Welfare Reform and the Intergenerational Transmission of Dependence*

Robert Paul Hartley
Center on Poverty and Social Policy
Columbia University

Carlos Lamarche
Department of Economics
University of Kentucky

James P. Ziliak
Center for Poverty Research and
Department of Economics
University of Kentucky

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Abstract

We estimate the effect of welfare reform on the intergenerational transmission of welfare participation, and related economic outcomes, using a long panel of mother-daughter pairs over the survey period 1968-2013 in the Panel Study of Income Dynamics. Because states implemented welfare reform at different times starting in 1992, the cross-state variation over time permits us to quasi-experimentally separate out the effect of mothers' welfare participation during childhood on daughters' economic outcomes in adulthood in the pre- and post-welfare reform periods. We find that a mother's welfare participation increased her daughter's odds of participation as an adult by roughly 25 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 we 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. These results are obtained by addressing nonrandom selection into welfare and are robust to other potential threats to identification from misclassification error, life-cycle age effects, and cross-state mobility.

* Address correspondence to James P. Ziliak, University of Kentucky, Center for Poverty Research, Gatton Building Suite 234, 550 South Limestone St., Lexington, KY 40506-0034. Email: jziliak@uky.edu. We are grateful for comments from the editor, James Heckman, seven anonymous reviewers, Anna Aizer, Richard Blundell, Chris Bollinger, Paul Devereux, Steven Durlauf, Hilary Hoynes, Mike Keane, Henrik Kleven, Robert Moffitt, Bill Neilson, Lara Shore-Sheppard, Jeff Smith, Lowell Taylor, Genia Toma, Marianne Wannamaker, and participants at the 2015 Summer School on Socioeconomic Opportunity, 2016 NBER Summer Institute, 2016 Barcelona GSE Summer Forum, 2016 APPAM Conference, 2016 Tennessee Empirical Applied Microeconomics Festival, Columbia University, Institute for Fiscal Studies, Lancaster University, London School of Economics, Ohio State University, Nuffield College (Oxford), University College Dublin, University of Oklahoma, University of Oregon, and VATT Institute for Economic Policy. All errors are our own.

I. 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 (Murray, 1984; DeParle, 2004; Haskins, 2007). 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 (Duncan, Hill, and Hoffman, 1988; Solon et al., 1988; Gottschalk, 1990, 1992, 1996; Levine and Zimmerman, 1996; Pepper, 2000; Page, 2004; Dahl, Kostøl, and Mogstad, 2014). Instead, the parent-child link in welfare participation could simply be a spurious by-product of incomes that are correlated across generations, by which some persistence in welfare participation could be attributed to a poverty trap as opposed to a welfare trap. 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 we would 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; Moffitt, 2003; Grogger and Karoly, 2005; Ziliak, 2016), but to date there has not been research on whether it achieved a key aim of ending generational welfare dependence. In this paper, we 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, we 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.

We begin our analysis by documenting descriptive correlations in intergenerational income mobility and welfare participation using rolling cohorts of mother-daughter pairs over the survey period 1968-2013 in the Panel Study of Income Dynamics (PSID). We focus on mother-daughter pairs both because over 90 percent of the cases on the main welfare program—Aid to Families with Dependent Children (AFDC)—were single mothers, 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 (Levine and Zimmerman, 2005; Cancian and Reed, 2009). The analysis highlights that in the period prior to welfare reform, declining income mobility coincided with rising generational correlations in welfare use, and after reform, stagnating income mobility coincided with declining correlations in AFDC but stable correlations when welfare use is more broadly defined to include food and disability assistance. This suggests that the poverty trap argument of welfare dependence cannot be ruled out ex ante.

We then develop an empirical framework that builds on a canonical Becker-Tomes (1979) transmission model that separates state dependence (welfare trap) from unobserved heterogeneity (poverty trap) in order to identify a causal pathway of welfare exposure during childhood on participation in adulthood. Specifically, we employ a difference-in-difference-type specification whereby the economic outcome of the daughter during adulthood is regressed on the prior welfare participation of the mother, a variable reflecting the implementation of welfare reform in the mother's state, and the interaction of the welfare-reform variable with mother's participation. Our identification strategy exploits the quasi-experimental variation provided by the 1990s reforms to the AFDC program. Starting in 1992, states began implementing substantive changes to their AFDC programs with waivers from federal rules, and by 1996, most 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 in passage of the Personal Responsibility and Work Opportunity Reconciliation Act of 1996, which replaced AFDC with the 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, as mentioned, selection bias in welfare participation across generations can arise through the poverty trap mechanism, which may relate to 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 (Solon et al., 1988; Gottschalk, 1992, 1996; Lindbeck, Nyberg, and Weibull, 1999; Pepper, 2000). The second threat to identification comes from potential misclassification bias in survey responses (Bollinger and David, 1997; 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, the so-called life-cycle bias and the 'window problem' may affect intergenerational estimates of economic status because we generally only observe snapshots of a parent and child and not their full life cycles (Wolfe et al., 1996; Page, 2004; Haider and Solon, 2006; Nybom and Stuhler, 2016). 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 (Levine and Zimmerman, 1999; Gelbach, 2004; McKinnish, 2007).

In our empirical model, we address potential endogenous selection into welfare by instrumenting for mother's welfare use. Because selection is likely to be time-varying, we instrument mother's welfare participation with the state maximum AFDC/TANF benefit standard when daughters are ages 12 to 18. The instrument is 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. Fundamentally, this aggregated measure of state-level policies identifies the portion of a mother's participation decision that is related to her welfare status separately from conditions related to her poverty status, and consequently, her daughter's future poverty status.

Next, we 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. We 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. We 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). Moreover, we 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. As a sensitivity check, we 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, we examine the sensitivity of estimates to possible endogenous migration by examining various subsamples of non-movers.

Our 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 25 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, we find that childhood exposure to welfare substantively increased 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, panel specific trends, attrition, life-cycle age adjustments, and misclassification error.

II. Welfare Reform

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. 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 had authority to set benefit standards (maximum benefit levels increasing 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 about 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. Several states filed waiver applications under President George H.W. Bush's administration, and then more states 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 (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 the new TANF program, 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 noncompliance is to deny the application. Seventeen states 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 family-size benefit (Ziliak, 2016).

II.A. Trends in Aggregate Participation

Figure 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 (Ziliak et al., 2000; Grogger, 2003). 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 (Grogger, Haider, and Klerman, 2003).

[Figure 1 here]

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. This categorical eligibility remained after the introduction of TANF for those receiving cash assistance. 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 members on AFDC and some on SSI (and still also qualify for food stamps). These provisions remain after welfare reform.

Figure 1 also presents trends in the number of recipients on SNAP and SSI. There was a marked drop in SNAP 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 (Ziliak, 2015; Ganong and Liebman, 2018). 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.

II.B. Intergenerational Correlations in Economic Status

As motivating evidence for the possible role of welfare reform on the intergenerational transmission of dependence, we first examine overall trends in daughter's relative economic mobility vis-a-vis her mother's economic status. These correlations are of independent interest as much of the literature has either focused on the mobility of sons, has daughter estimates that mostly pre-date welfare reform, or in the case of some of the recent literature, relies primarily on tax data and thus does not

capture welfare income because it is generally not taxable (Solon, 1999; Mazumder, 2005; Black and Devereaux, 2011; Lee and Solon, 2009; Chetty et al., 2017).

For this preliminary work, we construct a series of rolling cohorts of mother-daughter pairs from the PSID similar to that used by Page (2004). Specifically, in each year *t* we select daughters ages 27-42 years old who have formed their own family unit, and we estimate the correlation coefficient ρ_t by regressing an outcome of the daughter, denoted by y_{it}^d, on her mother's outcome, y_{it}^m.[1] Our economic outcomes are: (1) poverty status defined as family income-to-needs ratio less than 1, where needs are defined by the U.S. Census Bureau official poverty threshold that varies by family size; (2) poverty status defined as family income-to-needs ratio less than 2; and, (3) the logarithm of family income. We select the two poverty measures because they represent the part of the income distribution that most welfare recipients are drawn from, while the log family income model will give us standard estimates of intergenerational elasticities. For the first two outcomes, we take the average of annual income-to-needs across daughter's years from the age of independence through age 27, while the mother's income-to-needs is averaged across all years before the daughter forms her own family unit. The dependent and independent variables are dichotomized to equal 1 if any of the average poverty conditions are met, and 0 otherwise. The third outcome measures income continuously as the log of average family income for the respective daughters' and mothers' observation windows.[2]

[Figure 2 here]

Figure 2 presents estimates of correlation coefficients starting in 1978, the first year with enough daughters to estimate the correlations. The figure shows that in the two decades from the late 1970s to 2000, the income mobility of daughters declined substantially across all three measures. For example, in the log-log model the elasticity of a daughter's income with respect to her mother's income more than doubled from 0.25 in 1980 to 0.55 in 2000. Over the same period, the odds of a daughter having income under twice the poverty line (income-to-needs < 2) if her mother also had similarly low income went from 0.15 to 0.40. Since 2000, the income correlations stabilized, suggesting no further deterioration in mobility. However, it is transparent that the economic fortunes of young-adult daughters are closely tied to those of their mothers, and did not improve in the period after welfare reform.

Given the strong intergenerational association in incomes between mothers and daughters, we next examine whether that coincides with a strong correlation in welfare participation. Figure 3 uses the same sample as in Figure 2, but now the variable of interest is whether the daughter or mother participated during their respective observation windows in AFDC/TANF. Figure 3 shows that the

[1] The correlations are estimated by running the regression $y_{it}^d = \alpha_t + \rho_t y_{it}^m + \epsilon_{it}^d$ in each year.
[2] The daughters who formed their own family unit may be single or married, and thus income contains their own income and that of a spouse if present.

intergenerational correlation in AFDC 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 about 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 SNAP or SSI in addition to AFDC/TANF (mother's welfare remains defined by AFDC/TANF use), then we see a very different pattern. The intergenerational correlation for broader welfare participation is relatively constant after welfare reform much like we saw with incomes in Figure 2.

[Figure 3 here]

One possibility for the declining intergenerational correlation in AFDC/TANF is that it is simply a mechanical by-product of the declining baseline probability of participation as depicted in Figure 1. Note that the correlations in Figures 2 and 3 had mother-daughter pairs crossing the pre- and post-welfare reform regimes (with several mothers participating before reform and daughters participating after), thus potentially confounding a mechanical effect of changing program access and the behavioral effect of changing transmission after reform. To attempt to rule out the possibility that the mechanical effect of the reform is the primary driver of the declining AFDC/TANF correlation, we construct a sample of mother-daughter pairs who are observed solely in one welfare regime or the other. Presumably, if the within-regime mother-daughter correlation is the same before and after reform, then the decline in Figure 3 likely captures changes in the baseline probability of welfare participation. For the pre-reform regime, we use only daughter observations before she or her mother experienced welfare reform, and for the post-reform regime, we use daughters whose entire critical exposure period from age 12 onward (if not earlier) is after reform. We restrict the before- and after-reform samples to daughters at most 27 years old in order to make the two samples comparable in age distributions, and this age range is also consistent with estimates shown in Figures 2 and 3, as well as Page (2004). We measure the mother's participation when the daughter is between the ages of 12-18, the typical age for measuring exposure in the literature, and the daughter's participation as an adult is measured at ages 19-27. Participation of mothers is measured as an indicator for any participation when the daughter is aged 12-18, and for adult daughters, participation is measured as the pooled mean across observation years. This comparison provides a correlation within each welfare regime, before or after reform, based on fixed windows of each mother-daughter pair.

[Table 1 here]

Table 1 shows eight specifications, four where welfare participation is defined as AFDC/TANF and four where welfare participation is expanded to include SNAP and SSI. Within each grouping, we show estimates without and with family demographic variables and state-level economic controls that are standard in the welfare participation literature (Moffitt, 1992; Ziliak et al., 2000; Grogger, 2003; Bitler

and Hoynes, 2016a).[3] For the AFDC/TANF models, there is clear evidence of a decline in the intergenerational correlation in the post-welfare reform period, falling by two-thirds whether with or without controls. On the other hand, when we expand the definition of welfare to include food and disability assistance, the within-regime correlations actually increase slightly.[4] While these are descriptive correlations only, they do provide suggestive evidence that the decline in the intergenerational correlation in AFDC/TANF is not simply a mechanical artifact of falling cross-sectional participation.

III. Estimating Intergenerational Transmission Pre- and Post-Reform

The descriptive evidence points to the possibility that welfare reform succeeded in reducing the transmission of AFDC/TANF use across generations, but welfare use more broadly defined has not changed. To identify the intergenerational dependence parameter, one has to separate the poverty trap from the welfare trap. That is, the correlations presented in Figure 3 may simply reflect persistence in poverty status—the mother may have been endowed with a lack of human capital investment, which then translated into weak labor force attachment and social capital, and thus her poverty status led to a need for social assistance. In this model of welfare, the mother's poverty status limits the opportunities to invest in her own daughter's human capital, and thus perpetuates the cycle of poverty and need for assistance. Alternatively, welfare receipt may affect family values and attitudes, or social norms, and thus become a welfare dependence trap across generations (see, e.g., Murray, 1984; Duncan et al., 1988; Antel, 1992; Lindbeck et al., 1999; Durlauf and Shaorshadze, 2014). Moreover, a child on welfare can observe and learn from her mother how the program 'works'. This becomes a form of human capital investment in the daughter regarding how to navigate a complicated and often-times onerous application and recertification process for assistance. If the reform caused mothers to inform their adult daughters that welfare benefits are no longer worth the costs, as suggested in the careful qualitative studies of welfare by Edin and Lein (1997), DeParle (2004), and Halpern-Meekin et al. (2015), then reform can change the transmission of a potential culture of welfare dependency.

The framework we employ to causally separate the poverty trap story from the welfare trap story is the dynastic family decision-making model of Becker and Tomes (1979, 1986). In this model, the parent has weighted altruistic preferences over the income of their child along with their own consumption, and allocates lifetime resources toward own consumption and investment in the child. The child's human capital investment likely involves development of both cognitive and noncognitive skills,

[3] The control variables include mean values of quadratics in daughter's and mother's age, indicators for number of children (1, 2, 3, 4 or more), and state-level controls for the daughter's AFDC/TANF benefit standard, maximum federal/state EITC, poverty rate, unemployment rate, and AFDC/TANF recipiency rate.

[4] We note however that this increment is not significantly different from zero. Table 1 also shows that the decline in the intergenerational correlation of AFDC/TANF use in the post-welfare reform period is significant in all variants of the model.

each of which can influence poverty and welfare status (Heckman, Stixrud, and Urzua, 2006). With standard assumptions on the human capital technology, the corresponding canonical statistical model involves regressing the outcome of interest of the child on the corresponding outcome of the parent (Solon, 2014). This model has been used in scores of papers on intergenerational transmission of economic status, whether it is earnings, education, health, income, wealth, or in our case, welfare participation (see Black and Devereux, 2011).

The preliminary evidence in Figure 3 and Table 1 suggests a structural break in AFDC/TANF 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

$$W_{ist}^{d} = \alpha + \beta' \boldsymbol{x}_{ist}^{d} + \delta W_{is,\forall j<t}^{m} + \gamma R_{st}^{m} + \theta R_{st}^{m} W_{is,\forall j<t}^{m} + \mu_{s}^{d} + \kappa_{t}^{d} + v_{ist}^{d}, \quad (1)$$

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; $\boldsymbol{x}_{ist}^{d}$ is a vector of control variables that includes observed demographic characteristics of the daughter, as well as state-level policy and labor-market variables in the daughter's state of residence; 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 error term.[5] The state effects μ_{s}^{d} control for permanent differences in states such as natural endowments that affect economic opportunities, while the time effects κ_{t}^{d} control for macroeconomic and policy changes affecting all daughters the same in a given year.

In equation (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 in the sections below we 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

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

other adult economic outcomes of the daughter, we also estimate models where we replace the dependent variable with an indicator for broader safety net participation on AFDC/TANF, SNAP, or SSI, as well as indicators for low educational attainment, nonemployment, and poverty and near poverty status.[6]

In equation (1), δ is the intergenerational effect of welfare participation, and $\delta + \theta$ is the effect after welfare reform. This specification is akin to a difference-in-difference model whereby we exploit the quasi-experimental variation induced by the fact that different states adopted welfare reform at different times starting in the early 1990s.[7] That is, the indicator R_{st}^{m} turns on when state *s* implements a waiver and remains on thereafter. By adopting this functional form, we 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.[8] If welfare reform succeeded in reducing the transmission across generations, then we expect that $\theta < 0$. We next discuss two key threats to identification of the transmission parameters—selection bias and misclassification error—and in the results section we discuss additional challenges from life-cycle bias and geographic mobility.

III.A. Selection Bias

The conditional mean independence assumption for consistent causal estimates of the intergenerational parameters δ and θ, $E\left[v_{ist}^{d} \middle| W_{is,\forall j<t}^{m}, \bullet\right] = 0$, will break down if there are unobserved characteristics common to the mother and daughter that affect the decision to participate.[9] 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_{ist}^{d} can be correlated with mother's welfare use by endogenous selection. 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 and unobserved heterogeneity.

There have been several efforts over the years to control for endogenous selection in intergenerational welfare participation. Solon et al. (1988) used pairs of sisters in order to control for shared family background, Antel (1992) adopted a version of Heckman's (1978) selection model, and Levine and Zimmerman (1996) used state (e.g. welfare generosity) and local (e.g. county unemployment

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

[7] 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.

[8] 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). When we include additional controls for welfare reform characteristics in the baseline specification, our transmission estimates are approximately unaffected.

[9] If we backdate equation (1) by a generation, say $-t$, and write a model of the mother's participation as a function of her demographics, $\boldsymbol{x}_{is,-t}^{m}$, and the welfare choice of her mother (i.e. daughter's grandmother, $W_{is,\forall k<-t}^{g}$), then shared tastes for work and welfare within families would imply $E\left[v_{ist}^{d} v_{is,-t}^{m} \middle| \boldsymbol{x}_{ist}^{d}, W_{is,\forall j<t}^{m}, R_{st}^{m}, \boldsymbol{x}_{is,-t}^{m}, W_{is,\forall k<-t}^{g}\right] \neq 0$.

rate) variables as instruments for mother's welfare participation. Gottschalk (1996) addressed unobserved heterogeneity by modeling the event histories of daughter's and mother's welfare usage, and Dahl et al. (2014) used the random assignment of appellate-court judges as an instrumental variable to identify parent's disability participation on child's disability insurance claims.

Our approach to address possible endogenous selection within welfare regimes is to extend the prior point identification literature by exploiting the comparatively long histories now available in the PSID and estimate equation (1) via instrumental variables. Specifically, we instrument for mother's previous welfare participation using the policy parameter defined by the state AFDC/TANF benefit standard, or maximum benefit guarantee. The AFDC/TANF benefit standard is set by state legislatures and varies across states, time, and family size. The variable speaks to the prospect of the welfare trap, since a higher AFDC/TANF benefit standard means that all else equal welfare is more attractive to the mother. To ensure that the policy instruments are most salient to the mother's welfare choice, we 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, as well as controls for the daughter's contemporaneous benefit policies, 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; Moffitt, 1992; Levine and Zimmerman, 1996). We use two measures of welfare generosity for our instruments: the average and maximum of the state-specific AFDC/TANF benefit standard for families of 2, 3, or 4 or more persons. In equation (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. We test both the first-stage strength and the validity of overidentifying restrictions in the results section as well as further robustness tests in the online supplement.

III.B. Misclassification Bias in Models with and without Endogenous Variables

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; Meyer and Mittag, 2014, 2015). 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 assign a monetary value.

Remedies for classification bias are not straightforward in the context of dichotomous variables. A number of approaches have been proposed for cross-sectional data either for measurement error in the dichotomous dependent variable, or the independent variable, though we have potentially mismeasured dichotomous variables on both the left- and right-hand sides of the equation.[10] We consider several potential remedies.

First, evidence in Bollinger and David (2005) shows that false negative survey responses decrease with length of panel participation. Since in our sample (described in the next section) we follow mothers for at least 13 years on average and daughters for 24 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 we measure if the mother *ever* participates, which is likely to be less noisy than contemporaneous participation.[11] Moreover, the instrumental variables approach discussed in the prior section, which is not consistent if a binary independent variable is measured with error, can be interpreted as providing an upper bound of the effect of mother's participation, following Frazis and Loewenstein (2003). It should be noted however that they derive bounds in a cross-sectional model where the dependent variable is measured without error, which may not apply to our paper. Third, for left-hand-side classification error, we consider parametric bias-corrections along the lines proposed in Bollinger and David (1997) and Hausman, Abrevaya, and Scott-Morton (1998). Specifically, we follow Hausman et al. (1998) and note that the partial effect of mother's participation on daughter's participation from observed data is equal to

$$(2) \qquad \mathrm{P}\left(W_{ist}^{d}=1\middle|W_{is,\forall j<t}^{m}=1,\ \bullet\right)-\mathrm{P}\left(W_{ist}^{d}=1\middle|W_{is,\forall j<t}^{m}=0,\ \bullet\right)=(1-\tau_{0t}-\tau_{1t})(\delta+\theta R_{st}^{m}),$$

where • 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, we set the false positive rate to 0, and for the linear probability models rescale all the right-hand-side variables in equation (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 the online supplement. A convenient aspect of the proposed methodology is that it allows us to estimate

[10] A standard approach for continuous variables with classical measurement error is to take 3- or 5-year averages of parent's (and possibly child's) income (Solon, 1992; Mazumder, 2005). While such averages could reduce biases in participation models, our problem is nonclassical. Some have proposed a likelihood function approach to incorporate misclassification (Hausman et al., 1998; Meyer and Mittag, 2014), while others have proposed partial-identification techniques (see, e.g., Kreider et al., 2012).

[11] For further support that the mother's prior welfare participation is measured accurately, the online supplement shows that the probability of ever misreporting tends to zero as the number of mother observations increases.

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. The online supplement includes further development of the misclassification model.

IV. 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, Gottschalk, and Moffitt, 1998a,b; Fitzgerald, 2011). 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. We focus on linked mother-daughter pairs over the entire life of the PSID survey years from 1968 to 2013, and in order to ensure adequate sample sizes we 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).[12] 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 (Moffitt and Gottschalk, 2002; Lee and Solon, 2009). Other examples have used weights for estimators that are based on frequency counts (Solon et al., 1988; Page, 2004), as a sensitivity check (Solon, 1992), or in the main estimation (Hoynes and Schanzenbach, 2012). A primary concern for our estimates is the potential heterogeneity of welfare participation transmission by race coupled with overrepresented low-income, minority families, and our model maintains a fairly parsimonious structure that may not adequately account for this source of bias. Therefore, we provide weighted estimates in all of our estimation results.[13]

Our 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

[12] See PSID documentation for background on survey selection procedures and sample weight construction.
[13] In the online supplement, we also demonstrate that the results are robust to unweighted regression, or restriction to the SRC subsample.

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 (Solon et al., 1988; Gottschalk, 1996; Pepper, 2000; Page, 2004). Part of this stems from data needs; that is, if we require observing early childhood as well as enough years in adulthood, then we will 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 et al., 1998; Duncan and Brooks-Gunn, 2000; Ziol-Guest et al., 2012; Elango et al., 2016). But this research has not separated out the independent role of welfare in this process. As such, we follow convention and focus on the ages 12-18 as a key period of welfare exposure for our baseline models, and then we explore how the estimates change as the length of exposure changes. 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.[14] This yields a baseline sample of 2,961 mother-daughter pairs spanning 56,068 observation years of the daughter as an adult. On average we observe mothers and daughters co-residing for 13 years during childhood, and the daughter for 24 years during her adulthood. These long observation windows should help mitigate both measurement error in program participation as well as life-cycle bias in age of participation. Appendix Table S.1-1 contains the key variables from the baseline sample used in estimation, both pooled and separated into the pre- and post-welfare reform eras, and weighted by the daughter's core longitudinal weight. Appendix Table S.1-2 shows that the average daughter on SNAP or SSI is more disadvantaged after reform, underscoring the importance of our use of IVs to separate out the welfare trap from poverty trap. We supplement the PSID data with program data from official reports by the U.S. Department of Health and Human Services (USDHHS), and our dates for the implementation of welfare reform are based on Crouse (1999).

V. Estimates of Welfare Reform on Intergenerational Transmission

In presenting the empirical results, we first advance the descriptive analysis presented in Figures 2-3 and Table 1 by correcting for the influence of nonrandom selection and misclassification error on AFDC/TANF participation, and then expand the outcomes to include participation in additional transfer programs as well as human capital and employment. We then assess the robustness of the findings to life-

[14] Our estimates are robust to only using daughters at least 18 years old.

cycle bias and cross-state mobility, among other threats to identification. All models control for daughter's age, age squared, mother's average age during her potential welfare observation years, mother's average age squared, and indicators for number of children equal to 1, 2, 3, or at least 4. In addition, we include contemporaneous time-varying policy and economic controls for the daughter's state of residence, including the AFDC/TANF benefit standard, Earned Income Tax Credit (EITC) federal/state maximum credit, Supplemental Poverty Measure (SPM) poverty rate, AFDC/TANF recipiency rate, and unemployment rate. We also include state and year effects. The standard errors are robust to heteroscedasticity and clustered at the state level given the focus on state welfare reforms.

V.A. Baseline Estimates

The first four columns of Table 2 contain the baseline estimates of the parameters of interest in equation (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 0.253 (s.e. = 0.050), meaning that if the daughter's mother ever participated in AFDC then the daughter is 25 percentage points more likely to participate as an adult.[15] This estimate, which corrects for correlated unobservables between mother and daughter, 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). That correlation falls 67 percent after welfare reform to 0.082 (=0.253-0.171). This suggests changes in the probability of AFDC/TANF participation that are similar to the results obtained in Table 1, which are robust to the mechanical effect of welfare reform participation. The after-welfare reform variable has a positive effect on daughter's participation, suggesting that, conditional on year effects, in the absence of welfare reform the trend increase in intergenerational transmission would have continued.[16] We note, however, that dropping year effects results in a negative coefficient, which aligns with priors from Figure 1, indicating that there were other macroeconomic forces affecting participation decisions of daughters.

[Table 2 here]

While our baseline estimates address misclassification of the mother's welfare participation by design (longer panels of nonattriters, 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

[15] Since the PSID survey years switch to biennial interviewing after 1997, our data on welfare participation includes both responses for the prior observation year (T-1) and, after 1997, for the two-year retrospective (T-2). We found that the T-2 estimates are slightly attenuated toward zero yet qualitatively equivalent to our baseline estimates. See the online supplement for results on the sensitivity to T-2 retrospective sample.

[16] There is the possibility that the positive coefficient reflects positive entry effects onto welfare among previous nonparticipants as they seek out employment and training (Moffitt, 1996). While some states did offer employment and training as part of their welfare reforms, most adopted a "work first" strategy, and Grogger et al. (2003) show caseload entry fell after welfare reform.

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 50 percent, or 31 percent over the baseline odds of participation.[17]

A standard concern with IV estimates is the quality and exogeneity of the instruments. In the middle of Table 2, we present standard tests of instrument strength and exogeneity. The null hypothesis of weak instruments is strongly rejected using the Kleibergen-Paap (2006) rank test, while the null of valid overidentifying restrictions from the Hansen J-test is not rejected, suggesting our IV estimates are consistent. In the online supplement, we subject the baseline IV estimates to a number of specification checks, and we present the first-stage estimates of the effect of the instruments on the mother's participation decision in the pre-reform period.

In all variants of equation (1) estimated in the first four columns of Table 2, we 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, e.g., Dahl et al., 2014). Generally, the OLS estimate can be different from the IV estimate for, at least, three reasons: selection bias, heterogeneous effects, and measurement error. In our setting, it is difficult a priori to predict the sign of the bias of OLS. For instance, we may expect upward-biased OLS estimates under the assumption that unobservables are positively correlated over generations. However, the effects could be heterogeneous, too. Our sample includes a subpopulation of mothers who are not likely to be affected by the instruments because their family income is above the federal poverty line over the entire period of analysis. As our models control for both selection bias and misclassification error, based on the results shown in the online supplement to this paper, we conclude that the difference between IV and OLS estimates are likely attributed to heterogeneous effects.[18]

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[17] The change over the baseline is defined as $(a/b) - 1$, where a is the post-reform effect $(\hat{\delta} + \hat{\theta})$ rescaled by the average post-reform participation rate, and b is the pre-reform effect $(\hat{\delta})$ rescaled by the average participation over the whole period. We 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 our sample consists of a long panel of non-attriters who tend to be more accurate in reporting (Bollinger and David, 2005).

[18] The IV estimate identifies the mother's participation effect by instrumenting for her policy-induced participation, which means that our IV estimates can be interpreted as a local average treatment effect (Imbens and Angrist, 1994). Figure S.3-1 in the online supplement shows, as expected, that mothers exposed to higher ADFC/TANF benefits were more likely to participate in welfare, with the exception of mothers whose average family income is more than twice the poverty line. Further, Figure S.3-2 shows that the IV estimates are increasing for subsamples in which the mother is ever below low percentages of the federal poverty line, yet the OLS estimates are relatively flat across these groups of mothers who are marginally more likely to receive welfare.

V.B. 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 reform affects the intergenerational transmission of welfare participation defined more generally. In columns (5)-(8) of Table 2, we 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 include the same regressors 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 22-35 percentage points. But this is where the similarity with columns (1)-(4) end as we find no evidence that this transmission channel was changed after welfare reform. In results not tabulated we obtain a similar result if we also define mother's participation as any cash, food, or disability assistance.

[Table 3 here]

In addition to reducing welfare participation, the architects of welfare reform aimed to improve the long-term economic outcomes of children. In Table 3, we present least squares and instrumental variables estimates of equation (1) where we alternately replace the dependent variable of daughter's welfare participation with indicators equal to 1 for (a) whether her family currently has zero earnings, (b) whether her current family earnings are less than the poverty line, (c) whether her family earnings are less than twice the poverty line, and (d) whether her educational attainment is less than or equal to high school (12 years). For these adult outcome estimates, we restrict the sample to only daughters at least 19 years old with nonmissing earnings data, and thus a slightly smaller sample than in Table 2. We present IV estimates because of possible shared unobservables that spill over from mother to daughter in these wider economic domains. Here we find a consistent pattern that daughters exposed to welfare are at risk of worse economic outcomes in adulthood. The IV estimates suggest they are 15 percentage points more likely to have episodes of nonemployment compared to daughters not exposed, 33 percentage points more likely to have earnings below poverty in a given year, 41 points more likely to have episodes of near poverty, and 51 percentage points more likely to have lower human capital attainment. The findings in Table 3 indicate that the 1996 reform to welfare did not substantively alter these risks for daughters.

The analysis presented in Table 2 does not directly identify the mechanisms of transmission that simultaneously explain the attenuation of AFDC/TANF transmission and the stability of transmission patterns in the use of the broader safety net. While it is possible that daughters participated less in TANF after the reform because the program became more restrictive, the evidence in Table 1 that separated mother-daughter pairs within welfare regimes suggests it was not a mechanical effect of access. The high

rates of participation in the broader safety net could be associated with (i) welfare reform not improving the prospects of employment and self-sufficiency, as we demonstrate in Table 3 (and as suggested descriptively in Online Supplement Table S.1-2), and/or (ii) the disutility of participation not being significantly different across welfare programs. The latter interpretation fits the framework developed by Lindbeck et al. (1999) that focuses on the interplay between social norms and economic incentives in the modern welfare state. Although contemporary social norms discourage dependence on public transfers, the policy expansion of food stamps/SNAP and court-ordered SSI expansion could have the consequence of weakening this social norm, and thus creating incentives for daughters to continue participation in other programs.

V.C. Robustness of Baseline Estimates

In this subsection, we explore four possible sources of misspecification in equation (1). We first explore how the IV estimates vary once we introduce corrections for life-cycle bias. This is then followed by sensitivity tests of the results to daughters' geographic movements that may be an endogenous response to the welfare climate in the state. We then examine whether latent trends or other confounders drive identification of the parameter of interest, followed by an investigation of potential attrition bias.

V.C.1. Life-Cycle Windows

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 (Jenkins, 1987; Haider and Solon, 2006; 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; Wolfe et al., 1996; Page, 2004), 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 over-represented in the short window and long-term exposure matters more for transmitting dependency.

Our 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. Recall that for each mother-daughter pair, we observe the daughter as head/spouse of her own family unit for 24 years on average and for as long as 38 years. In addition, we observe the mother and daughter co-residing for 13 years on average with at least 5 years during the daughter's ages 12-18 when the potential for welfare learning is heightened. Thus, we come much closer to covering the entire life cycle of welfare participation.[19]

We begin by showing the sensitivity of our previous results to restricting the window of observations by age to be the same for all mothers and daughters. By imposing this restriction, we ensure that within-generation differences in age do not drive the results. Table 4 shows estimates restricted to the observation window of the mother over ages 25 to 45, and of the daughter through age 27 (consistent with Table 1). The transmission effects are somewhat larger in magnitude compared to our baseline results, yet the percent reduction in levels transmission in column (2) is a comparable 51 percent.[20]

[Table 4 here]

We next present estimates that implement a life-cycle age adjustment proposed by Lee and Solon (2009) in the context of income mobility. Specifically, we 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 our setting, and therefore, in the IV model of column (4), we instrument the interaction variable using the detrended quartic in daughter's age times the average and maximum of mother's AFDC/TANF benefit standard when the daughter was living with the mother and she was between 12 and 18 years old, and we also use these instruments interacted with reform. Because fertility rates among low-income women peak in their mid-20s, we detrend around daughter's age of 25. Comparing the OLS and IV estimates in columns (1) and (2) of Table 2 to columns (3) and (4) in Table 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 absolute value) in the post-reform era.

[19] As a first check, we examine the window problem by extending the minimum requirement that the pairs be observed for at least 10 and 15 years, respectively. In the online supplement, we show that the reduction in the level of mother's transmission after reform is comparable to the estimates reported in Table 2.

[20] As shown in the online supplement, the results are robust to using other observation windows for the mother.

V.C.2. Migration

Our models to this point 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 (Levine and Zimmerman, 1999; Gelbach, 2004; McKinnish, 2007), yet when effects are found, they are very small in magnitude. Also, 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.

[Table 5 here]

As a test on our baseline sample, we consider three alternatives to our IV model in column (2) of Table 2 by restricting the sample of daughters to: those who reside in the same state as their birth state, those residing in the same state as their mothers, and those who never move during their observed lifetime. Table 5 shows that both the direct effect of mothers' participation and the interaction with welfare reform are larger in absolute value compared to estimates in Table 2, yet the changes are relatively proportional such that the percent reduction in levels of transmission after welfare reform is roughly the same. Notably, the magnitudes of estimates in Table 5 get successively larger in absolute value as we 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.

V.C.3. State-specific Trends

We also examine whether latent trends or other confounders drive identification of the parameter of interest. We begin by extending model (1) to explore intergenerational dynamics for long-run welfare participation. Using a generalized difference-in-difference-type model, we estimate transmission effects by years before and after reform. In Figure 4, we plot OLS and IV estimates of the mother's participation effect interacted with years relative to reform, and we also show results of our IV model first with state trends and then with both linear and quadratic state trends.[21] There are two main findings. First, the impact of welfare reform is clear given the absence of pre-trend effects followed by a distinct drop in

[21] The augmented models follows closely the discussion in Wolfers (2006), although an important difference is that we estimate the model using instrumental variables. We also investigated the sensitivity of the results shown in Table 2 to the inclusion of linear and quadratic state time trends (see Table S.4-1 in the online supplement). Controlling for state-specific trends does not significantly change the results.

welfare transmission after the implementation of reform. The OLS and IV point estimates for the pre-reform years are not significantly different from zero and seem to fluctuate around the flat dashed line. In contrast, we find a weakly significant shift in participation within the first years after reform, and the effect of reform does not diminish over time. Second, the figure also shows that the point estimates in the baseline specification are robust to the inclusion of state-specific trends.

[Figure 4 here]

V.C.4. Attrition

It has been extensively documented that survey weights effectively address non-random sampling, although they may not fully correct for attrition (Fitzgerald, 2011). Under selection on observables, Fitzgerald et al. (1998a) suggest weights based on the inverse probability of responding to the survey, and to then use those weights in a second stage where parameters of interest in the structural model are estimated (see also Wooldridge, 2007).

[Figure 5 here]

Figure 5 presents evidence on the sensitivity of our results to attrition, by showing IV estimates of mother's participation and reform interaction based on different samples of daughters. The figure shows point estimates and corresponding pointwise confidence intervals for samples that range from daughters who do not attrit (1,906 daughters) to the full sample of daughters with no condition on attrition behavior (2,961 daughters).[22] The figure also shows results for the full sample based on an estimator that uses inverse probability weighting in addition to survey weights as in the previous tables.[23] The figure demonstrates that the baseline estimates are not sensitive to the proportion of daughters who are attriters given the stable estimates of the mother's participation effect and the percent reduction in transmission after welfare reform. There are only small differences between our baseline results and the results obtained by rescaling survey weights with inverse probability weights.

VI. Heterogeneity of Policy Effects

The evidence thus far points to a reduced transmission in AFDC/TANF participation across generations after welfare reform but not on the wider use of the safety net or risk of worse economic outcomes in adulthood. In this section we focus on potentially heterogeneous mechanisms of the AFDC/TANF transmission pathway. Specifically, we focus on how the estimates vary based on the

[22] The attrition rate in our sample is 34.6 percent. The online supplement presents additional evidence on attrition, including regression results that are similar to the results presented in Figure 5.

[23] The following variables are used in the first-stage model of the daughter being a panel non-attriter: an indicator for whether the daughter belongs to the SEO subsample, the logarithm of daughter's family income (in 2012 dollars), mother's welfare participation, a linear and quadratic term in daughter's age and mother's age, indicators for number of children, and state-level policy and economic variables for the daughter introduced in Table 2. Consistent with the literature, we find that a daughter's income is an important determinant of answering the PSID survey, and that SEO daughters are more likely to attrit than SRC daughters.

timing of a daughter's exposure period as well as her length of welfare exposure. This is then followed by estimates that vary by race, and by the stringency of the welfare reform environment.

VI.A. Timing of Welfare Transmission Effects

We first investigate the timing of welfare transmission by age and duration of exposure by examining how the base-case IV estimates in Table 2 change if we restrict the daughter's potential welfare exposure to only periods of co-residence. Recall that in Table 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 column of Table 6, we see that the pre-reform transmission effect is little changed relative to the baseline in Table 2, and again, the post-reform interaction changes proportionally. This implies that welfare reform had a similar percent reduction of welfare transmission whether the daughters' exposure is measured only during co-residence or in any prior year.

[Table 6 here]

As a further exploration of exposure, column (2) in Table 6 presents panel-data fixed-effects estimates of the welfare transmission. Specifically, we 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. We 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 or 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 home to form her own family unit. Essentially, word-of-mouth learning can be influential for a daughter previously exposed to welfare yet continually interpreting her own potential benefits and costs of participation based on her ongoing relationship with her mother, including passed along knowledge from recent experience. This follows from our 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 we 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 (2) of Table 6 is about 30 percent of the size of the estimate from column (1) of Table 2, suggesting that word-of-mouth transmission can still be sizable even after the daughter leaves home. Notably, the total effect after welfare reform is negative

(0.076 – 0.120), indicating that welfare reform shut down this transmission channel.[24] That is, the mother's communicated welfare experience after the daughter left home seems to have changed from somewhat beneficial before reform to costs that completely outweigh the benefits after reform. The fact that reform eliminates word-of-mouth transmission and not co-resident exposure transmission suggests that reform may have affected incentives to participate more than it addressed participation take-up barriers that can be reduced by learning program rules through direct household exposure.

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 mothers' duration in years. In order to allow the mother's effect to vary by duration, we successively redefine a mother's welfare participation as greater than 1 year and greater than 5 years, and we re-estimate the model with each specification.

The last two columns of Table 6 show the effects of a 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. The IV estimates 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 reform effect on transmission is also getting larger in absolute value as the length of exposure increases, the percent reduction in transmission after welfare reform is fairly stable post-reform.

VI.B. Racial Differences

There is a vast literature on the socioeconomic differences between blacks and whites (see, for example, Smith and Welch, 1989; Donohue and Heckman, 1991), 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.

[Table 7 here]

Panel A of Table 7 presents OLS and IV results for the transmission of AFDC/TANF from mother to daughter estimated separately by race where the daughter identifies as either black or white. Given racial disparities in the propensity to be poor, we compare the transmission effects for a subsample

[24] It is known however that fixed-effects methods exacerbate attenuation bias relative to pooled OLS, so it is common to find estimates lower in absolute value. In results not shown here, once we make time-varying corrections for misclassification, the mother's direct effect slightly dropped relative to the estimate in column (3) of Table 2, and we found that the level and percentage of the word-of-mouth transmission channel declines significantly.

in which the mothers ever previously had income below 200 percent of the federal poverty line. The first two columns of Table 7 suggest that the pre-reform effect of welfare transmission was stronger among blacks than whites (in column (2), 0.372 compared to 0.206). However, while the transmission channel was substantively reduced among both blacks and whites after welfare reform, the percent change is larger among whites. While the IV estimates by race in column (2) are less precise than full-sample estimates, the magnitudes are reasonable compared to baseline estimates in Table 2.

VI.C. State Policy Stringency

States differed in the timing of implementation and in the degree of aggressiveness in implementation of welfare reform, both in the waiver era and after TANF. Since the Welfare Reform Act was signed into law in 1996, the majority of states implemented reform in 1996 or 1997. Therefore, we define the 19 states that had already implemented waiver reforms by 1995 as early reformers. While there is no agreed upon measure of strictness in the literature, we 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. We repeat our main estimation separately by indicators for these measures of welfare reform timing or stringency to test whether there were differences in intergenerational transmission in those states that adopted reforms earlier or adopted relatively stricter reforms.[25]

Panel B of Table 7 reports estimates corresponding to the effects of interest based on state reform timing and aggressiveness. The transmission mechanisms between mother and daughter before welfare reform were generally smaller in late reform states than in early reform states, and in less-strict-reform states than in strict-reform states, however, these differences are not statistically significant.[26] While there is weak evidence that states implementing stricter reforms have lower welfare transmission before and after reform, the timing of welfare reform does not appear to be related to reform strictness or differential effects on intergenerational welfare participation.

VII. Discussion and Conclusion

A focal aim of policymakers with the 1990s welfare reform was to end dependence on welfare. In addition to documenting the descriptive changes in welfare dependence across generations, we provide causal estimates of welfare transmission that imply daughters are about 25 to 35 percentage points more

[25] The sets of states whose reforms are defined as early (19 states) or strict (13 states) by these criteria have little overlap: only Mississippi, Nebraska, and Virginia are categorized as both early and strict.

[26] These results are qualitatively similar when excluding state-level controls or state fixed effects, however, the IV estimates in column (6) suggest that strict-reform states appear to have lower magnitudes for transmission and reform effects yet similar percent changes relative to states not classified as having strict reforms.

likely to participate if their mothers had participated in welfare. These estimates are larger than those found by Dahl et al. (2014), a prominent recent study of causal welfare transmission, yet our context is public assistance for mothers and daughters in the United States, whereas Dahl et al. examine disability insurance receipt in Norway. Viewed narrowly from the lens of participation in the AFDC/TANF program, we 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 we face 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 decline in daughters' TANF participation after welfare reform 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. Our 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 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. Our results 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, 2016b). 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.

We conclude by noting that implicit in most discussions surrounding welfare reform is that the transmission of welfare reliance from parent to child is inherently a bad outcome. However, the socially efficient intergenerational correlation of welfare outcomes is not obvious. 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 (Ziliak, 2015; Ganong and Liebman, 2018). 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.

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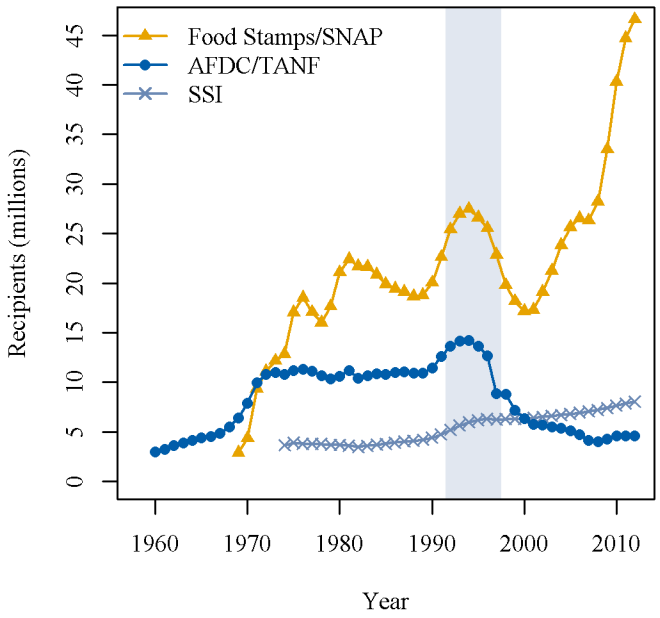
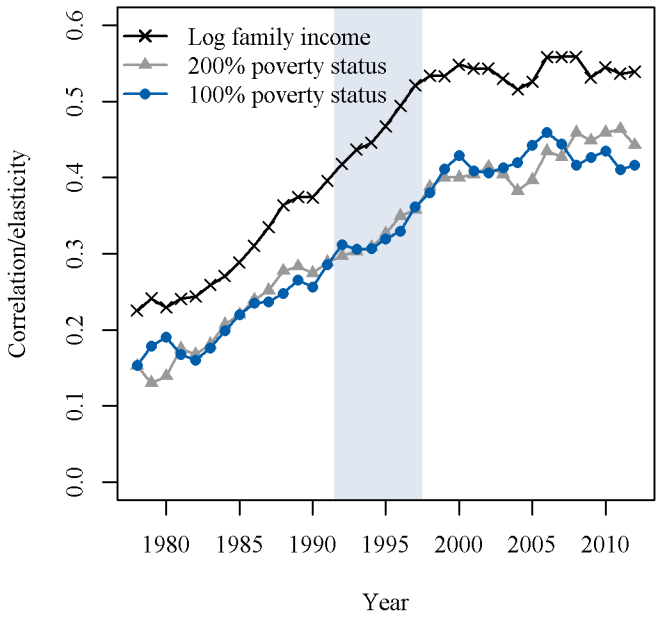
FIGURE 1. TRENDS IN AFDC/TANF, FOOD STAMP/SNAP, SSI RECIPIENTS



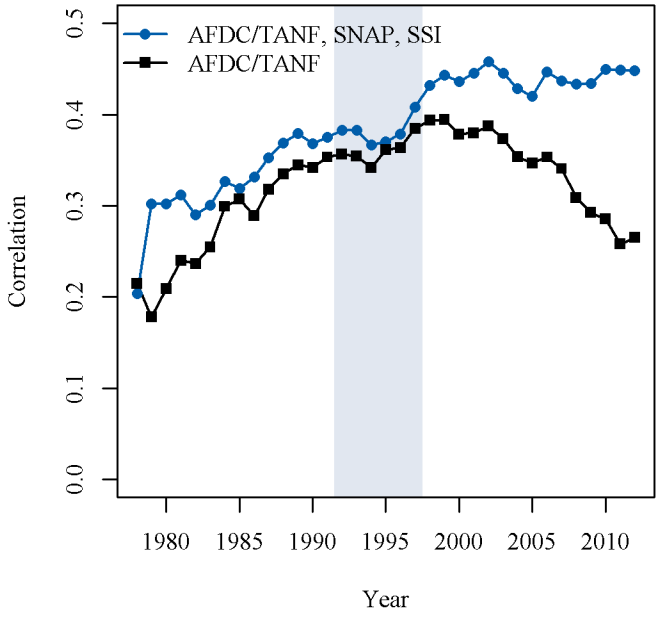
Notes: The major waiver period of welfare reform is indicated by the shaded region. AFDC/TANF denotes Aid to Families with Dependent Children/Temporary Assistance for Needy Families, SNAP denotes Supplemental Nutrition Assistance Program, and SSI denotes Supplemental Security Income. Authors' tabulations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

FIGURE 2. TRENDS IN INTERGENERATIONAL CORRELATION OF POVERTY STATUS AND FAMILY INCOME



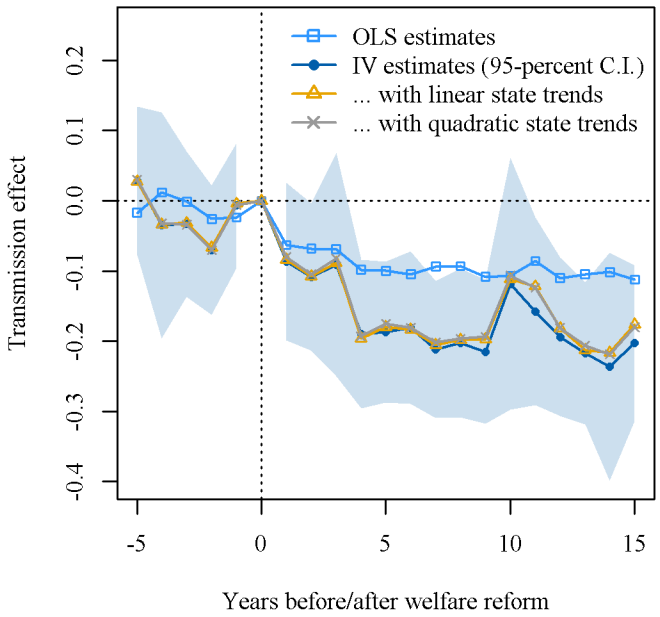
Notes: The correlation of poverty status is obtained from a model for whether an individual's mean family income is below 100 or 200 percent of the mean federal poverty threshold by age 27, and the correlation of family income is based on a log-log model of a daughter's average income through age 27 and the average of all of her mother's family income before the daughter begins her own family.

FIGURE 3. TRENDS IN THE INTERGENERATIONAL CORRELATION OF WELFARE PARTICIPATION



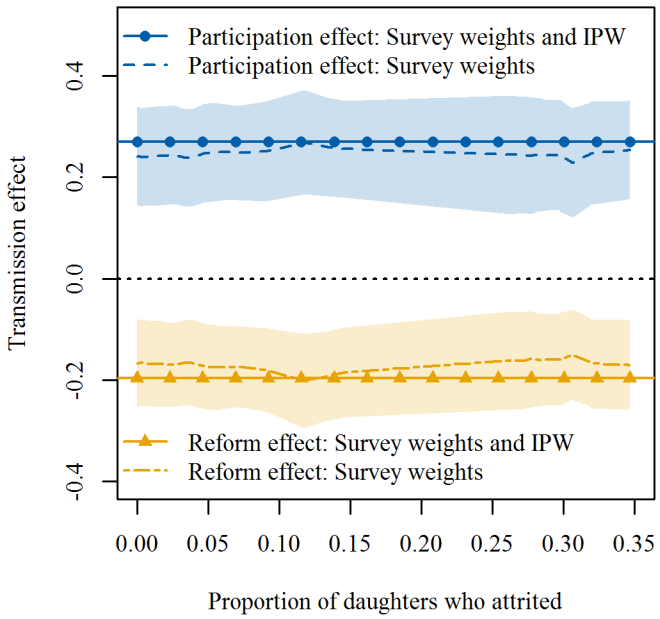
Notes: The major waiver period of welfare reform is indicated by the shaded region. SNAP denotes Supplemental Nutrition Assistance Program and SSI denotes Supplemental Security Income. The dependent variable indicates whether a daughter ever participated in AFDC/TANF (or, AFDC/TANF, SNAP, or SSI) in any year after forming her own family through age 27. The independent variable indicates whether the mother ever participated in AFDC/TANF when the child is observed living at home. The trends are obtained for daughters aged 27-42 in each year.

FIGURE 4. TIMING OF WELFARE REFORM



Notes: Estimates are shown with the baseline set of control variables reported in notes to Table 2 and mother's prior AFDC/TANF participation interacted with reform indicators by year before/after implementation. A 95-percent pointwise confidence interval is shown based on state-clustered estimates for the main effects without additional controls for state-specific trends.

FIGURE 5. SENSITIVITY OF RESULTS TO ATTRITION IN MODELS WITH INVERSE PROBABILITY WEIGHTS



Notes: Results above using survey weights only are estimated for samples restricted by the proportion of attrition sample allowed where the baseline results correspond to a 34.6 percent attrition rate. These results are compared to an estimate of transmission effects for the baseline sample when using survey weights and inverse probability weights for attrition. 95-percent pointwise confidence intervals are shown based on state-clustered estimates for the estimates with survey weights only.

TABLE 1. INTERGENERATIONAL WELFARE PARTICIPATION CORRELATIONS WITHIN WELFARE REGIMES PRE- OR POST-REFORM

Dependent variable:	Daughter's mean welfare participation							
	AFDC/TANF				AFDC/TANF, SNAP, SSI			
Welfare reform timing:	Before (1)	After (2)	Before (3)	After (4)	Before (5)	After (6)	Before (7)	After (8)
Mother's participation in any year while daughter is aged 12-18	0.229 (0.024)	0.075 (0.039)	0.146 (0.019)	0.036 (0.031)	0.303 (0.029)	0.336 (0.062)	0.189 (0.023)	0.203 (0.059)
Conditional on controls?	No	No	Yes	Yes	No	No	Yes	Yes
Effect of welfare reform		-0.153		-0.110		0.033		0.013
p-value		0.002		0.006		0.611		0.844
Percent change in levels		-67%		-75%		11%		7%
p-value		0.001		0.004		0.596		0.814
Number of daughters	1943	512	1943	512	1943	512	1943	512
Observations	1943	512	1943	512	1943	512	1943	512

Notes: Robust standard errors with state clustering are shown in parentheses. Estimation is restricted to daughters who can be observed at least 5 years during the critical exposure period, ages 12-18. Daughters observed before reform include only those mother-daughter pairs in which neither experiences welfare reform through the daughter's age 27. The after-reform sample is defined by daughters who are observed during the welfare reform era from age 12 onward. When estimates are conditional on control variables, these are averaged over the daughter's adult observation years. Daughter's welfare participation variable is the average participation during ages 19-27, and mother's welfare participation is 1 if she participates in any year when the daughter is aged 12-18 and 0 otherwise. P-values are obtained by a bootstrap procedure with 1000 replications.

TABLE 2. INTERGENERATIONAL TRANSMISSION OF MOTHER'S AFDC/TANF PARTICIPATION

Daughter's outcome variable:	AFDC/TANF				AFDC/TANF, SNAP, SSI			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mother's participation	0.139	0.253	0.228	0.394	0.218	0.284	0.285	0.350
	(0.013)	(0.050)	(0.020)	(0.084)	(0.018)	(0.071)	(0.024)	(0.096)
After welfare reform	0.035	0.064	0.046	0.076	0.000	-0.014	-0.015	-0.049
	(0.008)	(0.020)	(0.016)	(0.032)	(0.014)	(0.026)	(0.022)	(0.037)
Mother's participation ×	-0.095	-0.171	-0.126	-0.197	-0.037	0.037	-0.012	0.144
after welfare reform	(0.015)	(0.045)	(0.030)	(0.078)	(0.021)	(0.069)	(0.030)	(0.097)
Instrumental variables	No	Yes	No	Yes	No	Yes	No	Yes
Misclassification correction	No	No	Yes	Yes	No	No	Yes	Yes
Weak IV test statistic		22.436		21.070		22.436		21.417
p-value		0.000		0.000		0.000		0.000
Hansen J statistic		1.411		1.502		3.856		4.063
p-value		0.494		0.472		0.145		0.131
Percent change in levels	-68%	-67%	-55%	-50%	-17%	13%	-4%	41%
p-value	0.000	0.000	0.000	0.001	0.059	0.626	0.686	0.257
Percent change over baseline	-44%	-43%	-38%	-31%	-12%	20%	-3%	42%
p-value	0.002	0.040	0.014	0.142	0.218	0.478	0.745	0.248
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 models control for daughter's age, age squared, mother's average age during potential welfare observation years, mother's average age squared, indicators for number of children, controls for the daughter's state AFDC/TANF benefit standard, EITC federal/state maximum credit, state-level SPM poverty rate, AFDC/TANF recipiency rate, unemployment rate, and state and year effects. Instrumental variables include average and maximum measures of the mother's AFDC/TANF benefit standard 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 the online appendix for details). Daughters' PSID core longitudinal weights are used in estimation.

TABLE 3. MOTHER'S AFDC/TANF PARTICIPATION EFFECT ON DAUGHTER'S HUMAN CAPITAL AND LABOR MARKET OUTCOMES, AGES 19 AND OVER

Daughter's outcome variable:	No earnings		Earnings below 100% poverty		Earnings below 200% poverty		High school educ. or less	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mother's participation	0.127	0.150	0.243	0.333	0.307	0.407	0.264	0.505
	(0.018)	(0.059)	(0.022)	(0.090)	(0.024)	(0.114)	(0.034)	(0.191)
After welfare reform	0.007	0.000	0.007	-0.009	0.015	-0.029	0.045	0.072
	(0.013)	(0.018)	(0.020)	(0.031)	(0.022)	(0.040)	(0.028)	(0.063)
Mother's participation ×	-0.014	0.023	-0.042	0.050	-0.038	0.164	-0.010	-0.040
after welfare reform	(0.017)	(0.051)	(0.022)	(0.087)	(0.032)	(0.110)	(0.032)	(0.156)
Instrumental variables	No	Yes	No	Yes	No	Yes	No	Yes
Weak IV test statistic		22.512		22.512		22.512		22.512
p-value		0.000		0.000		0.000		0.000
Hansen J statistic		3.305		2.705		1.896		0.608
p-value		0.192		0.259		0.388		0.738
Number of daughters	2960	2960	2960	2960	2960	2960	2960	2960
Observations	54636	54636	54636	54636	54636	54636	54636	54636

Notes: See Table 2 notes. For these adult outcome estimates, we restrict the sample to only daughters at least 19 years old with non-missing earnings data.

TABLE 4. INTERGENERATIONAL TRANSMISSION OF AFDC/TANF PARTICIPATION WITH ADJUSTMENTS FOR POTENTIAL LIFE-CYCLE BIAS

	Mothers ages 25 to 45, daughters through age 27		Lee-Solon-type (2009) age adjustment	
	(1)	(2)	(3)	(4)
Mother's participation	0.184	0.444	0.109	0.242
	(0.020)	(0.107)	(0.011)	(0.038)
After welfare reform	0.064	0.118	0.017	0.045
	(0.020)	(0.042)	(0.007)	(0.023)
Mother's participation × after welfare reform	-0.115	-0.226	-0.060	-0.133
	(0.033)	(0.105)	(0.015)	(0.051)
Instrumental variables	No	Yes	No	Yes
Weak IV test statistic		16.581		18.760
p-value		0.001		0.066
Hansen J statistic		1.649		13.583
p-value		0.438		0.193
Number of daughters	2086	2086	2961	2961
Observations	15718	15718	56068	56068

Notes: See Table 2 notes. 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 for the Lee-Solon-type estimates additionally include the baseline set of instrumental variables interacted with a quartic in daughter's detrended age.

TABLE 5. INTERGENERATIONAL TRANSMISSION OF AFDC/TANF PARTICIPATION BY DAUGHTER'S GEOGRAPHIC MOBILITY STATUS

	Same state as birth		Same state as mother		Never moves states	
	(1)	(2)	(3)	(4)	(5)	(6)
Mother's participation	0.159	0.288	0.196	0.334	0.176	0.391
	(0.017)	(0.072)	(0.017)	(0.079)	(0.021)	(0.102)
After welfare reform	0.042	0.077	0.044	0.067	0.051	0.108
	(0.011)	(0.028)	(0.013)	(0.023)	(0.014)	(0.047)
Mother's participation × after welfare reform	-0.112	-0.208	-0.138	-0.221	-0.128	-0.290
	(0.018)	(0.067)	(0.019)	(0.073)	(0.024)	(0.103)
Instrumental variables	No	Yes	No	Yes	No	Yes
Weak IV test statistic		16.959		18.148		12.310
p-value		0.001		0.000		0.006
Hansen J statistic		1.423		2.210		0.660
p-value		0.491		0.331		0.719
Number of daughters	2618	2618	2757	2757	1961	1961
Observations	44122	44122	36823	36823	36404	36404

Notes: See Table 2 notes.

TABLE 6. INTERGENERATIONAL TRANSMISSION OF AFDC/TANF PARTICIPATION BY EXPOSURE MECHANISM TIMING AND DURATION

	Exposure only during co-residence	"Word-of-mouth" learning	Mother's longest spell > 1 year	Mother's longest spell > 5 years
	(1)	(2)	(3)	(4)
Mother's participation	0.278	0.076	0.234	0.376
	(0.084)	(0.021)	(0.050)	(0.115)
After welfare reform	0.063	0.037	0.042	0.049
	(0.037)	(0.010)	(0.013)	(0.015)
Mother's participation × after welfare reform	-0.207	-0.120	-0.147	-0.274
	(0.113)	(0.018)	(0.042)	(0.087)
Instrumental variables	Yes	No	Yes	Yes
Daughter fixed effects	No	Yes	No	No
Weak IV test statistic	11.259		19.667	15.198
p-value	0.010		0.000	0.002
Hansen J statistic	2.750		2.855	4.223
p-value	0.253		0.240	0.121
Number of daughters	2961	2961	2961	2961
Observations	56068	56068	56068	56068

Notes: See Table 2 notes.

TABLE 7. HETEROGENEOUS INTERGENERATIONAL TRANSMISSION OF AFDC/TANF PARTICIPATION

Transmission effects by:	A. Race		B. Timing or aggressiveness of state reforms			
	(1)	(2)	(3)	(4)	(5)	(6)
	Black		Early (1992-1995)		Strict reform	
Mother's participation	0.114	0.372	0.148	0.289	0.136	0.256
	(0.020)	(0.188)	(0.026)	(0.065)	(0.014)	(0.058)
Mother's participation × after welfare reform	-0.073	-0.151	-0.093	-0.218	-0.095	-0.176
	(0.039)	(0.216)	(0.028)	(0.075)	(0.024)	(0.057)
Weak IV test statistic		7.461		9.239		6.205
p-value		0.059		0.026		0.102
Hansen J statistic		1.942		0.290		0.197
p-value		0.379		0.865		0.906
Number of daughters	1317	1317	1566	1566	945	945
Observations	25446	25446	25870	25870	16933	16933
	White		Late (1996-1997)		Less strict	
Mother's participation	0.065	0.206	0.128	0.228	0.138	0.249
	(0.013)	(0.112)	(0.011)	(0.076)	(0.017)	(0.065)
Mother's participation × after welfare reform	-0.055	-0.143	-0.100	-0.134	-0.095	-0.164
	(0.015)	(0.106)	(0.019)	(0.058)	(0.018)	(0.051)
Weak IV test statistic		10.606		8.560		18.159
p-value		0.014		0.036		0.000
Hansen J statistic		0.405		3.770		1.749
p-value		0.817		0.152		0.417
Number of daughters	1151	1151	1843	1843	2143	2143
Observations	21924	21924	30198	30198	39135	39135
Instrumental variables	No	Yes	No	Yes	No	Yes

Notes: Panel A: The sample is restricted to daughters whose race is either indicated as black or white, and whose mothers ever had family income below 200 percent of the federal poverty line. Panel B: "Early" means implementation occurred in years 1992-1995 (19 states) and "Late" in years 1996-1997 (32 states) according to Crouse (1999). The aggressiveness of state reforms is defined by whether states reforms were considered strict (13 states) according to criteria defined in Grogger and Karoly (2005).

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Online Supplement to:

Welfare Reform and the Intergenerational Transmission of Dependence*

Robert Paul Hartley
Center on Poverty and Social Policy
Columbia University

Carlos Lamarche
Department of Economics
University of Kentucky

James P. Ziliak
Center for Poverty Research and
Department of Economics
University of Kentucky

May 2019

*Address correspondence to James P. Ziliak, University of Kentucky, Center for Poverty Research, Gatton Building Suite 234, 550 South Limestone St., Lexington, KY 40506-0034. Email: jziliak@uky.edu.

This supplementary appendix provides additional information on the data and results reported in our paper "Welfare Reform and the Intergenerational Transmission of Dependence". We begin with data description in Section S.1, and then, in Section S.2, we present our approach to misclassification. As referenced throughout the paper, this supplement also introduces additional results that explore the sensitivity of our main findings on the effects of a mother's prior welfare participation on the daughter's current welfare status. Section S.3 presents first-stage results and detailed sensitivity analysis on the instrumental variables results presented in the manuscript, including a falsification exercise for addressing heterogeneity in correlations with a mother's future welfare participation. Section S.4 offers additional empirical evidence obtained by estimating variants of the difference-in-difference-type specification presented in equation (1) as well as a placebo-type test of the randomness of trend effects with respect to the timing of welfare reform implementation. Also, we show further evidence that our difference-in-difference design implies no effects of reform on other welfare program transmission patterns. Section S.5 examines the impact of longitudinal survey weights and biennial interviewing on the baseline estimates presented in Table 2. Section S.6 investigates the relevance of attrition for estimates of intergenerational transmission of welfare participation. Lastly, Section S.7 includes extensions of models related to exposure timing and potential life-cycle bias.

To summarize the empirical evidence presented in this supplementary appendix, we find that our results are robust to variations of the baseline model of intergenerational transmission of welfare presented in the manuscript. In particular, the qualitative results of welfare reform are consistent: there is a causal influence from mother's welfare participation to daughter's participation, and reform attenuates this intergenerational transmission.

S.1. Data Description

We use data from the Panel Study of Income Dynamics (PSID), with a specific focus on linked mother-daughter pairs obtained over the survey years 1968-2013. The baseline sample includes 2,961 daughters spanning 56,068 observation years of the daughter as an adult. Table S.1-1 contains the key variables from the baseline sample used in estimation of equation (1), separated into the pre- and post-welfare reform eras, and weighted by the daughter's core longitudinal weight, which is appropriate when combining PSID subsamples from the Survey Research Center (SRC) and Survey of Economic Opportunity (SEO).

TABLE S.1-1. DESCRIPTIVE STATISTICS

A. Daughter's characteristics as an adult	Before	After	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)
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)
Policy/economy measures when daughter observed as an adult			
AFDC/TANF benefit standard	0.372	0.277	0.310
(in thousands of 2012 dollars)	(0.333)	(0.268)	(0.296)
EITC maximum credit	1.280	2.812	2.279
(in thousands of 2012 dollars)	(1.144)	(2.100)	(1.966)
Poverty rate	0.154	0.139	0.144
	(0.041)	(0.033)	(0.037)
Unemployment rate	0.066	0.061	0.062
	(0.019)	(0.022)	(0.021)
AFDC/TANF recipiency rate	0.046	0.019	0.029
	(0.015)	(0.015)	(0.020)
B. Mother's characteristics	Before	After	Pooled
Any prior welfare by period?			
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)
Age (average for prior observation years)	42.472	59.357	45.103
	(8.841)	(10.512)	(8.626)
Policy/economy measures when daughter observed during ages 12-18			
AFDC/TANF benefit standard, average	0.736	0.393	0.724
(in thousands of 2012 dollars)	(0.334)	(0.213)	(0.336)
AFDC/TANF benefit standard, maximum	0.913	0.476	0.904
(in thousands of 2012 dollars)	(0.363)	(0.226)	(0.365)
Mean daughter-as-child observations			13.164
Mean daughter-as-adult observations			23.828
Number of daughters	2212	2372	2961
Total observations	25331	30737	56068

Notes: Sample averages are weighted by the daughter's PSID core longitudinal weights for both daughters' and mothers' statistics. 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: Aid to Families with Dependent Children/Temporary Assistance for Needy Families (AFDC/TANF), Food Stamps/Supplemental Nutrition Assistance Program (SNAP), Supplemental Security Income (SSI), and Earned Income Tax Credit (EITC).

While 4.4 percent of daughters receive AFDC/TANF (Aid to Families with Dependent Children, or Temporary Assistance for Needy Families) 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. On the other hand,

there is much more stability over time in participation in any of the three means-tested programs, with 13.2 percent receiving AFDC/TANF, food stamps/SNAP (Supplement Nutrition Assistance Program), or SSI (Supplemental Security Income) before reform and 11.2 percent afterwards. Almost all of the additional uptake in welfare use is from SNAP. The bottom panel of Table S.1-1 shows that about 27 percent of mothers received any prior AFDC/TANF transfers before welfare reform, and 6.6 percent during the period after reform, while those figures jump to 43 and 19 percent, respectively, if the mother received any prior AFDC/TANF, SNAP, or SSI. Note that it is possible for the mother to first participate in 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 in the household with her mother, or from indirect "word of mouth" once the daughter forms her own family unit as discussed in the manuscript.

The other focal regressor in equation (1) of the manuscript is the indicator for welfare reform. As discussed in Section II of the paper, states began reforming AFDC in earnest starting in 1992, four years prior to passage of the Personal Responsibility and Work Opportunity Reconciliation Act (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). We 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 we 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 our data, the implementation of welfare reform is encoded as the earliest year in which at least 3 quarters of the year are observed after *state-wide* reform, implying that the reform spans 1993-1998.

Table S.1-1 also contains demographic characteristics of the daughter and mother, as well as our main instrumental variables. Daughters are 28 years old on average before reform and 39 after reform, while mothers (when welfare choices are observed) are 42 and 59 years old, respectively, highlighting the long observation windows we observe families compared to prior research. The nominal values of the maximum guarantees and credit are converted to real 2012 dollars using the personal consumption expenditure deflator. The average real maximum AFDC/TANF benefit standard facing mothers was $736 before welfare reform 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).

TABLE S.1-2. DAUGHTER'S AVERAGE PRE- AND POST-REFORM SOCIOECONOMIC CHARACTERISTICS

	Pre-reform			Post-reform		
	No welfare (1)	Some AFDC (2)	SNAP/SSI only (3)	No welfare (4)	Some TANF (5)	SNAP/SSI only (6)
	A. Unconditional on mother's welfare participation					
Years on welfare	0.000	4.591	1.867	0.000	5.080	2.715
Number of children	0.539	1.707	1.147	0.438	2.037	1.314
Married	0.657	0.407	0.649	0.591	0.209	0.296
Family income (median)	51.073	23.591	30.457	61.320	26.557	28.005
No family earnings	0.008	0.267	0.043	0.042	0.280	0.180
Earnings < 100% poverty	0.062	0.595	0.259	0.131	0.656	0.547
Earnings < 200% poverty	0.215	0.804	0.627	0.308	0.869	0.747
Same state as birth	0.778	0.837	0.819	0.734	0.888	0.789
High school or less	0.475	0.855	0.696	0.312	0.708	0.647
	B. Conditional on mother participating in AFDC/TANF					
Years on welfare	0.000	5.657	2.273	0.000	5.607	3.172
Number of children	0.733	2.006	1.353	0.660	2.034	1.045
Married	0.631	0.303	0.699	0.443	0.117	0.224
Family income (median)	43.344	19.355	25.820	46.765	24.738	21.613
No family earnings	0.023	0.393	0.056	0.012	0.301	0.291
Earnings < 100% poverty	0.116	0.717	0.328	0.143	0.755	0.760
Earnings < 200% poverty	0.340	0.885	0.716	0.435	0.928	0.850
Same state as birth	0.825	0.905	0.812	0.671	0.858	0.757
High school or less	0.674	0.869	0.840	0.260	0.761	0.754

Notes: These statistics correspond to the samples used in Table 1 of the manuscript in which daughters are observed during adulthood through age 27. The before reform period indicates before either the mother's or daughter's state ever implements reform, and the after-reform period indicates that the daughter has experienced welfare reform since age 12 at least. Mother's AFDC/TANF participation in panel B corresponds to the daughter's critical exposure period similarly to Table 1 in the manuscript.

Table S.1-2 investigates whether the observed characteristics of daughters considered in the empirical analysis changed after the reform. We present descriptive statistics that focus on the sample of daughters observed within welfare reform eras, before and after, according to the estimation samples used in Table 1 of the manuscript. The top panel contains sample means of daughters regardless of participation status of mother in AFDC/TANF, while the bottom panel is conditional on mother participating in AFDC/TANF. The first three columns refer to the pre-reform period for daughters who (i) did not receive any welfare in adulthood through age 27, (ii) received some AFDC/TANF and possibly SNAF or SSI, and (iii) received SNAP and/or SSI but not AFDC/TANF. The second three columns are for the same set of groups, but refer to sample averages in the post-reform era. The patterns pre/post reform are the same in that daughters not on any welfare are most advantaged, followed by those of SNAP or SSI alone, and the most disadvantaged are those with some AFDC/TANF. There are some notable differences in that post-reform the fraction married is much lower, though this is true for those never on welfare as well as those on welfare. Daughters who select into welfare participation appear to have more children after reform relative to the pre-/post-reform differences between daughters who did

not participate in welfare through age 27, though we control for number of children in our specifications. There is a secular rise in education attainment, but also nonemployment and earnings below poverty. This seems especially true among the sample of daughters on SNAP and/or SSI alone, which could help account for the transmission mechanism persisting after reform. It also underscores the importance in our causal model in equation (1) of the manuscript to separate out the poverty trap from welfare trap and motivates the inclusion of control variables and the use of IVs. We present additional evidence in the manuscript and throughout this supplement to test sensitivity of our main estimates to a daughter's educational attainment (Table 3), geographic mobility (Table 5), and controls for her mother's income and education (Table S.3-4).

S.2. Misclassification Bias Corrections

In this section, we demonstrate that potential misclassification of mother's prior participation does not lead to inconsistent estimates of the intergenerational transmission of welfare participation if (i) the probability that a mother reports accurately is greater than zero, and (ii) the mother is observed over a relatively long period. We also present the reporting rates used for estimation of models with misclassification.

Estimates based on equation (1) rely on self-reported data for a daughter's welfare participation at time t and her mother's self-reported participation at any time prior to t. Consider the main estimation equation

$$W_{ist}^{d} = \alpha + \beta' \boldsymbol{x}_{ist}^{d} + \delta W_{is,\forall j<t}^{m} + \gamma R_{st}^{m} + \theta R_{st}^{m} W_{is,\forall j<t}^{m} + \mu_{s}^{d} + \kappa_{t}^{d} + \upsilon_{ist}^{d},$$

where $W_{is,\forall j<t}^{m} = \max\{W_{is,t-1}^{m}, W_{is,t-2}^{m}, W_{is,t-3}^{m}, \ldots\}$. Let the true participation status be denoted $\widetilde{W}_{ist}^{d}$ for daughter at time t, $\widetilde{W}_{ist}^{m}$ for mother at time t, and $\widetilde{W}_{is,\forall j<t}^{m}$ 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, misclassification in $W_{is,\forall j<t}^{m}$ does not lead to inconsistent results as long as individuals have some positive probability of accurately reporting welfare participation at time t. To fix ideas, consider for simplicity $t = 3$ with $j \in \{1,2\}$ and let the contemporaneous probability of accurately reporting participation be defined as

$$q = \mathrm{P}\left(W_{ist}^{m} = 1 \middle| \widetilde{W}_{ist}^{m} = 1\right) > 0,$$

for all t. In this case, the mother's measure of any prior participation at $t = 3$ will be accurately reported with probability

$$\mathrm{P}\left(W_{is,\forall j<3}^{m} = 1 \middle| \widetilde{W}_{is,\forall j<3}^{m} = 1\right) = \mathrm{P}\left(W_{is1}^{m} = 1 \middle| \widetilde{W}_{is1}^{m} = 1\right) + \mathrm{P}\left(W_{is2}^{m} = 1 \middle| \widetilde{W}_{is2}^{m} = 1\right) - \mathrm{P}\left(W_{is1}^{m} = 1 \middle| \widetilde{W}_{is1}^{m} = 1\right)\mathrm{P}\left(W_{is2}^{m} = 1 \middle| \widetilde{W}_{is2}^{m} = 1, W_{is1}^{m} = 1, \widetilde{W}_{is1}^{m} = 1\right).$$

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

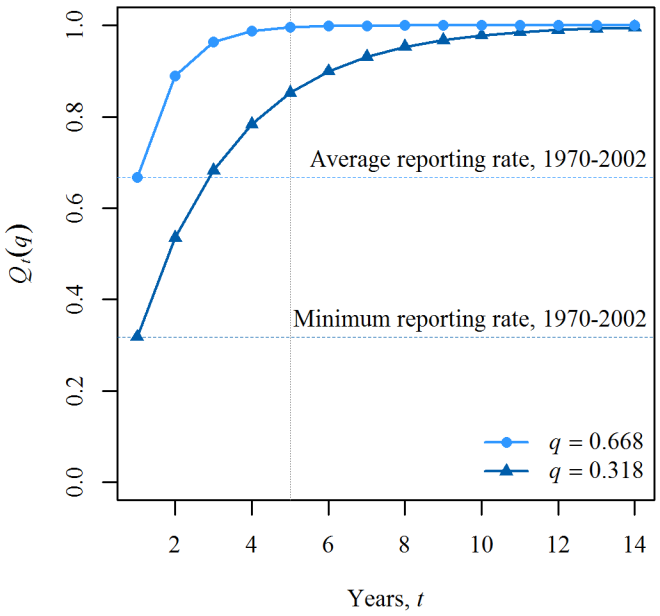
$$\mathrm{P}\left(W^m_{is,\forall j<3}=1\middle|\widetilde{W}^m_{is,\forall j<3}=1\right)=\ q\ (2-r)>q=\mathrm{P}\left(W^m_{is3}=1\middle|\widetilde{W}^m_{is3}=1\right).$$

We can now generalize the argument assuming, again for simplicity in exposition, that $q=r$. The probability of accurately reported welfare participation in any prior period under the above conditions can be expressed (based on the inclusion-exclusion principle for the union of finite events (Billingsley, 1995, p. 24)) as

$$Q_t(q)\equiv \mathrm{P}\left(W^m_{is,\forall j<t}=1\middle|\widetilde{W}^m_{is,\forall j<t}=1\right)=\sum_{j=1}^{t-1}(-1)^{j-1}\binom{t-1}{j}q^j\ ,\ \text{where}\ \binom{t-1}{j}=\frac{(t-1)!}{j!\,(t-1-j)!},$$

which is increasing in the number of time periods observed. For our analysis, the mother's minimum number of time periods is five years, and for the average reporting rate for 1970-2000 (see Table S.2-1 and 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 13 years on average prior to the daughter's participation decision, the probability that a mother accurately reports any prior participation tends to 1, as shown in Figure S.2-1.

FIGURE S.2-1. PROBABILITY OF ACCURATELY REPORTING WELFARE PARTICIPATION



Notes: Given a propensity, q, to report welfare participation accurately in a given year, the probability of reporting accurately when questioned repeatedly over t years is shown by $Q_t(q)$ where the values of q are taken as the average and minimum reporting rates for mothers over the years 1970 to 2002.

We focus instead on misclassification in the binary dependent variable for the daughter's current welfare status. The probability that a daughter reports participating in welfare can be written as

$$\mathrm{P}\left(W_{ist}^{d}=1\right)=\mathrm{P}\left(W_{ist}^{d}=1\middle|\widetilde{W}_{ist}^{d}=1\right)\mathrm{P}\left(\widetilde{W}_{ist}^{d}=1\right)+\mathrm{P}\left(W_{ist}^{d}=1\middle|\widetilde{W}_{ist}^{d}=0\right)\mathrm{P}\left(\widetilde{W}_{ist}^{d}=0\right),$$

where false negatives are defined as $\tau_{1,ist}\coloneqq\mathrm{P}\left(W_{ist}^{d}=0\middle|\widetilde{W}_{ist}^{d}=1\right)$ and false positives are defined as $\tau_{0,ist}\coloneqq\mathrm{P}\left(W_{ist}^{d}=1\middle|\widetilde{W}_{ist}^{d}=0\right)=0$ by assumption. 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. Note that whereas q is assumed fixed for the purposes of exposition above, false negatives here can be shown equivalently as $\tau_{1,ist}=1-q_{ist}$. Therefore, using equation (1) and $\tau_{1,ist}$, we can rewrite the daughter's probability of reported welfare participation as

$$\mathrm{P}\left(W_{ist}^{d}=1\right)=\left[1-\tau_{1,ist}\right]\left[\alpha+\beta'\boldsymbol{x}_{ist}^{d}+\delta W_{is,\forall j<t}^{m}+\gamma R_{st}^{m}+\theta R_{st}^{m}W_{is,\forall j<t}^{m}+\mu_{s}^{d}+\kappa_{t}^{d}\right].$$

We estimate the previous equation 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 $\mathrm{E}\left(\tau_{1,ist}\right)=\tau_{1t}$. In the second stage, we estimate the parameters of interest, (δ,γ,θ), by estimating the model of W_{ist}^{d} on weighted independent variables including a weighted intercept $[1-\hat{\tau}_{1t}]\alpha$, $[1-\hat{\tau}_{1t}]\mu_{s}^{d}$ and $[1-\hat{\tau}_{1t}]\kappa_{t}^{d}$.

Table S.2-1 shows PSID reporting rates for dollar amount in transfers and number of cases for AFDC/TANF and SNAP (obtained from Meyer et al., 2015). The estimation parameter used in misclassification bias correction estimates, $(1-\hat{\tau}_{1t})$, is the imputed reporting rate (or the greater of the two reporting rates for daughter's broader safety net estimates). This imputed rate is equal to the reporting rate for transfers in the first column inflated 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 SNAP. In years where we are missing both rates for amounts and cases, we linearly interpolate between observed years and use a two-year moving average for the last years.

TABLE S.2-1. PSID REPORTING RATES FOR MISCLASSIFICATION BIAS CORRECTION

	AFDC/TANF			Food stamps/SNAP		
	Meyer et al. (2015)		Estimation	Meyer et al. (2015)		Estimation
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).

S.3. Instrumental Variables: Additional Results and Sensitivity Analysis

This section presents a detailed analysis on the instrumental variables approach to equation (1). We begin the section presenting first-stage results associated with the estimates shown in Table 2. We then investigate the quality and exogeneity of the instruments, perform a falsification test, and investigate the interpretation of our findings. We end this section by reporting the sensitivity of our IV results to including additional mother's control variables and varying the minimum number of mother-daughter observations.

S.3.1. First-Stage Results

Table S.3-1 offers first-stage results for the IV estimates presented in Table 2 columns (2) and (4). These results correspond to a model for daughters' AFDC/TANF participation. The first stage corresponding to the daughter's broader welfare participation (Table 2 columns (6) and (8)) is no different except for small effects when using a different misclassification correction when accounting for SNAP reporting rates as well as AFDC/TANF. We report results on the variables used to instrument for the mother's AFDC/TANF participation decision. As expected, AFDC/TANF is a strong predictor of the

probability of mother's welfare participation, and the evidence is consistent with the commonly accepted premise that mother's welfare participation decision responds positively to greater average state-level AFDC/TANF benefit standards.

TABLE S.3-1. FIRST-STAGE INSTRUMENTAL VARIABLES ESTIMATES FOR MOTHER'S AFDC/TANF PARTICIPATION DECISION

	(1)	(2)
Average AFDC/TANF	0.523	0.526
	(0.087)	(0.093)
Reform × average AFDC/TANF	0.264	0.290
	(0.107)	(0.098)
Maximum AFDC/TANF	-0.364	-0.378
	(0.116)	(0.110)
Reform × maximum AFDC/TANF	-0.171	-0.193
	(0.090)	(0.083)
Misclassification correction	No	Yes
F test of excluded instruments	13.888	12.770
p-value	0.000	0.000
Weak IV test statistic	22.436	21.070
p-value	0.000	0.000
Number of daughters	2961	2961
Observations	56068	56068

Notes: Robust standard errors with state clustering are shown in parentheses. The weak IV test statistic is a Kleibergen-Paap (2006) rank statistic. Daughters' PSID core longitudinal weights are used in estimation.

The evidence presented in Table 2 and Table S.3-1 strongly suggests that state welfare benefit limits are valid instruments for the mother's participation decision. Nonetheless, using Table S.3-2 below, we explore the sensitivity of our estimates to other possible instruments including AFDC/TANF denial and hearings claims rates and macroeconomic variables used in other studies (see Levine and Zimmerman, 1996).

S.3.2. Policy Instruments and Macroeconomic Variables

Table S.3-2 compares estimates for the parameters of interest in equation (1) obtained from using different sets of instrumental variables, which are key to identifying the effect of mother's participation given her possible selection into welfare. In all the variations of the model, we instrument for mother's previous welfare participation using the policy parameters defined by the state AFDC/TANF benefit guarantee. The table also shows results by using other state-by-year instruments, including the overall application denial rate for AFDC/TANF, the application denial rate for procedural reasons, the rate at

which wrongful denials are overturned through favorable hearing claims, and the state unemployment rate over daughter's critical exposure ages 12-18. 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 trap from the poverty trap. Regardless, across the 6 columns in Table S.3-2, we get similar transmission effects both before and after welfare reform as in Table 2.

TABLE S.3-2. INTERGENERATIONAL TRANSMISSION OF AFDC/TANF PARTICIPATION WITH ALTERNATIVE INSTRUMENTAL VARIABLES

	(1)	(2)	(3)	(4)	(5)	(6)
Mother's participation	0.253	0.299	0.320	0.312	0.279	0.299
	(0.050)	(0.051)	(0.054)	(0.065)	(0.054)	(0.060)
After welfare reform	0.064	0.078	0.088	0.089	0.079	0.084
	(0.020)	(0.023)	(0.019)	(0.026)	(0.024)	(0.025)
Mother's participation × after welfare reform	-0.171	-0.212	-0.243	-0.247	-0.219	-0.235
	(0.045)	(0.049)	(0.042)	(0.057)	(0.053)	(0.055)
Instrumental variables (measured as of daughters ages 12-18):						
AFDC/TANF	X	X	X	X	X	X
AFDC/TANF application denial		X	X			
Unemployment rate			X			
AFDC/TANF procedural denial					X	X
AFDC/TANF favorable claims						X
Weak IV test statistic	22.436	24.263	27.568	17.149	21.674	23.631
p-value	0.000	0.001	0.004	0.001	0.003	0.014
Hansen J statistic	1.411	11.749	12.693	0.880	4.044	10.058
p-value	0.494	0.068	0.241	0.644	0.671	0.435
Number of daughters	2961	2951	2951	1422	1422	1422
Observations	56068	55873	55873	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 daughter's age, age squared, mother's average age during potential welfare observation years, mother's average age squared, indicators for number of children, daughter's state AFDC/TANF benefit standard, daughter's EITC federal/state maximum credit, state-level poverty rate, AFDC/TANF recipiency rate, and unemployment rate. 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. Given the limited data availability of procedural denial and favorable claims across years, estimates in columns (4)-(6) use a restricted sample of daughters who were ages 16-35 in 1991. The weak IV test statistic is a Kleibergen-Paap (2006) rank statistic. Daughters' PSID core longitudinal weights are used in estimation.

In a previous version of the manuscript, we also instrumented for mother's previous welfare participation using the combined federal and state EITC along with the state AFDC/TANF benefit guarantee. 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. A higher EITC means that work is more attractive than welfare since EITC eligibility is work conditioned. However, as pointed out by a reviewer, it is unclear that EITC payments during childhood

can be excluded from a daughter's participation decision as an adult. For instance, EITC payments can increase the daughter's likelihood of finishing high school, and that can affect welfare use as an adult. When AFDC/TANF benefit guarantee and EITC were used as instruments, the main conclusions of our investigation did not change and the empirical results were qualitatively similar to the results presented in Table S.3-2.

S.3.3. Mother's Future Participation and IVs: A Falsification Exercise

As extensively discussed in the manuscript, the OLS evidence of persistence in welfare participation could be attributed to a poverty trap as opposed to a welfare trap. Our consistent approach to estimation of the effect of welfare reform uses the variation of mother's participation that is related to her welfare status separately from conditions related to her poverty status by using policy instruments. Because low-income adult daughters are likely to have low-income mothers, and low-income mothers are likely to have low-income daughters, the "effect" of future participation of mothers on daughter's current participation is likely to be associated with the poverty trap and not with a welfare trap.

We begin this section by presenting results from a falsification exercise that includes mother's future welfare participation in the equation for daughter's current participation. The causal transmission effect of future welfare participation on current participation is zero. However, mother's future participation at $t + s$ for $s > 1$ and daughter's participation at t are likely to be correlated because daughter's and mother's incomes are correlated over time. The poverty trap drives this dependence, and the use of the policy instruments in our difference-in-difference-type specification is expected to consistently estimate a zero effect.

Using Table S.3-3, we investigate whether the mother's future welfare use in any year from $t + 5$ to $t + 11$ correlates with her daughter's welfare use at time t. We created a window for future participation that begins 5 years in the future and spans 7 years. For instance, a daughter's participation in 1990 would be compared to her mother's participation any time from 1995-2001. We only use observations for which the mother is observed for those years, which explains the smaller number of daughters shown in the table. We present OLS results in columns (1) and (4), and IV results in the other columns for AFDC/TANF participation. Columns (2) and (5) present IV results based on the set of instruments used in Table 2, in addition to the new instrumental variables used for future mother's participation in column (5). Mother's future instrumental variables are defined by the state AFDC/TANF benefit guarantee considering an equivalent window size to prior instrument measures over the critical exposure period for daughter's ages 12-18. Columns (3) and (6) present IV results based on the set of instruments used in Table 2, in addition to the application denial rate for procedural reasons and the rate at which wrongful denials are overturned through favorable hearing claims (Table S.3-2).

These alternative policy instruments are potentially strong instruments for separating the welfare trap from the poverty trap, and they lead to similar conclusions as shown in column (3).

TABLE S.3-3. INTERGENERATIONAL TRANSMISSION OF AFDC/TANF PARTICIPATION CONTROLLING FOR MOTHER'S FUTURE WELFARE PARTICIPATION

	(1)	(2)	(3)	(4)	(5)	(6)
Mother's prior participation	0.179	0.396	0.292	0.136	0.349	0.338
	(0.024)	(0.081)	(0.091)	(0.023)	(0.127)	(0.153)
After welfare reform	0.023	0.058	0.037	0.016	-0.059	0.047
	(0.012)	(0.029)	(0.023)	(0.012)	(0.029)	(0.029)
Mother's prior participation × after welfare reform	-0.105	-0.240	-0.152	-0.084	-0.292	-0.264
	(0.030)	(0.102)	(0.079)	(0.027)	(0.130)	(0.152)
Mother's future participation				0.007	-0.285	-0.083
				(0.023)	(0.565)	(1.035)
Mother's future participation × after welfare reform				-0.020	-0.232	-0.452
				(0.025)	(0.682)	(0.690)
Mother's prior × future participation				0.255	-0.267	-0.084
				(0.061)	(0.711)	(0.885)
Mother's prior × future × after welfare reform				-0.035	0.578	0.888
				(0.060)	(0.886)	(0.855)
Instrumental variables	No	Yes	Yes	No	Yes	Yes
Weak IV test statistic		21.664	20.601		9.290	5.044
p-value		0.024	0.038		0.232	0.655
Hansen J statistic		6.915	10.777		3.571	8.879
p-value		0.733	0.375		0.734	0.181
Number of daughters	1665	1665	1586	1665	1665	1586
Observations	15034	15034	14828	15034	15034	14828

Notes: Robust standard errors with state clustering are shown in parentheses. All specifications control for state and year effects in addition to daughter's age, age squared, mother's average age during potential welfare observation years, mother's average age squared, indicators for number of children, daughter's state AFDC/TANF benefit standard, daughter's EITC federal/state maximum credit, state-level poverty rate, AFDC/TANF recipiency rate, and unemployment rate. The baseline instrumental variables include average and maximum measures of the mother's AFDC/TANF benefit standard, and interactions of each with an indicator for welfare reform. Column (5) includes additional instruments for the mother's future participation using the baseline instrument measures constructed over future years $t+5$ to $t+11$, and columns (3) and (6) alternatively include instrument measures based on the AFDC/TANF procedural denial and AFDC/TANF favorable claims when the daughter is aged 12-18. The weak IV test statistic is a Kleibergen-Paap (2006) rank statistic. Daughters' PSID core longitudinal weights are used in estimation.

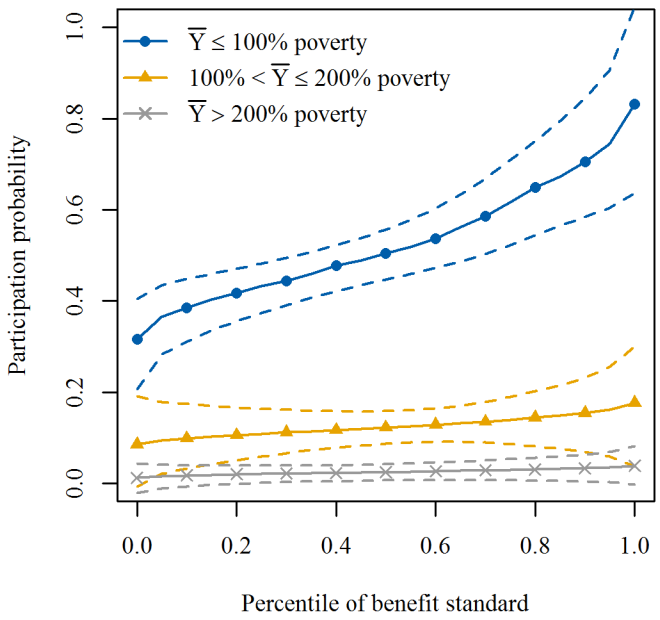
The OLS estimates suggest that among mothers who previously participated in welfare, future participation significantly increases the likelihood of daughter's current participation by 25 percentage points (column 4). This point estimate is naturally biased and a probable explanation is failure of controlling for a lack of economic opportunities, which creates dependence between mother's and

daughter's unobserved characteristics in the specification. On the other hand, using the policy instruments, we find IV estimates equal to -0.267 (column 5) and -0.084 (column 6) that are not statistically significantly different from zero. The results for the broader safety net, which are not presented here to save space, suggest similar conclusions. Overall, these results offer suggestive evidence that our IV approach seems to attenuate, and possibly eliminate, biases in the estimation of the impact of the welfare reform. That is, the use of policy instruments leads to an approach that is identified by variation in the mother's participation related to her welfare status and not to her poverty status.

S.3.4. Interpretation of Results and Heterogeneous Effects

Recall that in the first columns of Table 2, we find that the IV estimate of mother's participation is larger than the OLS estimate. One explanation of this result is that the model includes heterogeneous effects. Our 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. Therefore, our estimates might be interpreted as a local average treatment effect, as in Imbens and Angrist (1994).

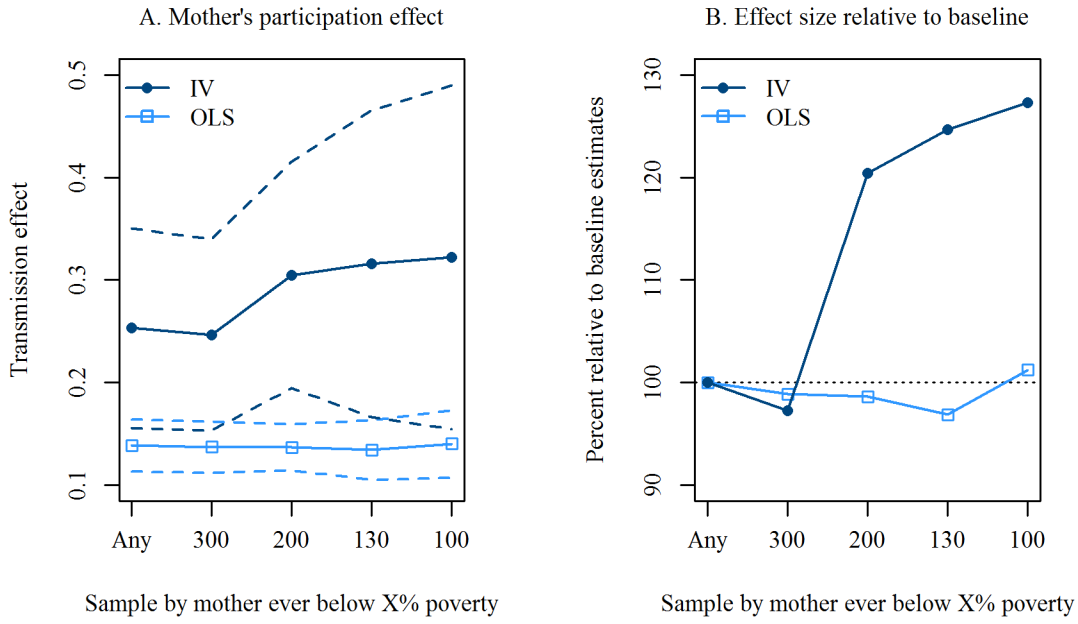
FIGURE S.3-1. MOTHER'S WELFARE PARTICIPATION RELATIVE TO AFDC/TANF BENEFIT LEVELS



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 across all observation years. Dashed lines represent 95-percent pointwise confidence intervals with state-level clustering.

Using Figure S.3-1, we investigate empirically the relationship between mothers' welfare participation and the main policy instrument of AFDC/TANF benefit generosity. As expected, 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. Additionally, Figure S.3-2 shows that the IV estimates of the mother's transmission effects are increasing in subsamples by mothers having income below lower thresholds of poverty, whereas the OLS estimates are generally flat across these same groups. Once again, the evidence supports the hypothesis of heterogeneous effects, which can explain the differences between the OLS and IV estimates in Table 2 of the manuscript.

FIGURE S.3-2. OLS AND IV ESTIMATES OF WELFARE TRANSMISSION EFFECTS BY SUBSAMPLES OF INCREASINGLY MARGINAL PARTICIPANTS



Notes: The subsamples are restricted by whether the mother ever previously had income below the given federal poverty thresholds, where "Any" corresponds to the baseline estimates shown in Table 2 columns (1) and (2).

Motivated by the heterogeneity of transmission by mother's characteristics, we return to estimation of the baseline IV model of Table 2 by 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. 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. Table S.3-4 shows that the results presented in Table 2 are little changed when we add controls for mother's background like education and income.

TABLE S.3-4. IV ESTIMATES OF INTERGENERATIONAL TRANSMISSION OF AFDC/TANF PARTICIPATION WITH CONTROLS FOR MOTHER'S CHARACTERISTICS

	(1)	(2)	(3)	(4)	(5)	(6)
Mother's participation	0.252	0.285	0.285	0.392	0.475	0.472
	(0.050)	(0.087)	(0.087)	(0.085)	(0.175)	(0.175)
After welfare reform	0.064	0.064	0.065	0.077	0.080	0.081
	(0.020)	(0.021)	(0.020)	(0.031)	(0.035)	(0.034)
Mother's participation × after welfare reform	-0.172	-0.181	-0.182	-0.198	-0.222	-0.224
	(0.045)	(0.050)	(0.049)	(0.077)	(0.093)	(0.092)
Mother's controls:						
At most high school education	X		X	X		X
Ever below 200% poverty		X	X		X	X
Misclassification correction	No	No	No	Yes	Yes	Yes
Weak IV test statistic	22.468	17.109	17.532	21.034	15.032	15.204
p-value	0.000	0.001	0.001	0.000	0.002	0.002
Hansen J statistic	1.387	1.877	1.872	1.483	1.527	1.514
p-value	0.500	0.391	0.392	0.476	0.466	0.469
Number of daughters	2961	2961	2961	2961	2961	2961
Observations	56068	56068	56068	56068	56068	56068

Notes: Robust standard errors with state clustering are shown in parentheses. Controls for mother's characteristics, used where indicated, include an indicator if the mother's educational attainment is less than or equal to 12 years, and an indicator for mother's family income has ever been below 200 percent the official poverty threshold by family size. All specifications control for state and year effects in addition to daughter's age, age squared, mother's average age during potential welfare observation years, mother's average age squared, indicators for number of children, daughter's state AFDC/TANF benefit standard, daughter's EITC federal/state maximum credit, state-level poverty rate, AFDC/TANF recipiency rate, and unemployment rate. Instrumental variables include average and maximum measures of the mother's AFDC/TANF benefit standard, 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. Daughters' PSID core longitudinal weights are used in estimation.

S.4. Difference-in-Difference-Type Approach: A Sensitivity Analysis

In this section, we re-estimate the main equation (1) under different assumptions. We first examine whether identification of the parameter of interest is driven by latent trends and confounders not properly controlled for in the model. We then subject the baseline difference-in-difference-type estimates to a number of specification checks, including a placebo-type falsification exercise and further null reform effects on means-tested welfare transmission beyond AFDC/TANF participation.

S.4.1. Latent State Trends

We begin this section by examining whether the identification of the transmission parameter and the effect of the reform are driven by unobserved state-specific time trends or other state-time variables

not properly controlled for in equation (1). Following closely Wolfers (2006), we augment the model estimated in Table 2 with linear and quadratic state trends and present the results in Table S.4-1. The table shows that the IV estimates of the AFDC/TANF transmission effect and the welfare reform effect are marginally attenuated. For instance, the transmission estimate in column (2) is slightly smaller than the 0.253 estimate in Table 2. It is clear that controlling for state-specific time trends does not significantly change the results.

The results presented in Table S.4-1 complement the evidence presented in Figure 4 in the manuscript. Recall that we performed an event-type investigation when we estimate a model of transmission effects interacted with years before and after reform in models with linear and quadratic state-specific trends. We did not find significant differences in the dynamic version of our equation (1) with or without state-specific trends.

TABLE S.4-1. SENSITIVITY OF RESULTS TO THE INCLUSION OF STATE-SPECIFIC TRENDS

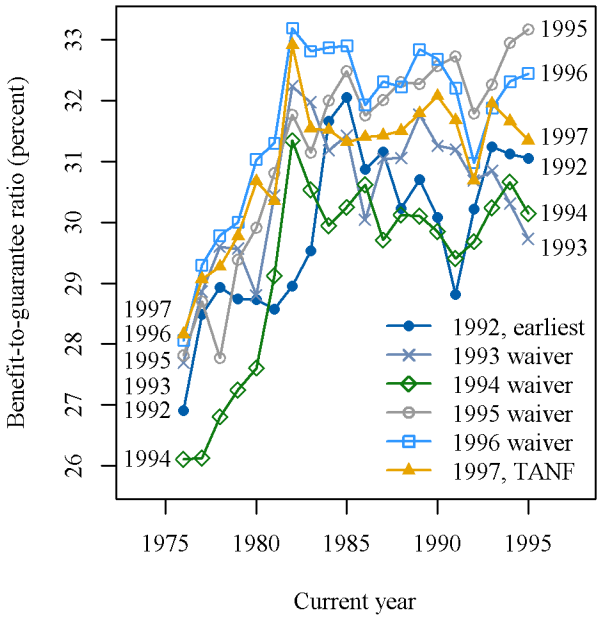
Daughter's outcome variable:	AFDC/TANF				AFDC/TANF, SNAP, SSI			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mother's participation	0.139	0.246	0.231	0.388	0.222	0.318	0.293	0.411
	(0.013)	(0.049)	(0.021)	(0.085)	(0.019)	(0.074)	(0.025)	(0.100)
After welfare reform	0.037	0.060	0.054	0.075	0.010	0.007	0.014	-0.001
	(0.008)	(0.018)	(0.014)	(0.030)	(0.013)	(0.027)	(0.019)	(0.038)
Mother's participation × after welfare reform	-0.093	-0.156	-0.125	-0.175	-0.041	-0.004	-0.022	0.068
	(0.016)	(0.044)	(0.031)	(0.080)	(0.022)	(0.074)	(0.031)	(0.105)
Instrumental variables	No	Yes	No	Yes	No	Yes	No	Yes
Misclassification correction	No	No	Yes	Yes	No	No	Yes	Yes
Linear state-specific trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quadratic state trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Weak IV test statistic		22.525		20.781		22.525		21.271
p-value		0.000		0.000		0.000		0.000
Hansen J statistic		1.106		1.365		4.281		4.552
p-value		0.575		0.505		0.118		0.103
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 daughter's age, age squared, mother's average age during potential welfare observation years, mother's average age squared, indicators for number of children, daughter's state AFDC/TANF benefit standard, daughter's EITC federal/state maximum credit, state-level poverty rate, AFDC/TANF recipiency rate, and unemployment rate. Instrumental variables include average and maximum measures of the mother's AFDC/TANF benefit standard, 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. Daughters' PSID core longitudinal weights are used in estimation. Abbreviations: Food Stamps/Supplemental Nutrition Assistance Program (SNAP), and Supplemental Security Income (SSI).

Next, we present a set of figures and tables to investigate the timing of the reforms. We begin by showing changes in the ratio of average benefits to the statutory maximum guarantee for a typical

recipient family (single parent with two children). This statistic offers a measure of how much states pay out to families conditional on eligibility, which could vary by state differences in program generosity, perhaps as a proxy for accessibility, or an indication of the depth of poverty among eligible families. Figure S.4-1 shows that there are no discernible differences in trends by groups of states who implemented reform earlier compared to later.

FIGURE S.4-1. STATE AFDC/TANF BENEFIT-TO-GUARANTEE RATIOS BY DATE OF WELFARE REFORM IMPLEMENTATION AND CURRENT YEAR



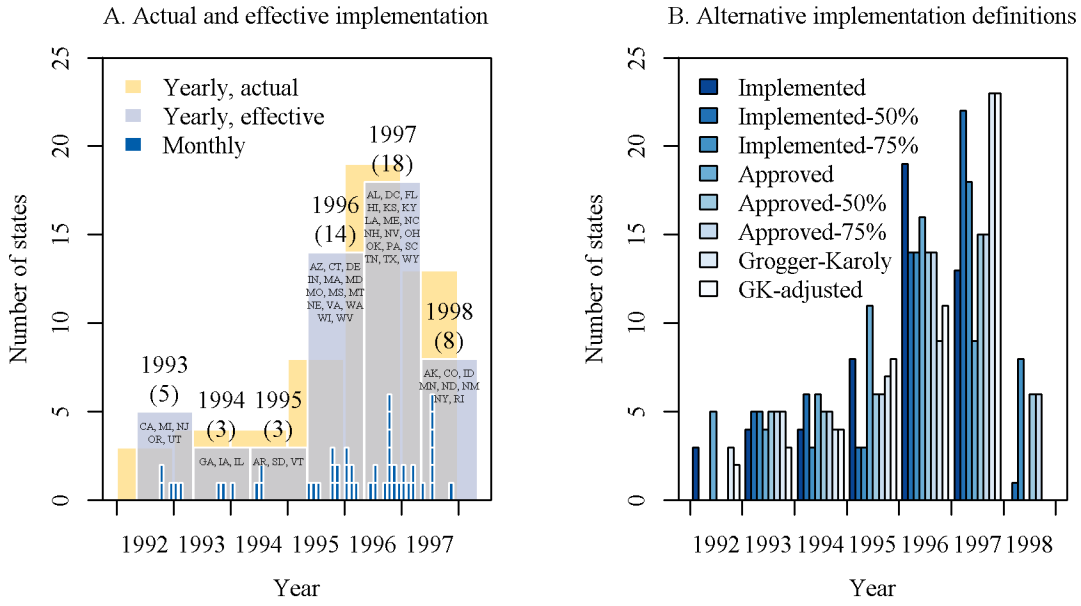
Notes: State-level benefit-to-guarantee ratios represent the average family benefit by state divided by the statutory maximum guarantee for a single-parent family with two children, which are shown grouped by year of state-wide welfare reform implementation.

We now explore the sensitivity of our findings by varying the definition of a post-welfare reform state. Recall that the definition of reform in the baseline specification is that mother's reform indicator only turns on when she is observed in a state-year after reform, though some mothers may have left the sample before reform and thus the indicator remains "before reform" even after the TANF years begin. We consider alternative reform-timing definitions based on dates reported in Crouse (1999) and Grogger and Karoly (2005), as well as using the earliest implementation in either the mother's or daughter's state of residence to define the reform variable.

Figure S.4-2 offers a visualization of the variation in welfare reform implementation dates in panel A, and in panel B, a visualization of reform implementation under various alternative definitions based on Crouse (1999) as well as Grogger and Karoly (2005). In panel A, the effective year of reform implementation represents states that introduced reform for at least 75 percent of the year, which

corresponds to the definition used in estimation throughout. In panel B, the number of states with welfare reform by year is compared based on the actual implementation date and the effective implementation as shown in panel A, and additional rules are shown such as at least 50 percent of the year is after implementation, or waiver approval instead of reform implementation, as well as two measures from Grogger and Karoly (2005) based on the first reform implementation (labeled "Grogger-Karoly") or the second implementation if states introduced more than one reform ("GK-adjusted").

FIGURE S.4-2. WELFARE REFORM IMPLEMENTATION DATES AND ALTERNATIVE DEFINITIONS



Notes: Panel A: The actual implementation dates are represented by the monthly and "Yearly, actual" bars, whereas the "Yearly, effective" bars indicate the number of states with at least 75 percent of the year under the implemented welfare reform policies. Panel B: "Implemented" denotes the Crouse (1999) implementation year; "Implemented-50" or "-75" denotes at least 50 or 75 percent of the year after implementation, respectively; "Approved" denotes the Crouse (1999) approval year; "Approved-50" and "-75" denotes at least 50 or 75 percent of the year after approval; "Grogger-Karoly" denotes the first year Grogger and Karoly (2005) list a state reform; "GK-adjusted" denotes the second year a reform bundle is listed if a state introduces reform more than once during the waiver period.

Next, we re-estimate the difference-in-difference-type model based on these varying definitions of reform timing using instrumental variables, with results shown in Table S.4-2. For these estimates, we add two more reform definitions: one defined by at least 75 percent of the year after reform based on the *daughter's* state of residence (DR), and the other defined by the earliest reform by either the daughter's or mother's state of residence (DR/R). Note that column (2) corresponds to our baseline results in Table 2 column (2) with the exception that the reform indicator equals 1 based on the mother's last observed state of residence if her current state variable is missing at the time of reform (otherwise, in the main results,

the reform indicator remains 0). The correlation between these alternative measures and the baseline reform ranges between 0.779 and 0.826 (Table S.4-2).

We present results using Table S.4-2, which shows estimates of the parameters of interest by different definitions in the timing of implementation of the reforms. Recall that the effect of mother's participation is 0.253 (s.e. 0.050) and the effect of the reform is -0.170 (s.e. 0.045) in Table 2 column (2) of the manuscript. Looking at the estimates in Table S.4-2, we find that the IV estimates are slightly larger in absolute value, with no substantive differences in terms of point estimates and the percent change of the transmission effect across all specifications of reform.

TABLE S.4-2. IV ESTIMATES OF WELFARE TRANSMISSION BY VARIATION IN RELEVANT DATE OF REFORM

Reform date:	Impl.	Impl-75	Approv.	Appr-75	GK-2005	GK-adj	DR	DR/R
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mother's participation	0.292	0.272	0.298	0.279	0.289	0.289	0.277	0.280
	(0.076)	(0.069)	(0.079)	(0.071)	(0.075)	(0.074)	(0.070)	(0.070)
After welfare reform	0.070	0.062	0.068	0.062	0.064	0.062	0.063	0.059
	(0.022)	(0.019)	(0.023)	(0.021)	(0.021)	(0.020)	(0.021)	(0.021)
Mother's participation ×	-0.210	-0.190	-0.218	-0.198	-0.209	-0.211	-0.198	-0.200
after welfare reform	(0.073)	(0.064)	(0.076)	(0.067)	(0.072)	(0.072)	(0.067)	(0.065)
Correlation with baseline reform	0.790	0.826	0.779	0.814	0.796	0.794	0.811	0.818
Weak IV test statistic	19.412	18.085	19.573	18.247	19.605	20.075	18.795	19.291
p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Hansen J statistic	2.102	1.919	2.536	2.281	2.341	2.528	2.561	2.555
p-value	0.350	0.383	0.281	0.320	0.310	0.283	0.278	0.279
Number of daughters	2961	2961	2961	2961	2961	2961	2961	2961
Observations	56068	56068	56068	56068	56068	56068	56068	56068

Notes: All specifications above differ from the baseline reform definition by which the mother's reform indicator only turns on when she is observed in a state-year after reform, though some mothers may have left the sample before reform and thus the indicator remains "before reform" even after the TANF years begin. "Impl." denotes the Crouse (1999) implementation year; "Impl-75" denotes at least 75 percent of the year after implementation; "Approv." denotes the Crouse (1999) approval year; "Appr-75" denotes at least 75 percent of the year after approval; "GK-2005" denotes the first year Grogger and Karoly (2005) list a state reform; "GK-adj" denotes the second year a reform bundle is listed if a state introduces reform more than once during the waiver period; "DR" denotes the first state-year the daughter experiences reform using the Crouse (1999) Impl-75 rule; and, "DR/R" denotes the earliest year either the mother or daughter experiences reform using the Crouse (1999) Impl-75 rule.

S.4.2. Timing of Welfare Reform: A Placebo-Type Falsification Exercise

In order to evaluate our assumption of random trends around the timing of reform implementation, we perform a falsification exercise in the form of a placebo-type test. Now, our primary objective is to evaluate the cross-state variation in states implementation of welfare reform, which allows us to quasi-experimentally separate out the effect of a mother's participation in welfare during her daughter's childhood on the daughter's participation as an adult. Previous work has shown that a state's decision to apply for an AFDC waiver was not an endogenous response to caseload size (see Ziliak et al.,

2000), but we perform this test for completeness. Moreover, as shown in Table S.4-2, variations in the definition of reform implementation dates do not affect the main findings of our study.

As explained in detail below, we randomly generate welfare reform dates to then estimate the parameters of interest using the same methods within an equivalent class of models. We are not aware of a similar placebo-type test in the literature, although our idea is somewhat related to the recent work of Hagemann (2019). Consider the model introduced in equation (1) for $t \in \{1,2,\dots,T\}$:

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

where W_{ist}^{d}, $W_{is,\forall j<t}^{m}$, $\boldsymbol{x}_{ist}^{d}$, μ_{s}^{d}, κ_{t}^{d}, and ν_{ist}^{d} are defined as before. Recall that 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. Let t_s^* be the year when the reform is implemented in state s. Note that for $t_s < t_s^*$, $R_{st}^{m} = 0$, and for $t_s > t_s^*$, $R_{st}^{m} = 1$. In what follows, we drop the dependence of t_s^* on s for notational convenience. Lastly, we split years before and after the reform into two sets: before-reform years $B = \{1,2,\dots,t^* - 1\}$, and after-reform years $A = \{t^*, t^* + 1, \dots, T\}$.

Let $a_t = \alpha + \kappa_t^d$. Note that for $t \in B$, the parameter δ can be estimated by instrumental variables using the following regression model,

$$W_{ist}^{d} = a_t + \beta' \boldsymbol{x}_{ist}^{d} + \delta W_{is,\forall j<t}^{m} + \mu_{s}^{d} + \nu_{ist}^{d},$$

while, for $t \in A$, the parameter $\Delta = \delta + \theta$ can be estimated by instrumental variables using the following regression model,

$$W_{ist}^{d} = b_t + \beta' \boldsymbol{x}_{ist}^{d} + \Delta W_{is,\forall j<t}^{m} + \mu_{s}^{d} + \nu_{ist}^{d},$$

where $b_t = a_t + \gamma$. Consequently, one can identify and consistently estimate θ considering the difference $\Delta - \delta$ obtained by estimating the last two equations. This relies on the assumption that t^* is conditionally random, or alternatively, that the timing of the reform does not depend on the participation of daughters and mothers, and/or there are no latent state-trends that generate dependence between daughter's participation and the timing of the reform. If t^* is conditionally random, the procedure is consistent, and consequently, we should obtain results similar to Table 2.

For the implementation of the approach, consider T^* years indicating the cardinality of the sets B and A, where T^* represents the number of years before and after a state implements welfare reform at time t^*. We perform our baseline estimation on a sample of daughter-mother pairs randomly drawn from only one time period before welfare reform, $t_b \in B$, and one period after, $t_a \in A$, and we repeat this estimation for randomly drawn years over $R = 1000$ samples. The estimator of our parameters of interest, $\{\delta, \gamma, \theta\}$, can be obtained by averaging $\{\widehat{\delta, \gamma, \theta}\}_r$ for $r = \{1,2,\dots,R\}$. The equations can be estimated separately, as introduced above, or jointly using a difference-in-difference-type specification. Because our interest is to compare the results with Table 2, we adopted the second approach, although the

results for δ and θ were similar for both approaches. To further explore the sensitivity of our placebo test, we allow the window of observations before/after reform to vary in length with $T^* \in \{5, 10, 15\}$.

The results are shown in Table S.4-3, where IV test statistics, number of daughters, and observations are taken as mean values across R samples. The table shows that OLS results based on the conditional independence condition on the timing of the reform estimates converge to our Table 2 column (1) estimates as T^* increases. Moreover, the IV estimates are similar to the baseline reform effects. Overall, Table S.4-3 presents evidence that is largely consistent with the estimates presented in columns (1) and (2) of Table 2.

TABLE S.4-3. PLACEBO ESTIMATES FOR A DIFFERENCE-IN-DIFFERENCE-TYPE MODEL

	5 years before/after		10 years before/after		15 years before/after	
	(1)	(2)	(3)	(4)	(5)	(6)
Mother's participation	0.117	0.222	0.126	0.214	0.140	0.223
	(0.013)	(0.031)	(0.013)	(0.061)	(0.027)	(0.074)
After welfare reform	0.025	0.044	0.035	0.052	0.048	0.056
	(0.014)	(0.030)	(0.019)	(0.033)	(0.102)	(0.051)
Mother's participation ×	-0.064	-0.122	-0.078	-0.117	-0.095	-0.125
after welfare reform	(0.017)	(0.066)	(0.017)	(0.074)	(0.029)	(0.081)
Instrumental variables	No	Yes	No	Yes	No	Yes
Weak IV test statistic		18.066		19.002		19.501
p-value		0.000		0.000		0.000
Hansen J statistic		2.612		1.778		1.849
p-value		0.369		0.497		0.493
Number of daughters	2958	2958	2760	2760	2493	2493
Observations	4036	4036	3637	3637	3191	3191

Notes: Estimates shown above correspond to our baseline specifications estimated only for daughters observed for a randomly drawn year before and random year after welfare reform within the timespan indicated above, either 5, 10, or 15 years pre-/post-reform. Statistics are constructed based on 1000 bootstrap replications.

S.4.3. Transmission of Other Means-Tested Program Participation

Our difference-in-difference design implies that welfare reform changed transmission of AFDC/TANF participation without reducing transmission of participation in a broader set of means-tested assistance programs (Table 2 in the manuscript). Here we provide further evidence on the transmission patterns across these other welfare programs, which further confirms the null effect of the 1990s welfare reform on means-tested programs besides AFDC/TANF. Table S.4-4 shows unconditional and conditional correlations between mother-daughter participation in: 1) AFDC/TANF, SNAP, SSI; 2) SNAP, SSI; 3) SNAP; and, 4) SSI. The conditional correlations for each of these categories is less than 0.1 (compared to about 0.15 for AFDC/TANF), and there is little difference between the correlations by SNAP/SSI and SNAP alone. The conditional correlation in SSI participation is similar to the short-term disability transmission effect in Dahl, Kostøl, and Mogstad (2014). For all of these categorical definitions

of welfare transmission, welfare reform has no statistically significant effect on transmission, and the economic effect is at most 1 percentage point difference.

TABLE S.4-4. INTERGENERATIONAL TRANSMISSION OF WELFARE PARTICIPATION BY PROGRAM: UNCONDITIONAL AND CONDITIONAL CORRELATIONS

Daughter's outcome:	AFDC/TANF, SNAP, SSI		SNAP, SSI		SNAP		SSI	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	A. Mother's AFDC/TANF participation							
Mother's participation	0.257	0.218	0.239	0.203	0.224	0.188	0.051	0.047
	(0.022)	(0.018)	(0.022)	(0.020)	(0.020)	(0.017)	(0.015)	(0.015)
After welfare reform	-0.012	0.000	-0.010	-0.005	-0.013	0.001	0.006	-0.012
	(0.008)	(0.014)	(0.007)	(0.014)	(0.007)	(0.014)	(0.003)	(0.010)
Mother's participation ×	-0.039	-0.037	-0.029	-0.028	-0.024	-0.025	-0.003	0.001
after welfare reform	(0.021)	(0.021)	(0.021)	(0.020)	(0.019)	(0.019)	(0.015)	(0.014)
Conditional on baseline controls:	No	Yes	No	Yes	No	Yes	No	Yes
Percent change in levels	-15%	-17%	-12%	-14%	-11%	-13%	-6%	1%
p-value	0.053	0.059	0.143	0.139	0.191	0.165	0.825	0.966
Percent change over baseline	-10%	-12%	-8%	-10%	-4%	-7%	-21%	-14%
p-value	0.239	0.218	0.356	0.314	0.648	0.518	0.376	0.559
Pooled probability	0.119	0.119	0.112	0.112	0.102	0.102	0.024	0.024
After-reform probability	0.112	0.112	0.107	0.107	0.095	0.095	0.028	0.028
Number of daughters	2961	2961	2961	2961	2961	2961	2961	2961
Observations	56068	56068	56067	56067	55891	55891	56058	56058
	B. Mother's welfare participation corresponding to daughter's outcome							
Mother's participation	0.194	0.154	0.191	0.154	0.184	0.149	0.084	0.077
	(0.013)	(0.012)	(0.015)	(0.013)	(0.015)	(0.013)	(0.027)	(0.025)
After welfare reform	-0.017	-0.013	-0.014	-0.015	-0.014	-0.006	0.005	-0.010
	(0.007)	(0.015)	(0.006)	(0.015)	(0.006)	(0.012)	(0.004)	(0.010)
Mother's participation ×	-0.014	-0.009	-0.010	-0.007	-0.009	-0.010	-0.005	-0.002
after welfare reform	(0.014)	(0.015)	(0.014)	(0.015)	(0.017)	(0.016)	(0.024)	(0.023)
Conditional on baseline controls:	No	Yes	No	Yes	No	Yes	No	Yes
Percent change in levels	-7%	-6%	-5%	-5%	-5%	-6%	-5%	-2%
p-value	0.297	0.538	0.493	0.628	0.589	0.532	0.842	0.937
Percent change over baseline	-2%	-0%	-1%	-0%	2%	1%	-20%	-17%
p-value	0.828	0.987	0.932	0.984	0.800	0.950	0.385	0.472
Pooled probability	0.119	0.119	0.112	0.112	0.102	0.102	0.024	0.024
After-reform probability	0.112	0.112	0.107	0.107	0.095	0.095	0.028	0.028
Number of daughters	2961	2961	2961	2961	2961	2961	2952	2952
Observations	56068	56068	56067	56067	55891	55891	55820	55820

Notes: Estimates in panel A column (2) correspond to Table 2 column (5) in the manuscript. Daughter's and mother's welfare participation are indicators for current or any prior participation, respectively, except that the definition of welfare program is allowed to vary by specification as indicated above.

S.5. Survey Weights and Biennial Interviewing

As mentioned in Section IV of the manuscript, the large number of mothers and daughters linked over the PSID survey years is comprised of both the SRC and SEO subsamples. Our sample includes

about 52 percent of daughters (48 percent of observations) from the SEO subsample, and 48 percent of daughters (52 percent of observations) from the SRC subsample. We use the core longitudinal weights throughout the analysis to correct for the oversample of low-income and minority families in the SEO. In this section, we investigate the sensitivity of our findings to the use of survey weights

TABLE S.5-1. INTERGENERATIONAL TRANSMISSION OF AFDC/TANF PARTICIPATION ESTIMATED WITHOUT PSID LONGITUDINAL WEIGHTS

	(1)	(2)	(3)	(4)
A. Full sample (SRC and SEO subsamples)				
Mother's participation	0.189	0.349	0.299	0.569
	(0.014)	(0.059)	(0.021)	(0.091)
After welfare reform	0.065	0.124	0.074	0.157
	(0.009)	(0.035)	(0.018)	(0.054)
Mother's participation × after welfare reform	-0.139	-0.233	-0.177	-0.299
	(0.016)	(0.061)	(0.031)	(0.097)
Instrumental variables	No	Yes	No	Yes
Misclassification correction	No	No	Yes	Yes
Weak IV test statistic		20.095		21.060
p-value		0.000		0.000
Hansen J statistic		2.023		0.972
p-value		0.364		0.615
Number of daughters	2961	2961	2961	2961
Observations	56068	56068	56068	56068
B. Survey Research Center (SRC) sample only				
Mother's participation	0.109	0.194	0.175	0.249
	(0.019)	(0.066)	(0.032)	(0.109)
After welfare reform	0.028	0.055	0.041	0.064
	(0.010)	(0.022)	(0.019)	(0.033)
Mother's participation × after welfare reform	-0.083	-0.187	-0.114	-0.205
	(0.021)	(0.065)	(0.037)	(0.104)
Instrumental variables	No	Yes	No	Yes
Misclassification correction	No	No	Yes	Yes
Weak IV test statistic		16.582		15.999
p-value		0.001		0.001
Hansen J statistic		4.570		4.436
p-value		0.102		0.109
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 daughter's age, age squared, mother's average age during potential welfare observation years, mother's average age squared, indicators for number of children, daughter's state AFDC/TANF benefit standard, daughter's EITC federal/state maximum credit, state-level poverty rate, AFDC/TANF recipiency rate, and unemployment rate. Instrumental variables include average and maximum measures of the mother's AFDC/TANF benefit standard, 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.

We re-estimate the baseline specifications from Table 2 in the manuscript 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 and minority families, and then for only the Survey Research Center (SRC) subsample, which is nationally representative (for detailed discussion related to the SEO sample, see Brown, 1996). Relative to Table 2, the results in Table S.5-1 are little changed when we do not weight the estimates (panel A) or when we drop the SEO oversample of low-income families (panel B). The unweighted estimates are larger in magnitude when including the SEO low-income oversample, suggesting that weights are needed for the estimates to be more comparable to the nationally-representative SRC subsample estimates.

The PSID carried out annual interviews from 1968 to 1996, and changed to biennial interviews from 1997 to date. Therefore, our data on welfare participation includes both responses for the prior observation year (T-1) and, after 1997, for the two-year retrospective (T-2). This might have an impact on the accuracy of answers, and in particular, might exacerbate issues associated with misclassification. Thus, we now examine the sensitivity of our findings to the change in the frequency of PSID interviews.

TABLE S.5-2. TRANSMISSION ESTIMATE SENSITIVITY TO T2-YEAR RETROSPECTIVE DATA AFTER SURVEY YEAR 1997

Observation years:	Even years (T1 only)	Odd years (T1 & T2)	All years	Even years (T1 only)	Odd years (T1 & T2)	All years
	(1)	(2)	(3)	(4)	(5)	(6)
Mother's participation	0.142	0.135	0.139	0.273	0.232	0.253
	(0.013)	(0.013)	(0.013)	(0.053)	(0.049)	(0.050)
After welfare reform	0.034	0.035	0.035	0.067	0.060	0.064
	(0.009)	(0.008)	(0.008)	(0.023)	(0.019)	(0.020)
Mother's participation ×	-0.099	-0.090	-0.095	-0.186	-0.156	-0.171
after welfare reform	(0.016)	(0.015)	(0.015)	(0.051)	(0.045)	(0.045)
Instrumental variables	No	No	No	Yes	Yes	Yes
Weak IV test statistic				22.300	22.476	22.436
p-value				0.000	0.000	0.000
Hansen J statistic				1.152	2.075	1.411
p-value				0.562	0.354	0.494
Number of daughters	2961	2961	2961	2961	2961	2961
Observations	28276	27792	56068	28276	27792	56068

Notes: After 1997, biennial PSID survey questions include one- and two-year retrospectives, T-1 and T-2. Columns (1) and (4) represent only the T-1 questions for the even observation years (from odd survey years 1969-2013). Columns (2) and (5) represent the odd observation years, which include T-1 questions for even survey years 1968-1996, and T-2 questions in the biennial survey years 1999-2013. Columns (3) and (6) use all available data and correspond to our baseline estimates in Table 2.

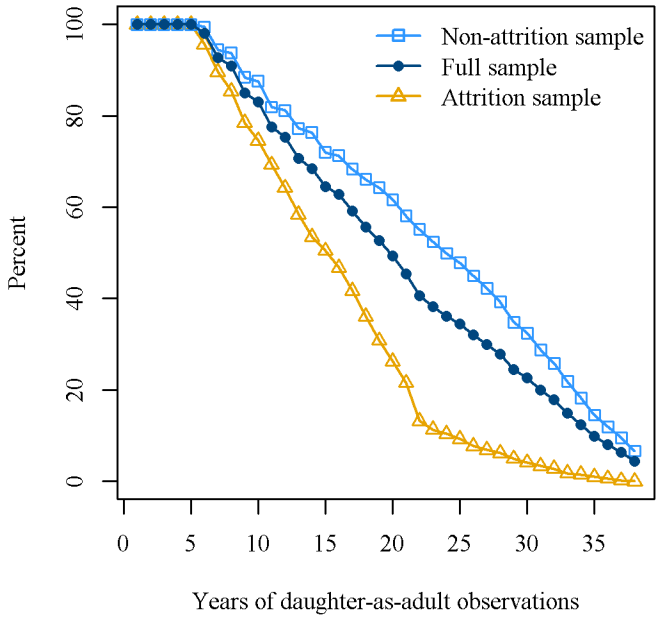
Table S.5-2 presents results of comparing T-1 years and T-2 years as applied to the entire PSID time period. Here, we are defining even years as the even observation years corresponding to the odd survey years 1969-2013, which are the T-1 years of reported economic activity in the prior year. The odd years represent observations from the even survey years 1968-1996 and the T-2 retrospective data from survey years 1999-2013. The columns for all years, (3) and (6), correspond to our baseline OLS and IV estimates of Table 2 in the manuscript. If we assume that the T-1 series is more reliable, then using T-2 years attenuates the magnitude of our results toward zero. However, the size of this potential bias is small.

S.6. Sample Attrition

The high annual PSID response rates have been critical in the success and continued use of the survey since its creation. In long panel studies, however, the representativeness of the sample can be compromised if a significant number of respondents attrit from the survey over time. In studies using the PSID, outcomes for daughter-mother pairs are known to suffer from some degree of attrition bias (Fitzgerald, Gottschalk, and Moffitt, 1998a,b; Ziliak and Kniesner, 1998; and Fitzgerald, 2011), and relatively high attrition rates have been found among low-income adult children with low-income parents (Schoeni and Wiemers, 2015).

We begin our investigation by documenting how attrition affects the number of years an adult daughter is observed in our sample. Figure S.6-1 shows the percentage of daughters who respond to the survey in our sample by the attrition status of the daughter. Recall that the baseline sample restriction requires all adult daughters to be observed at least 5 years (to attenuate measurement issues as discussed in Section IV), so the probability of observing a daughter for 5 consecutive years is 100 percent as shown in the figure. In our sample, daughters are observed for 24 years on average, although a significant number of daughters are observed over a longer period. It is also interesting to see that about half of the daughters who attrit are observed 15 years, which illustrates the relatively high annual response rates and the advantage of using the PSID for the study of intergenerational welfare dependence.

FIGURE S.6-1. UNCONDITIONAL ESTIMATES OF THE PROBABILITY OF RESPONDING TO THE PSID SURVEY BY ATTRITION STATUS



Notes: The attrition samples represents daughters who ever attrit compared to those who never attrit. Daughters in the sample are restricted to a minimum of five years of observation as an adult.

Although the use of the PSID survey weights can reduce potential biases arising from the attrition of daughters observed in Figure S.6-1, we now investigate if this possibly non-random attrition is an important threat to identification of the parameters of interest. We first provide descriptive evidence for the full sample of daughters and the sample of daughters who never attrit from the PSID sample. We end the section by providing evidence on the transmission effect estimated by inverse probability weighting (IPW) (see, among others, Robins, Rotnitzky, and Zhao, 1995; Fitzgerald, Gottschalk, and Moffitt, 1998; Wooldridge, 2007), and investigate the sensitivity of transmission results to different assumptions on the missing data process.

As it is standard in the literature (see, e.g., Fitzgerald, Gottschalk, and Moffitt, 1998; Schoeni and Wiemers, 2015), we present descriptive statistics associated with the observable characteristics of daughters and mothers. The evidence is presented in Table S.6-1, which shows sample mean and standard deviation for the full sample of daughters, the sample of daughters who never atttrited, and the sample of daughters who attrited anytime between 1968 and 2012. The first three columns show descriptive statistics obtained by PSID survey weights, while the last three columns show values obtained by combining survey weights and inverse probability weighting. The probability model for the binary variable indicating whether the daughter never attrits includes the independent variables used in model (1), an indicator for whether the daughter belongs to the SEO subsample, and the logarithm of daughter's

family income (in 2012 dollars). For the weights in Table S.6-1 columns (4)-(6), we estimate a linear probability model that incorporates survey weights to avoid possible biases arising from the overrepresented low-income SEO subsample. Later, for comparison, we show results obtained by estimating first-stage probabilities based on a logit link function and sample averages of the observable variables.

TABLE S.6-1. MOTHER AND DAUGHTER CHARACTERISTICS BY ATTRITION STATUS

	PSID survey weights			PSID survey weights + IPWs		
	All daughters (1)	Never attrited (2)	Ever attrited (3)	All daughters (4)	Never attrited (5)	Ever attrited (6)
	Daughter's characteristics					
Current AFDC/TANF participation	0.044	0.040	0.061	0.045	0.040	0.055
	(0.206)	(0.195)	(0.239)	(0.207)	(0.195)	(0.229)
Age	35.041	36.061	31.242	34.612	35.987	31.812
	(9.400)	(9.543)	(7.747)	(9.262)	(9.517)	(8.021)
Number of children	1.208	1.217	1.173	1.196	1.212	1.165
	(1.238)	(1.248)	(1.201)	(1.235)	(1.246)	(1.212)
Family income	76.576	77.190	74.697	77.334	77.063	77.796
	(106.685)	(90.879)	(144.700)	(121.097)	(91.370)	(159.456)
Same state as birth	0.723	0.714	0.754	0.724	0.714	0.745
	(0.448)	(0.452)	(0.430)	(0.447)	(0.452)	(0.436)
Married	0.654	0.652	0.659	0.653	0.651	0.657
	(0.476)	(0.476)	(0.474)	(0.476)	(0.477)	(0.475)
Non-teen birth	0.805	0.801	0.821	0.808	0.799	0.830
	(0.396)	(0.399)	(0.383)	(0.394)	(0.401)	(0.376)
High school or less	0.484	0.448	0.564	0.493	0.449	0.551
	(0.500)	(0.497)	(0.496)	(0.500)	(0.497)	(0.497)
	Mother's characteristics					
Any prior AFDC/TANF participation	0.271	0.282	0.229	0.261	0.280	0.223
	(0.444)	(0.450)	(0.420)	(0.439)	(0.449)	(0.416)
Age	45.103	45.283	44.433	44.979	45.257	44.411
	(8.626)	(8.846)	(7.717)	(8.496)	(8.818)	(7.768)
Number of children	1.649	1.602	1.823	1.655	1.604	1.761
	(1.125)	(1.095)	(1.216)	(1.122)	(1.090)	(1.177)
Family income	72.169	72.156	72.216	72.618	72.136	73.599
	(83.142)	(89.581)	(52.644)	(80.145)	(89.960)	(54.963)
Same state as birth	0.635	0.619	0.693	0.641	0.620	0.684
	(0.481)	(0.486)	(0.461)	(0.480)	(0.485)	(0.465)
Married	0.732	0.721	0.774	0.738	0.722	0.771
	(0.310)	(0.306)	(0.320)	(0.310)	(0.305)	(0.317)
Non-teen birth	0.696	0.686	0.737	0.704	0.686	0.743
	(0.460)	(0.464)	(0.440)	(0.456)	(0.464)	(0.437)
High school or less	0.649	0.639	0.686	0.649	0.638	0.671
	(0.477)	(0.480)	(0.464)	(0.477)	(0.480)	(0.470)
Survey of Economic Opportunity sample	0.152	0.104	0.329	0.166	0.104	0.291
	(0.359)	(0.306)	(0.470)	(0.372)	(0.306)	(0.454)
Observations	56068	41498	14570	56068	41498	14570

Notes: Standard deviations are in parentheses. Daughters' PSID core longitudinal weights are used to obtain the descriptive statistics in the first three columns and daughters' PSID core longitudinal weights combined with inverse probability weighting (IPW) are used to obtain the last three columns.

We take two important conclusions from Table S.6-1. First, there are small differences between the group of all daughters and the daughters who never attrit (columns (1) and (2)). Consistent with the literature, survey weights appear to be important when practitioners combine SRC and SEO subsamples, and the use of these weights can help reduce observable differences between pairs of daughters and mothers classified by attrition status. Second, when we consider weighting observations by the inverse probability of remaining in the survey, in addition to using survey weights, the differences remain small for most of the variables, but there are some minor improvements when comparing the full sample to those who do not attrit.

The descriptive evidence presented in Table S.6-1 led us to perform an additional robustness check, although it is reassuring that the composition of the subsample of daughters appears to be similar across groups. We also estimate equation (1) using the inverse probability of remaining in the survey in addition to using survey weights. These results are reported in Figure 5 in the manuscript and are shown in more detail in Table S.6-2.

Table S.6-2 shows results obtained two different first-stage methods. In panel A, we estimate the inverse probability weights by a logit model, while in panel B, we estimate the weight using a linear probability model with survey weights. By using survey weights in the first stage, we attempt to correct for possible inconsistencies arising from the overrepresentation of low-income families in the SEO sample. Table S.6-2 also shows results obtained from three different weighting schemes. Columns (1) and (2) show the baseline OLS and IV results from estimators that used survey weights, as in Table 2. Columns (3) and (4) show two-stage results in which the first-stage estimates the probability that a daughter remains in the survey during the period of analysis, and the second-stage estimates rely on the resulting inverse propensity weights only (without using survey weights). The binary response variable is defined as 1 if the daughter never attrits, and 0 otherwise. The independent variables are: mother's welfare participation, a linear and quadratic in age of the mother and daughter, indicators for number of children, policy and economic variables for the daughter as described in Table S.1-1, an indicator for whether the daughter belongs to the SEO subsample, and the logarithm of the daughter's family income (in 2012 dollars). The last two columns show OLS and IV results obtained by using survey weights rescaled by the estimated inverse probability function.

The main empirical takeaway from our regression results presented in Table S.6-2 is that the main findings of our investigation are not significantly different if one addresses the possibility of attrition. The results in the last column of Table S.6-2 (panel B) are shown in Figure 5, which demonstrates that the baseline estimates shown in Table 2 are not sensitive to imposing restrictions in the proportion of daughters who ever attrit that are included in the estimation sample. This can be explained by the large proportion of daughters who are observed over the entire duration of the sample (approximately 65

percent, implying an attrition rate of roughly 35 percent) and by the similar average characteristics of the daughters by attrition status, as shown in Table S.6-1.

TABLE S.6-2. WELFARE TRANSMISSION ESTIMATES AND THE EFFECT OF ATTRITION

	(1)	(2)	(3)	(4)	(5)	(6)
	A. Based on first-stage logit estimates without survey weights					
Mother's participation	0.139	0.253	0.201	0.450	0.133	0.293
	(0.013)	(0.050)	(0.021)	(0.086)	(0.015)	(0.066)
After welfare reform	0.035	0.064	0.076	0.198	0.034	0.090
	(0.008)	(0.020)	(0.013)	(0.056)	(0.012)	(0.034)
Mother's participation ×	-0.095	-0.171	-0.151	-0.348	-0.087	-0.238
after welfare reform	(0.015)	(0.045)	(0.018)	(0.086)	(0.016)	(0.062)
Instrumental variables	No	Yes	No	Yes	No	Yes
Survey weights	Yes	Yes	No	No	Yes	Yes
Inverse probability weight	No	No	Yes	Yes	Yes	Yes
Weak IV test statistic		22.436		19.238		17.257
p-value		0.000		0.000		0.001
Hansen J statistic		1.411		1.633		1.781
p-value		0.494		0.442		0.411
Number of daughters	2961	2961	1935	1935	1935	1935
Observations	56068	56068	41498	41498	41498	41498
	B. Based on first-stage linear probability estimates with survey weights					
Mother's participation	0.139	0.253	0.188	0.379	0.133	0.270
	(0.013)	(0.050)	(0.018)	(0.065)	(0.015)	(0.052)
After welfare reform	0.035	0.064	0.069	0.149	0.033	0.076
	(0.008)	(0.020)	(0.008)	(0.039)	(0.011)	(0.026)
Mother's participation ×	-0.095	-0.171	-0.135	-0.262	-0.086	-0.197
after welfare reform	(0.015)	(0.045)	(0.014)	(0.065)	(0.016)	(0.047)
Instrumental variables	No	Yes	No	Yes	No	Yes
Survey weights	Yes	Yes	No	No	Yes	Yes
Inverse probability weight	No	No	Yes	Yes	Yes	Yes
Weak IV test statistic		22.436		17.934		18.059
p-value		0.000		0.000		0.000
Hansen J statistic		1.411		1.585		1.432
p-value		0.494		0.453		0.489
Number of daughters	2961	2961	1935	1935	1935	1935
Observations	56068	56068	41498	41498	41498	41498

Notes: Standard deviations are in parenthesis. Columns (1) and (2) present results using survey weights as in Table 2. Columns (3)-(6) present second-stage results based on the inverse probability weights constructed in a first stage as described by panel above.

S.7. Exposure Timing and Life-Cycle Windows

This section presents two extensions to the empirical analysis presented in our manuscript. We first investigate whether within-generation differences in age drive the baseline results. Lastly, we estimate intergenerational transmission effects by extending the minimum number of mother-daughter observation pairs in our sample.

In Table 4, we showed estimates of the baseline specifications of Table 2 restricted to the observation window of the daughter-as-adult through age 27 and the mother for different age ranges over the span from 25 to 45. By imposing this restriction, we ensured that within-generation differences in age do not drive the results. In this section, we extend the evidence presented in Table 4 by restricting the samples to the observation windows of the mother over ages 25 to 45 in eight different specifications (see Table S.7-1).

The interquartile range for mother's age during critical exposure is 36 to 45 in the full sample, 36 to 45.5 for pre-reform observations, and 35.5 to 43 for post-reform observations. We use the ages 25 to 45 for satisfying weak IV test requirements for this smaller sample, yet the estimates of the effect of reform on transmission are robust to the choice in mother's age range, as can be seen in Table S.7-1. The transmission effects are somewhat larger in magnitude compared to our baseline results, yet the percent reduction after reform is consistent around 50 percent in levels.

TABLE S.7-1. IV ESTIMATE ROBUSTNESS TO MOTHERS' AGES AROUND CRITICAL EXPOSURE FOR AFDC/TANF TRANSMISSION TO DAUGHTERS-AS-ADULTS THROUGH AGE 27

Mother's age range:	25-35	25-40	25-45	30-35	30-40	30-45	35-40	35-45
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	A. Ordinary least squares							
Mother's participation	0.178	0.170	0.184	0.181	0.174	0.188	0.211	0.212
	(0.032)	(0.023)	(0.020)	(0.038)	(0.024)	(0.021)	(0.026)	(0.022)
After welfare reform	0.041	0.056	0.064	0.035	0.051	0.061	0.058	0.061
	(0.022)	(0.019)	(0.020)	(0.023)	(0.021)	(0.021)	(0.023)	(0.022)
Mother's participation ×	-0.100	-0.106	-0.115	-0.090	-0.098	-0.113	-0.136	-0.127
after welfare reform	(0.040)	(0.033)	(0.033)	(0.053)	(0.037)	(0.037)	(0.041)	(0.041)
	B. Instrumental variables							
Mother's participation	0.312	0.429	0.444	0.336	0.457	0.471	0.436	0.489
	(0.099)	(0.128)	(0.107)	(0.109)	(0.139)	(0.115)	(0.127)	(0.122)
After welfare reform	0.048	0.108	0.118	0.045	0.106	0.119	0.081	0.104
	(0.033)	(0.039)	(0.042)	(0.033)	(0.043)	(0.044)	(0.042)	(0.044)
Mother's participation ×	-0.121	-0.215	-0.226	-0.126	-0.216	-0.235	-0.154	-0.219
after welfare reform	(0.102)	(0.088)	(0.105)	(0.115)	(0.098)	(0.110)	(0.140)	(0.142)
Weak IV test statistic	13.027	14.196	16.581	12.362	11.714	12.111	5.593	9.755
p-value	0.005	0.003	0.001	0.006	0.008	0.007	0.133	0.021
Hansen J statistic	1.742	0.777	1.649	1.934	0.728	1.495	1.042	0.903
p-value	0.419	0.678	0.438	0.380	0.695	0.474	0.594	0.637
Number of daughters	1384	1798	2086	1370	1793	2084	1745	2063
Observations	10433	13504	15718	10330	13461	15697	13123	15547

Notes: Robust standard errors with state clustering are shown in parentheses. All specifications control for state and year effects in addition to daughter's age, age squared, mother's average age during potential welfare observation years, mother's average age squared, indicators for number of children, daughter's state AFDC/TANF benefit standard, daughter's EITC federal/state maximum credit, state-level poverty rate, AFDC/TANF recipiency rate, and unemployment rate. Instrumental variables include average and maximum measures of the mother's AFDC/TANF benefit standard, 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.

In Table S.7-2, we examine the windows problem by extending the minimum requirement that the pairs be observed for at least ten and fifteen years, respectively. There we see that the reduction in the level of mother's transmission after welfare reform is at least as large as that reported in Table 2 of the manuscript (and reproduced in column (1) of Table S.7-2).

TABLE S.7-2. 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	0.253	0.394	0.326	0.551	0.256	0.423
	(0.050)	(0.084)	(0.073)	(0.132)	(0.069)	(0.121)
After welfare reform	0.064	0.076	0.099	0.143	0.076	0.100
	(0.020)	(0.032)	(0.031)	(0.050)	(0.027)	(0.045)
Mother's participation × after welfare reform	-0.171	-0.197	-0.267	-0.391	-0.170	-0.218
	(0.045)	(0.078)	(0.069)	(0.123)	(0.058)	(0.105)
Misclassification correction	No	Yes	No	Yes	No	Yes
Weak IV test statistic	22.436	21.070	18.609	16.199	15.702	16.334
p-value	0.000	0.000	0.000	0.001	0.001	0.001
Hansen J statistic	1.411	1.502	2.279	1.841	0.496	0.422
p-value	0.494	0.472	0.320	0.398	0.780	0.810
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 N_F, represents years when the mother is observed living with the daughter before her daughter has formed her own family unit (the baseline minimum restriction used throughout is $N_F \geq 5$). Robust standard errors with state clustering are shown in parentheses. All specifications control for state and year effects in addition to daughter's age, age squared, mother's average age during potential welfare observation years, mother's average age squared, indicators for number of children, daughter's state AFDC/TANF benefit standard, daughter's EITC federal/state maximum credit, state-level poverty rate, AFDC/TANF recipiency rate, and unemployment rate. Instrumental variables include average and maximum measures of the mother's AFDC/TANF benefit standard, 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. Daughters' PSID core longitudinal weights are used in estimation.

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