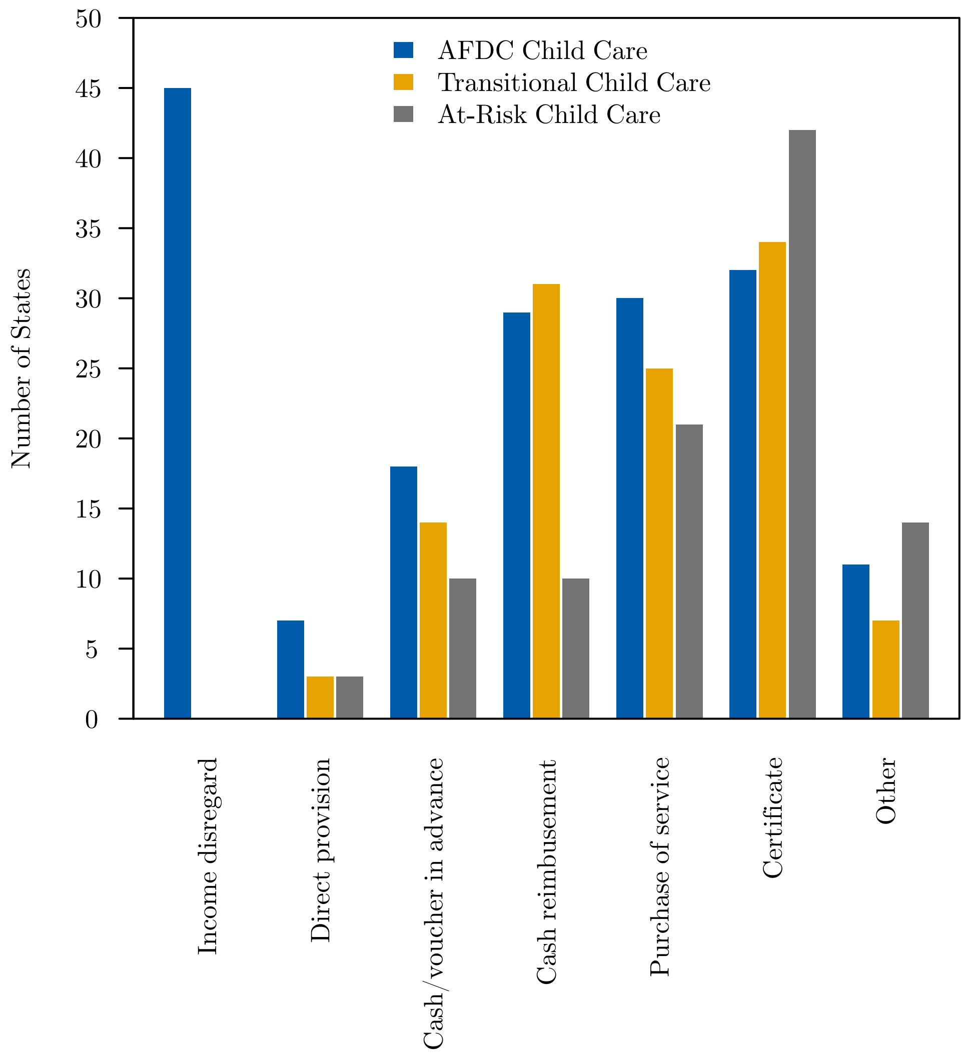
program, which gave states much more freedom to prioritize how welfare dollars were spent, including by what means to fund childcare assistance (for more background on the TANF program, see Moffitt, 2003; Ziliak, 2016).

Throughout this essay, the term "childcare assistance" is used generally to represent government transfers to income-eligible families in order to make child care affordable. In practice, the transfer could be in the form of direct provision of childcare services through a state-run facility, or more likely, it could be in some monetary form, whether by cash in advance, voucher, reimbursement, or payments to the service provider. In Figure 3.3, the prevalence for each form of transfer are shown for the major AFDC programs as of 1996, which also identifies the number of states (nearly all) that allow an income disregard for childcare expenses.[9] A major difference after PRWORA is not the method of transfer, but the policy rules governing eligibility of recipients and regulation of service providers, both of which may vary depending on the state and source of funding: TANF or CCDF.

Another change under TANF is the specific use of the term "assistance" versus "non-assistance". The key difference is that "assistance" refers to families who are not employed, and "nonassistance" to families who are employed. Based on this criterion, Figure 3.4 shows the trend for assistance and nonassistance childcare expenditures beginning in 1991 (where JOBS and non-JOBS participants with earnings are considered "non-assistance" during AFDC years). A glaring discontinuity occurs for reported expenditure in years 1997-1999, the transition period immediately after PRWORA. While the definition of assistance during this time period was less clear statutorily, states were reporting childcare expenditure as assistance when payments were made for child care,

[9]For a study of what types of child care welfare participants use, see Fuller et al. (2002).

Figure 3.3. Number of States by AFDC Childcare Provision Method, 1996



Notes: States typically use more than one method of childcare provision. Specific income disregards related to family childcare expenses are not included in childcare expenditure categories for financial reporting, though this is another means of transferring income that is used in most states. *Source:* Background Material and Data on Programs within the Jurisdiction of the Committee on Ways and Means; author's tabulations.

and nonassistance for childcare-related services such as referrals or counseling.[10] A large distinction in the definition of nonassistance is that these transfers neither contribute to the caseload nor time limits associated with TANF participation, and nonassistance participation is also not included in state reporting requirements.[11]

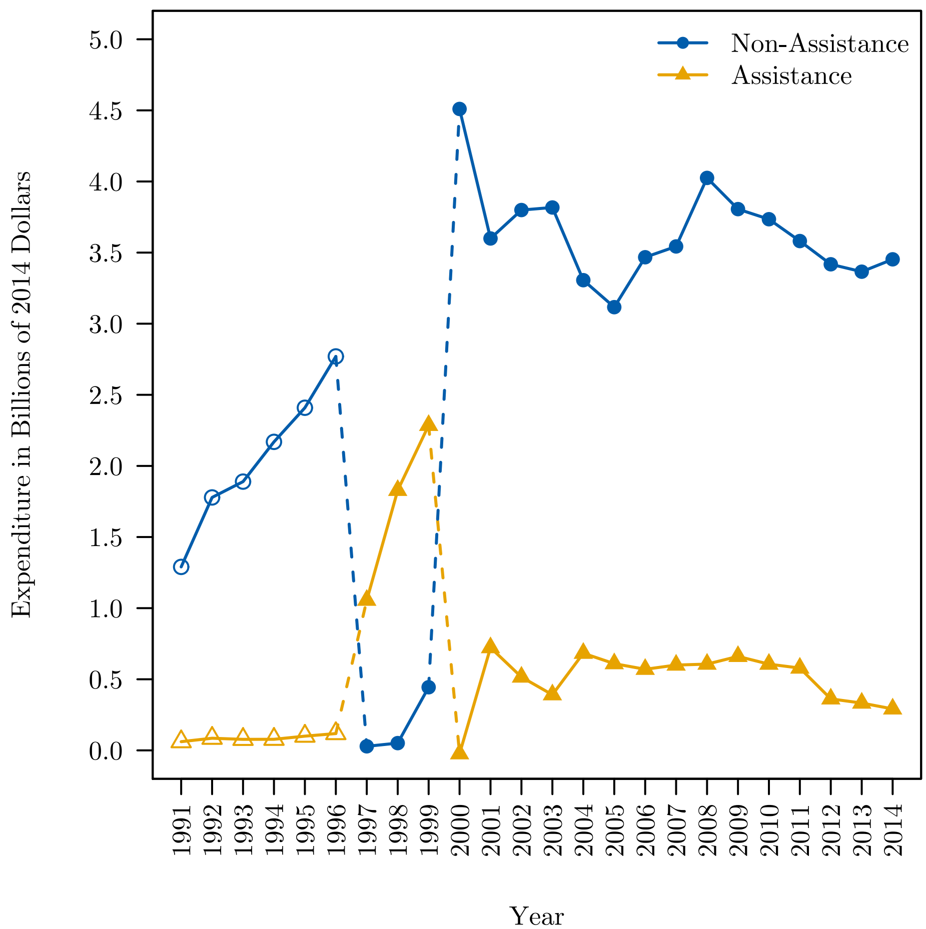
When states allocate TANF block grant dollars to child care, there are three main spending channels. One option is to simply transfer the funds to CCDF, which was established to consolidate childcare assistance programs into one primary mechanism. Up to 30 percent of a state's federally allocated TANF block grant may be transferred to CCDF in a given year (additionally, total funds transferred to CCDF and the SSBG are not to exceed 30 percent). It should be noted that SSBG continues to provide childcare services, as well, though this role has diminished greatly after welfare reform.[12] A second option for TANF childcare is expenditure on nonassistance transfers, which enables the state to boost work participation rates in partial fulfillment of state obligations under federal law. A third option, states can allocate funds toward assistance transfers, which

[10]See Appendix Table G.1 for spending across all assistance, nonassistance, and transfer categories for select years; many nonassistance categories show zero expenditure in 1997. The definition of "assistance" is given in the Code of Federal Regulations (U.S. Government Publishing Office, 2015) Title 45.B.II.§260.31: "cash, payments, vouchers, and other forms of benefits designed to meet a family's ongoing basic needs (i.e., for food, clothing, shelter, utilities, household goods, personal care items, and general incidental expenses)." According to this section of the federal code, TANF expenditures for transportation and child care may be classified as assistance when provided to families who are not employed, or as nonassistance when provided to families who are employed. The current definition of assistance entered the federal code as of 2000, yet the original use of the term was less clear for the first three years of TANF, 1997-1999. According to the Final Rule (Federal Register, Vol. 64, No. 69, 1999, pg. 17775): "The general legislative history for this title indicated that Congress meant for this term to encompass more than cash assistance, but did not provide much specific guidance (H.R. Rep. No. 725, 104th Cong., 2d sess.). Likewise, our pre-NPRM [Notice of Proposed Rule-Making] consultations did not provide clear guidance or direction." Therefore, the TANF transition years 1997-1999 may be a difficult time period to make reliable inference based on nonassistance program data.

[11]Note that some families may receive both cash assistance and nonassistance child care, so the only families not represented in official caseload statistics would be those receiving transitional child care or considered at risk of needing government assistance.

[12]In 2014, the total amount of TANF transfers to CCDF was about $1.4 billion, whereas the TANF transfers to SSBG were about $1.2 billion out of which $237 million was spent on child care along with another $63 million of SSBG-allocated funds. The total SSBG funds spent on child care in 2014 comprise about 2.7 percent of all childcare assistance.

Figure 3.4. AFDC/TANF Childcare Expenditure
by "Assistance" Case Status, 1991-2014



Notes: The terms "assistance" and "nonassistance" child care refer to AFDC/TANF cases in which families are unemployed or employed, respectively. For years 1991-1996, assistance and nonassistance expenditures are shown according to financial reports by JOBS/employment status. For years 1997-1999, financial reports categorize spending by assistance, though the definitions understood by states at that time was unclear. For categorical expenditure reports from 2000 onward, assistance is explicitly defined by employment status in the federal code. *Source:* U.S. Department of Health and Human Services; Background Material and Data on Programs within the Jurisdiction of the Committee on Ways and Means. (See Data Appendix for details.)

may apply to parents looking for work or participating in some job training, education, or community service. TANF assistance funds for child care may also be used for family counseling or for expanding existing early education programs such as Head Start as well as after-school and summer programs.[13] Transferring funds to CCDF might be preferable, for example, if the state wants to support state-wide child care quality enhancement, which may not directly satisfy TANF goals (Schumacher, Greenberg, and Duffy, 2001). The discretion states have for allocating TANF block grant funds follows directly from the overarching goal of PRWORA with respect to child care: to give states "maximum" flexibility to serve families' needs and to give families the ability to choose the best child care according to their own needs.[14]

In addition to state spending through TANF and CCDF, there exist other TANF policy mechanisms that can make child care more accessible to the working poor. Foremost, income disregards for childcare expenses can, depending on the state, either increase the eligibility threshold, the benefit amount determination, or both. Other state-level TANF policies related to child care and development include transitional childcare benefits (level and duration) for families leaving welfare, school attendance and parental involvement policies for dependent children, and the allocation of non-recurrent transfers (or, diversion payments) for child care while a family member searches for employment. Further, state CCDF programs define several relevant policy parameters for childcare assistance, for example: initial and continuing eligibility income thresholds by family size; copay amounts by income threshold and family size, as well as copay exemptions related to TANF status,

[13] Few studies look at the important gap in summer child care with the exception of Hardy and Gershenson (2016).

[14] For further discussion of state incentives to allocate childcare-related funds to either TANF or CCDF, see Greenberg (1998) and Lynch (2016).

poverty status, or adjustments for multiple children or part-time care; and, regulations and reimbursement rates by type of childcare provider.[15]

Lastly, to put the post-PRWORA era in perspective, total childcare assistance expenditure has gone from almost nonexistent before the 1970s to almost equivalent to the AFDC/TANF program on all other expenditures. Meanwhile, as seen in Figure 3.5, welfare programs for some of the nation's most vulnerable families has remained flat in real terms as real education and total public welfare spending has climbed.[16] In 2014, childcare assistance amounts to about 2 percent of total public welfare spending.

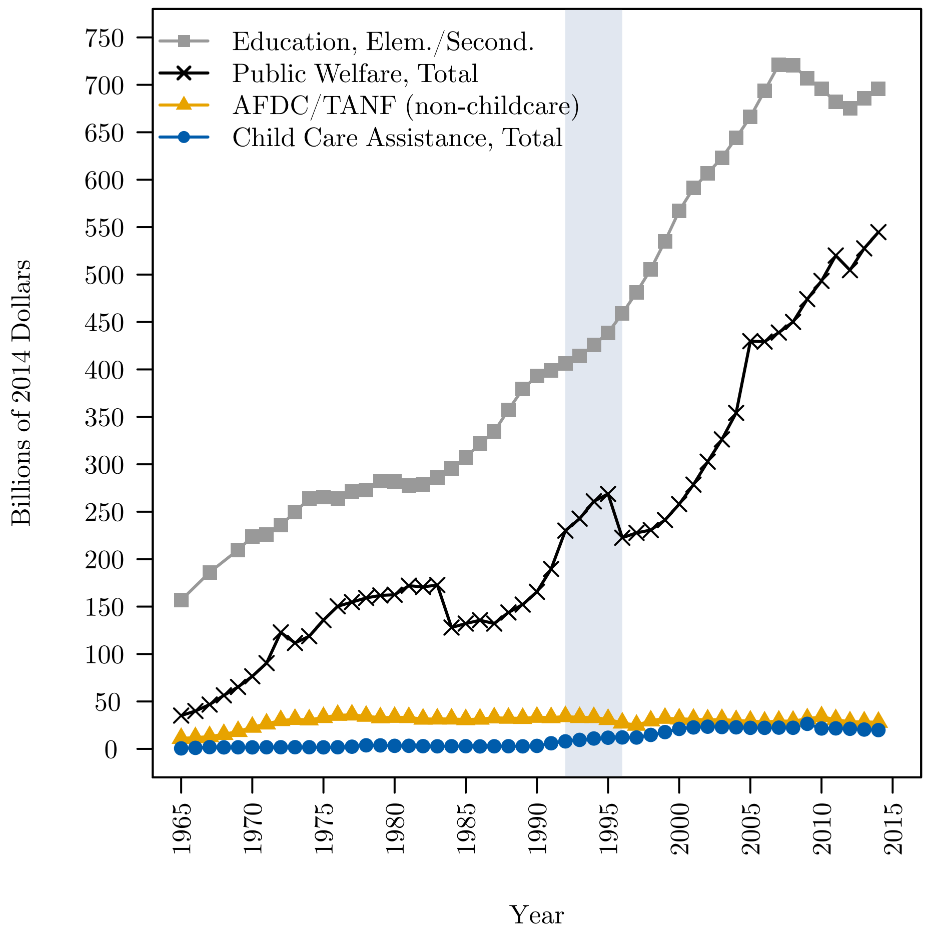
3.3 Related Literatures in Early Childhood Intervention and Development

Evidence for the importance of early childhood development for later life outcomes has become an important topic for researchers interested in persistent poverty across generations (see Currie and Almond, 2011; Elango et al., 2016; Heckman and Mosso, 2014). In particular, studies have noted the importance of early investments in social ability, so-called noncognitive skills, as well as more academic (or cognitive) skills, as determinants of successful life outcomes in adulthood (Cunha and Heckman, 2007; Cunha et al., 2006; Heckman and Kautz, 2012). Other studies have focused on the role of parental time investments (Del Boca et al., 2014; Del Boca, Monfardini, and Nicoletti, 2017), the causal effect of income transfers (Aizer et al., 2016; Dahl and Lochner, 2012), or environmental inputs to child development, such as school quality, social networks, and neighborhood

[15]TANF policies on such dimensions as eligibility, copay, and regulations are not as readily available as those of CCDF, yet these programs coordinate services in all states and CCDF programs consult with TANF officials on policy development in at least a third of states. It is likely that states contrast policies in these programs strategically to meet different goals with respect to target populations and federal requirements.

[16]Total public welfare spending represents U.S. expenditures by all governments (federal, state, and local) on assistance and subsidy programs including health and food assistance, notably Medicaid and food stamps.

Figure 3.5. Education and Public Welfare Expenditure Trends Relative to Cash and Childcare Assistance, 1965-2014



Notes: Education expenditure includes capital and current spending related to elementary and secondary education. AFDC/TANF expenditures exclude direct childcare spending and transfers to CCDF, which are both represented in total childcare assistance expenditure along with Head Start and Title XX/Social Services Block Grant childcare expenditure. The major waiver period of welfare reform is indicated by the shaded region. *Source:* U.S. Census Bureau, Annual Survey of State and Local Government Finances and Census of Governments; Statistical Abstract of the United States; and, various other government publications. (See Data Appendix for details.)

environment (Chetty et al., 2014; Durlauf, 1996, 2006; Fryer and Katz, 2013). For background on childcare subsidies and the demand for quality care, see Appendix E.

The economics literature on child care has been bolstered by evidence from two prominent experiments, the Abecedarian Project (see Barnett and Masse, 2007) and the Perry Preschool Program (Heckman et al., 2010; Heckman, Pinto, and Savelyev, 2013). Given the demonstrated benefits of quality early childhood education for disadvantaged children, there has been a growing public discourse on ways to improve childcare assistance through reforms to the tax code (Ziliak, 2014) as well as a universal child allowance (Shaefer et al., 2016). There have been several studies to analyze experiments and case studies for the provision of universal child care (Baker, Gruber, and Milligan, 2008; Havnes and Mogstad, 2015; Herbst, 2017b; Kottelenberg and Lehrer, 2016; Shaefer et al., 2016). Also, Olivetti and Petrongolo (2017) survey a range of family policies across high-income countries, though this literature routinely examines female labor supply instead of child outcomes.

A similar, and extensive, stream of research has examined the effect of education financing on student outcomes, particularly as a result of equalization reforms (see Cascio and Reber, 2013; Gordon, 2014; Hoxby, 2001).[17] In a more general study on the effectiveness of public expenditures (Harknett et al., 2005) find positive effects for state-level spending on child outcomes such as health and test scores.

Several studies have directly addressed child poverty and development after welfare reform (Bennett, Lu, and Song, 2004; Danziger, 2003; Danziger and Danziger, 2008; Duncan

[17]For more extensive coverage of heterogeneous effects, distributional effects, and misspecification issues, among others, see Baicker and Gordon (2006); Candelaria and Shores (2017); Dewey, Husted, and Kenny (2000); Figlio and Fletcher (2012); Hanushek and Somers (2001); Jackson, Johnson, and Persico (2016); Lafortune, Rothstein, and Schanzenbach (2016).

and Brooks-Gunn, 2000; Kalil and Dunifon, 2007; Morris, Gennetian, and Duncan, 2005). Morris and Hendra (2009) look at child wellbeing and academic achievement after time limits have expired. Miller and Zhang (2009, 2012) present the first studies to directly test welfare reform effects on children's academic achievement and educational attainment, respectively, given nationally representative data.[18] For academic achievement, the authors use restricted data on mathematics test scores from the National Assessment of Educational Progress program and find relative test score gains for low-income students based on changes-in-changes estimates.[19] Further, they use a value-added analysis to show the robustness of the effects to changes in EITC benefits. For educational attainment, Miller and Zhang use the Common Core of Data, produced by the National Center for Education Statistics, and continue to find positive effects of welfare reform on child outcomes. The gains in test scores are increasing with the duration of welfare participation. These findings contradict some experimental results showing adolescents experience losses, which the authors attribute to their access to longer time horizons suggesting again that duration matters (see Morris et al., 2009). Bruins (2016) provides structural estimates suggesting that TANF child care is poorly targeted to the most needy families, and Herbst (2017a) uses TANF's age-of-youngest-child exemption to identify a negative effect of work requirements on young children's cognitive development.

3.4 Model

Given that most of this analysis is descriptive evidence to motivate a first-stage relationship between childcare policy and family behavior, the framework here follows from a

[18] Levine and Zimmerman (2005) look at the effects before welfare reform using National Longitudinal Survey of Youth 1979 data, and they find no significant effects of welfare exposure on children's test scores or behavioral indices when using instrumental variables, fixed effects, or sibling comparisons.

[19] "Changes-in-changes" refers to a nonlinear difference-in-differences approach (see Athey and Imbens, 2006).

simple linear probability model for the choice of childcare arrangement. Let the child's outcome variable, C_{ist}, be an indicator for whether the family of child i uses center-based or family home day care while living in state s at time t. The child is considered to receive formal child care if $C_{ist} = 1$ and informal care otherwise. The baseline estimation equation is

$$C_{ist} = \beta' \mathbf{x}_{ist} + \gamma E_{st} + \psi' \mathbf{s}_{st} + \eta_s + \theta_t + \alpha_i + u_{ist}, \tag{3.1}$$

where $\mathbf{x}_{ist}$ is a vector of individual or family characteristics, E_{st} is the state-level expenditure on childcare assistance per child in poverty, and $\mathbf{s}_{st}$ is a vector of other time-varying state-level expenditure and policy controls. Controls for fixed state and year effects are represented by η_s and θ_t, respectively, α_i is an individual fixed effect, and u_{ist} is the unobserved error term.

For states with generous childcare spending, families should have more ability to choose quality child care. Holding constant time-varying state policies in $\mathbf{s}_{st}$ and differencing out fixed state and year effects, I expect $\gamma > 0$ for target families with low earnings and young children if they perceive formal care to be higher quality than informal, and $\gamma \approx 0$ for ineligible families regardless of which type of care arrangement is preferred. An advantage of state-level spending is that it should offer an exogenous form of variation whereas local spending may be more closely correlated with families' poverty status or accessibility of child care. The test will be whether state spending has strong enough predictive power for family behavior.

In the following sections, I first describe the data and then provide more detailed trends in childcare assistance across states over time. Then I present preliminary estimates for the first-stage model using variants of equation (3.1) where I begin with the assumption that neither confounding state policies nor any fixed individual heterogeneity are related to a family's chosen care arrangement. Then I progressively add controls for time-varying state policies and then individual effects. Next, I test the sensitivity of this baseline estimation by target population as well as timing effects relative to the major welfare reform.

3.5 Data

In order to characterize the relationship between individual-level outcomes and state-level policy, I use the Panel Study of Income Dynamics supplemented by various sources for state-level policy and combined state/federal expenditure by year. Below is a summary of the main variables, and further source details are available in Appendix F.

3.5.1 Individual-Level Data: Panel Study of Income Dynamics

The Panel Study of Income Dynamics (PSID) provides a panel of family observations for socioeconomic data that include family childcare spending and childcare arrangements in certain years. For example, in the biennial survey years from 1999 to 2013, the main family survey asks whether families used center-based or family home child care during the previous year. Beginning in survey year 1997, the PSID added a Child Development Supplement (CDS) questionnaire for a subset of families with children ages 0-12. Following up on this initial wave, the CDS re-interviewed families in 2002 when the children were 5-17, again in 2007/2008 for those children who were under age 18 in 2007, and then another recent release in 2014. The CDS supplement adds rich information about the child's daycare arrangements in addition to detailed questions on topics of school, neighborhood, and home, as well as the child's relationship with different family members. As these children aged beyond 18, the PSID added another extension called the Transition to Adulthood (TA) questionnaire, which complements the CDS. The TA has been administered every other year from 2005 to 2015 (data are currently available through the 2013 survey). TA data provide standardized college-entry exam scores and college enrollment choices. Also, the CDS/TA surveys record educational attainment expectations held by the child's parents and teachers as well as the child's own expectations and aspirations as a young adult.[20]

The most consistent measure of child care utilization during the welfare reform era is the family survey question of whether the family uses center-based care. As shown in Table 3.1 for years 1998-2012, about 23 percent of all families with children under age 13 use formal child care, and among single-mother families, the probability of using formal care is increasing with income. Single mothers below poverty, based on the U.S. Census poverty threshold by family size, use formal care in 21 percent of the sample compared to 24 and 30 percent for single mothers below 200-percent poverty and above 300-percent, respectively. Also, Table 3.1 shows that as single-mother family income decreases, younger children are more prevalent and the number of children is larger. However, lower-income single mothers are also more likely to have some other adult present in the household. The likelihood of single mothers having any family labor income during this period is about 64 percent for those below 100-percent poverty and about 76 percent for those below 200-percent poverty.

As a standard measure of educational attainment, the individual survey collects each person's years of education completed, which can be linked across generations, and further, the TA supplement specifically asks about high school graduation, the GED, and

[20] In addition, PSID has also implemented the Childhood Retrospective Circumstances Supplement for survey year 2014. These data cover broad topics about one's environment while growing up, for example, whether the child has had any negative interactions with the justice system.

Table 3.1. Descriptive Statistics for Children Under Age 13, 1998-2012

Sample Definition:	Single Mother Families			Full Sample
Poverty Status:	< 100%	< 200%	≥ 300%	
Center-Based Child Care (%)	0.214	0.240	0.301	0.234
	(0.410)	(0.427)	(0.459)	(0.424)
Age of Child	7.083	7.160	8.072	6.729
	(3.312)	(3.286)	(2.975)	(3.395)
Age of Youngest Child in Family	5.624	5.925	8.175	5.716
	(3.556)	(3.549)	(3.170)	(3.599)
Number of Children in Family	2.786	2.581	1.673	2.440
	(1.388)	(1.290)	(0.772)	(1.092)
Number of Adults in Household	1.322	1.293	1.133	1.914
	(0.642)	(0.604)	(0.357)	(0.542)
Mother's Education (years completed)	12.872	13.063	14.653	14.257
	(1.895)	(1.858)	(1.894)	(2.128)
Any Family Labor Income? (%)	0.642	0.756	1.000	0.941
	(0.479)	(0.430)	(0.000)	(0.235)
Expenditures below shown in thousands of dollars per child in poverty:				
AFDC/TANF Child Care (Direct)	0.271	0.270	0.316	0.295
	(0.318)	(0.321)	(0.350)	(0.329)
AFDC/TANF Child Care (incl. Transfers)	0.664	0.669	0.782	0.749
	(0.689)	(0.699)	(0.773)	(0.721)
CCDF (Total incl. TANF Transfers)	0.662	0.674	0.766	0.760
	(0.283)	(0.290)	(0.349)	(0.341)
AFDC/TANF and CCDF Childcare Assistance	0.947	0.960	1.098	1.071
	(0.509)	(0.519)	(0.578)	(0.561)
Number of Individuals	2350	3031	309	7778
Total Observations	5134	7227	491	25009

Notes: Sample averages are weighted by the child's PSID core longitudinal weights, and standard deviations are shown below in parentheses. The full sample corresponds to all children whose age is greater than 0 and less than 13 during years 1998-2012, though most estimations that follow restrict this sample to single mothers with family labor income, and the PSID family survey measure for whether a child's family uses center-based or family home day care is available only for the even years in this time period. Poverty status is determined by family earnings as a percent of the U.S. Census poverty threshold by family size and year.

college attendance. In terms of measures of achievement, the supplemental surveys provide a more detailed perspective on both achievement test scores as well as attitudes and expectations for educational attainment. During the first wave of the CDS in 1996, children were administered the Woodcock-Johnson Test with percentile rank scores reported separately for letter-word recognition, passage comprehension, applied problems, and calculation.[21] The CDS also records passage comprehension raw scores for the child's primary caregiver, which is the mother 95% of the cases and either the mother or father in 98%. Another intergenerational achievement measure is available from the 1972 main family survey in which the PSID administered the Lorge-Thorndike Sentence Completion Test (validated as a measure of intelligence/ability as part of the Detroit Survey). Since each child observed in the CDS/TA surveys has a family identification number for every survey year, a child's educational outcome in 2013 can easily be matched for some measure of family intelligence in 1972, likely a measure for a grandparent. These family test scores may serve as controls for hereditary ability across generations.

3.5.2 State-Level Expenditure and Program Policy Data

In this analysis, the primary interest is the state-level amount of spending relative to the target population. All expenditure measures throughout this essay are given as real 2014 dollars based on the personal consumption expenditures deflator (excluding food and energy) obtained via the U.S. Bureau of Economic Analysis. In estimation, the expenditures are expressed as spending per capita by state, which is defined as children

[21]See Duffy and Sastry (2014) for details regarding achievement tests in the PSID-CDS. In addition to these early educational achievement measures, CDS also collected information on a child's self-concept in math and reading, that is, measures of one's perceptions of own ability in these subjects. An additional measure of intelligence in the CDS is the WISC digit span raw score, which represents the total for the longest forward-sequence and backward-sequence span of digits the child can recall after hearing (Wechsler Intelligence Scale for Children–Revised: The WISC Digit Span Test for Short-Term Memory, © Psychological Corporation). Other achievement measures for high school students include grade point average, SAT and ACT scores.

living below the U.S. Census poverty threshold for means-tested expenditure categories, or all children under age 18 for broader spending categories such as education.

3.5.2.1 AFDC/TANF and CCDF Childcare Assistance Data

During the AFDC years 1991-1996, state-level expenditure data, including pre-PRWORA childcare spending, are available via the Background Material and Data on Programs within the Jurisdiction of the Committee on Ways and Means (Green Book). For the TANF years, detailed expenditure data are available for every fiscal year via the U.S. Department of Health and Human Services (USDHHS) website. The Green Books also provide childcare caseload data in certain years, though caseloads during the TANF era only correspond to cash assistance cases. The only indications of the number of TANF childcare cases come from a measure of the percent of active TANF cases with subsidized child care in the Characteristics and Financial Circumstances of TANF Recipients, 2000-present, and state TANF reports on Separate State Programs and Maintenance of Effort (MOE) activity, which are available for years 2005 and 2010.[22] For earlier years of AFDC childcare assistance, state-level expenditure or caseload data are much less available, though the total level of childcare spending is also much lower before 1991. (See Appendix F for more details and additional data sources.)

From 1999 to present, CCDF program data are available for both expenditure and caseloads by state via the USDHHS website, and earlier data (including predecessor programs) are available for years 1991-1998 via Green Book publications. Given that some states allocate a generous proportion of TANF expenditure toward CCDF block grant

[22]While state reports for MOE activity are presumably collected in other years, only two have been available recently via government websites or select Internet archive sites. In some states, the total MOE spending for child care matches the total state expenditure in official budget reports, which means that the childcare caseloads in those states should be representative.

transfers, the TANF program can be a significant contributor to the variance of CCDF programming. For example, in Wisconsin since 1997, the proportion of TANF transfers to CCDF relative to total CCDF expenditure implies that TANF contributes toward about 8,700 children receiving child care on average per month out of a total of about 26,000. Another data source for childcare assistance participation is a series of USDHHS briefs providing estimates of childcare eligibility and receipt. In 2012, for instance, around 14 million children were eligible for subsidized care according to simulations under federal rules, almost 9 million of those were eligible under more strict state rules, and the number of children served was a little over 2 million (15 percent of those federally eligible).[23]

While there is little information about specific childcare policies within the TANF program, the Urban Institute maintains the CCDF Policy Rules Database in coordination with USDHHS. These policies include information on the amount of copays and eligibility for copay exemption (for example, some states exempt TANF participants), the presence of family-size adjustments for copays, whether relatives are authorized to provide subsidized in-home care, or the length of eligibility redetermination periods. Another Urban Institute project is the Welfare Rules Database, which does not offer childcare-specific policy rules, yet does have state-level TANF policy information such as the size of childcare earnings disregards, whether states impose rules on school attendance or parental

[23]Take up for childcare subsidies has been historically low. Evidence from certain pilot AFDC experiments provided early evidence that childcare subsidies can incentivize low-income families to use center-based child care, yet the costs born by families after subsidization was still a significant burden, as was the prospect of transitioning out of public assistance (Committee on Ways and Means, U.S. House of Representatives, 2000; Michalopoulos, 2010). Also, an increasing complication of using childcare assistance when eligible is that low-wage workers frequently experience variable scheduling subject to changes and difficult hours, such as contract or temp-to-hire work (Henly and Lambert, 2005; Katz and Krueger, 2016). Even though TANF conditions assistance on mothers working, the percent of active TANF cases receiving subsidized child care has stayed close to 10 percent over the last decade.

involvement, and whether transitional child care after TANF participation is limited by income or time.

3.5.2.2 Other State-Level Controls

In order to study the effect of childcare expenditure and policy on family behavior, it is important to control for other variation within states over time that may also be correlated with outcomes related to child care and education. In terms of additional program expenditures, the most relevant for low-income families are the Social Services Block Grant (SSBG), Head Start, elementary/secondary public education, and total public welfare.

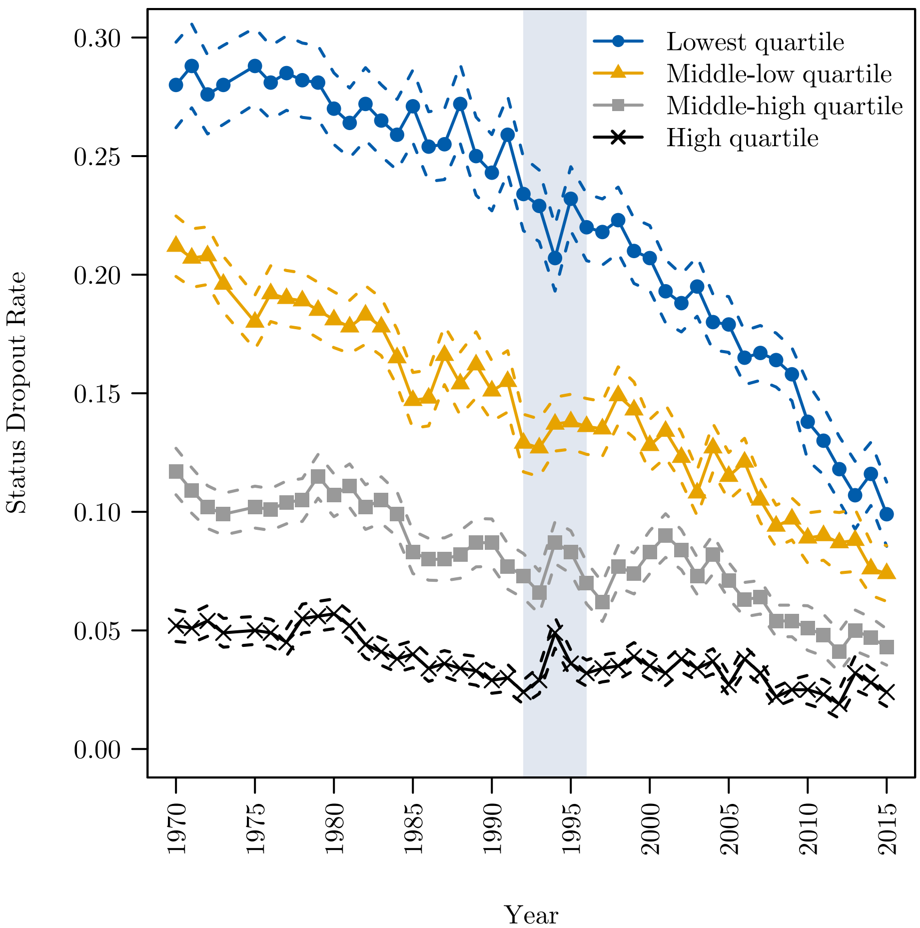
SSBG funds are primarily directed toward child-wellbeing services such as foster care or protection from abuse, yet the program allows states to fund a wide variety of social services, which also includes providing for childcare assistance. Before the welfare reform era, SSBG (or Title XX), was the leading funder of government subsidized child care, especially for AFDC recipients. Today, SSBG childcare makes up almost 20 percent of total SSBG spending, which amounts to less than 3 percent of the total spending from TANF and CCDF combined. Head Start, meanwhile, is a large program that increased dramatically during the 1990s (along with total childcare spending) and is comparable in magnitude to total CCDF spending. In the analysis, I ignore SSBG spending given the small size of the program and its shrinking role after welfare reform, and I assume that Head Start spending varies consistently across states and thus control only for the aggregate increases in Head Start allocations over time.

In addition to controlling for state-level expenditures as child development inputs, controls for potentially correlated outcomes, such as high school graduation rates, can further identify the relationship between childcare-specific spending and later education outcomes. In more recent years, the National Center for Education Statistics (NCES) has begun measuring high school graduation using a 4-year adjusted cohort graduation rate (ACGR), which is reported in the Common Core of Data. The income gap for high school completion has narrowed considerably in the last two decades, as shown in Figure 3.6. However, for economically disadvantaged students as of 2012, the ACGR can range from 58 percent in Nevada to 85 percent in Indiana.

3.6 Descriptive Evidence on State-Level Childcare Assistance Expenditure

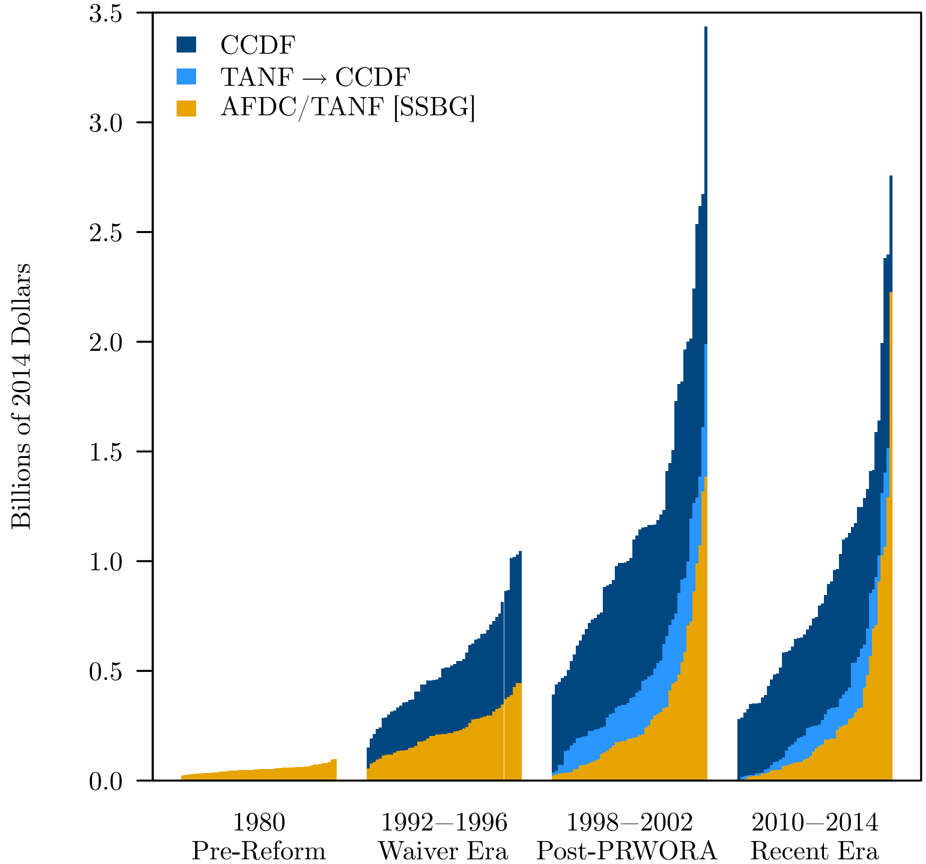
Public assistance for child care increased greatly after the Family Support Act of 1988, which resulted in more welfare participants working under the guarantee of subsidized child care between 1991 and 1996. This period of time coincides with the AFDC waiver era during which states began implementing state-wide reforms. After PRWORA was signed in 1996, childcare assistance increased again. However, the post-PRWORA period was also marked by increased state discretion on how to allocate TANF block grant funds (as opposed to the match grant AFDC program). Figure 3.7 shows the distribution of childcare assistance spending per child in poverty for the 50 U.S. states averaged within four time periods. In the period well before welfare reforms introduced work requirements, childcare spending was low and flat across states. The waiver era saw increased state generosity and more variation across states, and the post-PRWORA then saw more spending and even more variation. The most generous state in the five years after PRWORA spent over three times as much as the most generous state in the five years before PRWORA. Also evident in the years after 1997, especially in the most recent era, is that some states allocate almost no funds to TANF direct childcare assistance while other states use TANF as the predominant source of funds. The degree to which states

Figure 3.6. High School Status Dropout Rates
by Family Income Quartile, 1975-2012



Notes: High school status dropout rates shown above correspond to 16-24 year olds who have not completed high school and are not currently enrolled. The major waiver period of welfare reform is indicated by the shaded region. *Source:* Table 219.75. Percentage of high school dropouts among persons 16 to 24 years old (status dropout rate), by income level, and percentage distribution of status dropouts, by labor force status and years of school completed: 1970 through 2015, Digest of Education Statistics: 2015. U.S. Department of Education, National Center for Education Statistics.

Figure 3.7. State Distribution of Childcare Assistance per Child in Poverty, by Program and Time Period



Notes: For each time period above, the bars represent the distribution of average state expenditures on childcare assistance per child in poverty for the 50 U.S. states excluding the District of Columbia. The categories of expenditure are cumulative and ordered separately, that is, TANF→CCDF represent total AFDC/TANF [SSBG] direct childcare expenditure plus TANF transfers to CCDF. The major waiver period of welfare reform is indicated by "Waiver Era", and the "Post-PRWORA" period indicates the years after the Personal Responsibility and Work Opportunity Reconciliation Act of 1996. *Source:* U.S. Census Bureau, Annual Survey of State and Local Government Finances and Census of Governments; Statistical Abstract of the United States; and, various other government publications. (See Data Appendix for details.)

fund childcare assistance through TANF or CCDF relates directly to the degree to which those funds are restricted by federal policies.

Looking at the post-PRWORA period as a whole, Figure 3.8 illustrates the geographic variation for different measures of states' generosity for TANF childcare spending. The maps in Figure 3.8 show real TANF expenditure (including transfers to CCDF) per child in poverty in Panel A, the percent of TANF funds allocated toward child care in

Panel B, and the percent of total childcare assistance that comes from TANF in panel C. From a visual comparison, these measures seem to be highly correlated.[24] Florida stands out as being more generous than average for southern states, Oklahoma more than its midwestern neighbors, and New England tends to have the largest concentration of generous states in general. Some states like Idaho and Mississippi stand out as less generous in terms of spending per child, yet they allocate a higher than average portion of TANF spending toward child care. In panel C, note that the darkest shaded states are ones that contribute around half of all childcare assistance through the TANF program. In fact, five states fund over 50 percent of childcare assistance via TANF.[25]

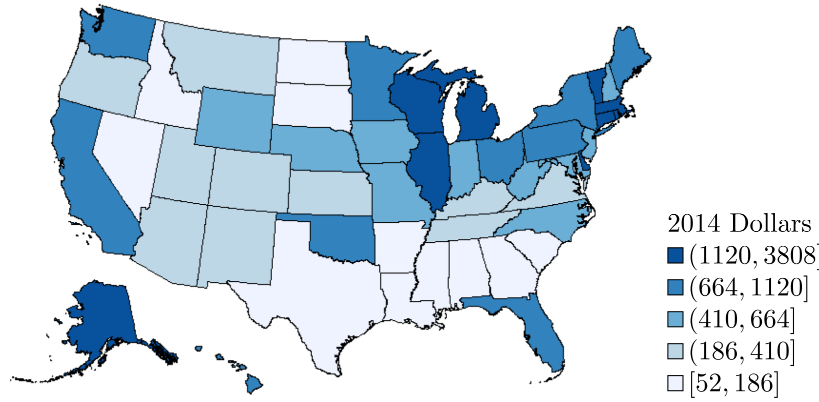
The degree to which the childcare assistance landscape changed after welfare reform is often understood as an increase in spending, yet the increase in variance of spending is a striking feature of reform's impact. Figure 3.9 shows that the median state spending shifted upward after PRWORA, and in the most recent years it has shifted back closer to 1996 levels. However, the upper tail of spending per child took off during the period from 1999 to 2002, and while it has tapered off since then, it has remained almost twice as high as the maximum spending in 1996. Regarding within state variation during the post-PRWORA period after 1996, most states have varied widely in spending per child. Figure 3.10 compares states along with a line depicting the U.S. median spending of approximately $897 per child in poverty. Connecticut has a large range over this period, which includes the maximum spending for all states and a minimum that is above the U.S. median. Texas has the smallest range of per-child expenditure, and it is also one

[24]The highest correlation is between panels A and C at 0.854, and the lowest correlation is between panels A and B at 0.648.

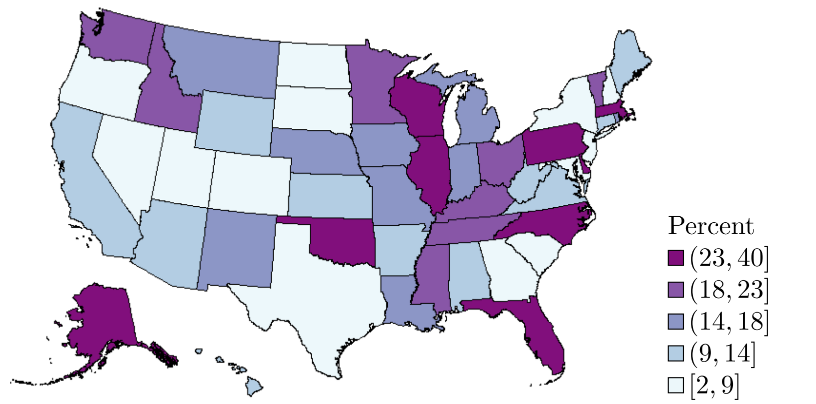
[25]TANF childcare expenditure and transfers are the majority funding sources for Michigan, Delaware, Massachusetts, Illinois, and Wisconsin, ranging from 50 to 54 percent of total expenditure. The District of Columbia funds 68 percent of childcare assistance through the TANF program.

Figure 3.8. Geographic Variation in Average Measures of TANF Childcare Generosity, 1997-2014

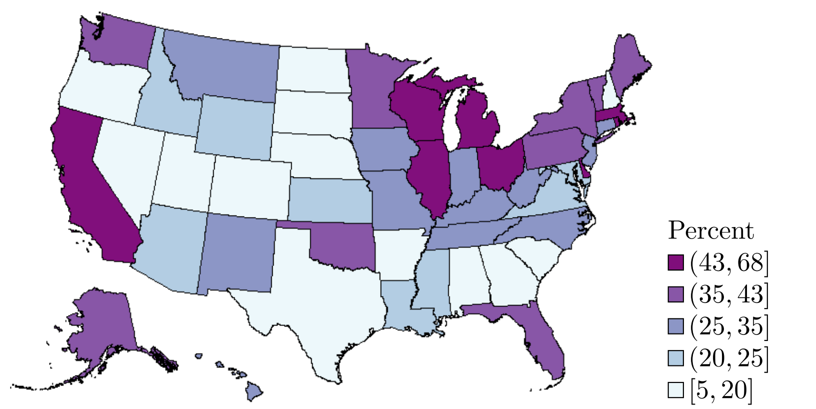
A. TANF Childcare Expenditure and CCDF Transfer Per Child in Poverty



B. TANF Childcare Expenditure and CCDF Transfer By Percent of Total TANF Expenditure



C. TANF Childcare Expenditure and CCDF Transfer By Percent of Total TANF and CCDF Childcare Expenditure



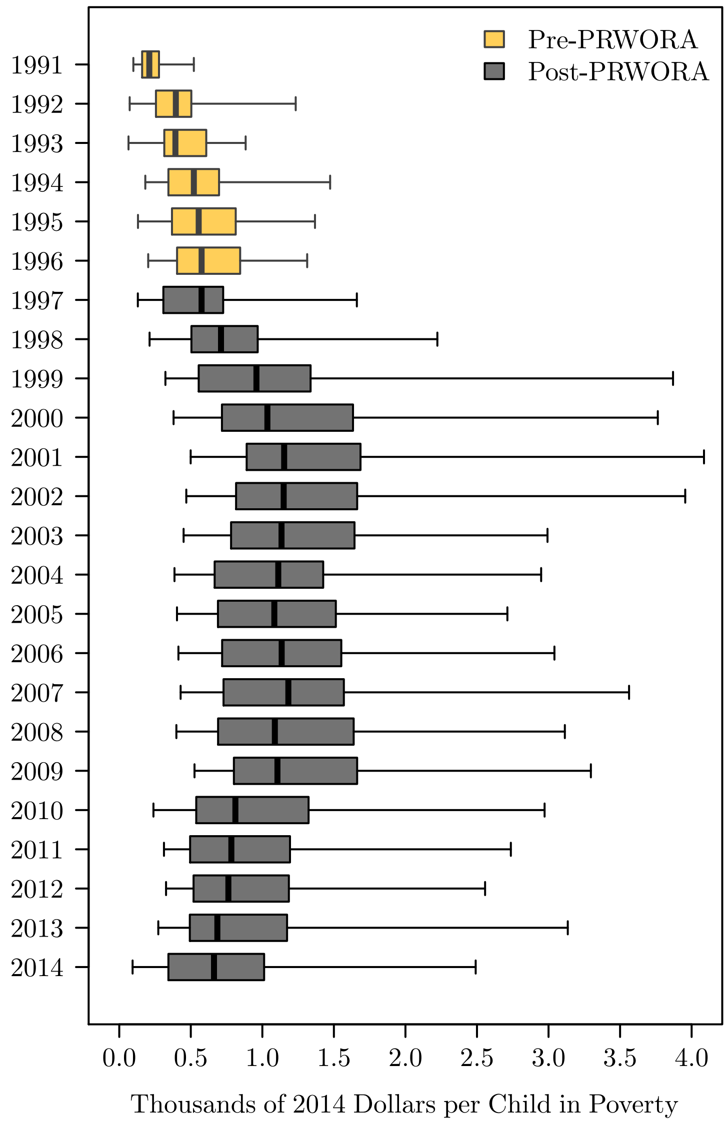
Source: U.S. Department of Health and Human Services; author's tabulations.

of the lowest funders of childcare assistance per child. Based on the variation evident in Figures 3.9 and 3.10, there is more fluctuation in state-level childcare policy from year-to-year than simply post-reform funds increasing and states separating in allocation behavior. The state allocation decision seems to be dynamic over time, and possibly related to changing priorities according to political administrations with the ability to shift block grant funds to meet different agendas.

After PRWORA, TANF has become less of a cash assistance program in comparison to AFDC, and it has taken up a significant role in childcare assistance alongside CCDF. Before proceeding to the effects of state spending on family childcare choices, the following figures explore the degree to which states allocate TANF funds in general. Figure 3.11 shows the percent of TANF allocations spent in broad categories by state over the period 1997 to 2014. States are ordered by the total percent allocated to childcare assistance and CCDF transfers, shown in light shades increasing from the right side of the figure, and cash assistance is shown from the left. Notice that for states like Delaware, Illinois, Wisconsin, and Oklahoma, childcare-related spending dominates cash assistance. States near the bottom of the figure, states like Texas and South Carolina are typically marked by large nonassistance expenditure with very little going toward child care and still small portions going toward cash assistance. Arkansas spends an especially large portion on nonassistance to the neglect of both cash and child care. Few states still prioritize TANF dollars on cash assistance, and only California and Maine spend over half in cash.

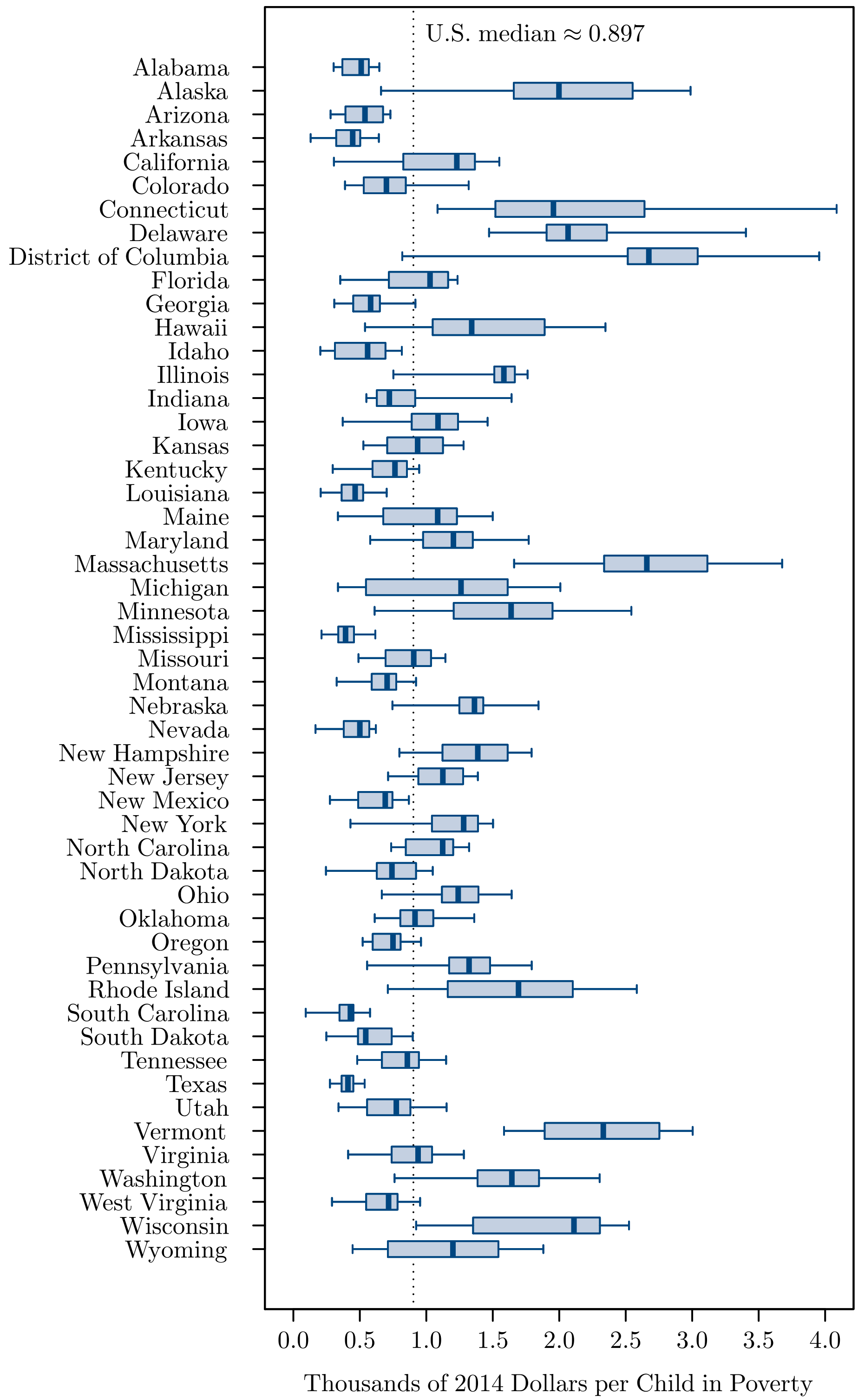
Just as childcare spending fluctuates over time, so do TANF allocations across all categories. For total U.S. TANF expenditures, Figure 3.12 provides a stacked plot where the height of each category contributes to the cumulative spending. This stratified visual

Figure 3.9. Within-Year State Variation in AFDC/TANF and CCDF Total Childcare Expenditure per Child in Poverty, 1991-2014



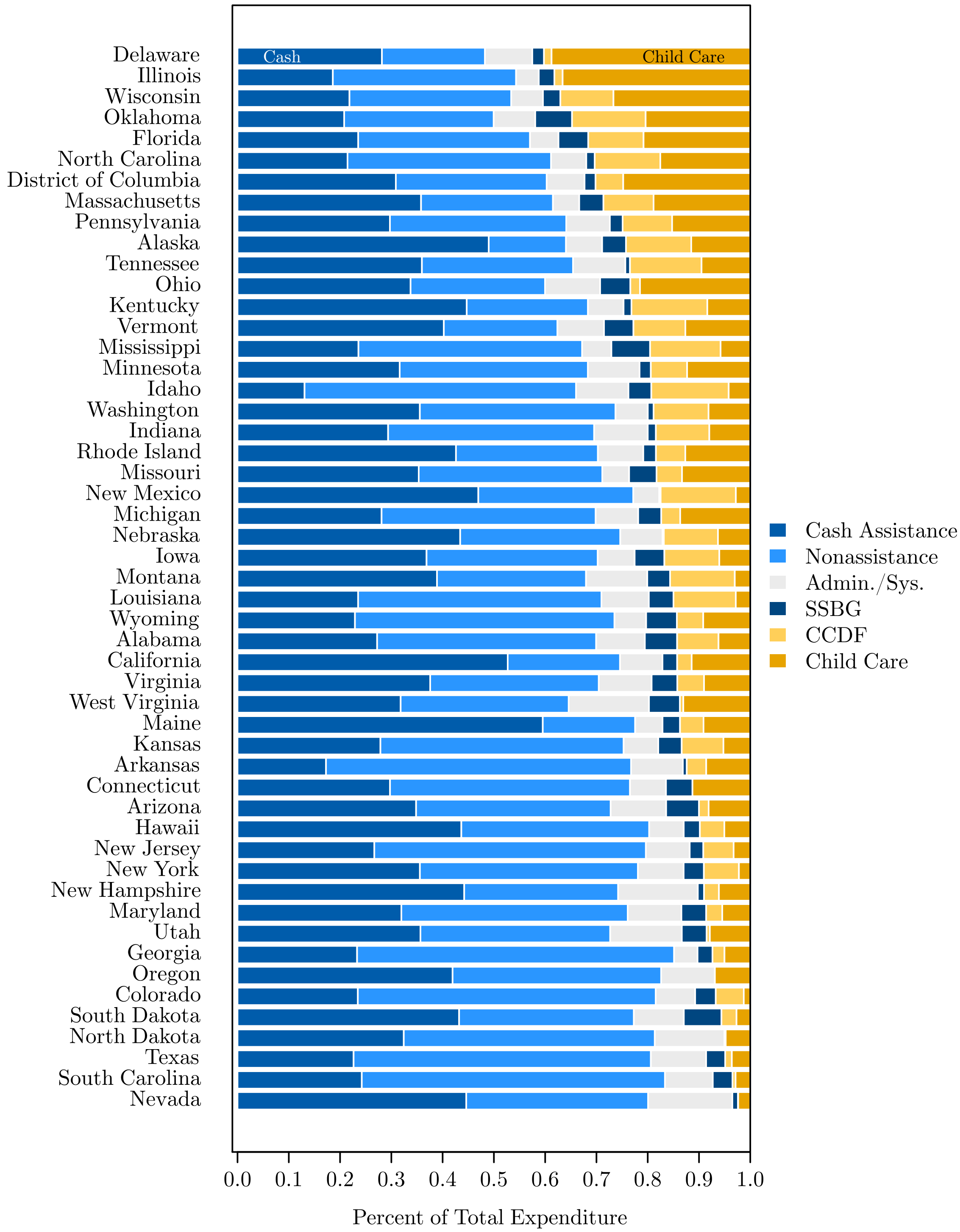
Notes: The box plot above represents the interquartile range and median of the distribution along with the minimum and maximum values. Different colors are used to highlight distributional differences pre- and post-PRWORA, the Personal Responsibility and Work Opportunity Reconciliation Act of 1996. *Source:* U.S. Department of Health and Human Services; Background Material and Data on Programs within the Jurisdiction of the Committee on Ways and Means. (See Data Appendix for details.)

Figure 3.10. Longitudinal Variation in State AFDC/TANF and CCDF Total Childcare Expenditure per Child in Poverty, 1997-2014



Notes: The box plot above represents the interquartile range and median of the distribution along with the minimum and maximum values. *Source:* U.S. Department of Health and Human Services; Background Material and Data on Programs within the Jurisdiction of the Committee on Ways and Means. (See Data Appendix for details.)

Figure 3.11. Distribution of Average TANF Expenditure
in Broad Categories by State, 1997-2014



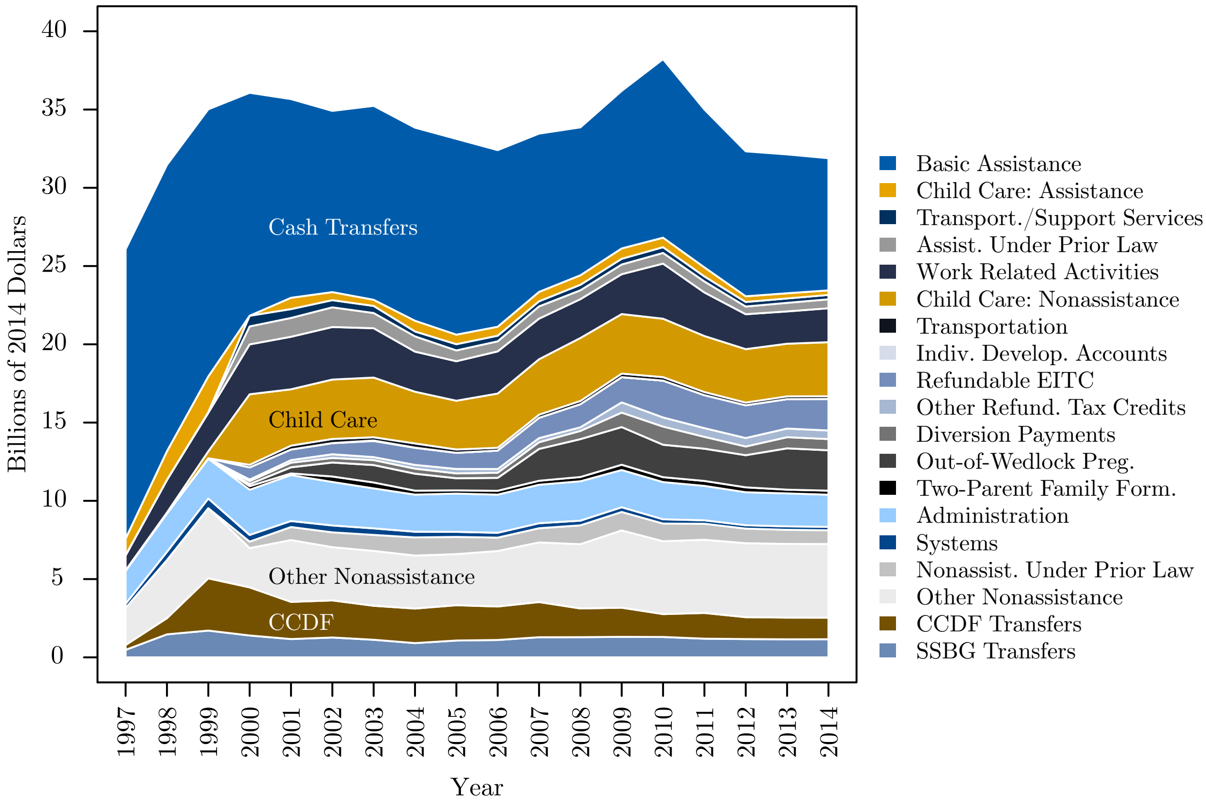
Notes: For the percentages of total expenditure above, "Cash Assistance" represents basic assistance only, and "Nonassistance" includes all other expenditure categories except for administration and systems ("Admin./Sys."), transfers to Social Service Block Grant (SSBG) and CCDF, and both "assistance" and "nonassistance" expenditure on child care. *Source:* U.S. Department of Health and Human Services; author's tabulations.

illustrates the rise in spending diversification during the early years of TANF, 1997 to 2000, and a general shrinking of cash assistance over time. After 2006, there seems to be a shift upward of some nonassistance categories, and again, a slight shrinking of CCDF transfers. The more telling visual of this change over time is to plot these allocations trends by state. Appendix Figure G.1 provides a similar figure, though instead of stacking the cumulative total spending, it depicts cumulative percent spending by category so that all plots compare state TANF allocation distributions from 0 to 100 percent. This figure alone, with plots for the 50 U.S. states from 1997 to 2014, is a strong motivation to understand more about how TANF funds are spent and what outcomes follow for the most vulnerable low-income families.

3.7 Childcare Assistance Expenditure Effects on Formal Care Arrangement

Along with an increasing trend in female labor force participation (Figure 3.1), the work requirements of 1990s welfare reforms provide a clear motivation for increases in childcare demand among single mothers. In particular, there is a pronounced increase in women with young children entering the labor force between 1993 and 1996. The reform effect is more evident among black families according to panel A of Figure 3.1, which is related to the higher prevalence of poverty and single-parent families with young children for this subsample. If the type of child care that working-poor single mothers choose matters for child development and second-generation outcomes, then a first-stage test would be to evaluate the extent to which childcare assistance affects the care utilization choices of this target population. Further, the time trends of state allocations toward child care may suggest differential outcomes for low-income families with young children.

Figure 3.12. TANF Expenditure by Detailed Categories of Assistance, Nonassistance, and Transfers, 1997-2014



Notes: Expenditure totals are stacked cumulatively to show the composition change in TANF spending over time. TANF expenditure categories of interest are cash assistance shown at the top, and childcare expenditure shown separately in assistance (just below cash transfers at the top), nonassistance (labeled in the middle), and transfers to CCDF (labeled near the bottom). Also, "other" nonassistance plays a large and growing role despite having no specific designation. *Source:* U.S. Department of Health and Human Services; author's tabulations.

3.7.1 Main First-Stage Results

In the main results, shown in Table 3.2, I consider the decision to use center-based child care for the sample of low-earning single mothers with children under 13 years old. The estimation sample corresponds to the post-PRWORA years 1998-2012, which abstracts away from some larger structural changes to welfare work requirements and childcare demand. State generosity for childcare assistance appears to have a robustly positive influence on a family's likelihood of using formal child care instead of unpaid or relative care. All specifications control for a basic set of family demographics—child's age, number of children, parent's education—along with fixed state and year effects.[26] Using basic family controls and state-year effects, column (1) indicates that an additional thousand dollars of childcare assistance per child in poverty would make families 14.2 percentage points more likely to use formal child care. After adding in further state-level controls for spending on education, public welfare, Head Start, and specific TANF and CCDF policies, column (2) shows a larger effect of childcare assistance spending with a 27.6 percentage point increase in formal care. The effects of family demographics on center-based care use are decreasing for older children and the presence of other adults, and increasing with mother's education.

The remaining columns of Table 3.2 apply stronger controls for family heterogeneity without weakening the influence of state childcare generosity. In column (3), detailed family controls from the PSID CDS are included to control for effects of the child's home, neighborhood, and parental influence. The subsample with CDS data is about one fourth of the baseline sample size such that columns (2) and (3) are not directly comparable, though the estimates are qualitatively similar with a somewhat larger point estimate: a 31.8 percentage point increase in formal care use. Columns (4)-(5) repeat the same specifications as (1)-(3) with the addition of individual fixed effects. Overall, the estimates are quantitatively similar regardless of the fixed-effect specification, especially in columns (2) and (5) with the full sample and state policy controls, which both have point estimates of 0.276 with standard errors of (0.083) and (0.141), respectively. The fixed-

[26]The full baseline set of individual/family controls used in the empirical specifications include: a quadratic in the child's age, an indicator for whether the child is female, an indicator for whether the child's race is reported as black, indicators for the number of children in the family unit and whether there is more than one adult, an indicator for urban residence, mother's educational attainment in years, and parents' marital status.

Table 3.2. Childcare Assistance Expenditure Effects on Center-Based Care for Children Under Age 13 with Low-Earnings Single Mothers, 1998-2012

	(1)	(2)	(3)	(4)	(5)	(6)
Childcare Assistance	0.142	0.276	0.318	0.160	0.276	0.400
(thousands per child)	(0.075)	(0.083)	(0.134)	(0.126)	(0.141)	(0.197)
Age of Child	−0.007	−0.009	−0.026	−0.047	−0.045	−0.049
	(0.010)	(0.010)	(0.033)	(0.021)	(0.025)	(0.048)
Age of Youngest Child	−0.033	−0.034	−0.039	−0.018	−0.019	−0.010
	(0.004)	(0.004)	(0.004)	(0.010)	(0.011)	(0.013)
Number of children = 2	0.024	0.021	0.032	0.025	0.030	0.210
	(0.026)	(0.025)	(0.048)	(0.071)	(0.072)	(0.141)
Number of children > 2	−0.063	−0.067	−0.077	0.015	0.018	0.198
	(0.030)	(0.030)	(0.050)	(0.079)	(0.079)	(0.164)
Other Adults in the Home	−0.058	−0.057	−0.086	−0.014	−0.011	0.074
	(0.031)	(0.032)	(0.057)	(0.045)	(0.045)	(0.090)
Mother's Education:	0.020	0.021	0.004			
High School	(0.029)	(0.029)	(0.030)			
Mother's Education:	0.077	0.079	0.047			
More than High School	(0.027)	(0.027)	(0.052			
Basic Family Controls	X	X	X	X	X	X
State Expenditures and Policies		X	X		X	X
Detailed CDS Family Controls			X			X
Individual Fixed Effect	No	No	No	Yes	Yes	Yes
Number of Individuals	2629	2604	664	1678	1661	624
Observations	5558	5501	1461	3872	3833	1383

Linear Probability Estimates for Center-Based Child Care Use by Low-Income Families At Percentiles of Average State Assistance Expenditure Per Child in Poverty

0.10	Nevada: $519/child	0.218	0.158	0.100	0.183	0.128	0.066
		(0.034)	(0.037)	(0.063)	(0.060)	(0.066)	(0.091)
0.25	Montana: $731/child	0.248	0.216	0.168	0.216	0.187	0.151
		(0.018)	(0.020)	(0.034)	(0.033)	(0.036)	(0.049)
0.50	Florida: $1,030/child	0.290	0.299	0.263	0.264	0.269	0.270
		(0.004)	(0.005)	(0.006)	(0.005)	(0.006)	(0.010)
0.75	Nebraska: $1,436/child	0.348	0.411	0.392	0.329	0.381	0.432
		(0.035)	(0.039)	(0.060)	(0.056)	(0.063)	(0.090)
0.90	Wisconsin: $2,145/child	0.448	0.607	0.617	0.443	0.577	0.716
		(0.088)	(0.097)	(0.155)	(0.146)	(0.163)	(0.229)

Notes: Robust standard errors with state clustering are shown in parentheses. The dependent variable is an indicator for whether the child's family used center-based or family home child care, and the sample is restricted to low-income families with positive earnings below 200 percent of the U.S. Census poverty threshold. The independent variable of interest is the total TANF and CCDF childcare assistance expenditure per child in poverty. Basic family controls used in all specifications include state and year fixed effects in addition to the child's age, age squared, sex, race, urban residence status, indicators for number of siblings and other adults living in the household, and indicators for parent's educational attainment. State expenditure and policy controls include measures of state spending per child with respect to Head Start, elementary/secondary education, and total public welfare; state TANF policies regarding school attendance, parental school involvement, childcare earnings disregards, and whether transitional childcare assistance is limited by earnings or time; and, CCDF policies regarding copay amounts, the presence of TANF exemptions for copay, family-size adjustments for copay, whether relatives are authorized to receive payments for in-home care, whether redetermination period is no longer than 6 months. Additional detailed family controls from the Child Development Supplement (CDS) include a home environment score, neighborhood score, measure of parental style (warmth), an indicator for low birth weight, and intelligence measures for parent and grandparent.

effect estimate loses some statistical precision, though both estimates are statistically significant at the 10-percent level (column (2) at the 1-percent level).

The lower panel of Table 3.2 estimates the linear probability for in-sample families' use of center-based child care given different levels of state assistance generosity based on percentiles of average expenditure during the sample time period. For example, based on column (5), if all states were as generous as Wisconsin, where $2,145 per child in poverty is at the 90th percentile of state spending, then approximately 58 percent of working-poor single mothers would use center-based care. At the median level of state generosity, about 27 percent would use formal care, and at the 10th percentile the number drops below 13 percent. As a comparison for these predicted probabilities, the sample average for center-based care use is about 28 percent, and the 10th and 90th percentile probabilities are 18 and 51 percent, respectively.[27]

3.7.2 Supplemental Results

Given that state-level childcare assistance has a strong effect on type of care arrangement, the following results explore how the effects vary by family poverty status, timing effects by source of funds, and a comparison just before and after PRWORA implementation.

As a direct comparison using the same specifications as in Table 3.2 columns (2) and (5), the results in Table 3.3 allow the sample to vary based on the family's earned income as a percent of the U.S. Census poverty threshold. The effect of state childcare assistance generosity on families below 100-percent poverty is larger than for families below 200-percent poverty when considering the linear probability estimates in columns (1) and (3), though not for the fixed-effect estimates in columns (2) and (4). These differences may

[27]Note that some states have rates of formal childcare utilization at over 70 percent. Sample means in Table 3.1 offer another comparison, although these statistics are not conditional on the mother working.

Table 3.3. Childcare Assistance Effects on Center-Based Care by Poverty Status for Children Under Age 13 with Working Single Mothers, 1998-2012

Poverty Status:	$< 100\%$		$< 200\%$		$\geq 300\%$	
	(1)	(2)	(3)	(4)	(5)	(6)
Childcare Assistance	0.310	0.181	0.276	0.276	0.128	0.054
(thousands per child)	(0.095)	(0.108)	(0.083)	(0.141)	(0.113)	(0.106)
Age of Child	0.002	−0.043	−0.009	−0.045	0.017	0.077
	(0.013)	(0.033)	(0.010)	(0.025)	(0.041)	(0.064)
Age of Youngest Child	−0.038	−0.018	−0.034	−0.019	−0.042	−0.016
	(0.005)	(0.011)	(0.004)	(0.011)	(0.016)	(0.045)
Number of children = 2	0.002	0.155	0.021	0.030	−0.172	0.078
	(0.036)	(0.072)	(0.025)	(0.072)	(0.061)	(0.250)
Number of children > 2	−0.112	−0.002	−0.067	0.018	−0.378	0.463
	(0.036)	(0.109)	(0.030)	(0.079)	(0.095)	(0.379)
Other Adults in the Home	−0.118	−0.083	−0.057	−0.011	0.164	0.473
	(0.036)	(0.053)	(0.032)	(0.045)	(0.108)	(0.133)
Mother's Education:	0.080		0.021		0.148	
High School	(0.045)		(0.029)		(0.093)	
Mother's Education:	0.099		0.079		0.241	
More than High School	(0.032)		(0.027)		(0.105)	
Individual Fixed Effect	No	Yes	No	Yes	No	Yes
Number of Individuals	1859	1171	2604	1661	306	209
Observations	3440	2332	5501	3833	485	338

Notes: Robust standard errors with state clustering are shown in parentheses. The dependent variable is an indicator for whether the child's family used center-based or family home child care. The independent variable of interest is the total TANF and CCDF childcare assistance expenditure per child in poverty. All specifications control for state and year fixed effects in addition to the child's age, age squared, sex, race, urban residence status, indicators for number of siblings and other adults living in the household, indicators for parent's educational attainment, as well as controls for state expenditures and policies. Specifically, state-specific measures include spending per child with respect to Head Start, elementary/secondary education, and total public welfare; state TANF policies regarding school attendance, parental school involvement, childcare earnings disregards, and whether transitional childcare assistance is limited by earnings or time; and, CCDF policies regarding copay amounts, the presence of TANF exemptions for copay, family-size adjustments for copay, whether relatives are authorized to receive payments for in-home care, whether redetermination period is no longer than 6 months. Poverty status is determined by family earnings as a percent of the U.S. Census poverty threshold by family size and year.

not be statistically significant, yet it would be reasonable to consider that family decisions for child care exhibit more heterogeneity for poorer families. The less financial (or social capital) resources a family has may cause more idiosyncratic responses to managing work, transportation, and accessible child care.

It is plausible that levels of government childcare funding could affect markets for providing quality care by changing the demand for services or by imposing regulations on providers, which could potentially make childcare assistance relevant beyond just the lower distribution of earners. Given that columns (5) and (6) of Table 3.3 still represent working single mothers, this might be the group most likely to be affected by such market changes for child care. However, the results indicate that single mothers with earnings above 300 percent of the poverty line do not seem to make childcare arrangements based on the level of state assistance. Another notable difference for single mothers by poverty status is that the mother's education becomes a much bigger decision-making factor for high earners who are about 24.1 percentage points more likely to use formal care if the mother has more than a high school education. Also, higher-earning single mothers seem to have an opposite response to additional adults in the home compared with lower-earning mothers. A potential explanation of the additional-adult effect is that the presence of unmarried partners in a home could increase family earnings that make child care more affordable without making shared childcare responsibilities more likely.

Returning now to focus on single-mother families below 200 percent poverty of the poverty line, Table 3.4 presents the effects of childcare assistance expenditure by source of funds and interacted with indicators by year in order to highlight trend effects for the period 2000-2012. This time period excludes 1998—the period just after reform—and interactions by year follow an assumption that families' time-varying unobserved reasons for choosing care arrangements is stable across years. Overall, the results in Table 3.4 suggest a decreasing trend in the effectiveness of childcare assistance at encouraging formal care among low-income single mothers. Columns (1) and (2) represent TANF direct childcare expenditure excluding block grant transfers to CCDF, columns (3) and (4) represent CCDF expenditure inclusive of TANF transfers, and total combined expenditures are shown in columns (5) and (6). The point estimates for TANF's effect on formal child care use are generally below CCDF estimates, which likely reflects the increased flexibility states have to allocate funds through TANF without being regulated by CCDF policies. TANF funds may be more targeted toward moving mothers into work than moving children into center-based care. Note, also, that during this time period, states are decreasing CCDF expenditure on average and keeping TANF childcare assistance relatively consistent. These parallel trends highlight the fact that state funds to aid low-income families may face reallocations over time to meet heterogeneous priorities for different states.

In another comparison of childcare assistance funds across years, Table 3.5 compares the effect of state spending just before and after PRWORA.[28] Columns (1)-(2) show childcare spending for AFDC in 1996 or TANF in 1998 (excluding transfers to CCDF), columns (3) and (4) show CCDF spending inclusive of TANF transfers in 1998 (where CCDBG is the CCDF pre-cursor), again the total combined spending of these programs is shown in the remaining columns (5)-(6). In order to compare the pre- and post-PRWORA periods, I use a difference-in-difference-type specification with childcare spending interacted with an indicator for whether the year is 1998. Therefore, the coefficients on *Childcare Assistance* correspond to pre-PRWORA effects, the interaction term compares the effect sizes before and after, and the calculated 1998 effect is shown below along with standard errors constructed by delta method. There is no statistically significant evidence that childcare assistance affects the care arrangement chosen by low-income single mothers before PRWORA, however, the magnitudes for CCDF and total childcare assistance in the fixed-effect models are comparable to some later-year estimates in Table 3.4. The coefficients on the post-PRWORA interaction term indicate that TANF childcare had a

[28]While previous estimates have used a center-based care variable from the PSID family survey, those data are available only for 1998 onward. The variable for childcare arrangement in 1996 comes from the CDS survey.

Table 3.4. Childcare Assistance Effects by Year, 2000-2012, on Center-Based Care of Children Under Age 13 with Low-Earnings Single Mothers

Expenditure Source:	TANF		CCDF		Total Combined	
	(1)	(2)	(3)	(4)	(5)	(6)
Childcare Assistance ×						
($Year = 2000$)	0.436	0.361	0.393	0.603	0.172	0.455
	(0.139)	(0.200)	(0.110)	(0.202)	(0.361)	(0.183)
($Year = 2002$)	0.315	0.311	0.380	0.409	0.039	0.391
	(0.178)	(0.168)	(0.118)	(0.203)	(0.282)	(0.169)
($Year = 2004$)	0.463	0.320	0.409	0.628	0.046	0.437
	(0.198)	(0.187)	(0.131)	(0.239)	(0.268)	(0.187)
($Year = 2006$)	0.265	0.194	0.310	0.433	−0.045	0.353
	(0.221)	(0.176)	(0.134)	(0.261)	(0.263)	(0.186)
($Year = 2008$)	0.179	0.228	0.301	0.285	−0.111	0.288
	(0.175)	(0.205)	(0.127)	(0.225)	(0.245)	(0.163)
($Year = 2010$)	0.137	0.142	0.258	0.199	−0.290	0.220
	(0.231)	(0.261)	(0.159)	(0.185)	(0.314)	(0.175)
($Year = 2012$)	0.129	0.104	0.246	0.133	−0.408	0.169
	(0.143)	(0.255)	(0.131)	(0.174)	(0.386)	(0.194)
Individual Fixed Effect	No	Yes	No	Yes	No	Yes
Number of Daughters	1401	2321	2321	2321	2321	2321
Observations	3146	4767	4767	4767	4767	4767

Notes: Robust standard errors with state clustering are shown in parentheses. The dependent variable is an indicator for whether the child's family used center-based or family home child care, and the sample is restricted to low-income families with positive earnings below 200 percent of the U.S. Census poverty threshold. The independent variables of interest are the interactions of indicators by year with the childcare assistance expenditure per child in poverty, which is defined as TANF direct expenditures in columns (1)-(2), CCDF expenditures including TANF transfers in columns (3)-(4), and total TANF and CCDF expenditures in columns (5)-(6). All specifications control for state fixed effects in addition to the child's age, age squared, sex, race, urban residence status, indicators for number of siblings and other adults living in the household, indicators for parent's educational attainment, as well as controls for state expenditures and policies. Specifically, state-specific measures include spending per child with respect to Head Start, elementary/secondary education, and total public welfare; state TANF policies regarding school attendance, parental school involvement, childcare earnings disregards, and whether transitional childcare assistance is limited by earnings or time; and, CCDF policies regarding copay amounts, the presence of TANF exemptions for copay, family-size adjustments for copay, whether relatives are authorized to receive payments for in-home care, whether redetermination period is no longer than 6 months.

Table 3.5. Childcare Assistance Effects on Center-Based Care for Children Under Age 13 with Low-Earnings Single Mothers, Pre- and Post-PRWORA

Expenditure Source:	AFDC/TANF		CCDBG/CCDF		Total Combined	
	(1)	(2)	(3)	(4)	(5)	(6)
Childcare Assistance	−0.267	0.088	−0.075	0.242	−0.046	0.139
	(0.237)	(0.288)	(0.292)	(0.270)	(0.130)	(0.137)
Post-PRWORA (1998)	0.171		0.155		0.152	
	(0.082)		(0.085)		(0.085)	
Childcare Assistance ×	0.900	0.699	0.339	0.121	0.259	0.139
Post-PRWORA (1998)	(0.193)	(0.263)	(0.251)	(0.264)	(0.112)	(0.130)
Individual Fixed Effect	No	Yes	No	Yes	No	Yes
Assistance Effect in 1998:	0.804	0.788	0.419	0.363	0.365	0.279
	(0.181)	(0.163)	(0.134)	(0.095)	(0.104)	(0.052)
Number of Daughters	1401	2321	2321	2321	2321	2321
Observations	3146	4767	4767	4767	4767	4767

Notes: Robust standard errors with state clustering are shown in parentheses. The dependent variable is an indicator for whether the child's family used center-based or family home child care, and the sample is restricted to low-income families with positive earnings below 200 percent of the U.S. Census poverty threshold. The independent variables of interest are the interactions of indicators by year with the childcare assistance expenditure per child in poverty, which is defined as TANF direct expenditures in columns (1)-(2), CCDF expenditures including TANF transfers in columns (3)-(4), and total TANF and CCDF expenditures in columns (5)-(6). All specifications control for state fixed effects in addition to the child's age, age squared, sex, race, urban residence status, indicators for number of siblings and other adults living in the household, indicators for parent's educational attainment, as well as controls for state expenditures and policies. Specifically, state-specific measures include spending per child with respect to Head Start, elementary/secondary education, and total public welfare; state TANF policies regarding school attendance, parental school involvement, childcare earnings disregards, and whether transitional childcare assistance is limited by earnings or time; and, CCDF policies regarding copay amounts, the presence of TANF exemptions for copay, family-size adjustments for copay, whether relatives are authorized to receive payments for in-home care, whether redetermination period is no longer than 6 months.

much increased impact over the AFDC program, and the CCDF program has a large and statistically significant impact after PRWORA, as well. Note that the effects of childcare assistance before and after PRWORA are negatively correlated, which is evident by comparing the smaller standard errors for the estimated impact in 1998 (for example, s.e. $= 0.052$ in column (6), which is smaller than both the childcare estimate and its interaction with year).

One puzzle going forward is how to evaluate the impact of TANF childcare assistance considering that (i) participation in TANF is difficult to measure given existing survey questions and the prevalence of misclassification error; and, (ii) it is also inherently difficult to measure both the quality of, and intensity of exposure to, child care. As an example of some descriptive evidence, Table 3.6 shows the probability that an individual over age 18 from a low-income, single-mother family graduates high school or attends any college (by 2015) conditional on TANF participation and child care use during ages 5-12 using data from the year 2000. Without controlling for confounding factors by state or family, the unconditional means in columns (1) and (2) imply that formal child care correlates with higher educational attainment for this population. The informal child care group graduates at a rate of 76 percent compared to about 90 percent for those using formal care.[29] The story on TANF participation is mixed. Participating in TANF and using formal child care is still better than TANF and informal care, though it is worse than having neither TANF nor formal care. The fact that TANF children face unconditionally lower educational attainment is a common result of negative selection into public assistance. However, the problem of disentangling the effects of selection and misclassification can be exacerbated by data constraints. If obtaining quality child care is a function of search time and transportation time, then the effects of childcare assistance may be diminishing at the lowest end of the income distribution, or particularly, for families selecting into TANF participation.

3.8 Discussion and Future Work

Conditional on state childcare policies and related expenditure, as well as rich family controls plus fixed state and year effects, public assistance for child care significantly influences its target population, low-earning single mothers, toward more formal childcare arrangements such as center-based or family home care. A prominent effect of 1990s welfare reform is that these single mothers are working more and substituting their own

[29] For comparison, among all families above the 200-percent poverty threshold, the high school graduation rate is about 93 percent, and the rate of any college attendance is about 85 percent.

Table 3.6. Education Outcomes Conditional on Prior TANF Participation and Child Care Among Children Ages 5-12 in Low-Income, Single Mother Families in 2000

Sample Means in 2015	(1)	(2)	(3)	(4)
Graduating High School	0.760	0.898	0.625	0.704
	(0.429)	(0.305)	(0.486)	(0.459)
Attending Any College	0.620	0.768	0.326	0.592
	(0.487)	(0.426)	(0.470)	(0.494)
TANF Participation?	No	No	Yes	Yes
Center-Based Child Care?	No	Yes	No	Yes
Observations	119	53	149	85

Notes: Sample means are weighted by the individuals PSID core longitudinal weight, and standard deviations are shown in parentheses. Children are considered low-income in 2000 if family earnings are below 200 percent of the U.S. Census poverty threshold by family size.

time with children for either formal or informal care arrangements. Given that a large portion of childcare assistance is funded via block grants to states, there is substantial heterogeneity in how resources are allocated, which may have implications for the quality of care options available to economically vulnerable families.

After establishing the potential for childcare assistance expenditure and state policies as first-stage determinants of family childcare arrangements, a logical next step would be to use this variation to instrument for endogenous family care choices as they affect educational outcomes for the children. Studies have shown the long-term benefits of quality child care and education for disadvantaged children, yet there has been no nationally-representative evidence from non-experimental data demonstrating this effect. Moreover, I plan to explore potential heterogeneous effects relative to TANF and CCDF funding, which may be related to diverging program goals across states. Another issue for children who receive early interventions is that they may subsequently continue along in an educational system that delivers lower-quality preparation for economic success. In terms of data availability, the PSID offers rich family characteristics in the CDS and

TA surveys, though even more specific data on families' county of residence, local school quality, and educational outcomes can be obtained in the restricted-use PSID.

An important policy relevance goal of this work is to shed light on the longer-term implications of some recent trends in childcare assistance. Early on during the welfare reform era, large shares of expenditures were allocated toward supporting childcare costs for the working poor. Since then, total spending has declined and the proportion of spending allocated under TANF has increased. Moreover, the variation over states and time in the levels of childcare assistance is a byproduct of the block grant funding mechanism. Since block grants and work-conditioned welfare loom large in current welfare debates, more evidence is warranted on how well the most vulnerable families are able to invest in their children's development.

APPENDIX FOR ESSAY 1

A Notes on Misclassification Bias Corrections

Estimates based on equation (1.1) rely on self-reported data for a daughter's welfare participation at time t and her mother's self-reported participation at any point prior to time t,

$$W_{ist}^{d} = \alpha + \beta' \mathbf{x}_{ist}^{d} + \delta W_{is,\forall jt}^{m} + \gamma R_{st}^{m} + \theta R_{st}^{m} W_{is,\forall jt}^{m} + \mu_{s}^{d} + \rho_{t}^{d} + \nu_{ist}^{d},$$

where $W_{is,\forall jt}^{m} = \max \left\{ W_{is,t-1}^{m}, W_{is,t-2}^{m}, W_{is,t-3}^{m}, \ldots \right\}$. Let the true participation status be denoted $\tilde{W}_{ist}^{d}$ for daughter at time t, $\tilde{W}_{ist}^{m}$ for mother at time t, and $\tilde{W}_{is,\forall jt}^{m}$ for mother at any time prior to time t. In principle, both W_{ist}^{d} and W_{ist}^{m} can be affected by misclassification error. However, as demonstrated below, $W_{is,\forall jt}^{m}$ does not represent a challenge for point estimation as long as individuals have some positive probability of truthfully reporting welfare participation at time t.

To fix ideas, consider for simplicity $t = 3$ with $j \in \{1, 2\}$ and let the probability of truthfully reporting participation be defined as $q = \Pr\left(W_{ist}^{m} = 1 \middle| \tilde{W}_{ist}^{m} = 1 \right) 0$. In this case, the mother's measure of any prior participation at $t = 3$ will be accurately reported with probability

$$\Pr\left(W_{is,\forall j3}^{m} = 1 \middle| \tilde{W}_{is,\forall j3}^{m} = 1 \right) = \Pr\left(W_{is1}^{m} = 1 \middle| \tilde{W}_{is1}^{m} = 1 \right) + \Pr\left(W_{is2}^{m} = 1 \middle| \tilde{W}_{is2}^{m} = 1 \right)$$
$$- \Pr\left(W_{is1}^{m} = 1 \middle| \tilde{W}_{is1}^{m} = 1 \right) \Pr\left(W_{is2}^{m} = 1 \middle| \tilde{W}_{is2}^{m} = 1, W_{is1}^{m} = 1, \tilde{W}_{is1}^{m} = 1 \right).$$

Denoting $\Pr\left(W_{is2}^{m} = 1 \middle| \tilde{W}_{is2}^{m} = 1, W_{is1}^{m} = 1, \tilde{W}_{is1}^{m} = 1 \right) = r$, it follows that

$$\Pr\left(W_{is,\forall 3}^{m} = 1 \middle| \tilde{W}_{is,\forall j3}^{m} = 1 \right) = q(2 - r)q = \Pr\left(W_{is3}^{m} = 1 \middle| \tilde{W}_{is3}^{m} = 1 \right).$$

The argument can now be generalized assuming, again for simplicity in exposition, that $q = r$. The probability of ever truthfully reporting welfare participation under the above conditions can be expressed (based on the inclusion-exclusion principle for the union of finite events (Billingsley, 1995, pg. 24)) as

$$Q_t(q) \equiv \Pr\left(W^m_{is,\forall jt} = 1 \middle| \tilde{W}^m_{is,\forall jt} = 1\right) = \sum_{j=1}^{t-1} (-1)^{j-1} \binom{t-1}{j} q^j,$$

$$\text{where } \binom{t-1}{j} = \frac{(t-1)!}{j!(t-1-j)!},$$

which is increasing in the number of time periods observed. For my analysis, the mother's minimum number of time periods is five years, and for the average reporting rate for 1970-2000 (Meyer, Mok, and Sullivan, 2015), the probability is $Q_5(q = 0.668) \approx 0.996$, or for the minimum reporting rate over that time period, $Q_5(q = 0.318) \approx 0.852$. Given that mothers are observed for about 14 years on average prior to the daughter's participation decision, the probability that a mother truthfully reports any prior participation tends to 1, as shown in Figure A.1.

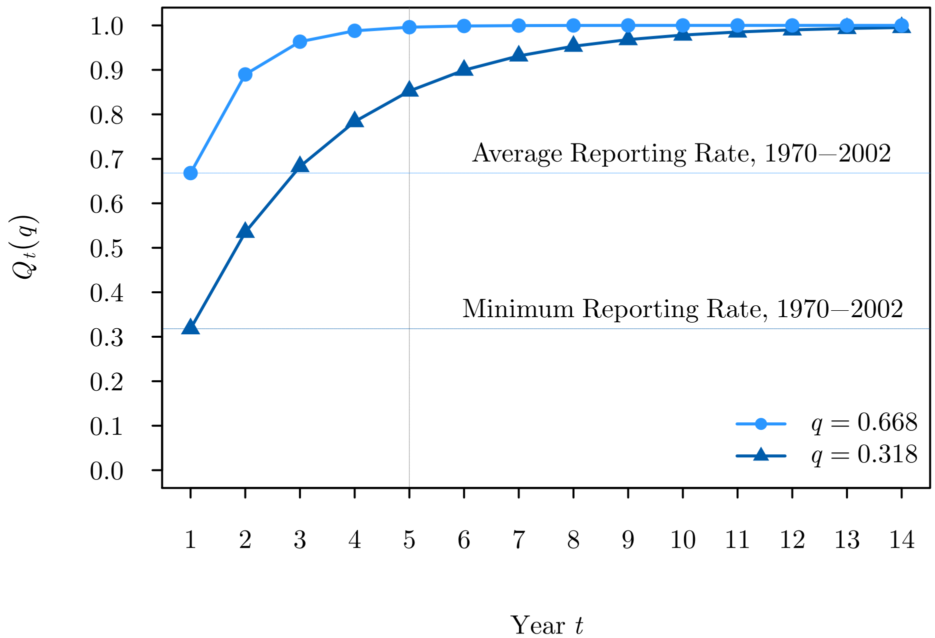
Therefore, the following bias-correction approach focuses on misclassification in the binary dependent variable for daughter's current welfare status. The probability that a daughter reports participating in welfare can be written as

$$\begin{aligned}\Pr\left(W^d_{ist} = 1\right) = \Pr\left(W^d_{ist} = 1 \middle| \tilde{W}^d_{ist} = 1\right) \Pr\left(\tilde{W}^d_{ist} = 1\right) \\ + \Pr\left(W^d_{ist} = 1 \middle| \tilde{W}^d_{ist} = 0\right) \Pr\left(\tilde{W}^d_{ist} = 0\right),\end{aligned}$$

where false negatives are defined as $\tau_{1,ist} := \Pr\left(W^d_{ist} = 0 \middle| \tilde{W}^d_{ist} = 1\right)$ and false positives are defined as $\tau_{0,ist} := \Pr\left(W^d_{ist} = 1 \middle| \tilde{W}^d_{ist} = 0\right) = 0$ by assumption.[1] This assumption is standard in the literature as false positive reports are relatively small, and these misreports typically correspond to individuals who mistake the source or timing of actual welfare participation.

[1]Note that whereas q is assumed fixed for the purposes of exposition above, false negatives here can be shown equivalently as $\tau_{1,ist} = 1 - q_{ist}$.

Figure A.1. Mother's Welfare Participation Reporting Rate for Periods $1, \ldots, t$ as a Function of the Number of Periods t Observed



Notes: Average and minimum PSID reporting rates for AFDC/TANF participation are summary statistics based on estimates from Meyer et al. (2015); see Table A.1. The estimation sample selects only those mothers observed at least 5 years, $t \geq 5$, while the daughter is ages 12-18 and living at home.

Therefore, using equation (1.1) and $\tau_{1,ist}$, the daughter's probability of reported welfare participation can be rewritten as

$$\Pr\left(W_{ist}^{d}=1\right)=\left[1-\tau_{1,ist}\right]\left[\alpha+\beta'\mathbf{x}_{ist}^{d}+\delta W_{is,\forall jt}^{m}+\gamma R_{st}^{m}+\theta R_{st}^{m}W_{is,\forall jt}^{m}+\mu_{s}^{d}+\rho_{t}^{d}\right].$$

Estimation of the previous equation proceeds in two steps. The first step estimates misclassification probabilities based on estimates of AFDC/TANF reporting rates in the PSID by Meyer et al. (2015) considering that $\mathbb{E}\left[\tau_{1,ist}\right]=\tau_{1t}$. Table A.1 shows the reporting rates used in estimation. In the second stage, I estimate the parameter of interest, (δ,γ,θ), by estimating the model of W_{ist}^{d} on weighted independent variables including a weighted intercept $\left[1-\hat{\tau}_{1t}\right]\alpha$, $\left[1-\hat{\tau}_{1t}\right]\mu_{s}^{d}$, and $\left[1-\hat{\tau}_{1t}\right]\rho_{t}^{d}$.

Table A.1. PSID Reporting Rates Taken as Given
for Misclassification Bias Correction Estimates

	AFDC/TANF			Food Stamps/SNAP		
	Meyer et al. (2015)			Meyer et al. (2015)		
Year	Transfers	Cases	Parameter	Transfers	Cases	Parameter
1975	0.646		0.722	0.779		0.773
1976	0.662		0.740	0.734		0.728
1977	0.630		0.704	0.754		0.748
1978	0.661		0.739	0.772		0.766
1979	0.642		0.717	0.782		0.776
1980	0.700		0.782	0.761	0.782	0.755
1981	0.699		0.781	0.761	0.780	0.755
1982	0.679		0.759	0.832	0.841	0.826
1983	0.708		0.791	0.808	0.817	0.802
1984	0.631		0.705	0.830	0.784	0.824
1985	0.594		0.664	0.817	0.786	0.811
1986	0.587		0.656	0.818	0.841	0.812
1987	0.555		0.620	0.871	0.846	0.864
1988	0.620		0.693	0.862	0.847	0.855
1989	0.576		0.644	0.982	0.845	0.974
1990	0.586		0.655	0.857	0.770	0.850
1991	0.612		0.684	0.756	0.681	0.750
1992	0.600		0.671	0.731	0.720	0.725
1993	0.528	0.605	0.590	0.621	0.700	0.616
1994	0.474	0.569	0.530	0.662	0.686	0.657
1995	0.493	0.539	0.551	0.632	0.652	0.627
1996	0.541	0.572	0.605	0.572	0.604	0.568
1997			0.508	0.509	0.522	0.505
1998	0.369	0.403	0.412	0.563	0.561	0.559
1999			0.387	0.654	0.535	0.649
2000	0.323	0.445	0.361	0.617	0.583	0.612
2001			0.350	0.592	0.573	0.587
2002	0.303	0.343	0.339	0.744	0.595	0.738
2003	0.387	0.458	0.432	0.685	0.719	0.680
2004	0.487	0.510	0.544	0.718	0.807	0.712
2005	0.285	0.285	0.318	0.688	0.635	0.683
2006	0.395	0.365	0.441	0.693	0.758	0.688
2007			0.472	0.742	0.794	0.736
2008	0.450	0.497	0.503	0.777	0.791	0.771
2009			0.486	0.704	0.764	0.699
2010	0.419	0.504	0.468	0.648	0.713	0.643
2011			0.477			0.671
2012			0.473			0.657

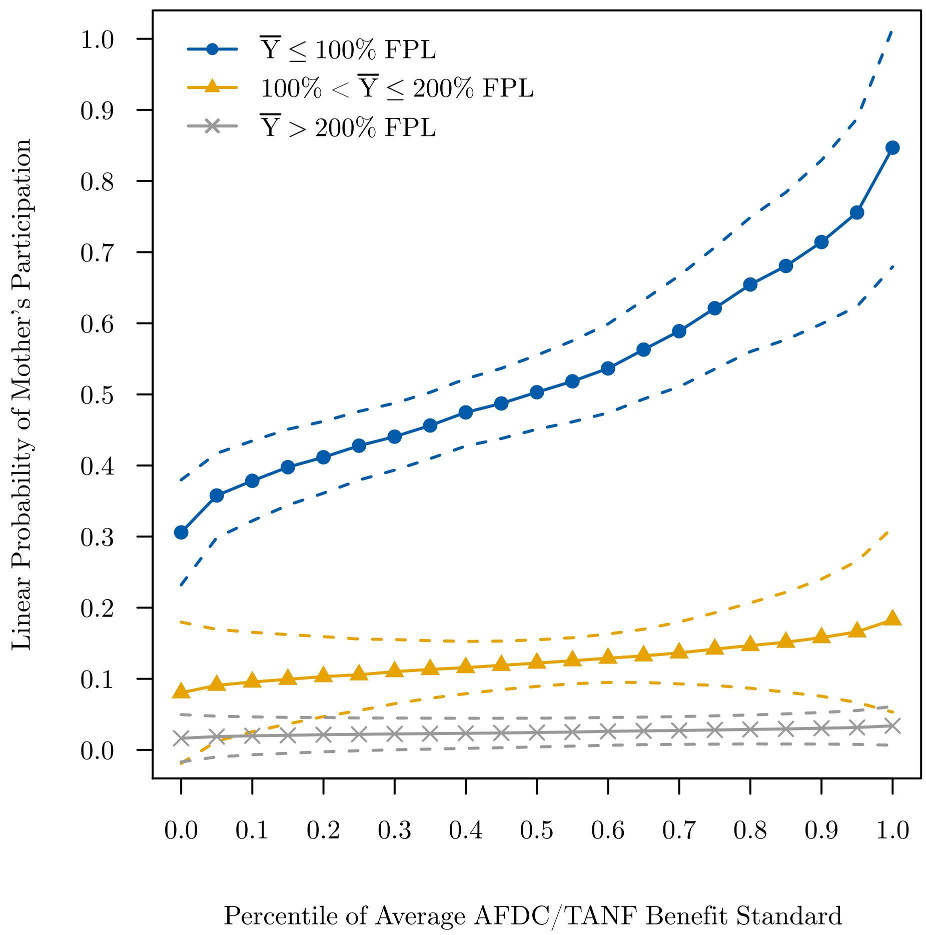
Notes: PSID reporting rates for dollar amount in transfers and number of cases for AFDC/TANF and food stamps/SNAP are estimated in Meyer et al. (2015). The estimation parameter used in misclassification bias correction estimates, $(1 - \hat{\tau}_{1t})$, is the imputed reporting rate. The imputed rate is equal to the reporting rate for transfers in the first column adjusted by the average ratio of the reporting rates for transfers and cases given the years with available data, which is approximately 1.118 for AFDC/TANF and 0.992 for food stamps/SNAP. In years where both rates for amounts and cases are missing, I linearly interpolate between observed years and use a two-year moving average for the last years.

B Additional Results and Robustness Checks

As referenced throughout the paper, the following section introduces additional results that explore the sensitivity of the main findings. The qualitative results of welfare reform are consistent: there is a causal influence from mother's welfare participation, and reform attenuates this transmission by more than 50 percent in levels and about 30 percent above baseline probabilities given the mechanical change in participation after reform.

Figure B.1 demonstrates the relationship between mother's welfare participation and the main policy instrument of AFDC/TANF benefit generosity. Table B.1 shows the first stage results for the mother's AFDC/TANF participation decision for instrumental variable estimates in Table 1.2. In Table B.2, I compare estimates for different sets of instrumental variables, which are key to identifying the effect of mother's participation given her selection into welfare. Then, in Table B.3 I re-estimate the baseline IV model including mother's variables related to her lifetime earnings ability: an indicator for less than high school education and an indicator for any prior family income below 200 percent of the Census poverty threshold by family size. Next in Table B.4, I re-estimate the baseline specifications from Table 1.2 without using the daughter's PSID core longitudinal survey weights, first for the full baseline sample including the Survey of Economic Opportunity (SEO), which oversamples low-income, minority families, and then for only the Survey Research Center (SRC) subsample, which is nationally representative. In Table B.5 I re-estimate the baseline results in Table 1.2 for a sample of eldest daughters only. Eldest daughters have the most opportunity to continue learning from their mothers' participation after leaving home since there may still be younger siblings living with the mother, and this sample abstracts away from larger families being overrepresented in the data. Table B.6 estimates the intergenerational transmission of welfare participation for the subsample of daughters whose mothers were more likely to participate based on lifetime education and family income. In Table B.7, I present a falsification exercise including a mother's future welfare participation. Lastly, in Table B.8 I present estimates of the baseline IV models imposing different levels of minimum years required for mother and daughter to be observed living together before the daughter forms her own family.

Figure B.1. Mother's Welfare Participation
Relative to AFDC/TANF Benefit Levels



Notes: Linear probability estimates are shown for the mother's indicator for any prior AFDC/TANF participation conditional on an average measure of AFDC/TANF benefit standard while the daughter is aged 12-18 along with the baseline controls of state and year effects as well as the daughter's quadratic in age and indicators for her number of children. The predicted probabilities are estimated for subsamples by the mother's ratio of mean family income, $\bar{Y}$, relative to the mean Federal Poverty Line (FPL) across all observation years. Dashed lines represent 95 percent pointwise confidence intervals with state-level clustering.

Table B.1. First Stage Instrumental Variable Estimates for
Mother's AFDC/TANF Participation Decision

Second Stage Dependent Variable:	Daughter's AFDC/TANF		Daughter's AFDC/TANF, SNAP, SSI	
	(1)	(2)	(3)	(4)
Avg. AFDC/TANF	0.548	0.532	0.741	0.733
	(0.087)	(0.096)	(0.081)	(0.082)
Reform × Avg. AFDC/TANF	0.199	0.231	0.263	0.281
	(0.122)	(0.114)	(0.108)	(0.104)
Max. AFDC/TANF	−0.385	−0.385	−0.637	−0.633
	(0.117)	(0.114)	(0.101)	(0.099)
Reform × Max. AFDC/TANF	−0.137	−0.170	−0.162	−0.179
	(0.108)	(0.104)	(0.108)	(0.106)
Avg. EITC	0.058	0.039	0.082	0.075
	(0.040)	(0.038)	(0.051)	(0.051)
Reform × Avg. EITC	−0.030	−0.018	−0.035	−0.030
	(0.046)	(0.041)	(0.053)	(0.053)
Max. EITC	−0.030	−0.023	−0.058	−0.055
	(0.030)	(0.027)	(0.029)	(0.029)
Reform × Max. EITC	0.023	0.015	0.018	0.014
	(0.034)	(0.032)	(0.032)	(0.031)
Misclassification Correction	No	Yes	No	Yes
F Test of Excluded Instruments	10.200	9.337	10.200	9.539
p-value	0.000	0.000	0.000	0.000
Weak IV Test Statistic	23.092	21.083	23.092	21.739
p-value	0.002	0.004	0.002	0.003
Hansen J Statistic	2.370	2.069	9.970	9.792
p-value	0.883	0.913	0.126	0.134
Number of Daughters	2961	2961	2961	2961
Observations	56068	56068	56068	56068

Notes: Robust standard errors with state clustering are shown in parentheses. All specifications control for state and year effects in addition to time-varying controls for daughter's age, age squared, and indicators for number of children equal to 1, 2, 3, or at least 4. Instrumental variables include average and maximum measures of the mother's AFDC/TANF benefit standard and federal/state EITC maximum credit by family size, which are defined over the daughter's critical exposure ages 12-18, and interactions of each with an indicator for welfare reform. The weak IV test statistic is a Kleibergen-Paap (2006) rank statistic. Daughters' PSID core longitudinal weights are used in estimation. Abbreviations: Food Stamps/Supplemental Nutrition Assistance Program (SNAP), and Supplemental Security Income (SSI).

Table B.2. Intergenerational Transmission of AFDC/TANF Participation with Alternative Instrumental Variables

	(1)	(2)	(3)	(4)	(5)	(6)
Mother's Participation	0.281	0.316	0.330	0.332	0.297	0.313
	(0.056)	(0.055)	(0.057)	(0.079)	(0.065)	(0.067)
After Welfare Reform	0.072	0.081	0.087	0.085	0.077	0.080
	(0.022)	(0.022)	(0.020)	(0.030)	(0.025)	(0.026)
Mother's Participation × After Welfare Reform	−0.197	−0.221	−0.241	−0.239	−0.214	−0.224
	(0.050)	(0.048)	(0.046)	(0.069)	(0.055)	(0.056)
Instrumental Variables:						
AFDC/TANF	X	X	X	X	X	X
EITC	X	X	X	X	X	X
AFDC/TANF Application Denial Rate		X	X			
Unemployment Rate			X			
AFDC/TANF Procedural Denial Rate					X	X
AFDC/TANF Favorable Claims Rate						X
Weak IV Test Statistic	23.092	25.068	29.108	19.636	23.259	24.449
p-value	0.002	0.009	0.016	0.006	0.016	0.058
Hansen J Statistic	2.370	12.196	13.015	1.682	5.740	16.434
p-value	0.883	0.272	0.525	0.947	0.837	0.288
Percent Change in Levels	-70%	-70%	-73%	-72%	-72%	-72%
p-value	0.000	0.000	0.000	0.000	0.000	0.000
Percent Change over Baseline	-48%	-47%	-53%	-43%	-43%	-42%
p-value	0.004	0.001	0.000	0.098	0.071	0.062
Number of Daughters	2961	2961	2961	1422	1422	1422
Observations	56068	56068	56068	32988	32988	32988

Notes: Robust standard errors with state clustering are shown in parentheses. All specifications control for state and year effects in addition to time-varying controls for daughter's age, age squared, and indicators for number of children equal to 1, 2, 3, or at least 4. Instrumental variables vary by column and include average and maximum [or minimum for denial rates] measures of indicated variables, which are defined over the daughter's critical exposure ages 12-18, and interactions of each with an indicator for welfare reform. The weak IV test statistic is a Kleibergen-Paap (2006) rank statistic. Daughters' PSID core longitudinal weights are used in estimation.

Table B.3. IV Estimates of the Intergenerational Transmission of AFDC/TANF Participation with Controls for Mother's Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
Mother's Participation	0.284	0.320	0.325	0.435	0.498	0.514
	(0.059)	(0.078)	(0.081)	(0.098)	(0.148)	(0.155)
After Welfare Reform	0.072	0.072	0.072	0.086	0.088	0.087
	(0.021)	(0.021)	(0.021)	(0.032)	(0.032)	(0.032)
Mother's Participation ×	−0.195	−0.206	−0.205	−0.228	−0.248	−0.246
After Welfare Reform	(0.050)	(0.052)	(0.052)	(0.079)	(0.085)	(0.085)
Misclassification Correction	No	No	No	Yes	Yes	Yes
Mother's Characteristics:						
Less than High School Education	X		X	X		X
Ever Below 200% Poverty		X	X		X	X
Weak IV Test Statistic	21.826	17.134	16.483	19.923	14.095	13.295
p-value	0.003	0.017	0.021	0.006	0.050	0.065
Hansen J Statistic	2.256	3.066	2.960	1.901	2.553	2.409
p-value	0.895	0.800	0.814	0.929	0.862	0.878
Percent Change in Levels	-69%	-64%	-63%	-52%	-50%	-48%
p-value	0.000	0.000	0.000	0.000	0.000	0.000
Percent Change over Baseline	-45%	-37%	-35%	-34%	-30%	-28%
p-value	0.006	0.067	0.081	0.042	0.089	0.111
Number of Daughters	2946	2946	2946	2946	2946	2946
Observations	55946	55946	55946	55946	55946	55946

Notes: Robust standard errors with state clustering are shown in parentheses. All specifications control for state and year effects in addition to time-varying controls for daughter's age, age squared, and indicators for number of children equal to 1, 2, 3, or at least 4. Instrumental variables include average and maximum measures of the mother's AFDC/TANF benefit standard and federal/state EITC maximum credit by family size, which are defined over the daughter's critical exposure ages 12-18, and interactions of each with an indicator for welfare reform. The weak IV test statistic is a Kleibergen-Paap (2006) rank statistic. The misclassification correction uses reporting rates in the PSID to address potential misreporting for the daughter's welfare participation (see Appendix A for details). Daughters' PSID core longitudinal weights are used in estimation.

Table B.4. Intergenerational Transmission of AFDC/TANF Participation Estimated without PSID Sample Weights

	(1)	(2)	(3)	(4)
A. Full Sample				
Mother's Participation	0.202	0.391	0.312	0.606
	(0.017)	(0.061)	(0.023)	(0.098)
After Welfare Reform	0.077	0.150	0.088	0.179
	(0.010)	(0.036)	(0.018)	(0.053)
Mother's Participation ×	−0.158	−0.278	−0.202	−0.344
After Welfare Reform	(0.017)	(0.058)	(0.031)	(0.092)
Instrumental Variables	No	Yes	No	Yes
Misclassification Correction	No	No	Yes	Yes
Weak IV Test Statistic		21.100		21.184
p-value		0.004		0.004
Hansen J Statistic		8.222		6.932
p-value		0.222		0.327
Percent Change in Levels	-78%	-71%	-65%	-57%
p-value	0.000	0.000	0.000	0.000
Percent Change over Baseline	-55%	-40%	-45%	-32%
p-value	0.000	0.008	0.001	0.024
Number of Daughters	2961	2961	2961	2961
Observations	56068	56068	56068	56068
B. Survey Research Center (SRC) Sample Only				
Mother's Participation	0.115	0.212	0.182	0.275
	(0.021)	(0.067)	(0.034)	(0.106)
After Welfare Reform	0.030	0.054	0.043	0.063
	(0.010)	(0.022)	(0.018)	(0.034)
Mother's Participation ×	−0.089	−0.181	−0.121	−0.194
After Welfare Reform	(0.022)	(0.067)	(0.039)	(0.108)
Instrumental Variables	No	Yes	No	Yes
Misclassification Correction	No	No	Yes	Yes
Weak IV Test Statistic		19.330		16.900
p-value		0.007		0.018
Hansen J Statistic		6.711		6.519
p-value		0.348		0.368
Percent Change in Levels	-78%	-85%	-67%	-71%
p-value	0.000	0.000	0.000	0.001
Percent Change over Baseline	-55%	-71%	-49%	-55%
p-value	0.001	0.016	0.008	0.104
Number of Daughters	1422	1422	1422	1422
Observations	28917	28917	28917	28917

Notes: Robust standard errors with state clustering are shown in parentheses. All specifications control for state and year effects in addition to time-varying controls for daughter's age, age squared, and indicators for number of children equal to 1, 2, 3, or at least 4. Instrumental variables include average and maximum measures of the mother's AFDC/TANF benefit standard and federal/state EITC maximum credit by family size, which are defined over the daughter's critical exposure ages 12-18, and interactions of each with an indicator for welfare reform. The weak IV test statistic is a Kleibergen-Paap (2006) rank statistic. The misclassification correction uses reporting rates in the PSID to address potential misreporting for the daughter's welfare participation (see Appendix A for details).

Table B.5. Intergenerational Transmission of AFDC/TANF Participation for the Subsample of Eldest Daughters

	(1)	(2)	(3)	(4)
Mother's Participation	0.137	0.259	0.219	0.371
	(0.014)	(0.087)	(0.022)	(0.149)
After Welfare Reform	0.031	0.058	0.037	0.058
	(0.007)	(0.025)	(0.013)	(0.040)
Mother's Participation ×	−0.100	−0.176	−0.135	−0.183
After Welfare Reform	(0.017)	(0.070)	(0.030)	(0.119)
Instrumental Variables	No	Yes	No	Yes
Misclassification Correction	No	No	Yes	Yes
Weak IV Test Statistic		20.956		18.285
p-value		0.004		0.011
Hansen J Statistic		3.237		3.010
p-value		0.779		0.808
Percent Change in Levels	-73%	-68%	-62%	-49%
p-value	0.000	0.000	0.000	0.021
Percent Change over Baseline	-52%	-43%	-47%	-29%
p-value	0.000	0.096	0.001	0.336
Number of Daughters	1914	1914	1914	1914
Observations	36288	36288	36288	36288

Notes: Robust standard errors with state clustering are shown in parentheses. All specifications control for state and year effects in addition to time-varying controls for daughter's age, age squared, and indicators for number of children equal to 1, 2, 3, or at least 4. Instrumental variables include average and maximum measures of the mother's AFDC/TANF benefit standard and federal/state EITC maximum credit by family size, which are defined over the daughter's critical exposure ages 12-18, and interactions of each with an indicator for welfare reform. The weak IV test statistic is a Kleibergen-Paap (2006) rank statistic. The misclassification correction uses reporting rates in the PSID to address potential misreporting for the daughter's welfare participation (see Appendix A for details). Daughters' PSID core longitudinal weights are used in estimation.

Table B.6. IV Estimates of Intergenerational Transmission of Mother's AFDC/TANF Participation by Relevant Subsample

Daughter's Outcome:	AFDC/TANF				AFDC/TANF, SNAP, SSI			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mother's Participation	0.219	0.258	0.351	0.344	0.207	0.246	0.362	0.303
	(0.053)	(0.047)	(0.074)	(0.061)	(0.072)	(0.070)	(0.096)	(0.070)
After Welfare Reform	0.069	0.071	0.153	0.117	−0.004	−0.020	0.051	−0.009
	(0.019)	(0.020)	(0.038)	(0.030)	(0.041)	(0.032)	(0.051)	(0.040)
Mother's Participation×	−0.185	−0.195	−0.261	−0.254	0.038	0.061	−0.075	0.010
After Welfare Reform	(0.048)	(0.049)	(0.058)	(0.053)	(0.098)	(0.085)	(0.084)	(0.080)
Subsample by Mother:	Education 9 years	Education $\leq$ 12 years	Ever below 130% FPL	Ever below 200% FPL	Education 9 years	Education $\leq$ 12 years	Ever below 130% FPL	Ever below 200% FPL
Weak IV Test Statistic	23.305	21.932	20.521	21.828	23.305	21.932	20.521	21.828
p-value	0.002	0.003	0.005	0.003	0.002	0.003	0.005	0.003
Hansen J Statistic	2.304	1.690	3.240	1.709	12.036	11.423	5.747	5.979
p-value	0.890	0.946	0.778	0.944	0.061	0.076	0.452	0.426
Percent Change in Levels	-84%	-75%	-74%	-74%	18%	25%	-21%	3%
p-value	0.000	0.000	0.000	0.000	0.725	0.536	0.268	0.898
Percent Change over Baseline	-67%	-57%	-55%	-54%	30%	31%	-16%	9%
p-value	0.006	0.004	0.000	0.000	0.600	0.460	0.424	0.745
Number of Daughters	1328	2507	2213	2634	1328	2507	2213	2634
Observations	30168	49918	40997	49266	30168	49918	40997	49266

Notes: Robust standard errors with state clustering are shown in parentheses. All specifications control for state and year effects in addition to time-varying controls for daughter's age, age squared, and indicators for number of children equal to 1, 2, 3, or at least 4. Instrumental variables include average and maximum measures of the mother's AFDC/TANF benefit standard and federal/state EITC maximum credit by family size, which are defined over the daughter's critical exposure ages 12-18, and interactions of each with an indicator for welfare reform. The weak IV test statistic is a Kleibergen-Paap (2006) rank statistic. The misclassification correction uses reporting rates in the PSID to address potential misreporting for the daughter's welfare participation (see Appendix A for details). Daughters' PSID core longitudinal weights are used in estimation.

Table B.7. Intergenerational Transmission of Mother's AFDC/TANF Participation Controlling for Mother's Future Welfare Participation

Daughter's Outcome:	AFDC/TANF				AFDC/TANF, SNAP, SSI			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mother's Prior Participation	0.182	0.267	0.139	0.316	0.225	0.267	0.179	0.387
	(0.024)	(0.085)	(0.023)	(0.101)	(0.028)	(0.087)	(0.027)	(0.094)
After Welfare Reform	0.024	0.038	0.016	0.052	0.004	0.007	−0.004	0.032
	(0.012)	(0.019)	(0.012)	(0.027)	(0.015)	(0.023)	(0.015)	(0.033)
Mother's Prior Participation ×	−0.111	−0.167	−0.090	−0.223	−0.066	−0.076	−0.040	−0.098
After Welfare Reform	(0.031)	(0.077)	(0.027)	(0.095)	(0.032)	(0.078)	(0.033)	(0.102)
Mother's Future Participation			0.013	0.483			−0.007	0.896
			(0.023)	(0.579)			(0.035)	(0.732)
Mother's Future Participation ×			−0.025	−1.030			−0.030	−1.424
After Welfare Reform			(0.026)	(0.740)			(0.042)	(0.934)
Mother's Prior Participation ×			0.250	−0.490			0.281	−0.996
Future Participation			(0.063)	(0.810)			(0.072)	(1.030)
Mother's Prior × Future ×			−0.029	1.312			−0.060	1.540
After Welfare Reform			(0.063)	(0.992)			(0.079)	(1.260)
Instrumental Variables	No	Yes	No	Yes	No	Yes	No	Yes
Weak IV Test Statistic		12.565		18.900		12.565		18.900
p-value		0.083		0.463		0.083		0.463
Hansen J Statistic		5.106		19.109		4.959		15.920
p-value		0.530		0.385		0.549		0.598
Percent Change in Levels	-61%	-62%	-65%	-71%	-29%	-29%	-22%	-25%
p-value	0.000	0.000	0.000	0.000	0.023	0.203	0.183	0.255
Percent Change over Baseline	-31%	-34%	-38%	-49%	-3%	-2%	6%	2%
p-value	0.178	0.168	0.122	0.091	0.862	0.940	0.783	0.948
Number of Daughters	1665	1665	1665	1665	1665	1665	1665	1665
Observations	15034	15034	15034	15034	15034	15034	15034	15034

Notes: Robust standard errors with state clustering are shown in parentheses. All specifications control for state and year effects in addition to time-varying controls for daughter's age, age squared, and indicators for number of children equal to 1, 2, 3, or at least 4. Instrumental variables include average and maximum measures of the mother's prior and future AFDC/TANF benefit standard and federal/state EITC maximum credit by family size, interactions of each variable's prior and future measure, and interactions of each with an indicator for welfare reform. Mother's future participation and instrumental variables are measured over years t+5 to t+11, which is arbitrarily distant from time t with an equivalent window size to prior instrument measures over the critical exposure period for daughter's ages 12-18. The weak IV test statistic is a Kleibergen-Paap (2006) rank statistic. Daughters' PSID core longitudinal weights are used in estimation.

Table B.8. IV Estimates of the Intergenerational Transmission of AFDC/TANF Participation by Minimum Number of Mother-Daughter Family Observations, N_F

	$N_F \geq 5$		$N_F \geq 10$		$N_F \geq 15$	
	(1)	(2)	(3)	(4)	(5)	(6)
Mother's Participation	0.281	0.428	0.328	0.541	0.281	0.462
	(0.056)	(0.093)	(0.071)	(0.115)	(0.064)	(0.111)
After Welfare Reform	0.072	0.087	0.094	0.129	0.087	0.117
	(0.022)	(0.033)	(0.027)	(0.041)	(0.027)	(0.045)
Mother's Participation ×	−0.197	−0.234	−0.253	−0.355	−0.195	−0.257
After Welfare Reform	(0.050)	(0.081)	(0.066)	(0.107)	(0.059)	(0.105)
Misclassification Correction	No	Yes	No	Yes	No	Yes
Weak IV Test Statistic	23.092	21.083	23.234	22.099	16.942	17.735
p-value	0.002	0.004	0.002	0.002	0.018	0.013
Hansen J Statistic	2.370	2.069	5.138	5.164	4.515	5.040
p-value	0.883	0.913	0.526	0.523	0.607	0.539
Percent Change in Levels	-70%	-55%	-77%	-66%	-69%	-56%
p-value	0.000	0.000	0.000	0.000	0.000	0.001
Percent Change over Baseline	-48%	-37%	-60%	-52%	-53%	-43%
p-value	0.004	0.031	0.007	0.018	0.009	0.040
Number of Daughters	2961	2961	2466	2466	1806	1806
Observations	56068	56068	43733	43733	28903	28903

Notes: The minimum number of mother-daughter family observations, denoted NF, represents years when the mother is observed living with the daughter before her daughter has formed her own family unit (the minimum restriction throughout the rest of the paper is NF=5). Robust standard errors with state clustering are shown in parentheses. All specifications control for state and year effects in addition to time-varying controls for daughter's age, age squared, and indicators for number of children equal to 1, 2, 3, or at least 4. Instrumental variables include average and maximum measures of the mother's AFDC/TANF benefit standard and federal/state EITC maximum credit by family size, which are defined over the daughter's critical exposure ages 12-18, and interactions of each with an indicator for welfare reform. The weak IV test statistic is a Kleibergen-Paap (2006) rank statistic. The misclassification correction uses reporting rates in the PSID to address potential misreporting for the daughter's welfare participation (see Appendix A for details). Daughters' PSID core longitudinal weights are used in estimation.

APPENDIX FOR ESSAY 2

C Experimental Sample

This section presents additional descriptive statistics of the experimental sample used in our investigation. To complement the evidence presented in Section 2.4, Table C.1 presents evidence by participant type.

D Technical Appendix

D.1 Tuning Parameter

In the empirical analysis of Section 2.5.2, we estimate the restricted case by setting $\lambda_0 = 1$ for the control group and $\lambda_1 = 0.01$ for Jobs First participants. This restriction imposes values of λ that would directly correspond to model assumptions of individual effects relevant only to the treatment group who are welfare-eligible at higher earnings (and presumably higher hours of labor supply). In this case, the choice of $\lambda_1 = 0.01$ is small enough to allow individual effects that can have a "fixed" effects interpretation for the treatment group, and for the control group, $\lambda_0 = 1$ is set to the standard condition that the variance of α_i is equal to the variance of u_{it} (Koenker, 2005, pg. 281). For the unrestricted case, λ is determined using a data-driven approach to be approximately 0.718 for the earnings dependent variable and 0.673 for total income. Under the assumption that α_i and D_{i0} are independent, which holds here by experimental design, the tuning parameter λ is estimated to minimize the variance of the QTE estimator (Lamarche, 2010). Under further assumptions, λ is equal to the ratio σ_u/σ_α which can be easily estimated by random effects or maximum likelihood methods. Apart from choosing the tuning parameter according to standard values in the literature (as in the restricted case) or by optimizing some objective function (as in the minimum variance estimator

Table C.1. Descriptive Statistics by Participant Type: Applicants and Recipients

	Levels		Differences		
Variables	Recipients	Applicants	Unadjusted	Adjusted	N
Newhaven County (urban)	0.794	0.695	0.099*	0.097*	4803
	(0.404)	(0.460)	(0.013)	(0.013)	
Never married	0.655	0.584	0.072*	0.072*	4803
	(0.475)	(0.493)	(0.014)	(0.015)	
HS dropout	0.350	0.278	0.072*	0.072*	4803
	(0.477)	(0.448)	(0.014)	(0.014)	
More than two children	0.266	0.140	0.125*	0.124*	4803
	(0.442)	(0.348)	(0.011)	(0.013)	
Mother younger than 25	0.237	0.380	−0.144*	−0.147*	4803
	(0.425)	(0.486)	(0.014)	(0.013)	
Mother older than 34	0.323	0.247	0.075*	0.075*	4803
	(0.468)	(0.432)	(0.013)	(0.013)	
Currently working $\geq$ 30 hours	0.253	0.351	−0.098*	−0.092*	989
	(0.435)	(0.478)	(0.030)	(0.029)	
Hourly wage	6.392	7.120	−0.728*	−0.700*	973
	(2.224)	(2.628)	(0.160)	(0.165)	
Public or subsidized housing	0.451	0.191	0.260*	0.260*	4520
	(0.498)	(0.393)	(0.013)	(0.015)	
Ever on AFDC as a child	0.264	0.235	0.030*	0.031*	4491
	(0.441)	(0.424)	(0.013)	(0.013)	
Ever received AFDC at prior quarter 7	0.758	0.196	0.562*	0.562*	4803
	(0.428)	(0.397)	(0.012)	(0.015)	
Length in months of 1st AFDC spell	17.705	13.435	4.269*	4.127*	3607
	(10.332)	(9.980)	(0.345)	(0.362)	
Number of AFDC spells	1.243	1.122	0.121*	0.115*	4803
	(0.595)	(0.689)	(0.019)	(0.020)	
Long-term recipient (2 years)	0.730	0.302	0.428*	0.429*	4706
	(0.444)	(0.459)	(0.014)	(0.014)	
		Pre-Treatment Quarters			
Average quarterly earnings	413.937	1227.859	−813.922*	−813.649*	4803
	(1041.982)	(1771.295)	(45.170)	(43.484)	
Average quarterly cash welfare	1290.907	197.443	1093.464*	1095.108*	4803
	(659.755)	(462.488)	(16.208)	(25.845)	
Fraction of quarters with earnings	0.259	0.456	−0.197*	−0.199*	4803
	(0.323)	(0.399)	(0.011)	(0.011)	
Fraction of quarters with cash welfare	0.827	0.142	0.684*	0.685*	4803
	(0.309)	(0.296)	(0.009)	(0.014)	
		Experimental Quarters 1-7			
Average quarterly earnings	1014.784	1375.760	−360.976*	−351.282*	4803
	(1498.970)	(1781.270)	(49.562)	(53.643)	
Average quarterly cash welfare	1130.330	761.419	368.911*	360.858*	4803
	(607.787)	(617.241)	(18.139)	(20.269)	
Fraction of quarters with earnings	0.466	0.505	−0.039*	−0.044*	4803
	(0.398)	(0.396)	(0.012)	(0.011)	
Fraction of quarters with cash welfare	0.788	0.573	0.215*	0.211*	4803
	(0.319)	(0.393)	(0.011)	(0.011)	

Notes: Standard deviations are shown in parentheses, and * denotes statistically significant differences at the 10-percent level.

in the unrestricted case), another selection criterion could be to follow a grid search over plausible values of λ. This is essentially similar to our robustness results shown for the tuning parameter selection as explored graphically in Figure 2.8.

D.2 Standard Errors

We propose to use the bootstrap for inference about $\hat{\Delta}(\tau, \lambda)$. In what follows, for notational simplicity, we suppress the dependence of the QTE estimator on τ and λ. Given the different estimators used in this study, the bootstrap appears to have an advantage over the estimation of the covariance matrices of the limiting process $(NT)^{-1/2}(\hat{\Delta} - \Delta)$. In order to directly compare QTEs with previous estimates, we follow the block bootstrap method used in the panel quantile literature as well as in Bitler, Gelbach, and Hoynes (2006). We proceed by drawing a sample with replacement of N subjects including their T observations. Using these new pairs $(\mathbf{Y}_i^*, \mathbf{D}_i^*, \mathbf{x}_i^*)$, we recalculate inverse-propensity weights based on $\mathbf{x}_i^*$ in each bootstrap sample, and then obtain Δ^* as the argument that minimizes the objective function. We reiterate this procedure B times to obtain a large sample of realizations $\{\Delta_b^*\}_{b=1}^B$. For a given quantile, we can obtain an estimate of the variance of $\hat{\Delta}(\tau)$ as the sample variance of $\{\Delta_b^*\}_{b=1}^B$. Moreover, a $100(1-2q)\%$ confidence interval can be obtained by constructing the qth quantile and $(1-q)$th quantile of $\{\Delta_b^*\}_{b=1}^B$. This pair bootstrap procedure is applied to compute the estimator standard errors based on $B = 1000$ replications.

D.3 Hypothesis Testing

To evaluate significance of differences across quantiles, it is possible to employ the Hausman-type statistic proposed in Harding and Lamarche (2014) or a Wald-type statistic (Koenker, 2005). We consider testing a basic general linear hypothesis on a vector ξ of the form

$H_0 : \mathbf{R}\xi = \mathbf{r}$, where $\mathbf{R}$ is a matrix that depends on the type of restrictions imposed. For instance, one might evaluate the null hypothesis of equality of effects across quantiles considering a vector $\xi = (\Delta(\tau_1), \ldots, \Delta(\tau_J))'$. More importantly, in Section 2.5.5 we test for an exogeneity condition of the treatment variable and the independent variables using the Hausman-type test for the null hypothesis that $\Delta(\tau_j)$ in equation (2.1) is equal to $\Delta(\tau_j)$ in equation (2.2) for $j = 1, \ldots, J$.

APPENDIX FOR ESSAY 3

E Economics of Childcare Subsidization

Morgan (1986) introduced the notion of a childcare trilemma, a set of interrelated problems that complicate parental work and family decisions: quality, accessibility, and affordability.[2] The main avenues for intervention related to these problems are regulation, information, and price subsidies. Through government regulation or private accreditation, childcare standards can be established for education, health, and safety, though these may prove to be weak floors for quality child investments, and measurement of quality is imperfect. Public or nonprofit agencies can support clearinghouses of childcare information to improve families' ability to match their children with higher quality programs. And lastly, the cost of child care can be subsidized, potentially in coordination with regulatory constraints.

Blau (2003) points out that price subsidies may have ambiguous effects on childcare quality demanded. As an example, Blau defines the price of child care as $p = \alpha + \beta q$ where q is the quality of child care demanded. An unconditional childcare subsidy would lower α and have a larger effect on increasing labor supply than a quality-conditioned subsidy on β. In practice, subsidies may set quality thresholds through eligible provider policies or reimbursement rates that differ by provider type, which would be similar to subsidizing β and more effective at improving quality than an unconditional subsidy. However, subsidizing either α or β would have unknown effects on quality demanded

[2] Morgan's trilemma was explicitly stated as quality, staff compensation, and parent affordability, though the term accessibility might summarize her broader themes of informational constraints, problems of "invisible" supply, and what to do with sick children. There remains an implicit tradeoff that finding an accessible supply that satisfies quality and affordability places an strain on staff compensation absent cost subsidies.

given the trade offs between a family's choice of quantity demanded as well as the relative quality of parental care (see also Blau and Currie, 2006; Blau and Hagy, 1998). For single mothers—especially those with lower education—center-based child care has been shown to improve children's cognitive development relative to the more common use of informal care among this demographic (Bernal and Keane, 2011).

F Data Appendix

Additional details and notes on data sources:

1. Temporary Assistance for Needy Families (TANF) Financial Data by Fiscal Year, U.S. Department of Health and Human Services, Administration for Children and Families, http://www.acf.hhs.gov/programs/ofa/programs/tanf/data-reports.

2. TANF expenditures are given in thousands of nominal dollars and include all federal and state spending by category. Note that some years may be negative in cases where states make adjustments for previous years.

3. Aid to Families with Dependent Children/Temporary Assistance for Needy Families (AFDC/TANF) Caseload Data by Fiscal Year, U.S. Department of Health and Human Services, Administration for Children and Families, http://www.acf.hhs.gov/programs/ofa/programs/tanf/data-reports.

4. Number of TANF child recipients for the United States in years 1998 and 1999 are interpolated based on the ratio of children to cases in the years just before and after.

5. Number of children in active TANF cases receiving subsidized child care is an imputed estimate based on the number of subsidized childcare cases in that year and the ratio of children to cases in the same year.

6. Number of AFDC children receiving subsidized childcare assistance in years 1969-1975 are taken from NCSS Report E-4 [HE17.609] "Child Care Arrangements of AFDC Recipients Under the Work Incentive Program as of the Last Day of the Quarter ended September 31, [YEAR]".

7. Table 7-4. Historical Trends in AFDC/TANF Enrollments, Fiscal Years 1970-99, Percent of all children who are on AFDC/TANF (pg. 376), Background Material and Data on Programs within the Jurisdiction of the Committee on Ways and Means, 2000.

8. Temporary Assistance for Needy Families (TANF) Active Cases - Percent Distribution of TANF Families Receiving Assistance [Subsidized Child Care; federal, state/local], Characteristics and Financial Circumstances of TANF Recipients [Table I-11 2000; Table 10:13 2001-2003; Table 13 2004-2012; Table 11 2013-2014], U.S. Department of Health and Human Services, Administration for Children and Families, http://www.acf.hhs.gov/programs/ofa/programs/tanf/data-reports.

9. State Temporary Assistance for Needy Families (TANF) and Maintenance of Effort (MOE) Annual Reports, 2005 and 2010, U.S. Department of Health and Human Services, Administration for Children and Families,
http://www.acf.hhs.gov/programs/ofa/programs/tanf/data-reports.

10. TANF MOE program data are organized by State program, with potentially more than one program of a given type per state. Expenditure is given in thousands of nominal dollars and cases are in thousands.

11. Child Care and Development Fund (CCDF) State Expenditure Data [1999-2014], U.S. Department of Health and Human Services, Administration for Children & Families, Office of Child Care, http://www.acf.hhs.gov/programs/occ/resource/ccdf-expenditure-data-all-years.

12. Table 9-28. Total Child Care and Development Fund (CCDF) and Predecessor Program Expenditures, by State, Fiscal Years 1992-98 (in thousands of dollars), Background Material and Data on Programs within the Jurisdiction of the Committee on Ways and Means, 2000 (pg. 625).

13. Table 17. State Allocations under the Child Care and Development Block Grant (CCDBG [CCDF]), 1991-93 (by fiscal years; in thousands of dollars), Background Material and Data on Programs within the Jurisdiction of the Committee on Ways and Means, 1993 (pg. 1013).

14. Social Services Block Grant (SSBG) Program Annual Reports, 2010-2014, U.S. Department of Health and Human Services, Administration for Children and Families, Office of Community Services, http://www.acf.hhs.gov/programs/ocs/programs/ssbg.

15. Table 1. Title XX Social Services Block Grant (SSBG) Funding Levels (in thousands of dollars), Fiscal Years 1977-2011, Background Material and Data on Programs within the Jurisdiction of the Committee on Ways and Means: 1998 (Table 10-1, pg. 714); 2012 (Table 10-1, pg. 483-484).

16. Table 2. Title XX Social Services Block Grant (SSBG) Allocations by State and Territory, Fiscal Years 1996-2012 (in thousands of dollars), Background Material and Data on Programs within the Jurisdiction of the Committee on Ways and Means: 1982 (Table 1, pg. 187-188); 1983 (Table I, pg. 344-345); 1984 (Table 2, pg. 412-413); 1986 (Table 2, pg. 513-515); 1987 (Table 2, pg. 546-545); 1988 (Table 2, pg. 558-559); 1990 (Table 2, pg. 745-746); 1992 (Table 2, pg. 831-832); 1996 (Table 11-2, pg. 681-683); 2004 (Table 10-2, pg. 10-3–10-5), 2008 (Table 10-2, pg. 10-4–10-5), 2012 (Table 10-2, pg. 485-487).

17. Table 4. Use of Title XX Social Security Block Grant (SSBG) Funds by Expenditure Category, Fiscal Years 1978-1979, 1983, 1986, 1988-1990, 1995-2009 (percent of total), Background Material and Data on Programs within the Jurisdiction of the Committee on Ways and Means: 1981 (Table 4, pg. 118), 1982 (Table 4, pg. 195), 1989 (pg. 724), 1991 (pg. 778), 1992 (pg. 834), 1993 (pg. 875), 1996 (pg. 686), 2000 (Table 10-4, pg. 641), 2004 (Table 10-4, pg. 10-9), 2012 (Table 10-4, pg. 490-491).

18. Table 4. Data from Pre-Expenditure Reports on Title XX Expenditures for Selected States for Three Selected Services, Fiscal Years 1980, 1983, 1986-1991, Background Material and Data on Programs within the Jurisdiction of the Committee on Ways and Means: 1989 (Table 4, pg. 728-729), 1992 (Table 4, pg. 838).

19. Annual Report to the Congress on Title XX of the Social Security Act [HV85 D46A], 1977 (Figure VIII, pg. 19; Appendix I, pg. 47); 1979 (Appendices K, L, M, O, pgs. 79-81, 83); 1980 (Tables 1C, 2C, C, J, K, pgs. 51-54, 63, 70-71).

20. Office of Head Start Data and Reports, U.S. Department of Health and Human Services, Administration of Children and Families, Early Childhood Learning & Knowledge Center (ECLKC), https://eclkc.ohs.acf.hhs.gov/hslc/data.

21. Barnett, W. S., & Friedman-Krauss, A. H., (2016). State(s) of Head Start. New Brunswick, NJ: National Institute for Early Education Research, http://nieer.org/headstart.

22. Kids Count Data Center, Annie E. Casey Foundation: The share of children under age 18 who live in families with incomes below the federal poverty level. The federal poverty definition consists of a series of thresholds based on family size and composition. In calendar year 2015, a family of two adults and two children fell in the "poverty" category if their annual income fell below $24,036. Poverty status is not determined for people in military barracks, institutional quarters, or for unrelated individuals under age 15 (such as foster children). The data are based on income received in the 12 months prior to the survey.

23. Population Reference Bureau, analysis of data from the U.S. Census Bureau, Census 2000 Supplementary Survey, 2001 Supplementary Survey, 2002 through 2015 American Community Survey. These data were derived from American Fact Finder table B17001 (factfinder2.census.gov/).

24. Statistics of Income (SOI) Bulletin Historical Table 1, Individual Income Tax Returns: Selected Income and Tax Items, 1999-2014, Internal Revenue Service (IRS) [Child and Dependent Care Tax Credit (CDCTC) and Child Tax Credit (CTC)], https://www.irs.gov/pub/irs-soi/histab1.xls.

25. Table 13-15. [Child and] Dependent Care Tax Credit (CDCTC): Number of Families and Amount of Credit, 1976-2001, Background Material and Data on Programs within the Jurisdiction of the Committee on Ways and Means, 2004 (pg. 13-44).

26. Table 1. Estimated Federal Income Tax Expenditures, Calendar Years 1967-1972, 1974-1975 (in thousands of dollars), Estimates of Federal Tax Expenditures, Committee on Ways and Means, Joint Committee on Internal Revenue Taxation, 1973, 1975, 1976 [Child and Dependent Care Tax Credit (CDCTC)], https://www.jct.gov/publications.html.

27. Table 1. Public high school 4-year adjusted cohort graduation rate (ACGR), by race/ethnicity and selected demographics for the United States, the 50 states, and the District of Columbia: School year 2010-11 through 2014-15, EDFacts/Consolidated State Performance Report, http://www2.ed.gov/admins/lead/account/consolidated/index.html.

28. The 4-year ACGR is the number of students who graduate in 4 years with a regular high school diploma divided by the number of students who form the adjusted cohort for the graduating class. From the beginning of 9th grade (or the earliest high school grade), students who are entering that grade for the first time form a cohort that is "adjusted" by adding any students who subsequently transfer into the cohort and subtracting any students who subsequently transfer out, emigrate to another country, or die.

29. Table 3. Poverty Status of People, by Age, Race, and Hispanic Origin: 1959 to 2015 (numbers in thousands), U.S. Bureau of the Census, Current Population Survey, Annual Social and Economic Supplements, https://www.census.gov/data/tables/time-series/demo/income-poverty/ historical-poverty-people.html.

30. Background Material and Data on Programs within the Jurisdiction of the Committee on Ways and Means, 1993: Table 11. Federal Payments to States for AFDC Child Care and Transitional Child Care, Fiscal Years 1991-1992 [1993-1994 est.] (in thousands); Table 13. Average Monthly Number of Children Receiving AFDC Child Care by Type of Care and by State, Fiscal Year 1991; Table 14. Average Monthly Number of Children and Families Receiving AFDC and Transitional Child Care, by Program Status, Fiscal Year 1991; Table 15. Monthly Number of Families Receiving Transitional Child Care, Oct 1990-Mar 1991 and Apr 1991-Sep 1991; Table 16. Federal Payments to States for At-Risk Child Care, Fiscal Years 1991-1992 [1993-1994 est.] (in thousands); Table 17. State Allocations under the Child Care and Development Block Grant, 1991-1992 [1993 est.] (in thousands); Table 25, Total AFDC Expenditures, Fiscal Years 1985-92. [For further data in this series of tables in later years, see Green Books 1994-1998.]

31. Table 4.2. Trends in Federal AFDC Expenditures, 1962-1996, Aid to Families with Dependent Children: The Baseline (Office of the Assistant Secretary for Planning and Evaluation (ASPE), U.S. Department of Health and Human Services, 1998).

32. Table: "Aid to Families with Dependent Children: Expenditures for assistance payments and administrative costs, by source of funds, fiscal years 1936 to date (in thousands)". Expenditures for public assistance payments and for administrative costs, by program and source of funds: Fiscal years 1936-1970. USDHEW, National Center for Social Statistics, NCSS Report F-5 (FY 36-70), July 6, 1971 [HE 17.632 1936-1970].

33. Table TANF 4. Total AFDC/TANF Expenditures on Cash Benefits and Administration, 1970-2002, U.S. Department of Health and Human Services, Administration for Children and Families, Office of Financial Systems,
http://aspe.hhs.gov/hsp/indicators04/apa-tanf.htm.

34. AFDC childcare data for years 1967-1990 are obtained from various sources on AFDC Work Incentive (WIN) program expenditures. Green Books 1982-1988; Work Incentive Program Annual Reports to Congress 1973-1979; "Work-Related Programs for Welfare Recipients", Congressional Budget Office Report, 1987; Child care arrangements of AFDC recipients under the Work Incentive Program as of the last day of the quarter ended September 30, [YYYY], NCSS Report E-4, USDHEW 1969-1973.

35. U.S. Census Bureau, Annual Survey of State and Local Government Finances and Census of Governments (1902-2014), https://www.census.gov/govs/local.

36. State and Local Government Finances and Employment, Statistical Abstract of the United States, 1974 (No. 402); 1980 (No. 481); 1985 (No. 435); 1986 (No. 457); 1987 (No. 428, 442); 1988 (No. 431, 441); 1989 (No. 447); 1990 (No. 466); 1994 (No. 476); 1995 (No. 489); 1996 (No. 486); 1997 (No. 494) [Total Expenditures, Education, Public Welfare (PW)] https://www.census.gov/library/publications/time-series/statistical_abstracts.html.

37. Series H 1-31. Social Welfare Expenditures under Public Programs (in thousands of dollars): 1890 to 1970, Bicentennial Edition: Historical Statistics of the United States,

Colonial Times to 1970 (pg. 340-341), https://www.census.gov/library/publications/1975/compendia/hist_stats_colonial-1970.html.

38. Table 106.10. Expenditures of educational institutions related to the gross domestic product, by level of institution: Selected years, 1929-30 through 2014-15, National Center for Education Statistics (NCES). U.S. Department of Education, National Center for Education Statistics, Biennial Survey of Education in the United States, 1929-30 through 1949-50; Statistics of State School Systems, 1959-60 through 1969-70; Revenues and Expenditures for Public Elementary and Secondary Education, 1970-71 through 1986-87; Common Core of Data (CCD), "National Public Education Financial Survey," 1987-88 through 2012-13; Higher Education General Information Survey (HEGIS), Financial Statistics of Institutions of Higher Education, 1965-66 through 1985-86; Integrated Postsecondary Education Data System (IPEDS), "Finance Survey" (IPEDS-F:FY87-99); and IPEDS Spring 2001 through Spring 2015, Finance component.

39. U.S. Bureau of Economic Analysis, Personal consumption expenditures (PCE) excluding food and energy (chain-type price index) [DPCCRG3A086NBEA], 2017, retrieved from FRED, Federal Reserve Bank of St. Louis, https://fred.stlouisfed.org/series/DPCCRG3A086NBEA.

40. Federal Percentages and Federal Medical Assistance Percentages (FMAP), U.S. Department of Health and Human Services, Assistant Secretary for Planning and Evaluation, https://aspe.hhs.gov/federal-percentages-and-federal-medical-assistance-percentages-fy-1961-fy-2011.

G TANF Categorical Expenditure Allocations Over Time

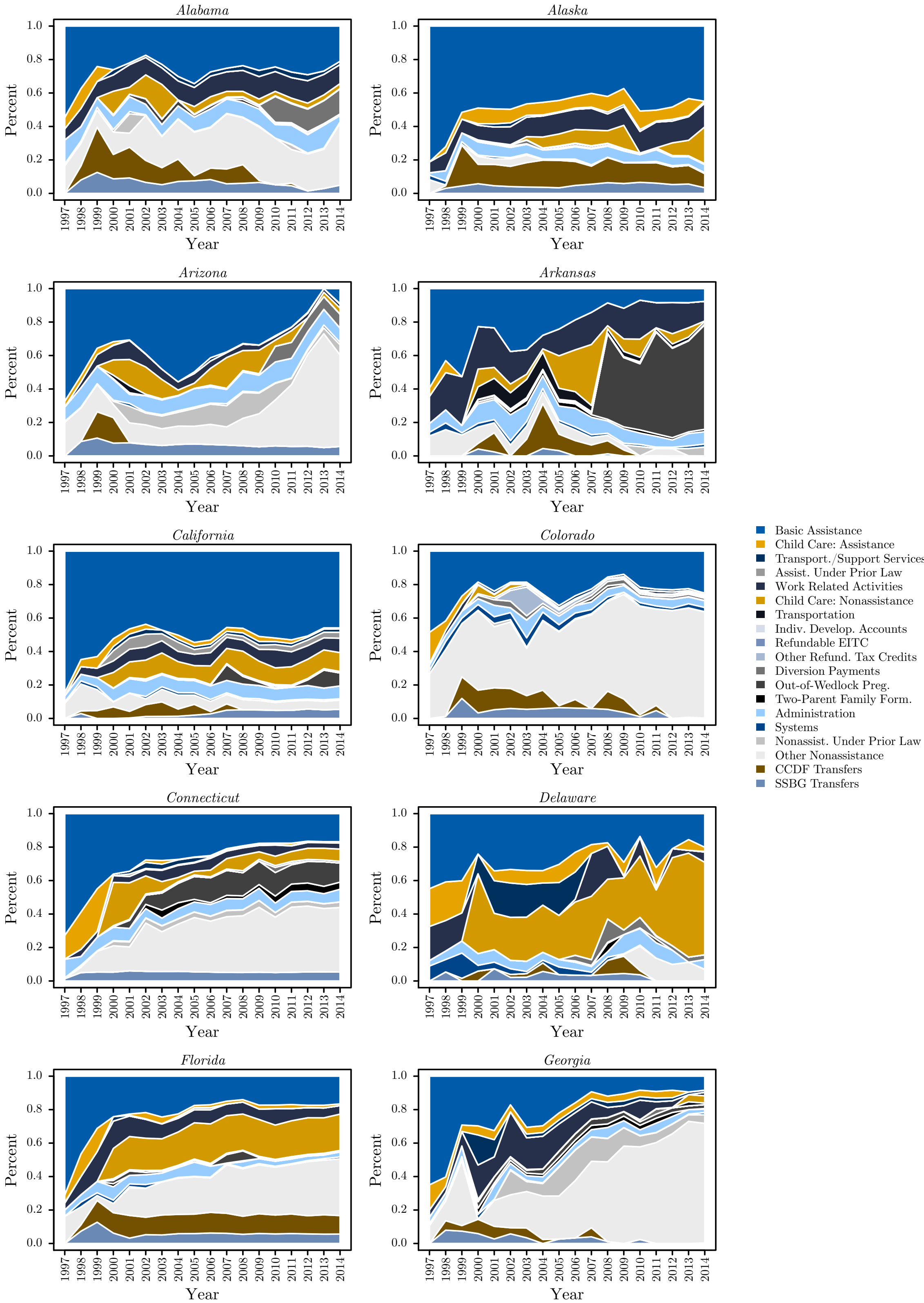
In order to emphasize the differences in TANF expenditures by category over time, Table G.1 gives an overview of U.S. spending in 1997, 2000, 2005, and 2010. Then, the following set of plots contained in Figure G.1 offers a more detailed breakdown of the U.S. aggregate allocations of TANF block grant funds by states from 1997-2014. For reference, the total U.S. spending by TANF category is shown in Table 3.12. While it may be difficult to easily identify every spending category, note that the main categories relevant to this study are as follows. Cash assistance is at the top in blue and all childcare-related expenditures and transfers are in some shade of gold (light for "assistance", medium for "nonassistance", and dark gold for CCDF transfers). Another prominent category, especially in some states, is "Other Nonassistance", which is shown in a light gray.

Table G.1. TANF Expenditure by Category in Millions of 2014 Dollars, Select Years

	1997	2000	2005	2010
Assistance	19,555	16,052	14,206	13,062
Basic Assistance	18,488	14,246	12,508	11,406
Child Care	1,055	-24	609	606
Transportation And Supportive Services	12	683	398	368
Assistance Under Prior Law	0	1,147	690	682
Non-Assistance	5,726	15,523	15,588	22,391
Work Related Activities/ Expenses	952	3,187	2,524	3,520
Child Care	29	4,507	3,114	3,732
Transportation	0	161	211	222
Individual Development Accounts	0	3	4	3
Refundable Earned Income Tax Credits	0	780	1,030	2,351
Other Refundable Tax Credits	0	98	282	585
Non-Recurrent Short-Term Benefits	0	187	311	1,160
Prevention of Out-of-Wedlock Pregnancies	0	137	776	2,071
Two-Parent Family Formation and Maintenance	0	225	203	313
Administration	2,111	2,889	2,445	2,378
Systems	270	427	323	273
Non-Assistance Under Prior Law	0	414	1,101	1,130
Other Non-Assistance	2,365	2,508	3,263	4,652
Transfer	789	4,471	3,331	2,764
Child Care And Development Fund	313	3,075	2,256	1,463
Social Services Block Grant	476	1,397	1,074	1,301
Total Expenditure	26,070	36,047	33,124	38,218

Notes: Author's tabulations from USDHHS financial reports. Total expenditures represent all federal and state expenditure (including separate state programs) for the 50 states and District of Columbia.

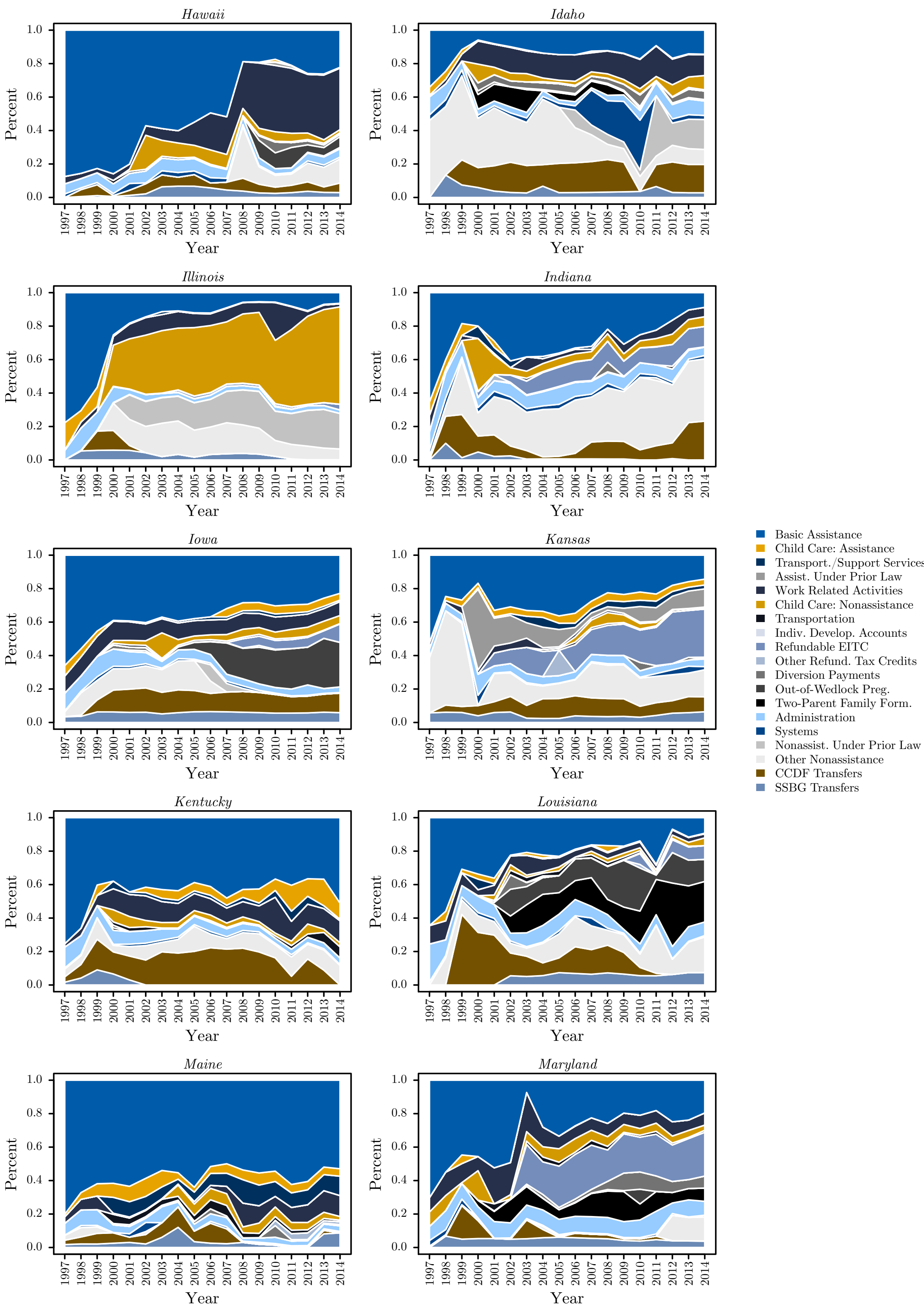
Figure G.1. TANF Category Expenditures by Percent of Total, 1997-2014



Source: U.S. Department of Health and Human Services; author's tabulations.

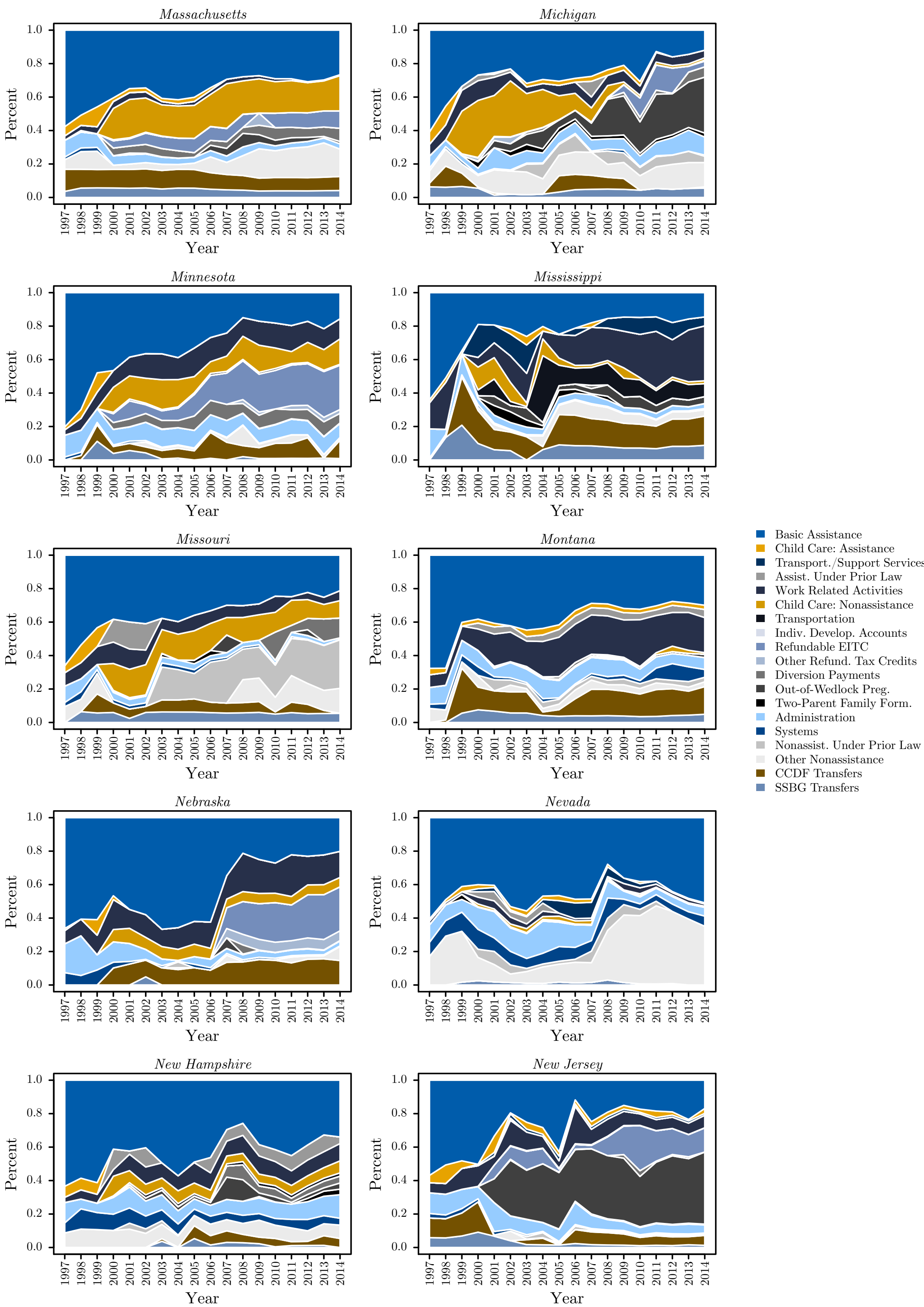
Figure G.1. TANF Category Expenditures by Percent of Total, 1997-2014

(*. . . continued*)



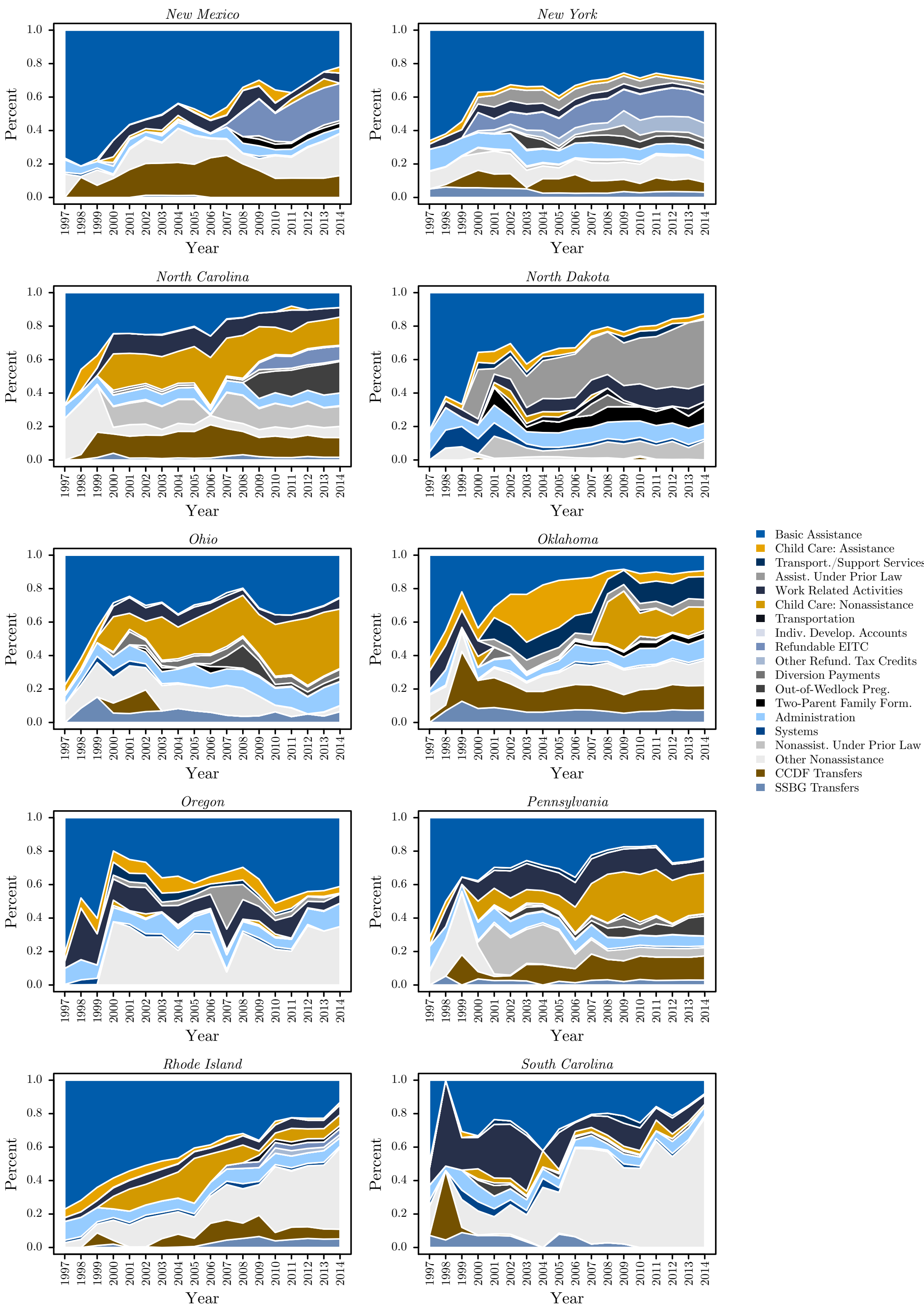
Source: U.S. Department of Health and Human Services; author's tabulations.

Figure G.1. TANF Category Expenditures by Percent of Total, 1997-2014
(*. . . continued*)



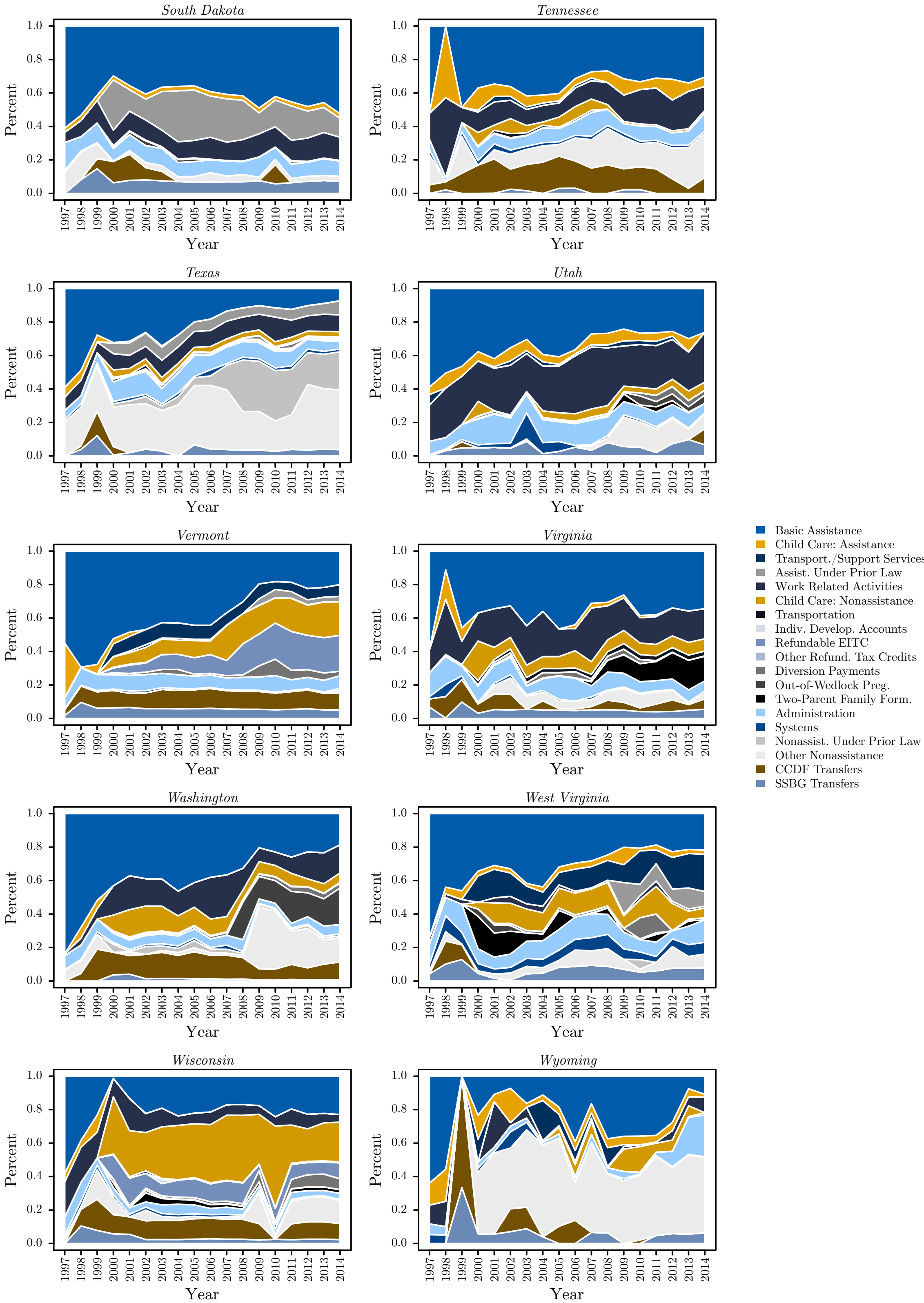
Source: U.S. Department of Health and Human Services; author's tabulations.

Figure G.1. TANF Category Expenditures by Percent of Total, 1997-2014
(*... continued*)



Source: U.S. Department of Health and Human Services; author's tabulations.

Figure G.1. TANF Category Expenditures by Percent of Total, 1997-2014
(... *continued*)



Source: U.S. Department of Health and Human Services; author's tabulations.

Antel, J. J. (1992): "The intergenerational transfer of welfare dependency: Some statistical evidence," *Review of Economics and Statistics*, 74, 467–473.

Becker, G. S. and N. Tomes (1979): "An equilibrium theory of the distribution of income and intergenerational mobility," *Journal of Political Economy*, 87, 1153–1189.

——— (1986): "Human capital and the rise and fall of families," *Journal of Labor Economics*, 4, S1–39.

Bitler, M. P. and H. W. Hoynes (2016): "Strengthening Temporary Assistance for Needy Families," The Hamilton Project, Policy Proposal 2016-04, Brookings Institution.

Black, D., M. Berger, and F. Scott (2000): "Bounding parameter estimates with nonclassical measurement error," *Journal of the American Statistical Association*, 95, 739–748.

Black, S. E. and P. J. Devereux (2011): "Recent developments in intergenerational mobility," *Handbook of Labor Economics*, 4, 1487–1541.

Blank, R. M. (2001): "What causes public assistance caseloads to grow?" *Journal of Human Resources*, 36, 85–118.

——— (2002): "Evaluating welfare reform in the United States," *Journal of Economic Literature*, 40, 1105–1166.

Bollinger, C. (1996): "Bounding mean regressions when a binary regressor is mismeasured," *Journal of Econometrics*, 73, 387–399.

Bollinger, C. and M. David (1997): "Modeling discrete choice with response error: Food stamp participation," *Journal of the American Statistical Association*, 92, 827–835.

——— (2001): "Estimation with response error and nonresponse: Food-stamp participation in the SIPP," *Journal of Business and Economic Statistics*, 19, 129–141.

——— (2005): "I didn't tell, and I won't tell: Dynamic response error in the SIPP," *Journal of Applied Econometrics*, 20, 563–569.

Borjas, G. J. and G. T. Sueyoshi (1997): "Ethnicity and the intergenerational transmission of welfare dependency," National Bureau of Economic Research, Research in Labor Economics 16: 271–295.

Brown, C. (1996): "Notes on the 'SEO' or 'Census' component of the PSID," *Report*, Survey Research Center, Institute for Social Research, University of Michigan, Ann Arbor, MI.

Cancian, M. and D. Reed (2009): "Family structure, childbearing, and parental employment: Implications for the level and trend in poverty," in *Changing Poverty, Changing Policies*, ed. by M. Cancian and S. Danziger, New York: Russell Sage Foundation, 92–121.

Committee on Ways and Means, U.S. House of Representatives (1994): "Section 10. Aid to Families with Dependent Children," *Background Material and Data on the Programs within the Jurisdiction of the Committee on Ways and Means*, (14th ed.), Washington, DC: U.S. Government Printing Office.

Corcoran, M., R. Gordon, D. Laren, and G. Solon (1992): "The association between men's economic status and their family and community origins," *Journal of Human Resources*, 27, 575–601.

Council of Economic Advisers (1997): "Explaining the decline in welfare receipt, 1993-1996," Technical report, Washington DC: Executive Office of the President.

Crouse, G. (1999): "State implementation of major changes to welfare policies, 1992-1998," Technical document, U.S. Department of Health and Human Services.

Currie, J. (2006): "The take up of social benefits," in *Public Policy and the Income Distribution*, ed. by A. Auerbach, D. Card, and J. M. Quigley, New York: Russell Sage, 80–148.

Dahl, G. B., A. R. Kostøl, and M. Mogstad (2014): "Family welfare cultures," *Quarterly Journal of Economics*, 129, 1711–1752.

DeParle, J. (2004): *American Dream: Three Women, Ten Kids, and a Nation's Drive to End Welfare*, New York: Penguin Books.

Donohue, J. J. and J. J. Heckman (1991): "Continuous versus episodic change: The impact of civil rights policy on the economic status of blacks," *Journal of Economic Literature*, 29, 1603–1643.

Duncan, G. J. and J. Brooks-Gunn (2000): "Family poverty, welfare reform, and child development," *Child Development*, 71, 188–196.

Duncan, G. J., M. S. Hill, and S. D. Hoffman (1988): "Welfare dependence within and across generations," *Science*, 239, 467–471.

Duncan, G. J. and S. Hoffman (1990): "Welfare benefits, economic opportunities, and out-of-wedlock births among black teenage girls," *Demography*, 27, 519–535.

Duncan, G. J. and W. J. Yeung (1995): "Extent and consequences of welfare dependence among America's children," *Children and Youth Service Review*, 17, 157–182.

Duncan, G. J., W. J. Yeung, J. Brooks-Gunn, and J. R. Smith (1998): "How much does childhood poverty affect the life chances of children?" *American Sociological Review*, 63, 406–423.

Durlauf, S. N. and I. Shaorshadze (2014): "Intergenerational mobility," in *Emerging Trends in the Social and Behavioral Sciences*, ed. by R. A. Scott and S. M. Kosslyn, Sherman Oaks: Sage Publishing, 505–537.

Elango, S., J. L. García, J. J. Heckman, and A. Hojman (2016): "Early childhood education," in *Economics of Means-Tested Transfer Programs in the United States*, ed. by R. A. Moffitt, University of Chicago Press, vol. II, 235–298.

Fitzgerald, J. M. (2011): "Attrition in models of intergenerational links using the PSID with extensions to health and to sibling models," *B.E. Journal of Economic Analysis & Policy*, 11, 1–63.

Fitzgerald, J. M., P. T. Gottschalk, and R. A. Moffitt (1998): "An analysis of sample attrition in panel data: The Michigan Panel Study of Income Dynamics," *Journal of Human Resources*, 33, 251–299.

Freeman, R. B. (1984): "Longitudinal analyses of the effects of trade unions," *Journal of Labor Economics*, 2, 105–128.

Frogner, B., R. A. Moffitt, and D. Ribar (2009): "How families are doing nine years after welfare reform: 2005 evidence from the Three-City Study," in *Welfare Reform and its Long-Term Consequences for America's Poor*, ed. by J. P. Ziliak, Cambridge University Press, 140–171.

Ganong, P. and J. Liebman (2013): "The decline, rebound, and further rise in SNAP enrollment: Disentangling business cycle fluctuations and policy changes," Working paper 19363, National Bureau of Economic Research.

Gelbach, J. (2004): "Migration, the life cycle, and state benefits: How low in the bottom?" *Journal of Political Economy*, 112, 1091–1130.

Gottschalk, P. T. (1990): "AFDC participation across generations," *American Economic Review: Papers & Proceedings*, 80, 367–371.

——— (1992): "The intergenerational transmission of welfare participation: Facts and possible causes," *Journal of Policy Analysis and Management*, 11, 254–272.

——— (1996): "Is the correlation in welfare participation across generations spurious?" *Journal of Public Economics*, 63, 1–25.

Gottschalk, P. T. and R. A. Moffitt (1994): "Welfare dependence: Concepts, measures, and trends," *American Economic Review: Papers & Proceedings*, 84, 38–42.

Grawe, N. (2006): "Lifecycle bias in estimates of intergenerational earnings persistence," *Labour Economics*, 13, 551–570.

Griliches, Z. and J. A. Hausman (1986): "Errors in variables in panel data," *Journal of Econometrics*, 31, 93–118.

Grogger, J. (2003): "The effects of time limits, the EITC, and other policy changes on welfare use, work, and income among female-headed families," *Review of Economics and Statistics*, 85, 394–408.

Grogger, J., S. J. Haider, and J. Klerman (2003): "Why did the welfare rolls fall during the 1990s?" *American Economic Review: Papers & Proceedings*, 93, 288–292.

Grogger, J. and L. A. Karoly (2005): *Welfare Reform: Effects of a Decade of Change*, Cambridge, MA: Harvard University Press.

Haider, S. J. and J. Klerman (2005): "Dynamic properties of the welfare caseload," *Labour Economics*, 12, 629–648.

Haider, S. J. and G. Solon (2006): "Life-cycle variation in the association between current and lifetime earnings," *American Economic Review*, 96, 1308–1320.

Haskins, R. (2007): *The Inside Story of the 1996 Welfare Reform Law*, Washington, DC: Brookings Institution Press.

Hausman, J. A., J. Abrevaya, and F. Scott-Morton (1998): "Misclassification of the dependent variable in a discrete-response setting," *Journal of Econometrics*, 87, 239–269.

Heckman, J. J. (1978): "Dummy endogenous variables in a simultaneous equation system," *Econometrica*, 46, 931–959.

Hoynes, H. and D. Schanzenbach (2012): "Work incentives and the Food Stamp Program," *Journal of Public Economics*, 96, 151–162.

Jenkins, S. (1987): "Snapshots versus movies: 'Lifecycle biases' and the estimation of intergenerational earnings inheritance," *European Economic Review*, 31, 1149–1158.

Kennan, J. and J. R. Walker (2010): "Wages, welfare benefits, and migration," *Journal of Econometrics*, 156, 229–238.

Kleibergen, F. and R. Paap (2006): "Generalized reduced rank tests using the singular value decomposition," *Journal of Econometrics*, 133, 97–126.

Kreider, B., J. V. Pepper, C. Gundersen, and D. Jolliffe (2012): "Identifying the effects of SNAP (food stamps) on child health outcomes when participation is endogenous and misreported," *Journal of the American Statistical Association*, 107, 958–975.

Kreider, B., J. V. Pepper, and M. Roy (2016): "Identifying the effects of WIC on food security among infants and children," *Southern Economic Journal*, 82, 1106–1122.

Kubik, J. (1999): "Incentives for the identification and treatment of children with disabilities: The Supplemental Security Income program," *Journal of Public Economics*, 73, 187–215.

Lee, C.-I. and G. Solon (2009): "Trends in intergenerational income mobility," *Review of Economics and Statistics*, 91, 766–772.

Levine, P. B. and D. J. Zimmerman (1996): "The intergenerational correlation in AFDC participation: Welfare trap or poverty trap?" Discussion paper 1100-96, University of Wisconsin Institute for Research on Poverty.

——— (1999): "An empirical analysis of the welfare magnet debate using the NLSY," *Journal of Population Economics*, 12, 391–409.

——— (2005): "Children's welfare exposure and subsequent development," *Journal of Public Economics*, 89, 31–56.

Lopoo, L. (2007): "Fertility trends, maternal characteristics, and poverty in the American South," *Journal of Poverty*, 11, 23–46.

Manski, C. F. (1995): *Identification Problems in the Social Sciences*, Cambridge, MA: Harvard University Press.

Mazumder, B. (2005): "Fortunate sons: New estimates of intergenerational mobility in the United States using social security earnings data," *Review of Economics and Statistics*, 87, 235–255.

McKinnish, T. (2007): "Cross-border welfare migration: New evidence from micro data," *Journal of Public Economics*, 91, 437–450.

McLanahan, S. (1988): "Family structure and dependency: Early transitions to female household headship," *Demography*, 25, 1–16.

Meyer, B., R. Goerge, and N. Mittag (2014): "Errors in survey reporting and imputation and their effects on estimates of Food Stamp Program participation," Mimeo.

Meyer, B. and N. Mittag (2014): "Misclassification in binary choice models," Working paper 20509, National Bureau of Economic Research.

——— (2015): "Using linked survey and administrative data to better measure income: Implications for poverty, program effectiveness, and holes in the safety net," Working paper 21676, National Bureau of Economic Research.

Meyer, B., W. Mok, and J. Sullivan (2015a): "Household surveys in crisis," *Journal of Economic Perspectives*, 29, 199–226.

——— (2015b): "The under-reporting of transfers in household surveys: Its nature and consequences," Working paper 15181 (updated version June 2015), National Bureau of Economic Research.

Moffitt, R. A. (1983): "An economic model of welfare stigma," *American Economic Review*, 73, 1023–1035.

——— (1992): "Incentive effects of the U.S. welfare system: A review," *Journal of Economic Literature*, 30, 1–61.

——— (2003): "The Temporary Assistance to Needy Families program," in *Means-Tested Transfer Programs in the United States*, ed. by R. A. Moffitt, Chicago: University of Chicago Press, 291–364.

Moffitt, R. A. and P. T. Gottschalk (2002): "Trends in the transitory variance of earnings in the United States," *Economic Journal*, 112, C68–C73.

Molinari, F. (2008): "Partial identification of probability distributions with misclassified data," *Journal of Econometrics*, 144, 81–117.

Murray, C. (1984): *American Social Policy, 1950-1980*, New York: Basic Books.

Nybom, M. and J. Stuhler (2016): "Heterogeneous income profiles and life-cycle bias in intergenerational mobility estimation," *Journal of Human Resources*, 51, 239–268.

Page, M. E. (2004): "New evidence on the intergenerational correlation in welfare participation," in *Generational Income Mobility in North America and Europe*, ed. by M. Corak, Cambridge University Press, 226–244.

Panel Study of Income Dynamics (2016): Public use dataset. Produced and distributed by the Survey Research Center, Institute for Social Research, University of Michigan, Ann Arbor, MI.

Pepper, J. V. (2000): "The intergenerational transmission of welfare receipt: A nonparametric bounds analysis," *Review of Economics and Statistics*, 82, 472–488.

Schmidt, L. and P. Sevak (2004): "AFDC, SSI, and welfare reform aggressiveness: Caseload reductions vs. caseload shifting," *Journal of Human Resources*, 39, 792–812.

Smith, J. P. and F. R. Welch (1989): "Black economic progress after Myrdal," *Journal of Economic Literature*, 27, 519–564.

Solon, G. (1992): "lntergenerational income mobility in the United States," *American Economic Review*, 82, 393–408.

——— (1999): "Intergenerational mobility in the labor market," *Handbook of Labor Economics*, 3, 1761–1800.

Solon, G., M. Corcoran, R. Gordon, and D. Laren (1988): "Sibling and intergenerational correlations in welfare program participation," *Journal of Human Resources*, 23, 388–396.

Solon, G., S. J. Haider, and J. M. Wooldridge (2015): "What are we weighting for?" *Journal of Human Resources*, 50, 301–316.

Wolfe, B. L., R. Haveman, D. Ginther, and C. B. An (1996): "The 'window problem' in studies of children's attainments: A methodological exploration," *Journal of the American Statistical Association*, 91, 970–982.

Ziliak, J. P. (2007): "Making work pay: Changes in effective tax rates and guarantees in U.S. transfer programs, 1983-2002," *Journal of Human Resources*, 42, 619–642.

——— (2015): "Why are so many Americans on food stamps? The role of the economy, policy, and demographics," in *SNAP Matters: How Food Stamps Affect Health and Well Being*, ed. by J. Bartfeld, C. Gundersen, T. Smeeding, and J. P. Ziliak, Stanford, CA: Stanford University Press, 18–48.

——— (2016): "Temporary Assistance for Needy Families," in *Economics of Means-Tested Transfer Programs in the United States*, ed. by R. A. Moffitt, University of Chicago Press, vol. I, 303–394.

Ziliak, J. P., D. Figlio, E. Davis, and L. Connolly (2000): "Accounting for the decline in AFDC caseloads: Welfare reform or the economy?" *Journal of Human Resources*, 35, 570–586.

Ziol-Guest, K. M., G. J. Duncan, A. Kalil, and W. T. Boyce (2012): "Early childhood poverty, immune-mediated disease processes, and adult productivity," *Proceedings of the National Academy of Sciences*, 109, 17289–17293.

ASHENFELTER, O. (1978): "Estimating the effect of training programs on earnings," *Review of Economics and Statistics*, 60, 47–57.

——— (1983): "Determining participation in income-tested social programs," *Journal of the American Statistical Association*, 78, 517–525.

BANERJEE, A., R. HANNA, G. KREINDLER, AND B. A. OLKEN (2017): "Debunking the stereotype of the lazy welfare recipient: Evidence from cash transfer programs," *World Bank Research Observer*, forthcoming.

BARGAIN, O. AND K. ORSINI (2006): "In-work policies in Europe: Killing two birds with one stone?" *Labour Economics*, 13, 667–697.

BECKER, G. S. (1965): "A theory of the allocation of time," *Economic Journal*, 75, 493–517.

BEFFY, M., R. BLUNDELL, A. BOZIO, G. LAROQUE, AND M. TO (2016): "Labour supply and taxation with restricted choices," Working paper w15/02, Institute for Fiscal Studies.

BELLONI, A. AND V. CHERNOZHUKOV (2011): "ℓ_1-penalized quantile regression in high-dimensional sparse models," *The Annals of Statistics*, 39, 82–130.

BELLONI, A., V. CHERNOZHUKOV, I. FERNÁNDEZ-VAL, AND C. HANSEN (2017): "Program evaluation and causal inference with high-dimensional data," *Econometrica*, 85, 233–298.

BERGOLO, M. AND G. CRUCES (2016): "The anatomy of behavioral responses to social assistance when informal employment is high," Discussion paper 10197, Institute for the Study of Labor (IZA).

BITLER, M. P., J. B. GELBACH, AND H. W. HOYNES (2006): "What mean impacts miss: Distributional effects of welfare reform experiments," *American Economic Review*, 96, 988–1012.

BLANK, R. M., D. E. CARD, AND P. K. ROBINS (2000): "Financial incentives for increasing work and income among low-income families," in *Finding Jobs: Work and Welfare Reform*, ed. by R. M. Blank and D. E. Card, New York, NY: Russell Sage Foundation, 373–419.

BLANK, R. M. AND P. RUGGLES (1996): "When do women use Aid to Families with Dependent Children and Food Stamps? The dynamics of eligibility versus participation," *Journal of Human Resources*, 31, 57–89.

BLOOM, D., S. SCRIVENER, C. MICHALOPOULOS, P. A. MORRIS, R. HENDRA, D. ADAMS-CIARDULLO, AND J. WALTER (2002): "Jobs First: Final report on Connecticut's welfare reform initiative," Report, MDRC, New York, NY.

Blundell, R., T. MaCurdy, and C. Meghir (2007): "Labor supply models: Unobserved heterogeneity, nonparticipation and dynamics," *Handbook of Econometrics*, 6, 4667–4775.

Bollinger, C., L. Gonzalez, and J. P. Ziliak (2009): "Welfare reform and the level and composition of income," in *Welfare Reform and Its Long-Term Consequences for America's Poor*, ed. by J. P. Ziliak, New York, NY: Cambridge University Press, 59–103.

Brewer, M., A. Duncan, A. Shephard, and M. J. Suárez (2006): "Did working families' tax credit work? The impact of in-work support on labour supply in Great Britain," *Labour Economics*, 13, 699–720.

Burtless, G. and J. A. Hausman (1978): "The effect of taxation on labor supply: Evaluating the Gary negative income tax experiment," *Journal of Political Economy*, 86, 1103–1130.

Cattaneo, M. D. (2010): "Efficient semiparametric estimation of multi-valued treatment effects under ignorability," *Journal of Econometrics*, 155, 138–154.

Chernozhukov, V. and H. Hong (2002): "Three-step censored quantile regression and extramarital affairs," *Journal of the American Statistical Association*, 97, 872–882.

Currie, J. (2006): "The take up of social benefits," in *Public Policy and the Income Distribution*, ed. by A. Auerbach, D. Card, and J. M. Quigley, New York: Russell Sage, 80–148.

Firpo, S. (2007): "Efficient semiparametric estimation of quantile treatment effects," *Econometrica*, 75, 259–276.

Fitzenberger, B. (1997): "A guide to censored quantile regressions," *Handbook of Statistics*, 15, 405–437.

González, L. (2008): "Single mothers, welfare, and incentives to work," *Labour*, 22, 447–468.

Gottschalk, P. T. (2005): "Can work alter welfare recipients' beliefs?" *Journal of Policy Analysis and Management*, 24, 485–498.

Harding, M. and C. Lamarche (2017): "Penalized quantile regression with semiparametric correlated effects: An application with heterogeneous preferences," *Journal of Applied Econometrics*, 32.

Heckman, J. J., H. Ichimura, J. Smith, and P. Todd (1998): "Characterizing selection bias using experimental data," *Econometrica*, 66, 1017–1098.

Heckman, J. J., J. Smith, and N. Clements (1997): "Making the most out of programme evaluations and social experiments: Accounting for heterogeneity in programme impacts," *Review of Economic Studies*, 64, 487–535.

Imai, K. and M. Ratkovic (2013): "Estimating treatment effect heterogeneity in randomized program evaluation," *Annals of Applied Statistics*, 7, 443–470.

Immervoll, H., H. J. Kleven, C. T. Kreiner, and E. Saez (2007): "Welfare reform in European countries: A microsimulation analysis," *Economic Journal*, 117, 1–44.

Kline, P. and M. Tartari (2016): "Bounding the labor supply responses to a randomized welfare experiment: A revealed preference approach," *American Economic Review*, 106, 972–1014.

Koenker, R. (2004): "Quantile regression for longitudinal data," *Journal of Multivariate Analysis*, 91, 74–89.

——— (2005): *Quantile Regression*, New York, NY: Cambridge University Press.

Koenker, R. and G. Bassett, Jr (1978): "Regression quantiles," *Econometrica*, 46, 33–50.

Lamarche, C. (2010): "Robust penalized quantile regression estimation for panel data," *Journal of Econometrics*, 157, 396–408.

Meghir, C. and L. Pistaferri (2004): "Income variance dynamics and heterogeneity," *Econometrica*, 72, 1–32.

Meyer, B. D. and D. T. Rosenbaum (2001): "Welfare, the Earned Income Tax Credit, and the labor supply of single mothers," *Quarterly Journal of Economics*, 116, 1063–1114.

Moffitt, R. A. (1983): "An economic model of welfare stigma," *American Economic Review*, 73, 1023–1035.

——— (2002): "Welfare programs and labor supply," *Handbook of Public Economics*, 4, 2393–2430.

——— (2003): "The Temporary Assistance to Needy Families program," in *Means-Tested Transfer Programs in the United States*, ed. by R. A. Moffitt, Chicago: University of Chicago Press, 291–364.

Mogstad, M. and C. Ponzato (2012): "Are lone mothers responsive to policy changes? Evidence from a workfare reform in a generous welfare state," *Scandinavian Journal of Economics*, 114, 1129–1159.

Powell, J. L. (1986): "Censored regression quantiles," *Journal of Econometrics*, 32, 143–155.

Rubin, D. B. (1977): "Assignment to treatment group on the basis of a covariate," *Journal of Educational Statistics*, 2, 1–26.

Saez, E. (2010): "Do taxpayers bunch at kink points?" *American Economic Journal: Economic Policy*, 2, 180–212.

Słoczyński, T. and J. M. Wooldridge (2017): "A general double robustness result for estimating average treatment effects," *Econometric Theory*, 1–22.

Ziliak, J. P. (2016): "Temporary Assistance for Needy Families," in *Economics of Means-Tested Transfer Programs in the United States*, ed. by R. A. Moffitt, University of Chicago Press, vol. I, 303–394.

Aizer, A., S. Eli, J. Ferrie, and A. Lleras-Muney (2016): "The long-run impact of cash transfers to poor families," *American Economic Review*, 106, 935–971.

Athey, S. and G. W. Imbens (2006): "Identification and inference in nonlinear difference-in-differences models," *Econometrica*, 74, 431–497.

Baicker, K. and N. Gordon (2006): "The effect of state education finance reform on total local resources," *Journal of Public Economics*, 90, 1519–1535.

Baker, M., J. Gruber, and K. Milligan (2008): "Universal child care, maternal labor supply, and family well-being," *Journal of Political Economy*, 116, 709–745.

Barnett, W. S. and L. N. Masse (2007): "Early childhood program design and economic returns: Comparative benefit-cost analysis of the Abecedarian program and policy implications," *Economics of Education Review*, 26, 113–125.

Bennett, N. G., H.-H. Lu, and Y. Song (2004): "Welfare reform and changes in the economic well-being of children," *Population Research and Policy Review*, 23, 671–699.

Bernal, R. and M. P. Keane (2011): "Child care choices and children's cognitive achievement: The case of single mothers," *Journal of Labor Economics*, 29, 459–512.

Bitler, M. P., H. W. Hoynes, and T. Domina (2014): "Experimental evidence on distributional effects of Head Start," Working Paper 20434, National Bureau of Economic Research.

Bruins, M. (2016): "Child care and child well-being in sole-parent families," Mimeo.

Candelaria, C. A. and K. A. Shores (2017): "Court-ordered finance reforms in the adequacy era: Heterogeneous causal effects and sensitivity," *Education Finance and Policy*, forthcoming.

Cascio, E. U. and S. Reber (2013): "The poverty gap in school spending following the introduction of Title I," *American Economic Review: Papers & Proceedings*, 103, 423–427.

Chang, Y. E., A. C. Huston, D. A. Crosby, and L. A. Gennetian (2007): "The effects of welfare and employment programs on children's participation in Head Start," *Economics of Education Review*, 26, 17–32.

Chetty, R., N. Hendren, P. Kline, and E. Saez (2014): "Where is the land of opportunity? The geography of intergenerational mobility in the United States," *Quarterly Journal of Economics*, 129, 1553–1623.

Committee on Ways and Means, U.S. House of Representatives (2000): "Appendix L. Monitoring the effects of pre- and post-TANF welfare reform initiatives," *Background Material and Data on the Programs within the Jurisdiction of the Committee on Ways and Means*, (17th ed.), Washington, DC: U.S. Government Printing Office.

Cunha, F. and J. J. Heckman (2007): "The technology of skill formation," *American Economic Review*, 97, 31–47.

Cunha, F., J. J. Heckman, L. Lochner, and D. V. Masterov (2006): "Interpreting the evidence on life cycle skill formation," *Handbook of the Economics of Education*, 1, 697–812.

Currie, J. and D. Almond (2011): "Human capital development before age five," *Handbook of Labor Economics*, 4, 1315–1486.

Dahl, G. B. and L. Lochner (2012): "The impact of family income on child achievement: Evidence from the Earned Income Tax Credit," *American Economic Review*, 102, 1927–1956.

Danziger, S. H. (2003): "After welfare reform and an economic boom: Why is child poverty so much higher in the U.S. than in Europe?" in *Children and Social Security*, ed. by J. Bradshaw, London: Ashgate, 3–35.

Danziger, S. K. and S. H. Danziger (2008): "Child poverty in economic and public policy contexts," in *Educating the Other America: Top Experts Tackle Poverty, Literacy, and Achievement in Our Schools*, ed. by S. B. Neuman, Baltimore: Paul H. Brookes Publishing Company, 19–34.

Del Boca, D., C. Flinn, and M. Wiswall (2014): "Household choices and child development," *Review of Economic Studies*, 81, 137–185.

Del Boca, D., C. Monfardini, and C. Nicoletti (2017): "Parental and child time investments and the cognitive development of adolescents," *Journal of Labor Economics*, 35, 565–608.

Deming, D. (2009): "Early childhood intervention and life-cycle skill development: Evidence from Head Start," *American Economic Journal: Applied Economics*, 1, 111–134.

DeParle, J. (2004): *American Dream: Three Women, Ten Kids, and a Nation's Drive to End Welfare*, New York: Penguin Books.

Dewey, J., T. A. Husted, and L. W. Kenny (2000): "The ineffectiveness of school inputs: A product of misspecification?" *Economics of Education Review*, 19, 27–45.

Duffy, D. and N. Sastry (2014): "Achievement tests in the Panel Study of Income Dynamics Child Development Supplement," Technical Series Paper #14-02, Survey Research Center - Institute for Social Research, University of Michigan.

Duncan, G. J. and J. Brooks-Gunn (2000): "Family poverty, welfare reform, and child development," *Child Development*, 71, 188–196.

Durlauf, S. N. (1996): "A theory of persistent income inequality," *Journal of Economic Growth*, 1, 75–93.

——— (2006): "Groups, social influences, and inequality: A memberships theory perspective on poverty traps," in *Poverty Traps*, ed. by S. Bowles, S. N. Durlauf, and K. Hoff, Princeton, NJ: Princeton University Press, 141–175.

Elango, S., J. L. García, J. J. Heckman, and A. Hojman (2016): "Early childhood education," in *Economics of Means-Tested Transfer Programs in the United States*, ed. by R. A. Moffitt, University of Chicago Press, vol. II, 235–298.

Figlio, D. N. and D. Fletcher (2012): "Suburbanization, demographic change and the consequences for school finance," *Journal of Public Economics*, 96, 1144–1153.

Fryer, Jr, R. G. and L. F. Katz (2013): "Achieving escape velocity: Neighborhood and school interventions to reduce persistent inequality," *American Economic Review*, 103, 232–237.

Fuller, B., S. L. Kagan, G. L. Caspary, and C. A. Gauthier (2002): "Welfare reform and child care options for low-income families," *Children and Welfare Reform*, 97.

Garces, E., D. Thomas, and J. Currie (2002): "Longer-term effects of Head Start," *American Economic Review*, 92, 999–1012.

Gelber, A. and A. Isen (2013): "Children's schooling and parents' behavior: Evidence from the Head Start Impact Study," *Journal of Public Economics*, 101, 25–38.

Gordon, N. (2014): "Explaining trends in high school graduation: The changing elementary and secondary education policy landscape and income inequality over the last half century," in *Human Capital and History: The American Record*, ed. by L. P. Boustan, C. Frydman, and R. A. Margo, Chicago: University of Chicago Press, 59–89.

Greenberg, M. H. (1998): "Spend or transfer, federal or state? Considerations in using TANF and TANF-related dollars for child care," Policy report, Center for Law and Social Policy, Washington, DC.

Hahn, H., O. Golden, and A. Stanczyk (2012): "State approaches to the TANF block grant: Welfare is not what you think it is," Working Families Paper 20, Urban Institute.

Hanushek, E. A. and J. A. Somers (2001): "Schooling, inequality, and the impact of government," in *The Causes and Consequences of Increasing Inequality*, ed. by F. Welch, Chicago: University of Chicago Press, 169–200.

Hardy, B. L. and S. Gershenson (2016): "Parental involvement and the intergenerational transmission of educational attainment," Working paper, American University.

Harknett, K., I. Garfinkel, J. Bainbridge, T. M. Smeeding, N. Fobre, and S. McLanahan (2005): "Are public expenditures associated with better child outcomes in the U.S.? A comparison across 50 states," *Analyses of Social Issues and Public Policy*, 5, 103–125.

Havnes, T. and M. Mogstad (2015): "Is universal child care leveling the playing field?" *Journal of Public Economics*, 127, 100–114.

Heckman, J. J. and T. Kautz (2012): "Hard evidence on soft skills," *Labour Economics*, 19, 451–464.

Heckman, J. J., S. H. Moon, R. Pinto, P. A. Savelyev, and A. Yavitz (2010): "The rate of return to the HighScope Perry Preschool Program," *Journal of Public Economics*, 94, 114–128.

Heckman, J. J. and S. Mosso (2014): "The economics of human development and social mobility," *Annual Review of Economics*, 6, 689–733.

Heckman, J. J., R. Pinto, and P. Savelyev (2013): "Understanding the mechanisms through which an influential early childhood program boosted adult outcomes," *American Economic Review*, 103, 1–35.

Henly, J. R. and S. Lambert (2005): "Nonstandard work and child-care needs of low-income parents," in *Work, Family, Health, and Well-being*, ed. by S. M. Bianchi, L. M. Casper, and R. B. King, Routledge, 473–492.

Herbst, C. M. (2017a): "Are parental welfare work requirements good for disadvantaged children? Evidence from age-of-youngest-child exemptions," *Journal of Policy Analysis and Management*, 36, 327–357.

——— (2017b): "Universal child care, maternal employment, and children's long-run outcomes: Evidence from the US Lanham Act of 1940," *Journal of Labor Economics*, 35, 519–564.

Hoxby, C. M. (2001): "All school finance equalizations are not created equal," *Quarterly Journal of Economics*, 116, 1189–1231.

Jackson, C. K., R. C. Johnson, and C. Persico (2016): "The effects of school spending on educational and economic outcomes: Evidence from school finance reforms," *Quarterly Journal of Economics*, 131, 157–218.

Kalil, A. and R. Dunifon (2007): "Maternal work and welfare use and child well-being: Evidence from 6 years of data from the Women's Employment Study," *Children and Youth Services Review*, 29, 742–761.

Katz, L. F. and A. B. Krueger (2016): "The rise and nature of alternative work arrangements in the United States, 1995-2015," Working paper 603, Princeton University, Industrial Relations Section.

Kline, P. and C. Walters (2016): "Evaluating public programs with close substitutes: The case of Head Start," *Quarterly Journal of Economics*, 131, 1795–1848.

Kottelenberg, M. J. and S. F. Lehrer (2016): "Targeted or universal coverage? Assessing heterogeneity in the effects of universal childcare," *Journal of Labour Economics*, forthcoming.

Lafortune, J., J. Rothstein, and D. W. Schanzenbach (2016): "School finance reform and the distribution of student achievement," Working paper 22011, National Bureau of Economic Research.

Levine, P. B. and D. J. Zimmerman (2005): "Children's welfare exposure and subsequent development," *Journal of Public Economics*, 89, 31–56.

Ludwig, J. and D. L. Miller (2007): "Does Head Start improve children's life chances? Evidence from a regression discontinuity design," *Quarterly Journal of Economics*, 122, 159–208.

Lynch, K. E. (2016): "Trends in child care spending from the CCDF and TANF," Report 44528, Congressional Research Service.

Michalopoulos, C. (2010): "Evaluation of child care subsidy strategies: Alternative eligibility and redetermination policies in Illinois," Administration for Children and Families, U.S. Department of Health and Human Services, Washington, D.C.

Michel, S. (1999): *Children's Interests/Mothers' Rights: The Shaping of America's Child Care Policy*, New Haven: Yale University Press.

Miller, A. R. and L. Zhang (2009): "The effects of welfare reform on the academic performance of children in low-income households," *Journal of Policy Analysis and Management*, 28, 577–599.

——— (2012): "Intergenerational effects of welfare reform on educational attainment," *Journal of Law and Economics*, 55, 437–476.

Moffitt, R. A. (2003): "The Temporary Assistance to Needy Families program," in *Means-Tested Transfer Programs in the United States*, ed. by R. A. Moffitt, Chicago: University of Chicago Press, 291–364.

Morris, P. A., L. A. Gennetian, G. Duncan, and A. Huston (2009): "How welfare policies affect child and adolescent school performance: Investigating pathways of influence," in *Welfare Reform and Its Long-term Consequences for America's Poor*, ed. by J. P. Ziliak, 255–289.

Morris, P. A., L. A. Gennetian, and G. J. Duncan (2005): "Effects of welfare and employment policies on young children: New findings on policy experiments conducted in the early 1990s," *Social Policy Report*, 19, 3–18.

Morris, P. A. and R. Hendra (2009): "Losing the safety net: How welfare time limits affect families and children?" *Developmental Psychology*, 45, 383–400.

Noble, C. (1997): *Welfare As We Knew It: A Political History of the American Welfare State*, New York, NY: Oxford University Press.

Olivetti, C. and B. Petrongolo (2017): "The economic consequences of family policies: Lessons from a century of legislation in high-income countries," *Journal of Economic Perspectives*, 31, 205–230.

Schumacher, R., M. Greenberg, and J. Duffy (2001): "The impact of TANF funding on state child care subsidy programs," Policy report, Center for Law and Social Policy, Washington, DC.

Shaefer, H. L., G. Duncan, K. Edin, I. Garfinkel, D. Harris, T. Smeeding, J. Waldfogel, C. Wimer, and H. Yoshikawa (2016): "A universal child allowance: A plan to reduce poverty and income instability among children," Mimeo.

Shaefer, H. L. and K. Edin (2013): "Rising extreme poverty in the United States and the response of federal means-tested transfer programs," *Social Service Review*, 87, 250–268.

Walters, C. (2015): "Inputs in the production of early childhood human capital: Evidence from Head Start," *American Economic Journal: Applied Economics*, 7, 76–102.

Ziliak, J. P. (2014): "Supporting low-income workers through refundable child care credits," in *Policies to Address Poverty in America*, ed. by M. Kearney and B. Harris, Brookings Institution: Hamilton Project, 109–118.

——— (2016): "Temporary Assistance for Needy Families," in *Economics of Means-Tested Transfer Programs in the United States*, ed. by R. A. Moffitt, University of Chicago Press, vol. I, 303–394.

REFERENCES for APPENDIX

BERNAL, R. AND M. P. KEANE (2011): "Child care choices and children's cognitive achievement: The case of single mothers," *Journal of Labor Economics*, 29, 459–512.

BILLINGSLEY, P. (1995): *Probability and Measure*, New York: John Wiley & Sons.

BITLER, M. P., J. B. GELBACH, AND H. W. HOYNES (2006): "What mean impacts miss: Distributional effects of welfare reform experiments," *American Economic Review*, 96, 988–1012.

BLAU, D. M. (2003): "Child care subsidy programs," in *Means-tested transfer programs in the United States*, ed. by R. A. Moffitt, University of Chicago Press, 443–516.

BLAU, D. M. AND J. CURRIE (2006): "Pre-school, day care, and after-school care: Who's minding the kids?" in *Handbook of the Economics of Education*, ed. by E. Hanushek, S. Machin, and L. Woessmann, Amsterdam: Elsevier, vol. 2, 1163–1278.

BLAU, D. M. AND A. P. HAGY (1998): "The demand for quality in child care," *Journal of Political Economy*, 106, 104–146.

HARDING, M. AND C. LAMARCHE (2014): "Estimating and testing a quantile regression model with interactive effects," *Journal of Econometrics*, 178, 101–113.

KLEIBERGEN, F. AND R. PAAP (2006): "Generalized reduced rank tests using the singular value decomposition," *Journal of Econometrics*, 133, 97–126.

KOENKER, R. (2005): *Quantile Regression*, New York, NY: Cambridge University Press.

LAMARCHE, C. (2010): "Robust penalized quantile regression estimation for panel data," *Journal of Econometrics*, 157, 396–408.

MEYER, B., W. MOK, AND J. SULLIVAN (2015): "The under-reporting of transfers in household surveys: Its nature and consequences," Working paper 15181 (updated version June 2015), National Bureau of Economic Research.

MORGAN, G. (1986): "Supplemental care for young children," in *In Support of Families*, ed. by M. W. Yogman and T. B. Brazelton, Cambridge, MA: Harvard University Press, 156–170.

VITA

Robert Paul Hartley

1. Education:

2007–2008	MA Economics, *Georgia State University*, Atlanta, GA
2001–2003	MDiv Theology [2013], *Emmanuel School of Religion*, Johnson City, TN
1996–2000	BS Industrial Engineering, *Georgia Institute of Technology*, Atlanta, GA

2. Fields: Labor economics, public economics

3. Research:

Hartley, Robert Paul, Lamarche, Carlos, and Ziliak, James P. (2017): "Welfare Reform and the Intergenerational Transmission of Dependence", University of Kentucky Center for Poverty Research Discussion Paper Series, DP2016-01.

Hartley, Robert Paul, and Lamarche, Carlos (2017): "Behavioral Responses and Welfare Reform: Evidence from a Randomized Experiment", University of Kentucky Center for Poverty Research Discussion Paper Series, DP2014-11.

4. Academic Experience:

Research Assistant

2013–2016	University of Kentucky Center for Poverty Research
2012–2013	Research assistant to Professor Gail Hoyt

Instructor of Record

2017	ECO 202: Principles of Economics II (1 section)
2013	ECO 391: Business & Economic Statistics (1 section)

Teaching Assistant

2012–2013	ECO 201: Introductory Microeconomics (2 sections)
2012–2013	ECO 391: Business & Economic Statistics (3 sections)
2012	ECO 401: Intermediate Microeconomics (1 section)

5. Honors:

2016–2017	Dissertation Year Fellowship, The Graduate School, University of Kentucky
2015–2016	Luckett Fellowship, Gatton College, University of Kentucky
2013–2015	Gatton Fellowship, Gatton College, University of Kentucky
2012–2013	Max Steckler Fellowship, Gatton College, University of Kentucky

6. Presentations:

Conferences & Workshops

2016	Southern Economic Association Meetings, Washington, DC
2016	Department of Economics Workshop, University of Kentucky
2016	Tennessee Empirical Applied Microeconomics Festival, University of Tennessee
2015	Kentucky Economic Association Conference, Frankfort, Kentucky
2015	Department of Economics Workshop, University of Kentucky
2015	Summer School on Socioeconomic Inequality, University of Chicago

Guest Lectures

2016	ECO 491G: Applied Econometrics, "Instrumental Variable Methods"
2016	ECO 731: Labor Economics, "Generational Mobility in Labor Market Outcomes"
2016	ECO 731: Labor Economics, "Methods for Static Labor Supply Estimation"
2016	PA 624: Government Information Systems, "Panel Study of Income Dynamics"
2014, 2016	ECO 201: Principles of Microeconomics, "Value of Trade Experiment"
2015	ECO 391: Business & Economic Statistics, "Interactive Regression Experiment"
2015	ECO 101: Contemporary Economic Issues, "Poverty and Income Inequality"
2014	ECO 731: Labor Economics, "Distributional Effects of Welfare Reform"
2013, 2014	ECO 499: Economics of Africa, "Microfinance and Development in Rwanda"

7. Related Experience & Professional Service:

2014	Supervisor for Undergraduate Research Assistant, Center for Poverty Research
2014	Microteaching Instructor, Graduate School Orientation, University of Kentucky
2008–2010	Savings and Credit Associations Specialist, HOPE International, Rwanda/U.S.
2007	Staff Advisor, Economics Empowerment Initiative, Georgia State University
2007	Graduate Assistant, Office of Civic Engagement, Georgia State University