# Welfare Reform and the Intergenerational Transmission of Dependence

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## Abstract

We estimate the effect of welfare reform on the intergenerational transmission of AFDC/TANF participation 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 over time variation permits us to quasi-experimentally separate out the effect of mothers' participation on daughters' welfare choice in the pre- and post-welfare reform periods. In addition, our empirical framework addresses potential biases in estimating a causal pathway from parent to child from endogenous selection into welfare, life-cycle factors, and misclassification error. Our estimates show that there is a causal transmission of AFDC/TANF participation from mother to daughter, but that welfare reform significantly attenuated this transmission by at least 50 percent. The causal pathway is stronger among black families than whites, with selection accounting for more of the transmission across generations among whites. The estimates are robust across a variety of specifications, including the length of mother-daughter observation window, the age of welfare exposure by the daughter when living at home, life-cycle age adjustments, and misclassification error. However, when we broaden the definition of welfare received by the daughter to also include assistance from food stamps or disability (Supplemental Security Income) then the transmission from mother to daughter does not substantively change after welfare reform. This seems to be a consequence of persistence in intergenerational poverty status.

The transmission of government welfare across generations is of longstanding interest to both research and policy (Duncan, Hill, and Hoffman 1988; McLanahan 1988; Solon, et al. 1988; Gottschalk 1990, 1992, 1996; Levine and Zimmerman 1996; Borjas and Sueyoshi 1997; Pepper 2000; Page 2004; Dahl, Kostol, and Mogstad 2014). Indeed, 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; Niskanen 1996; 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. Instead, the parent-child link in welfare participation could simply be a spurious by-product of incomes that are correlated across generations. That is, low economic mobility across generations means that children of parents with low incomes likely have low incomes themselves in adulthood, and both generations participate in means-tested programs solely because of their shared poverty status and not welfare exposure per se. If true, then we would not expect generational welfare participation to fall after reform unless poverty among the young declined.

In this paper, we provide new estimates of the intergenerational transmission of welfare dependence. Documenting trends in welfare correlations across generations is of interest in its own right as a barometer of family economic status and it serves as a useful complement to the much more voluminous research on trends in income mobility that shows either no change in mobility or a decline over time (Mazumder 2005; Lee and Solon 2009; Black and Devereux 2011; Chetty, et al. 2014; Durlauf and Shaorshadze 2014). However, we aim to go a step further by identifying whether the channel of welfare use from parent to child was causally changed

after welfare reform by exploiting the quasi-experimental variation provided by the 1990s reforms to the Aid to Families with Dependent Children (AFDC) program in the United States. AFDC was established during the Great Depression and was the main cash transfer program for families with dependent children. Conditional on low income and assets, along with presence of children under age 18, eligibility for assistance was an entitlement. Starting in 1992, states began implementing substantive changes to their AFDC programs with waivers from federal rules, and by 1996, 43 states had implemented some form of waiver affecting program features such as new work requirements, time limits on length of receipt, and caps on benefit generosity. These waivers culminated with passage of the Personal Responsibility and Work Opportunity Reconciliation Act of 1996, which replaced AFDC with the non-entitlement federal block grant program Temporary Assistance for Needy Families (TANF). Scores of papers have been written evaluating welfare reform (see surveys in Blank 2002; Moffitt 2003; Ziliak 2016), but what is not known, and thus is the focus of this study, is whether it achieved a key aim of ending the transmission of welfare across generations.

To estimate the effect of welfare reform on the intergenerational transmission of AFDC/TANF participation, we assemble a long panel of mother-daughter pairs over the survey period 1968-2013 in the Panel Study of Income Dynamics (PSID). We focus on mother-daughter pairs because over 90 percent of AFDC cases were headed by a single mother, and there has been a large secular increase since the 1960s in the fraction of first births to unmarried women in the U.S. from fewer than 1 in 10 to over 4 in 10 such that more than one third of U.S. children were exposed to welfare by age 10 (Levine and Zimmerman 2005; Cancian and Reed 2009). Our empirical framework augments a canonical transmission model whereby the welfare participation of the daughter during adulthood is regressed on the prior welfare participation of

the mother with a difference-in-difference type specification that also includes 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. Because states implemented reforms at different times starting in 1992, the cross-state over time variation permits us to separate out the effect of mothers' participation in the pre- and post-welfare reform periods.

Even though welfare reform provides exogenous variation in access to program benefits across epochs, identifying whether there is a causal pathway from parent to child in welfare use *within* epochs is complicated by three—potentially reinforcing—forms of bias. First, selection bias in welfare participation across generations can arise through possible unobserved correlations in labor market productivity between the parent and child, perhaps because of latent shared cognitive or noncognitive skills, or shared tastes for welfare relative to work (Solon et al. 1988; Gottschalk 1992, 1996; Pepper 2000). Second, so-called life-cycle bias and the 'windows problem' may affect intergenerational correlations of economic status because we generally only observe snapshots of a parent and child and not the full life cycle (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 the intergenerational correlation depending on whether the window of parent-child observations is dominated by families in the midst of long-term welfare spells. The third threat to identification comes from potential misclassification bias in survey responses (Bollinger and David 1997, 2001; Hausman, et a. 1998; Kreider, et al. 2012, 2016; 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 (2015) document a trend increase in misreporting across all major household surveys in the U.S., including the PSID.

We address potential endogenous selection into welfare both by controlling for person-specific unobserved heterogeneity in the welfare participation decision and by instrumenting for mother's welfare use. If the daughter has a latent and time-invariant propensity to participate in welfare, conditional on measured demographics, and part of this time-invariant heterogeneity is shared with the mother, then including daughter fixed effects will control for this form of selection. However, because some selection may be time-varying, we also instrument mother's welfare participation with the state unemployment rate and state maximum AFDC/TANF benefit guarantee in years that overlap with the mother's welfare choice. Research on welfare caseloads has identified the macroeconomy as the primary driver of welfare participation, even after welfare reform (Blank 2002; Grogger and Karoly 2005; Ziliak 2016). Local unemployment rates measured coincident with the mother's participation and prior to the welfare decision of the daughter should only affect the daughter's choice via its effect on her mother's participation decision (Antel 1992; Moffitt 1992; Levine and Zimmerman 1996; Pepper 2000).

We attempt to mitigate the influence of the life-cycle windows problem by using the long time series now available in the PSID. We require the mother and daughter to live together at least 5 years during the critical exposure period of ages 12-18, and to observe the daughter for at least five years after she forms her own family unit (either living independently of her mother or as a related subfamily in the same household). However, we observe some mother-daughter pairs for upwards of 38 years, and on average we observe daughters for nearly 25 years as head of their family. AFDC/TANF participation tends to peak at least a decade earlier than income and earnings because of the child-present requirement, and thus we observe the full welfare lifecycle for many mother-daughter pairs. 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. Finally, we explore the implications of misclassification bias by parametric methods using "extra-sample" information on reporting rates in the PSID (Meyer, et al. 2015).

Our estimates show that there is a causal transmission of AFDC/TANF participation from mother to daughter, but that welfare reform significantly attenuated that pathway by at least 50 percent. The transmission pathway is stronger among black families than whites, with selection accounting for more of the transmission across generations among whites. The estimates are robust across a variety of specifications, including the length of mother-daughter observation window, the age of welfare exposure by the daughter when living at home, life-cycle age adjustments, and misclassification error. However, when we broaden the definition of welfare received by the daughter to also include assistance from food stamps or disability (Supplemental Security Income), then the reduced transmission from mother to daughter after welfare reform is much more modest, likely owing to the persistence in intergenerational poverty status.

## II. Welfare Reform and Intergenerational Transmission

"Welfare" in the U.S. through the 1980s was largely defined by the Aid to Families with Dependent Children program (AFDC), which was established as part of the Social Security Act of 1935 to assist low-income families with children under age 18. Initially, assistance was restricted to the children of destitute widows and widowers, and then later was expanded to cover the guardian of the child, and eventually a second parent if present in the household. In well over 90 percent of the cases, the family was headed by a single mother. Eligibility for assistance (conditional on the presence of a dependent child under age 18) was determined by an income test, a liquid asset test, and a vehicle asset test. The federal government set rules on what counted as income or an asset, and also established limits on the dollar value of those resources. States

did have authority to set maximum benefit levels (which increased with family size) and so-called need standards used in assigning income eligibility. The program was an entitlement funded by a federal-state matching grant based on state per-capita income, with the federal government picking up over 60 percent of expenditure on average (Ziliak 2016).

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

The state-level waivers were codified into federal law with passage of the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) in August of 1996. PRWORA replaced AFDC with a new program called Temporary Assistance for Needy Families (TANF), which is not an entitlement. The new law established federal maximum guidelines

regarding funding, work requirements and time limits, but otherwise devolved much more program design authority to the states. For example, the federal lifetime time limit for benefits for an adult is five years, but nearly half the states opted to impose shorter limits. Nineteen states now require some form of mandatory job search at the point of benefit application, and in fourteen of those states the sanction for noncompliance is to deny the application. Seventeen states have opted to impose a family cap on benefit generosity, and thirty-two states introduced "diversion payments" that steer eligible applicants away from the official caseload and instead toward a lump-sum payment, typically valued at three months of the maximum benefit for a given family size (Ziliak 2016).

[Figure 1 here]

Figure 1 depicts trends in the number of persons on welfare, and on the right axis the corresponding expenditure in real 2012 dollars (based on the personal consumption expenditure deflator). The graph spans the AFDC program (1960-1991), the major waiver period (1992-1996), 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 (Council of Economic Advisers 1997; Ziliak et al. 2000; Blank 2001; 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; Haider and Klerman 2005; Frogner, Moffitt, and Ribar 2009).

### A. Intergenerational Transmission Pre- and Post-Reform

Contemporary empirical studies on intergenerational socioeconomic outcomes trace their intellectual foundation to the work of Becker and Tomes (1979, 1986), who provide a structural framework of dynastic family decision making. The corresponding canonical statistical model involves regressing the outcome of interest of the child on the corresponding outcome of the parent, whether it is earnings, education, health, income, wealth, or in our case, welfare (see surveys in Solon (1999) and Black and Devereux (2011)). The prima facie evidence in Figure 1 suggests a structural break in welfare participation starting during the welfare waiver era, and a straightforward modification to the canonical model of the intergenerational transmission of welfare before and after welfare reform as

$$W_{ist}^{d} = \alpha + \beta' \boldsymbol{x}_{ist}^{d} + \delta W_{ist}^{m} + \gamma P_{ist}^{m} + \theta P_{ist}^{m} W_{ist}^{m} + \varepsilon_{ist}^{d}, \qquad (1)$$

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* in time period *t* participates in welfare and 0 otherwise, $W_{ist}^{m}$ is a welfare indicator for the mother (*m*), $\boldsymbol{x}_{ist}^{d}$ is a set of observed demographic characteristics of the daughter, $P_{ist}^{m}$ 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 $\varepsilon_{ist}^{d}$ is an unobserved error term.

In the model of equation (1), $\delta$ is the intergenerational correlation of welfare participation, and $\delta + \theta$ is the correlation after welfare reform. This specification is akin to a difference-in-difference model whereby we exploit the quasi-experimental variation induced by

the fact that different states adopted welfare reform at different times starting in the early 1990s. That is, the indicator $P_{ist}^{m}$ "turns on" when the 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 who were early adopters of reform. This has been a standard assumption in the welfare reform literature, though in some cases researchers allow a trend break between the waiver era and TANF era (Blank 2002). If welfare reform succeeded in reducing the transmission across generations then we expect that $\theta < 0$.

### B. Identifying a Causal Pathway across Generations

A ubiquitous challenge across the intergenerational transmission literature has been establishing a causal pathway from parent to child because of potential biases from endogenous selection, life cycle factors, and measurement error. We discuss each of these threats to identification and how we propose to address them in our model.

*Selection Bias*

Estimation of equation (1) by least squares is the linear probability model, and under the conditional mean independence assumption that $E\left[\varepsilon_{ist}^{d} \middle| \boldsymbol{x}_{ist}^{d}, W_{ist}^{m}, P_{ist}^{m}\right] = 0$ yields a consistent causal estimate of the intergenerational parameters $\delta$ and $\theta$. The conditional mean independence assumption will break down, however, if there are unobservables common to the mother and daughter that affect the decision to participate. That is, if we backdate equation (1) by a generation 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. the grandmother, $W_{is,-t}^{g}$), there will also be an error term, $\varepsilon_{is,-t}^{m}$. If $E[\varepsilon_{ist}^{d}\varepsilon_{is,-t}^{m}|\boldsymbol{x}_{ist}^{d}, \boldsymbol{x}_{is,-t}^{m}] \neq 0$, then estimation of the linear probability model by least squares will yield biased estimates. In this case separating out the "welfare trap" (state

dependence) from “poverty trap” (unobserved heterogeneity) becomes a first order concern for causal identification. The quasi-experimental design of using cross-state over time variation in adoption of welfare reform allows us to separate the pre- versus post-reform eras, but within epochs there 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) is an early study, whereby they used pairs of sisters in order to control for shared family background (i.e. family fixed effects) in identifying the effect of parental welfare participation. Antel (1992) adopted a Heckman (1978) dummy endogenous variable model within the context of a two-limit tobit specification (the dependent variable was the number of months a daughter was on welfare). While the model is identified off of functional form, he also included exclusion restrictions in the mother’s reduced form equation such as the state’s monthly AFDC benefit guarantee and local labor market conditions as proxied by net migration flows. In lieu of exclusion restrictions, Gottschalk (1996) addressed unobserved heterogeneity by modeling the event histories of daughter’s and mother’s welfare usage, in addition to the time-path histories for covariates, in order to identify causal effects relative to a mother’s past participation. Levine and Zimmerman (1996), on the other hand, used mother’s background as additional control variates, as well as state (e.g. welfare generosity) and local (e.g. county unemployment rate) variables as instruments for mother’s welfare participation. Dahl, et al. (2014), who examined disability insurance in Norway and not welfare per se, used the random assignment of appellate-court judges as an instrumental variable to identify parent’s disability participation on child’s disability insurance claims. Pepper (2000) eschewed point identification methods of the latter authors in favor of nonparametric bounding techniques to control for selection as proposed by Manski (1995). To refine the bounds, he

imposed tenable assumptions such as monotone treatment selection (daughter's participation is nondecreasing in her exposure as a child) and monotone instrumental variables (mother's participation is nondecreasing in the local unemployment rate). Antel, Gottschalk, Pepper, and Dahl, et al. all conclude that parent's participation in welfare is causal for the child and not spurious, while Solon, et al. and Levine and Zimmerman provide evidence more in favor of welfare correlations as a poverty trap.[1]

Our approach to address possible endogenous selection is to extend the prior point identification literature by exploiting the comparatively long time histories now available in the PSID and estimating equation (1) via panel-data instrumental variables. Specifically, we admit error components into the model consisting of latent person-specific and time-invariant heterogeneity, time-invariant unobserved state heterogeneity, and common but time-varying heterogeneity as

$$\varepsilon_{ist}^{d} = \lambda_{i}^{d} + \mu_{s}^{d} + \rho_{t}^{d} + \nu_{ist}^{d}, \qquad (2)$$

where $\lambda_i^d$ is a time-invariant daughter fixed effect, $\mu_s^d$ is a time-invariant state fixed effect, $\rho_t^d$is a common year effect, and $\nu_{ist}^d$ 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, attitudes), as well as that which is daughter specific such as school quality and neighborhood. The state effect controls for permanent differences in states such as natural endowments that affect economic opportunities, while the time effect controls for macroeconomic and policy changes that affect all daughters the same in a given year.

Identification comes from the fact that welfare participation of the daughter and mother at a given age can change over time. That is, we arrange our data in terms of family, state, year, and

[1] Gottschalk's (1996) evidence in favor of causation is strongest for white families, while for black families a considerable fraction of the intergenerational link is spurious, though still causal.

age. For example, $W^{d}_{is1990}(25)$ takes a value of 1 or 0 depending on whether the daughter in family *i* and state *s* in the year 1990 who is age 25 participates in welfare or not, and $W^{m}_{is1990}$ takes a value of 1 if the mother of the 25 year old daughter has ever been on welfare *prior to* 1990 and 0 otherwise. The dependent variable can change between 0 and 1 as the daughter moves off and on welfare as she ages, while the mother's value remains 0 if she never joins welfare, but switches permanently to a value of 1 once the mother ever participates prior to a given age-year (whether while the daughter is a child living at home, or as an adult living on her own). Because we will control for daughter's age in the demographic characteristics, $\boldsymbol{x}^{d}_{ist}$, we face the common identification problem that it is not possible to separate out age, cohort, and year effects, and thus the year dummies are a convolution of both cohort and time effects, but for simplicity we just refer to time.

Standard panel data estimators provide consistent estimates of $\hat{\delta}$ and $\hat{\theta}$ if the only source of family selection is time invariant (controlling for state and time effects). However, this assumption is likely to be overly restrictive as it precludes any time-varying selection into welfare by the mother that may also be correlated with the daughter's structural error term; that is, $E[\nu^{d}_{ist} W^{m}_{ist} | \lambda^{d}_{i}, \mu^{d}_{s}, \rho^{d}_{t}] \neq 0$. Thus, we also instrument for mother's previous welfare participation using state labor market conditions and welfare program policies in effect while the daughter lives with the mother. These aggregate labor market and welfare policies while the daughter is young and living at home 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 describe these instruments in more detail below in the data section, and test both the first-stage strength and the validity of overidentifying restrictions in the results section.

*Life-Cycle Bias*

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 cycle. In the intergenerational income mobility literature this has come to be known as life-cycle bias (Jenkins 1987; Haider and Solon 2006; Grawe 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 presence of young children high, and participation is low when older (for the opposite reason of the young), again leading to attenuation in the generational correlation.

A related measurement issue, frequently referred to as the "windows 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 windows 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 the correlation if true participation is omitted, yet it could also lead to overestimation if long-term spells are overrepresented in the short window and long-term exposure matters more for transmitting dependency.

In the income mobility literature, Lee and Solon (2009) propose ameliorating the life-

cycle bias by including controls for parent's age, normalized child's age, and interactions between child's normalized age and parent's income. The normalized child's age is measured as the deviation around age 40, which is the point in the life cycle when annual income serves as a reasonable proxy for lifetime income. The window problem is then addressed by taking three- or five-year averages of parent's income (Solon 1992; Zimmerman 1992; Mazumder 2005). Gottschalk (1996) attempted to address the life-cycle problem by including a long event history of mother's AFDC participation, but only had a 6-year observation window for daughters (though this was a significant improvement over prior research). Page (2004) experimented with various window lengths and found that previous estimates of welfare transmission were downward biased because the daughters were observed for shorter windows earlier in their life cycle, and in particular, that length of daughter's window mattered more than length of mother's window (but both mattered). Nybom and Stuhler (2016) find a similar result for income, even after making the Lee and Solon adjustment.

Our solution to the life-cycle bias and window problem is to utilize the much longer time series now available in the PSID compared to prior studies. For each mother-daughter pair, we observe the daughter as head/spouse of her own family unit for 25 years on average and for as long as 38 years. In addition, we observe the mother and daughter co-residing for 14 years on average with at least 5 years during the daughter's ages 12-18 when the potential for welfare learning is heightened. Thus, we come much closer to approximating the life cycle of welfare participation, especially given the requirement of dependent children under age 18 and the fact that fertility of low-income mothers tends to peak in their early to mid 20s (Lopoo 2007). However, as a sensitivity check on the estimates of intergenerational transmission in the baseline specification, we also present estimates that incorporate the Lee and Solon (2009) age

adjustment, along with alternative observation windows that differentiate critical periods of welfare exposure.

*Misclassification Bias*

Another potential source of measurement error is misreporting by the survey respondent. Misclassification bias is present both at the extensive participation margin and the intensive dollar-reporting margin, pervades all social surveys, and has gotten worse over time (Meyer, Mok, and Sullivan 2015). 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 2001; Meyer, Goerge, and Mittag 2014; Meyer and Mittag 2015).

Returning to Figure 1, the right hand axis shows that real spending closely tracked recipiency until 1997, after which it diverged significantly. The latter stems from the fact that under PRWORA states are only required to report the number of persons receiving cash assistance, not in-kind help such as a child care subsidy, job training, or transportation voucher, but they are obligated to report all forms of spending. During AFDC about 70 percent of all spending was in the form of cash benefits, and the remainder was in kind. Under TANF, it is just the opposite—70 percent of spending is in kind and 30 percent is cash. This means the number of TANF recipients in Figure 1 is understated because it excludes those receiving in-kind help. Whether and to what extent this affects measurement of participation in household surveys like the PSID is not known. The main question in the PSID on TANF asks whether the family head (wife, and other members) received income from TANF in the prior year, and if so how much. If

the respondent received in-kind support, they may or may not respond in the affirmative. The PSID also has a secondary follow-up question asking of receipt of any other income from TANF, but again, whether the respondent includes in-kind assistance in answering the question is unknown. This ambiguity, coupled with evidence of underreporting of TANF, requires that our empirical framework control for possible misclassification bias.

Remedies for classification bias are not straightforward in the context of dichotomous variables. A standard approach for continuous variables in the intergenerational income literature with classical measurement error is to take 3- or 5-year averages of parent's (and possibly child's) income (Solon 1992; Mazumder 2005). While such averages are likely to improve things in dichotomous participation models, this is not ensured as the errors have been found to vary systematically with characteristics and are nonclassical. Some have proposed parametric or semiparametric adjustments to the likelihood function to incorporate misclassification (Bollinger and David 1997, 2001; Hausman et al. 1998; Meyer and Mittag 2014; Nguimkeu et al. 2015), while others have proposed partial-identification nonparametric bounding techniques (Bollinger 1998; Black et al. 2000; Molinari 2008; Kreider, et al. 2012, 2016). These solutions have been proposed for cross-sectional data either for measurement error in the dichotomous dependent variable, or the dichotomous independent variable, though in our case we have potentially mismeasured dichotomous variables on both the left- and right-hand sides of the equation, as well as panel data.

We consider several potential remedies for misclassification bias. First, evidence in Bollinger and David (2005) showed that respondents have a latent propensity to report or not report, and that cooperation increases with length of panel participation. This suggests that our control for daughter fixed effects should sweep out much of the person-specific propensity to

report or not. In addition, since we follow mothers for at least 14 years on average and daughters for 25 years, correct reporting should be more prevalent than in a sample with short observation windows. Second, for right-hand side mismeasurement of mother's participation, again recall that we measure if she *ever* participates, which is likely to be less noisy than contemporaneous participation. Moreover, the instrumental variables discussed in the prior section on selection bias are also likely to improve matters for misreports of mother's participation. Third, for left-hand side classification error, we consider parametric bias-corrections along the lines proposed in Bollinger and David (1997, 2001) and Hausman et al. (1998). Specifically, we follow Hausman et al. (1998) and assume that misreporting is independent of model covariates and constant across individuals, which implies that the partial effect of mother's participation on daughter's participation in equation (1) from observed data are proportional to the true partial effects,

$$\Pr\left(W_{ist}^{d}=1\middle|W_{ist}^{m}=1,\ \bullet\right)-\Pr\left(W_{ist}^{d}=1\middle|W_{ist}^{m}=0,\ \bullet\right)=(1-\tau_0-\tau_1)(\delta+\theta P_{ist}^{m}), \quad (3)$$

where • represents other controls, $\tau_0$ is the false positive reporting rate and $\tau_1$ is the false negative reporting rate. 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 $(1-\hat{\tau}_1)$, where $\hat{\tau}_1$ is based on estimates of reporting rates in the PSID by Meyer, Mok, and Sullivan (2015) as depicted in Appendix Table 1.[2] We treat the $\hat{\tau}_1$ as known data in estimation. For completeness, we also estimate a bias-corrected version of the probit maximum likelihood estimator proposed by Hausman, et al. (1998), again treating the false negatives as known data in

[2] Meyer, et al. (2015) only provide estimates of misreporting for participation rates in AFDC/TANF in the PSID for a limited number of years, but a much more complete time series for misreports of dollar amounts. In the years of overlap, misreporting of dollar amounts always exceeds that of participation. For the years where we have misreporting of dollars but not participation, we inflate the former by the median ratio of dollars to cases to fill in for the missing participation reporting rates. In years where we are missing both dollars and cases, we linearly interpolate between observed years. As seen in the last column of Appendix Table 1 there is strong evidence of a secular increase in misreporting.

estimation.[3]

*Multiple Program Participation*

A final specification issue pertains to the definition of what constitutes "welfare" and "dependence." That is, families that received AFDC were categorically eligible for food assistance from the Food Stamp Program. 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 (Green Book 1994). This categorical eligibility remained after the introduction of TANF, and also after the change in program name of food stamps to the Supplemental Nutrition Assistance Program (SNAP) in 2008. Also, while any given individual on AFDC could not simultaneously receive assistance from the disability program Supplemental Security Income (SSI), it was possible for families to combine benefits, with some on AFDC and some on SSI (and still also qualify for food stamps). These provisions remain after welfare reform.

[Figure 2 here]

Figure 2 presents trends in the number of recipients in food stamps and SSI for selected years. There was a marked drop in food stamp participation in the immediate aftermath of welfare reform, followed by a huge expansion in the subsequent decade. These swings have been attributed to changes in the macroeconomy, welfare and food stamp policies, and in program take-up rates among those eligible (Ganong and Leibman 2014; Ziliak 2015). There has also been growth in SSI, especially after 1990 when the Supreme Court's *Zebley Decision* expanded eligibility for children (Kubik 1999), and again after welfare reform, where there is some evidence that states systematically facilitated the applications of former AFDC recipients for SSI program benefits (Schmidt and Sevak 2004). The implication then is that even if welfare reform

[3] We implement the probit model using the MRPROBIT command in Stata written by Nikolas Mittag (see Meyer and Mittag 2014).

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. Thus, we first estimate our model of mother's AFDC/TANF transmission to daughter's AFDC/TANF participation, and then to daughter's AFDC/TANF, food stamps/SNAP, and/or SSI.[4]

## III. 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. This 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, et al. 1998; Fitzgerald 2011). The original sample drew about 60 percent of the families from the nationally representative Survey Research Center (SRC), and the other 40 percent from an oversample of low-income 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 1968-2013, and in order to ensure adequate sample sizes we include observations from both the SRC and SEO subsamples. However, our results below are robust to restricting mother-daughter pairs from the SRC sample alone.[5]

In an effort to address the issues of life cycle bias and the windows problem, our baseline sample consists of mother-daughter pairs that are observed living in the same household for at least five years while the daughter is in the critical exposure period spanning the ages of 12-18,

[4] The prior literature generally only provided estimates of AFDC with General Assistance (e.g. Gottschalk 1996), 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; Page 2004).

[5] Results available upon request from the authors

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 pervades the welfare transmission literature (Solon, et al. 1988; Duncan and Yeung 1995; 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 pairs because of attrition. The other reason for focusing on adolescence 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 five years observed during the ages 12-18 as a key period of welfare exposure. A daughter is considered an adult at first childbirth or when establishing a new family unit if she is at least age 14, though she may continue to live at home as a subfamily. This yields a baseline sample of 2,967 mother-daughter pairs spanning 56,206 observation years of the daughter as an adult. On average, we observe mothers and daughters co-residing during the ages of 12-18 for 6.3 years, and we observe daughters as adults for 25 years and mother-daughter pairs for 14 years on average. In the results section, we report estimates from specifications that both loosen and tighten the observation windows for both mothers and daughters.

[Table 1 here]

Table 1 contains the key variables from the baseline sample used in estimation of equation (1), separated into the pre- and post-welfare reform eras, weighted by the daughter's core longitudinal weight that is appropriate for combined SRC-SEO subsamples of the PSID. The dependent variable takes on a value of 0 or 1 based on whether the daughter participates in welfare at any time during a year after age 13 and when she has formed her own family unit. We consider two definitions of welfare, one which captures participation in AFDC/TANF or "other welfare" such as General Assistance (which we simply refer to AFDC/TANF for succinctness), and a second which captures participation in any of AFDC/TANF, food stamps/SNAP, or SSI.[6] This variable varies over time because of possible movements on and off welfare across her life course. While 4.4 percent of daughters receive AFDC/TANF as adults in an average year over the sample period, as seen in Table 1 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 programs, with 13.2 percent receiving AFDC/TANF, food stamps/SNAP, or SSI before reform and 11.2 percent afterwards. Almost all the additional uptake in welfare use is from food stamps/SNAP.

The key independent variable is mother's welfare participation found in the bottom panel of Table 1, which takes a value of 1 if the mother *ever* participates in welfare at any time *prior* to a given age-year of her daughter, and 0 otherwise. Once the mother participates, the 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

[6] The PSID asks of AFDC/TANF receipt of the family head and spouse, and other family members, as well as an "other welfare" category (not including SSI, food stamps, workers comp, housing, Social Security, etc…). This other welfare category can contain assistance from various public sources including General Assistance.

captures a longer window and thus attenuates potential measurement error. Table 1 shows that about 27 percent of mothers were ever-on AFDC/TANF prior to welfare reform, and 7 percent afterwards, while those figures jump to 43 and 19 percent, respectively, if the mother ever received AFDC/TANF, food stamps/SNAP, or SSI. In estimation, we restrict our measure of mother's welfare use to be AFDC/TANF (or General Assistance), and not food stamps/SNAP or SSI. The reason for this is that even though welfare reform did impose some direct changes on food stamps and SSI, the primary focus was on AFDC and thus we want to test whether changes to AFDC had spillover effects on the daughter's wider welfare participation beyond that program. Note, however, that it is possible for the mother to first participate on welfare after the daughter forms her own family unit. 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 support while the daughter resides in the household with her mother, or from indirect "word of mouth" once the daughter forms her own family unit. In the sensitivity section we estimate the model where we restrict exposure while mother and daughter co-reside.

The other focal regressor in equation (1) is the indicator for welfare reform. As discussed previously, states began reforming AFDC in earnest starting in 1992, four years prior to passage of PRWORA. States had to submit requests for waivers from Federal rules to the U.S. Department of Health and Human Services, e.g. to introduce a time limit on benefits or to expand asset limits for eligibility. If the waiver was approved, then there was generally a lag between the time of approval and when the policy was implemented. Indeed, some approved waivers never were implemented (Grogger and Karoly 2005). 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. We achieve identification because of cross-state variation over time in the welfare reform indicator. As seen in Table 1, for daughters 65 percent of person-years occur after welfare reform is implemented, while for mothers it is just about 14 percent.

Table 1 also contains demographic characteristics of the daughter and mother, including age, race, and number of children, as well as business-cycle conditions used in identifying the effect of mother's welfare participation. Daughters are 28 years old on average before reform and 39 after reform, while mothers are 43 and 59 years old, respectively, highlighting the long observation windows we observe families compared to prior research. Based on the welfare caseload research, the historical driver of participation was the state of the macroeconomy. Thus, we use two measures of business cycle conditions: the average state unemployment rate facing the mother in years when her welfare choice is observed prior to the current year, as well as the maximum of those state rates. Both instruments vary across states, and they also vary over time because of the rolling observation window of the mother-daughter pair. We use both the average and the maximum unemployment rates to capture "typical" business-cycle conditions as well as statewide shocks. In addition, the welfare participation literature generally includes the maximum benefit guarantee as a measure of "price" in the demand for welfare (Moffitt 1992). We thus also include the prior average and maximum AFDC/TANF benefit facing the mother as additional instruments. This measure of benefit generosity varies by family size, but due to data limitations we assume that the maximum benefit for a 4-person unit applies to any household with four or more members.

## IV. Results

### A. Graphical Evidence

We begin by presenting graphical evidence on changes in the intergenerational

transmission of welfare over time. Specifically, across rolling cohorts of mother-daughter pairs in each year we estimate

$$W_{it}^{d} = \alpha_t + \delta_t W_{it}^{m} + \epsilon_{it}^{d} \quad (4)$$

where $W_{it}^{d}$ and $W_{it}^{m}$ are the daughter's and mother's welfare indicator, respectively, and $\delta_t$ is the year-specific intergenerational correlation in welfare use. No attempt is made in equation (4) to separate out cause and effect, only correlation over time in order to anchor our estimates to those in the literature as summarized in Page (2004).[7]

[Figures 3 and 4 here]

Figure 3 presents trends in the intergenerational correlation when we impose no restriction on the minimum number of observations that a mother and daughter live together, and Figure 4 presents the same trends but now with a five-year minimum number of observations and thus the later start date. Figure 3 shows that the intergenerational correlation in welfare increased throughout the two decades leading up to passage of PRWORA, and indeed did not peak until 1998 when the correlation of 0.40 was more than double that of the late 1970s. The correlation between mothers and daughters AFDC/TANF use then fell precipitously afterwards to levels comparable to those in the early 1980s. However, if we expand the definition of daughter's welfare to include food stamps/SNAP or SSI (mother's welfare remains defined by AFDC/TANF use), then we see a very different pattern. The intergenerational correlation is relatively constant after welfare reform. Figure 4 repeats the exercise but with five-year minimum observation windows, and while stability in welfare correlations is in evidence for both

[7] Also, in order to make our estimates comparable to Page (2004) we redefine our sample and measures of welfare participation for the purposes of Figures 3 and 4. In each year *t* our sample consists of daughters ages 27-42 years old who are the heads of their family unit (as before subfamilies are allowed) and the dependent variable measures welfare use on or after age 14 and before age 27. The independent variable measures mother's welfare use prior to the daughter's matriculation to family headship. The regression model in equation (4) is weighted by daughter's sample weight as of age 27.

measures of daughter's welfare, like Figure 3, we observe a separation after welfare reform between the correlation trends depending on whether or not we include food stamps/SNAP and SSI. The descriptive evidence thus points to the possibility that welfare reform succeeded in reducing the transmission of AFDC/TANF use across generations, but dependence more broadly defined has not changed.

### B. Selection Effects

Table 2 contains the baseline estimates of the model in equation (1). As per the error components in equation (2), each specification in the table includes dummy variables for state of residence and year.[8] However, as one moves across columns, we demonstrate how the estimates change with and without daughter fixed effects and instruments for mother's participation, as well as time-varying demographic controls of the daughter including a quadratic in her age and indicators for the number of children in her home. For the sake of parsimony we focus our discussion on columns (7)-(10) that contain demographic controls of the daughter and separate effects of mother's participation pre- and post-reform. The standard errors are robust to heteroskedasticity and clustered for within-group correlation by state of residence.

[Table 2 here]

The baseline correlation of the effect of mother's AFDC participation prior to welfare reform in column (7) is 0.20, which is well within the range of estimates among studies from that era surveyed in Page (2004).[9] That correlation falls 79 percent after welfare reform to 0.043 (=0.201-0.158). Controlling for unobserved heterogeneity in the daughter's welfare choice in

[8]. For the estimation sample, approximately 72 percent of daughters reside in their state of birth during adulthood, 63 percent live in the same state as their mother, and 57 percent never change states during any previous observations (based on weighted means by daughter's PSID core longitudinal weight).

[9] Note that this estimate is lower than a simple average of the trend estimates in Figure 4 because the samples differ, with the figure depicting if the daughter is ever on welfare before age 27, while the sample used in estimating equation (1) is for any contemporaneous welfare use after forming a family unit, regardless of daughter age.

column (8) of Table 2 shows that time-invariant selection accounts for just over 40 percent of the intergenerational correlation in the pre-welfare reform period (0.117 compared to 0.201), and after welfare reform the transmission effect is eliminated altogether. Column (9) presents estimates from the more common approach in the welfare transmission literature of controlling for selection via instrumental variables. Both the direct effect of mothers participation and the interaction with welfare reform are instrumented (the interaction is identified by interacting the welfare reform indicator with the four instruments of average and maximum state unemployment rates and AFDC/TANF benefit guarantee). In this case, the pre-reform effect of mothers participation is about twice as large, but so is the post-reform interaction such that transmission mechanism is attenuated by 75 percent to 0.097 (=0.398-0.301). The null hypothesis of weak instruments is rejected, but the null of valid overidentifying restriction is not, suggesting our IV estimates are consistent. Finally, in column (10) we admit daughter fixed effects and also instrument mother's participation with the four state-by-year measures of the business cycle and welfare generosity. The pre-reform effect is weakly negative but statistically zero, while the post-reform interaction term remains economically and statistically significant. There is a substantial loss of efficiency in the pre-reform period with both fixed effects and instruments, and there is less evidence against the null that the instruments are weak. In Appendix Table 2 we present a series of alternative fixed-effects IV estimates where we explore various combinations of instruments (specification (6) coincides with specification (10) of Table 2). There it is apparent that as we overidentify, especially in the post-reform era, the main transmission coefficient diminishes. Even still, the pattern of a substantive decline in AFDC/TANF transmission after welfare reform persists.

[Figure 5]

In Figure 5 we use the IV specification from column (9) to ask whether the change in the effect of mother's AFDC/TANF participation was a short-run impact or one that persisted over time. Instead of the fraction of the year that welfare reform was in place in a given year, we now create a running variable akin to an event-study reflecting three years before implementation of reform up to nine years after implementation. Examining the pre-reform period serves as a falsification test on our model to make sure there were no significant pre-existing trends. The figure reports the coefficient on the interaction term, along with a 95 percent confidence interval testing the null that the interaction is zero. There we see that there were no substantive trends of mother's AFDC participation on the daughter's AFDC use prior to welfare reform, but there is a substantively and statistically significant decline upon implementation of reform in the state, and it persisted.

### C. 'Windows', Life-Cycle Bias, and Critical Exposure

We next examine whether and to what extent the baseline estimates are susceptible to life-cycle bias. We argued that our estimates were less vulnerable to this form of bias because we observe the typical daughter for two and a half decades after forming her own family unit, and that we impose the requirement that mothers and daughters co-reside at least five years while the daughter is aged 12 to 18. In Table 3 we examine the windows problem by both eliminating minimum restrictions and extending the minimum requirement that the pairs be observed for ten and fifteen years, respectively. As the bottom row of Table 3 indicates this comes at the cost of fewer years that the daughter is observed as an adult head, though it is still a robust 20 year period as an adult. We focus on models that parallel columns (7)-(9) in Table 2, i.e. without and with daughter fixed effects and then IV. Comparing the corresponding columns across Table 3 shows remarkable stability in the coefficient estimates both pre- and post-reform, suggesting that

the main qualitative result of a post-reform decline in welfare transmission holds.

[Table 3 here]

Next, we address potential life-cycle bias by following the method suggested in Lee and Solon (2009). In the first column of Table 4 we repeat the baseline linear probability model from column (7) of Table, and then in the remaining three columns we augment the model with Lee-Solon-type age adjustments by successively including a quartic in the average age of the mother, and quartic in the detrended daughter's age, and the interaction between mother's participation and the detrended quartic in daughter's age. Because fertility rates among low-income women peak in their mid 20s, we detrend around daughter's age of 25. We focus on the linear probability estimates in order to abstract from any convoluting effects of instruments and latent heterogeneity. The estimates in column (4) of Table 4 show that after age-adjustment, the qualitative story remains though the quantitative effect is attenuated. That is, the pre-reform effect of mother's participation is about 10 percent higher than in column (1), and the post-reform effect is about 20 percent smaller (in absolute), so that welfare reform lowers the transmission by about 50 percent.

[Table 4 here]

In our baseline models, we require mothers and daughters to co-reside at least five years during the ages of 12-18. As discussed in the Data Section, this age range was selected in part from convention in the literature, but there was little prior evidence on whether "age of exposure" mattered for welfare learning. In Figure 6, we present the first tests of age at critical exposure windows by using rolling five-year (left panel) and ten-year (right panel) windows from the first year of life through age 17. The figure presents IV estimates of the pre-welfare reform effect of mothers AFDC participation, the "after reform" indicator, the interaction

between mothers participation and reform, and the sum of the direct effect and interaction. The figure shows that the magnitude of the direct effect of the mother's participation increases as the age of first exposure increases, suggesting that the learning effect is stronger during adolescence and teen years relative to early childhood.

[Figure 6 here]

### D. Sensitivity Analyses

We next explore the sensitivity of our estimates to a variety of alternative specifications, including misclassification bias, racial differences, geographic mobility, welfare reform aggressiveness, and the definition of welfare.

*Misclassification*

Table 5 contains the estimates of the baseline linear probability model with and without bias corrections as described in equation (3), as well as the corresponding probit model. The linear probability estimates in columns (4)-(6) are generally at least double in absolute value compared to the corresponding estimates in columns (1)-(3), but there is little change in the pre/post-reform story. This is not surprising because the average rate of false positives in Appendix Table 1 is 41 percent, and even higher if we restrict attention to after 1985 where most of the data lie, and the bias-correction compresses the variable by one minus the reporting rate, and thus inflating the coefficient. Qualitatively we see a similar result with the probit model, but the marginal effects in the bottom panel increase a more modest amount compared to the linear probability as one might expect (Meyer and Mittag 2014). We note that the bias-corrected linear probability estimates are larger than the IV estimates in Table 2 and are likely to be upper-bounds because the estimates of false positives from Meyer et al. (2015) come from annual cross sections of the PSID but our sample consists of a long panel of stayers who tend to be more

accurate in reporting (Bollinger and David 2005).

[Table 5 here]

*Black-White Differences*

There is a vast literature on the socioeconomic differences between blacks and whites (see, for example, Smith and Welch 1989; Duncan and Hoffman 1990; 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 6 here]

Table 6 presents estimates for the transmission of AFDC/TANF from mother to daughter separated by blacks and whites. Specifically, we append the model with a dummy variable for whether the daughter is black and interact that with both mother's participation and welfare reform. As before, all models control for state and year effects, as well as a quadratic in age and indicators for the number of children in the daughter's family. The first two columns without and with controls for unobserved heterogeneity, as well as the IV estimates in column (3), suggest that the direct effect of welfare transmission was much stronger among blacks than whites. This is similar to that found in Gottschalk (1996). What differs from Gottschalk, however, is that we find more evidence that selection effects are stronger among whites than blacks. Indeed, the fixed effects estimate for whites is half that of the model without fixed effects, suggesting a strong degree of selection. In all three specifications, welfare reform eliminates the transmission mechanism among whites, and reduces it by at least 50 percent among blacks.

*Cross-State Mobility*

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 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; Kennan and Walker 2010). However, when effects are found they are very small in magnitude. Moreover, Ziliak, et al. (2000) show that states decisions to adopt waivers were not an endogenous response to the welfare caseload. However, as a test on our model we consider three alternatives—restricting the sample of daughters to those who reside in the same state as their birth state, restricting the sample of daughters residing in the same state as their mothers, and restricting the sample of daughters to those who never move as adults.

[Table 7]

Table 7 contains four specifications for each of the three sample splits, without and with fixed effects and without and with instrumental variables. Comparing the baseline estimates in columns (7)-(9) in Table 2 to the corresponding columns (1)-(3), (5)-(7), and (9)-(11) in Table 7, we see that both the direct effect of mothers participation and the interaction with welfare reform are larger in absolute value in Table 7 compared to those in Table 2. Moreover, the largest qualitative difference is with the fixed-effects IV estimates in columns (4), (8), and (12) of Table 7 compared to column (1) of Table 2. In the former case we find that the pre-reform effect of mother's participation is much larger than in Table 2, though still statistically zero owing to the conservative standard errors that adjust for within-state correlation. Overall, the estimates of

Table 7 are suggestive that the mobility of daughters across state lines can "undo" some of the intergenerational transmission of welfare.

*Welfare Reform Aggressiveness*

States differed dramatically in the degree of aggressiveness in implementation of welfare reform, both in the waiver era and after TANF. There is no agreed upon measure of strictness in the literature, and thus we follow Grogger and Karoly (2005, Table 4.2) and define strict states as those whereby all four 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) examines five different categories of welfare reform aggressiveness and concludes that the latter measure was the best proxy for strict policy reforms. We then include this measure of welfare reform stringency in a triple difference framework to test whether there were differences in intergenerational transmission in those states that adopted more strict reforms compared to states with less strict reforms.

[Table 8 here]

Table 8 reports the estimates of the triple difference model without and with fixed effects, and for the instrumental variables. Across all three specifications, the transmission mechanism before welfare reform between mother and daughter was qualitatively smaller (though not statistically so except in the IV model in column (3)) based on the negative coefficients for the interaction between mother's participation and strictness (GK3). This suggests that there was some permanent difference among residents in states adopting strict reforms versus less strict reforms. However, after reform, this difference was attenuated as the triple interaction is positive, suggesting some degree of convergence in welfare climates across states after welfare reform.

*The Wider Safety Net*

In our last set of tests we examine what effect welfare reform had on the decision to participate more broadly in the social safety net. In this case we estimate the effect of mother's AFDC/TANF participation on the daughter's participation in any of AFDC/TANF, food stamps/SNAP, and SSI. In Table 9 we present four sets of estimates that are parallel to columns (7)-(10) in Table 2; namely, all models control for observable daughter characteristics, state, and year effects, and then with and without daughter fixed effects and instruments for mother's participation.

[Table 9 here]

The estimates in Table 9 show that prior to welfare reform the correlation between mother's AFDC and daughter's use of the wider safety net was little different compared to AFDC alone in Table 2, with the notable exception of the IV fixed effects estimates which are qualitatively much larger in Table 9. However, after welfare reform, there is less evidence that the transmission channel was broken. Based on the interaction term of mother's participation and welfare reform, it is clear that the causal pathway was attenuated with welfare reform, but not nearly to the extent we observe with AFDC/TANF.

[Figure 7]

A possible explanation for this result is that the economic status of daughters did not improve enough after welfare reform relative to their mothers for them to attain self sufficiency. We explore this possibility in Figure 7 where we present trends in intergenerational correlations in mothers and daughters akin to Figures 3 and 4 but now for four measures of economic status: (1) log family income; (2) poverty status defined as an income-to-needs ratio less than 1, where needs is defined by the U.S. Census Bureau poverty line that varies by family size and is

inflation adjusted across years; (3) poverty status defined as an income-to-needs ratio less than 1.3; and (4) poverty status defined as an income-to-needs ratio less than 2. We present income-to-needs cutoffs of 1.3 and 2 in addition to 1 because the Federal guideline for gross income eligibility for food stamps is 130 percent of the poverty line, but after the year 2000 states have had the option to extend eligibility and thus we also show for a threshold of 200 percent of poverty. In the two decades from the mid 1970s to the mid 1990s, the income mobility of daughters declined (i.e. the correlation was increasing), but then stabilized thereafter. And while there is some evidence that mobility out of poverty increased in the post-welfare era, this is not true if you extend the distribution to 1.3, and in fact, mobility out of near poverty continued to worsen. This suggests that after welfare reform daughters had continued economic need for assistance from the wider safety net.

## V. Conclusion

A focal aim of policymakers with the 1990s welfare reform was to end dependence on welfare, and based on the metric of the intergenerational correlation between mother and daughter, the evidence presented here suggests partial success toward meeting that goal. Viewed from the lens of participation in the AFDC/TANF program, we find strong evidence across a variety of specifications that address major threats to identification including selection bias, life-cycle bias, and misclassification bias, that the transmission channel from mother to daughter was reduced by at least 50 percent. However, when the definition of welfare is expanded from AFDC/TANF to also include food and disability assistance programs, the post-welfare reform correlation falls by a more modest amount. We suggest that the latter result likely stems from the fact that broader economic mobility of daughters (e.g. total family income, near-poverty status) stagnated or got worse after welfare reform.

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

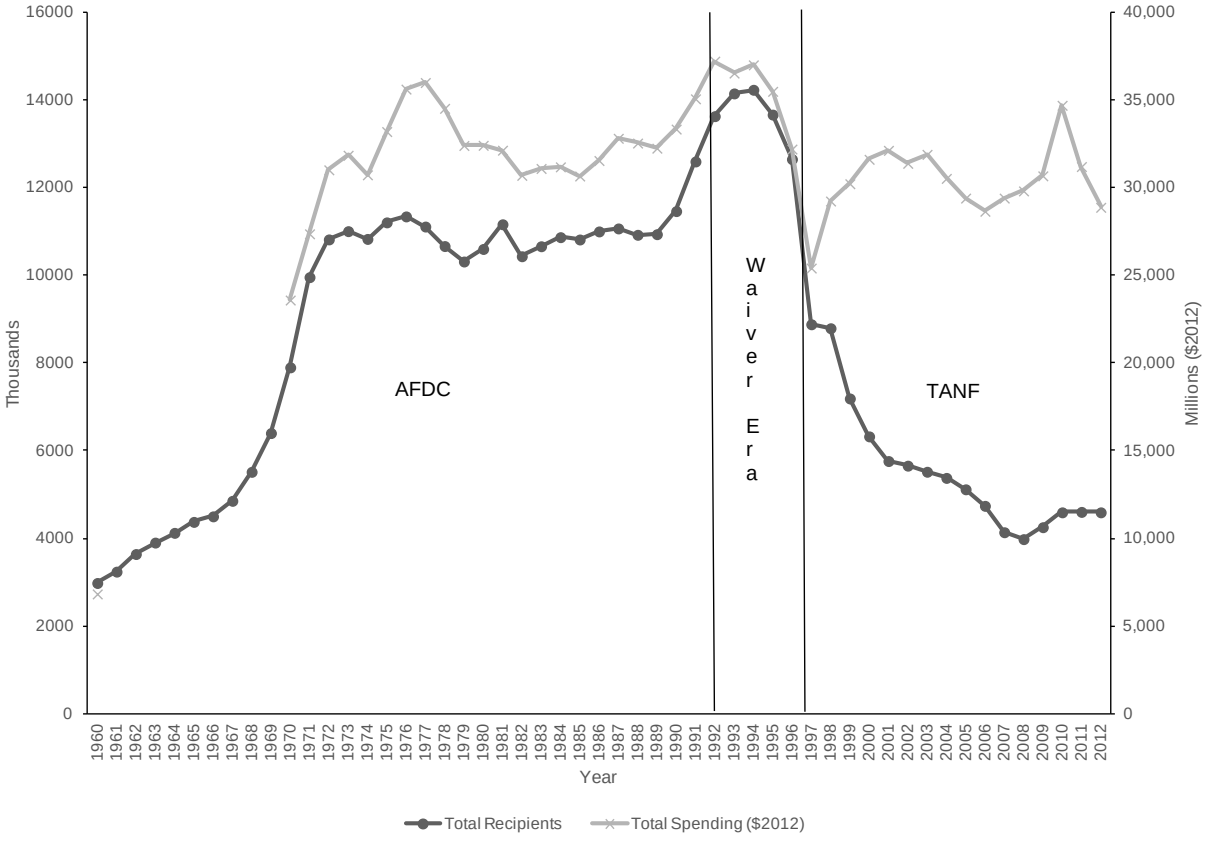


**Figure 2. Number of Recipients in Food Stamps/Supplemental Nutrition Assistance Program and Supplemental Security Income, Selected Years**

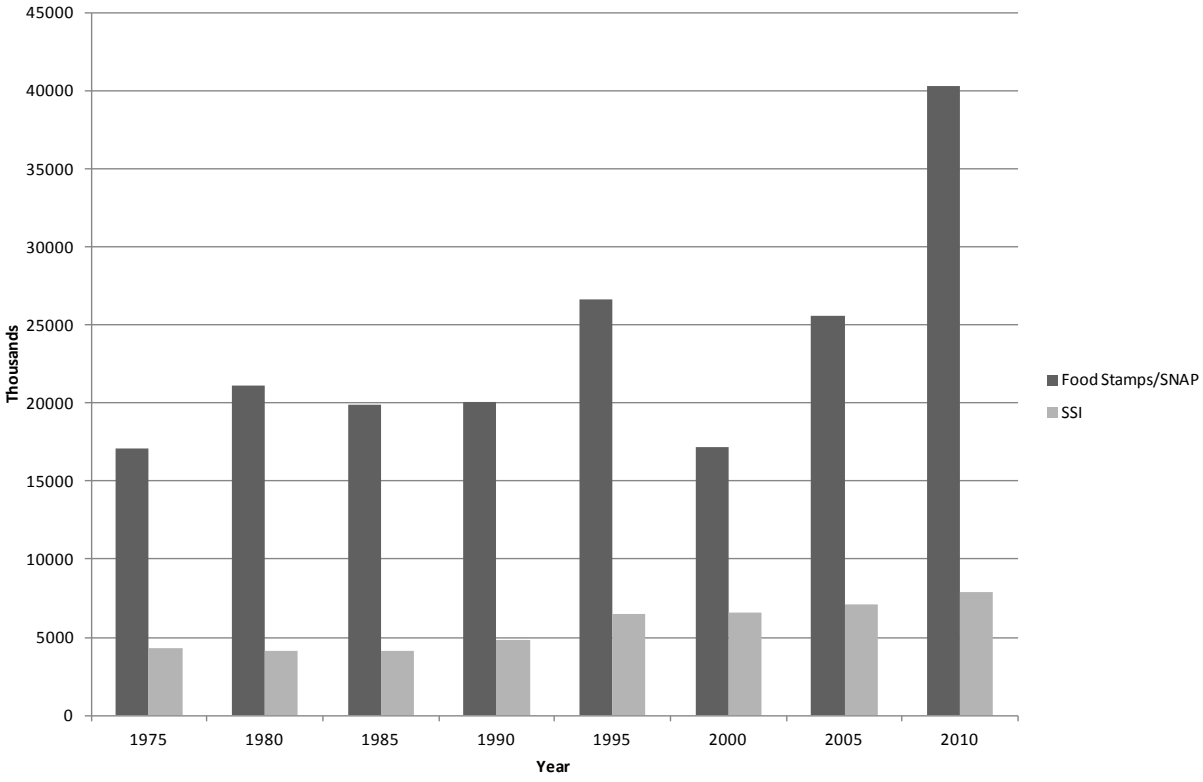
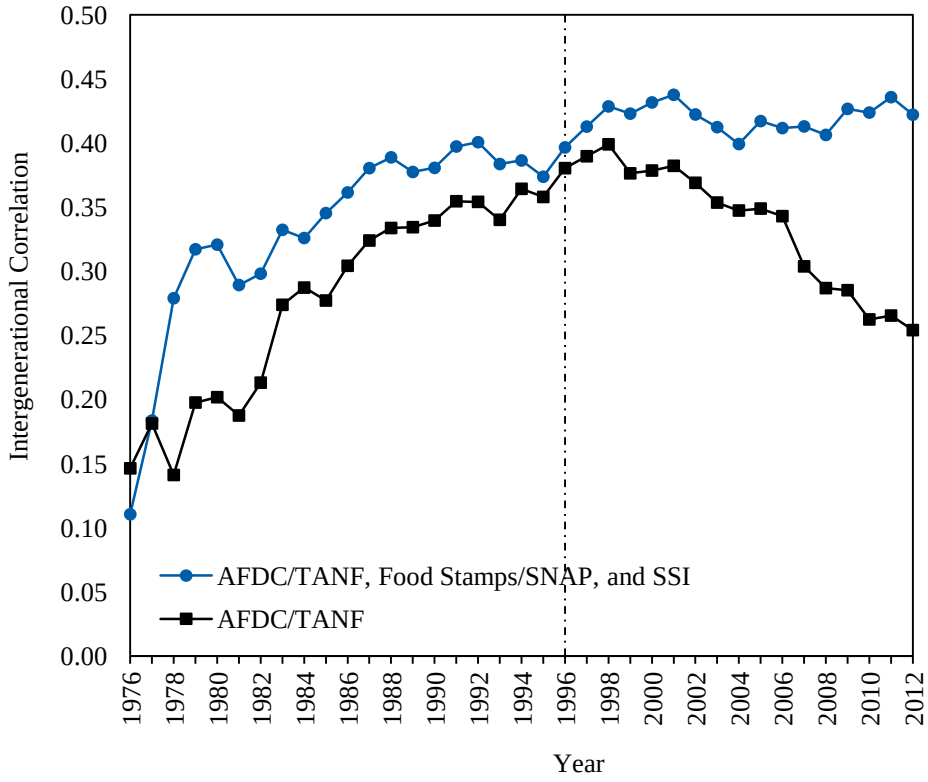
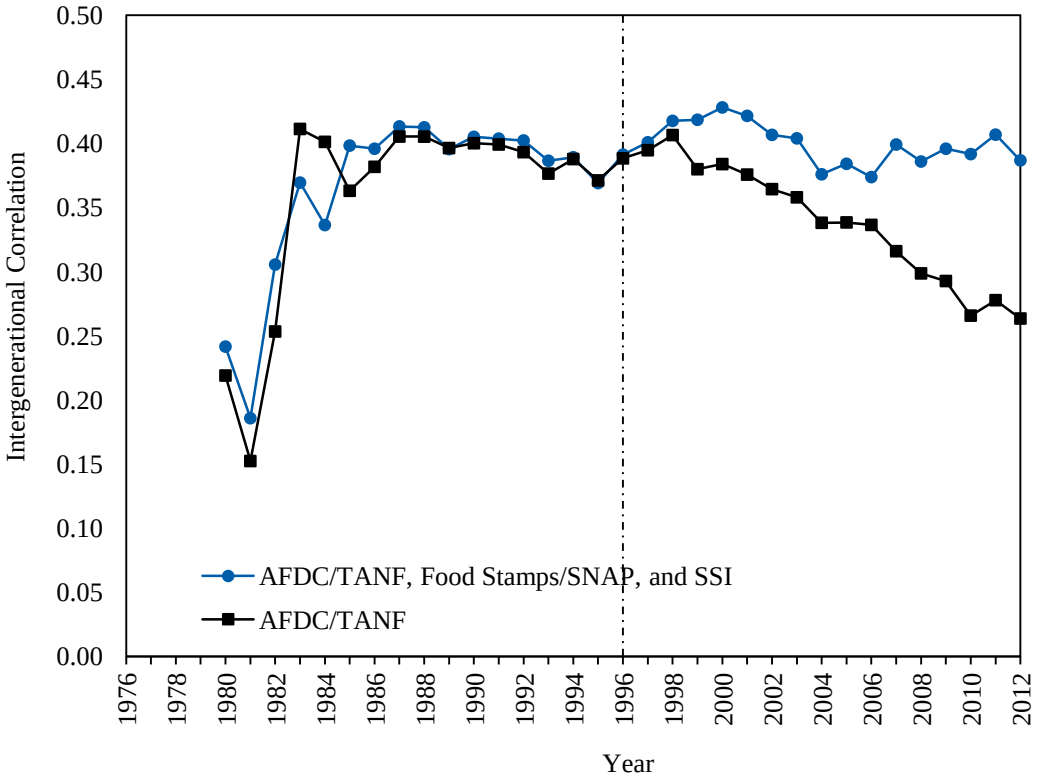


**Figure 3. Trends in Intergenerational Welfare Transmission: No Minimum Observation Window**



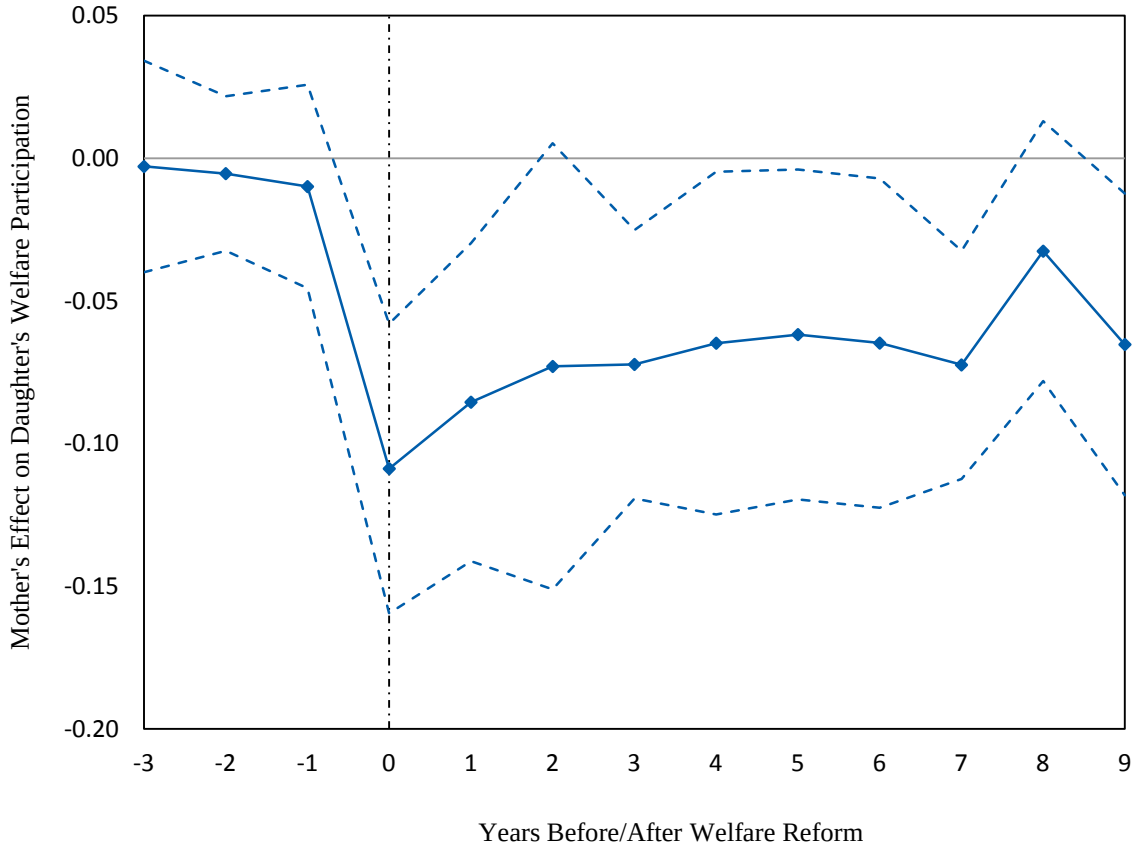

Note: The dependent variable for the daughter's welfare participation is an indicator for whether she participated in AFDC/TANF, or alternatively, inclusive of SSI and Food Stamps, from the child leaving home through age 26. The independent variable, for both series, is whether the mother ever participated in AFDC/TANF when the child is observed living at home. These trends reflect rolling cohort groups of daughters aged 27-42 in each year.

**Figure 4. Trends in Intergenerational Welfare Transmission: Five-Year Minimum Observation Window**




Note: The dependent variable for the daughter's welfare participation is an indicator for whether she participated in AFDC/TANF, or alternatively, inclusive of SSI and Food Stamps, from the child leaving home through age 26. The independent variable, for both series, is whether the mother ever participated in AFDC/TANF when the child is observed living at home. These trends reflect rolling cohort groups of daughters aged 27-42 in each year, but with a minimum 5-year observation window of mother-daughter co-residence during childhood.

**Figure 5. Mother's Participation Effect by Year Relative to Welfare Reform Implementation**

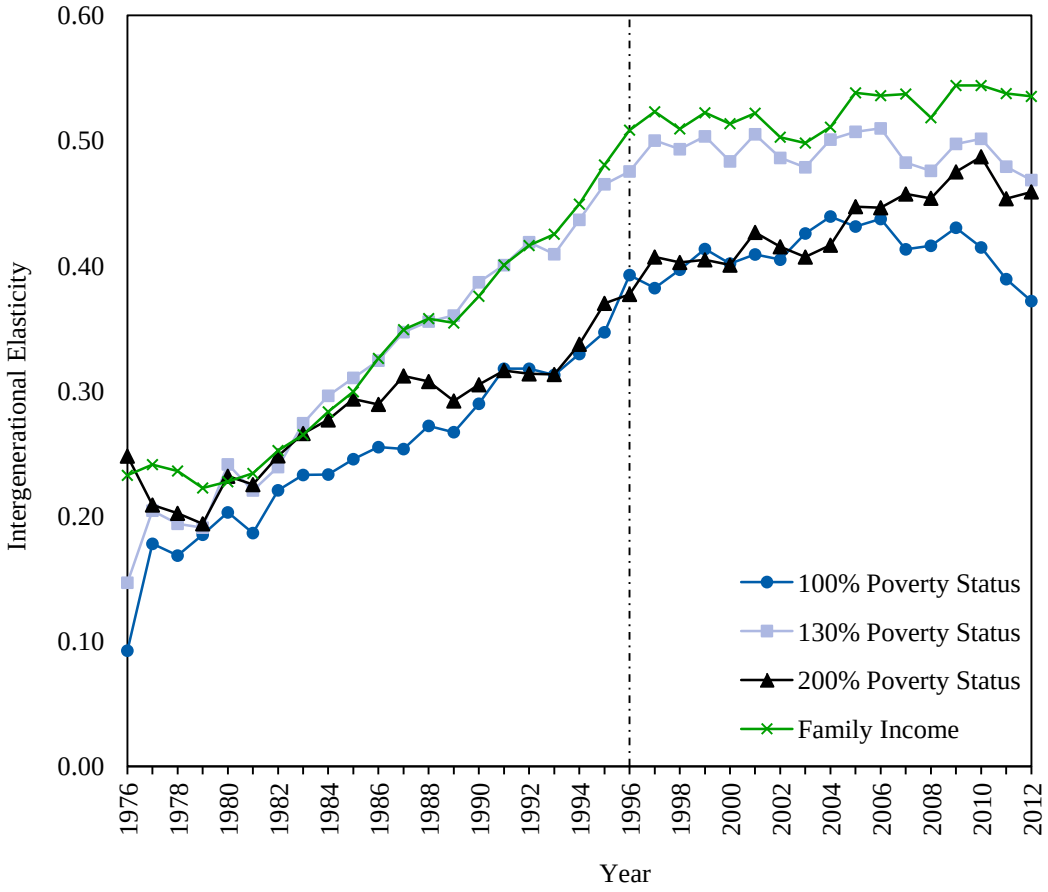



Note: Estimates represent coefficients on an interaction term for mother's participation and an indicator for the year relative to welfare reform. The dependent variable is daughter's current welfare status (AFDC/TANF), and other controls include any previous participation for the mother, an indicator for after welfare reform, state and year effects, daughter fixed effects, daughter time-varying controls, and instrumental variables. 95% confidence intervals are shown based on state-level clustering. Estimates are based on 56206 observations with weak IV statistic 26.628, Hansen J statistic 13.681, and p-value 0.622 for the J statistic.

**Figure 6. Critical Exposure Period for Transmission Through age 17**


Note: The dependent variable is daughter's current welfare status (AFDC/TANF), and the independent variables include any previous participation for the mother, an indicator for after welfare reform, an interaction term for mother's participation and after welfare reform, state and year effects, daughter fixed effects, daughter time-varying controls, and instrumental variables. The last four window estimates for each figure all pass the weak IV test at the 5% significance level and have a minimum J statistic p-value of 0.299.

**Figure 7. Intergenerational Transmission of Poverty Status and Family Income**




Note: The intergenerational transmission for poverty status represents linear probability model estimates based on indicators for whether an individual's family income is ever equal to or below 100, 130, or 200% of the federal poverty line by age 27, and the family income elasticity 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.

## Table 1. Descriptive Statistics

| | | | |
|---|---|---|---|
| Daughter | | | |
| | *Before Reform* | | *After Reform* |
| Currently Receiving Welfare? | | | |
| *AFDC/TANF* | 0.080 | | 0.025 |
| | (0.271) | | (0.157) |
| *AFDC/TANF, Food Stamps/SNAP, SSI* | 0.132 | | 0.112 |
| | (0.338) | | (0.315) |
| *Food Stamps/SNAP* | 0.115 | | 0.095 |
| | (0.319) | | (0.293) |
| Years After Welfare Reform (%) | 0.348 | | 0.652 |
| | (0.476) | | (0.476) |
| Age | 28.251 | | 38.684 |
| | (5.575) | | (9.017) |
| Number of Children | 1.246 | | 1.100 |
| | (1.166) | | (1.190) |
| Race | | | |
| *Black (%)* | 0.162 | | 0.171 |
| | (0.369) | | (0.376) |
| *White (%)* | 0.810 | | 0.804 |
| | (0.392) | | (0.397) |
| *Other (%)* | 0.027 | | 0.025 |
| | (0.162) | | (0.157) |
| Resides in Same State as Birth (%) | 0.759 | | 0.703 |
| | (0.428) | | (0.457) |
| Mother | | | |
| Any Previous Welfare? | | | |
| *AFDC/TANF* | 0.270 | | 0.068 |
| | (0.444) | | (0.251) |
| *AFDC/TANF, Food Stamps/SNAP, SSI* | 0.430 | | 0.192 |
| | (0.495) | | (0.394) |
| *Food Stamps/SNAP* | 0.374 | | 0.121 |
| | (0.484) | | (0.327) |
| Years [Before] After Welfare Reform (%) | 0.858 | | 0.142 |
| | (0.158) | | (0.158) |
| Age | 42.556 | | 59.352 |
| | (8.879) | | (10.501) |
| Unemployment Rate, Average | 5.537 | | 6.233 |
| | (1.134) | | (1.027) |
| Unemployment Rate, Maximum | 6.302 | | 6.571 |
| | (1.882) | | (2.045) |
| Mother-Child Family Unit Observations | | 14.202 | |
| | | (5.304) | |
| Daughter-as-Adult Observations | | 25.124 | |
| | | (9.014) | |
| Total Observations | 25,390 | | 30,816 |

PSID Core Longitudinal Weights are applied according to the daughter-year observation for both daughters' and mothers' statistics.

## Table 2. Intergenerational Transmission of AFDC/TANF Participation

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| Mother's Participation | 0.159** | 0.041+ | 0.331** | 0.236** | 0.111** | 0.498** | 0.201** | 0.117** | 0.398** | -0.063 |
| | (0.014) | (0.021) | (0.055) | (0.019) | (0.023) | (0.094) | (0.017) | (0.020) | (0.089) | (0.496) |
| After Welfare Reform | | | | 0.098** | 0.121** | 0.192** | 0.076** | 0.105** | 0.160** | 0.141** |
| | | | | (0.011) | (0.016) | (0.046) | (0.011) | (0.015) | (0.043) | (0.031) |
| Mother's Participation × After Welfare Reform | | | | -0.169** | -0.194** | -0.327** | -0.158** | -0.162** | -0.301** | -0.247** |
| | | | | (0.017) | (0.020) | (0.083) | (0.017) | (0.019) | (0.083) | (0.068) |
| Daughter Fixed Effects | No | Yes | No | No | Yes | No | No | Yes | No | Yes |
| Instrumental Variables | No | No | Yes | No | No | Yes | No | No | Yes | Yes |
| Daughter Controls | No | No | No | No | No | No | Yes | Yes | Yes | Yes |
| Weak IV Test Statistic | | | 20.240 | | | 22.670 | | | 21.330 | 11.453 |
| p-value (Weak IV) | | | 0.000 | | | 0.002 | | | 0.003 | 0.120 |
| Hansen J Statistic | | | 5.892 | | | 7.171 | | | 6.512 | 9.686 |
| p-value (J Statistic) | | | 0.117 | | | 0.305 | | | 0.368 | 0.139 |
| Number of Daughters | 2967 | 2967 | 2967 | 2967 | 2967 | 2967 | 2967 | 2967 | 2967 | 2967 |
| Observations | 56206 | 56206 | 56206 | 56206 | 56206 | 56206 | 56206 | 56206 | 56206 | 56206 |

Robust standard errors with state clustering are shown in parentheses. All specifications control for state and year effects. Daughter's time-varying controls include age, age squared, and indicator variables for number of children equal to 1, 2, 3, or at least 4. The weak IV test statistic is a Kleibergen-Paap (2006) rank statistic. ** p<0.01, * p<0.05, + p<0.1.

## Table 3. Life-Cycle AFDC/TANF Exposure by Minimum Number of Mother-Daughter Family Observations

| | No Restrictions | | | 5+ Observations of Mother and Daughter | | | 10+ Observations of Mother and Daughter | | | 15+ Observations of Mother and Daughter | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| Mother's Participation | 0.187** | 0.083** | 0.367** | 0.201** | 0.117** | 0.398** | 0.202** | 0.139** | 0.382** | 0.201** | 0.142** | 0.421** |
| | (0.014) | (0.020) | (0.072) | (0.017) | (0.020) | (0.089) | (0.020) | (0.023) | (0.105) | (0.022) | (0.027) | (0.109) |
| After Welfare Reform | 0.061** | 0.101** | 0.150** | 0.076** | 0.105** | 0.160** | 0.077** | 0.110** | 0.152** | 0.077** | 0.117** | 0.180* |
| | (0.009) | (0.012) | (0.033) | (0.011) | (0.015) | (0.043) | (0.010) | (0.018) | (0.056) | (0.016) | (0.021) | (0.072) |
| Mother's Participation × | -0.146** | -0.165** | -0.317** | -0.158** | -0.162** | -0.301** | -0.158** | -0.173** | -0.282** | -0.154** | -0.165** | -0.317** |
| After Welfare Reform | (0.013) | (0.018) | (0.072) | (0.017) | (0.019) | (0.083) | (0.015) | (0.021) | (0.100) | (0.018) | (0.020) | (0.112) |
| Daughter Fixed Effects | No | Yes | No | No | Yes | No | No | Yes | No | No | Yes | No |
| Instrumental Variables | No | No | Yes | No | No | Yes | No | No | Yes | No | No | Yes |
| Weak IV Test Statistic | | | 23.270 | | | 21.330 | | | 20.480 | | | 19.080 |
| p-value (Weak IV) | | | 0.002 | | | 0.003 | | | 0.005 | | | 0.008 |
| Hansen J Statistic | | | 11.290 | | | 6.512 | | | 7.727 | | | 8.038 |
| p-value (J Statistic) | | | 0.080 | | | 0.368 | | | 0.259 | | | 0.235 |
| Number of Daughters | 4229 | 4229 | 4229 | 2967 | 2967 | 2967 | 2469 | 2469 | 2469 | 1808 | 1808 | 1808 |
| Observations | 82771 | 84246 | 84122 | 56206 | 56206 | 56206 | 43785 | 43785 | 43785 | 28947 | 28947 | 28947 |
| Mean Daughter-as-Adult | 26.000 | 26.000 | 26.000 | 24.831 | 24.831 | 24.831 | 22.782 | 22.782 | 22.782 | 20.120 | 20.120 | 20.120 |
| Observations | (9.034) | (9.034) | (9.034) | (8.882) | (8.882) | (8.882) | 8.044 | (8.044) | (8.044) | 7.055 | (7.055) | (7.055) |
| Mean Daughter's Age | 34.195 | 34.195 | 34.195 | 32.932 | 32.932 | 32.932 | 32.152 | 32.152 | 32.152 | 31.078 | 31.078 | 31.078 |
| | (10.004) | (10.004) | (10.004) | (9.380) | (9.380) | (9.380) | (8.897) | (8.897) | (8.897) | (8.197) | (8.197) | (8.197) |

Robust standard errors with state clustering are shown in parentheses. All specifications control for state and year effects in addition to daughter's time-varying controls including age, age squared, and indicator variables for number of children equal to 1, 2, 3, or at least 4. The weak IV test statistic is a Kleibergen-Paap (2006) rank statistic. With the exception of the 'No Restrictions' case, these estimates all maintain a minimum threshold of observations such that the mother and daughter are observed in the same family unit at least 5 years during the daughter's ages 12-18. ** p<0.01, * p<0.05, + p<0.1.

**Table 4. Intergenerational Transmission of AFDC/TANF Participation with Lee-Solon-type Life-Cycle Adjustments**

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Mother's Participation | 0.201** | 0.201** | 0.200** | 0.222** |
| | (0.017) | (0.017) | (0.017) | (0.020) |
| After Welfare Reform | 0.076** | 0.078** | 0.079** | 0.058** |
| | (0.011) | (0.011) | (0.011) | (0.009) |
| Mother's Participation × | -0.158** | -0.158** | -0.157** | -0.126** |
| After Welfare Reform | (0.017) | (0.017) | (0.017) | (0.017) |
| Quartic on Mother's Mean Age | No | Yes | Yes | Yes |
| Quartic on Daughter's Current Age | No | No | Yes | Yes |
| Mother's Participation × Quartic on Daughter's Age | No | No | No | Yes |
| Number of Daughters | 2967 | 2967 | 2967 | 2967 |
| Observations | 56206 | 56206 | 56206 | 56206 |

Robust standard errors with state clustering are shown in parentheses. Mother's mean age applies to years in which her potential previous welfare participation is observed, and daughter's current age is detrended by age 25 (current age – 25). All specifications control for state and year effects as well as daughter's time-varying controls including age, age squared, and indicator variables for number of children equal to 1, 2, 3, or at least 4. ** $p<0.01$, * $p<0.05$, + $p<0.1$.

## Table 5. Intergenerational Transmission of Welfare Participation with Misclassification Bias Correction

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | *Linear Probability Estimates* | | | | | |
| | Without Bias Correction | | | With Bias Correction | | |
| Mother's Participation | 0.159** | 0.236** | 0.201** | 0.247** | 0.512** | 0.464** |
| | (0.014) | (0.019) | (0.017) | (0.028) | (0.051) | (0.047) |
| After Welfare Reform | | 0.098** | 0.076** | | 0.237** | 0.206** |
| | | (0.011) | (0.011) | | (0.025) | (0.025) |
| Mother's Participation × | | -0.169** | -0.158** | | -0.405** | -0.400** |
| After Welfare Reform | | (0.017) | (0.017) | | (0.042) | (0.043) |
| Daughter Controls | No | No | Yes | No | No | Yes |
| Number of Daughters | 2967 | 2967 | 2967 | 2967 | 2967 | 2967 |
| Observations | 56206 | 56206 | 56206 | 56206 | 56206 | 56206 |
| | *Probit Estimates* | | | | | |
| | Without Bias Correction | | | With Bias Correction | | |
| Mother's Participation | 1.124** | 1.232** | 1.064** | 1.352** | 1.440** | 1.251** |
| | (0.054) | (0.053) | (0.057) | (0.022) | (0.028) | (0.030) |
| After Welfare Reform | | 0.342** | 0.169 | | 0.376** | 0.146* |
| | | (0.121) | (0.132) | | (0.052) | (0.057) |
| Mother's Participation × | | -0.393** | -0.363* | | -0.442** | -0.380** |
| After Welfare Reform | | (0.133) | (0.143) | | (0.049) | (0.054) |
| Daughter Controls | No | No | Yes | No | No | Yes |
| Number of Daughters | 2963 | 2963 | 2963 | 2967 | 2967 | 2967 |
| Observations | 56012 | 56012 | 56012 | 56206 | 56206 | 56206 |
| | *Probit Marginal Effects* | | | | | |
| | Without Bias Correction | | | With Bias Correction | | |
| Mother's Participation | 0.151** | 0.172** | 0.132** | 0.273** | 0.282** | 0.212** |
| | (0.006) | (0.007) | (0.007) | (0.004) | (0.005) | (0.004) |
| After Welfare Reform | | 0.048** | 0.021 | | 0.068** | 0.023* |
| | | (0.017) | (0.017) | | (0.009) | (0.009) |
| Mother's Participation × | | -0.049** | -0.042** | | -0.076** | -0.057** |
| After Welfare Reform | | (0.015) | (0.015) | | (0.009) | (0.008) |
| Daughter Controls | No | No | Yes | No | No | Yes |
| Number of Daughters | 2963 | 2963 | 2963 | 2967 | 2967 | 2967 |
| Observations | 56012 | 56012 | 56012 | 56206 | 56206 | 56206 |

Robust standard errors are shown in parentheses where clustering by state is applied for all cases except bias-corrected Probit. All specifications control for state and year effects. Daughter's time-varying controls include age, age squared, and indicator variables for number of children equal to 1, 2, 3, or at least 4. ** p<0.01, * p<0.05, + p<0.1.

## Table 6. Intergenerational Transmission of AFDC/TANF Participation by Race

| | (1) | (2) | (3) |
|---|---|---|---|
| Mother's Participation | 0.085** | 0.046 | 0.057 |
| | (0.014) | (0.028) | (0.063) |
| After Welfare Reform | 0.086** | 0.105** | 0.117** |
| | (0.012) | (0.016) | (0.028) |
| Mother's Participation × | -0.082** | -0.041* | -0.179* |
| After Welfare Reform | (0.014) | (0.018) | (0.085) |
| Daughter's Race: Black | 0.062** | | -0.305+ |
| | (0.018) | | (0.170) |
| Black × Mother's Participation | 0.106** | 0.073+ | 0.615** |
| | (0.024) | (0.037) | (0.236) |
| Black × After Welfare Reform | -0.023 | -0.005 | 0.207 |
| | (0.016) | (0.010) | (0.157) |
| Black × Mother's Participation × | -0.073** | -0.146** | -0.315 |
| After Welfare Reform | (0.024) | (0.027) | (0.208) |
| Daughter Fixed Effects | No | Yes | No |
| Instrumental Variables | No | No | Yes |
| Weak IV Test Statistic | | | 22.950 |
| p-value (Weak IV) | | | 0.042 |
| Hansen J Statistic | | | 16.870 |
| p-value (J Statistic) | | | 0.154 |
| Number of Daughters | 2854 | 2854 | 2854 |
| Observations | 55094 | 55094 | 55094 |

Robust standard errors with state clustering are shown in parentheses. All specifications control for state and year effects in addition to daughter's time-varying controls including age, age squared, and indicator variables for number of children equal to 1, 2, 3, or at least 4. The weak IV test statistic is a Kleibergen-Paap (2006) rank statistic. ** p<0.01, * p<0.05, + p<0.1.

## Table 7. Intergenerational Transmission of AFDC/TANF Participation by Geographic Mobility Status

| | Daughter Same State as Birth | | | | Daughter Same State as Mother | | | | Daughter Never Moves States | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| Mother's Participation | 0.219** | 0.134** | 0.448** | 0.479 | 0.260** | 0.112** | 0.457** | 0.083 | 0.235** | 0.131** | 0.481** | 0.751 |
| | (0.021) | (0.023) | (0.102) | (0.584) | (0.020) | (0.025) | (0.089) | (0.288) | (0.024) | (0.028) | (0.098) | (0.582) |
| After Welfare Reform | 0.085** | 0.127** | 0.184** | 0.173** | 0.098** | 0.086** | 0.141** | 0.106** | 0.090** | 0.133** | 0.199** | 0.196** |
| | (0.014) | (0.018) | (0.054) | (0.039) | (0.018) | (0.020) | (0.039) | (0.033) | (0.017) | (0.018) | (0.057) | (0.045) |
| Mother's Participation × | -0.171** | -0.186** | -0.339** | -0.286** | -0.209** | -0.178** | -0.302** | -0.220** | -0.188** | -0.195** | -0.373** | -0.320** |
| After Welfare Reform | (0.019) | (0.020) | (0.100) | (0.069) | (0.019) | (0.017) | (0.066) | (0.046) | (0.023) | (0.019) | (0.100) | (0.074) |
| Daughter Fixed Effects | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |
| Instrumental Variables | No | No | Yes | Yes | No | No | Yes | Yes | No | No | Yes | Yes |
| Weak IV Test Statistic | | | 18.950 | 13.350 | | | 17.840 | 13.030 | | | 16.380 | 9.670 |
| p-value (Weak IV) | | | 0.008 | 0.064 | | | 0.013 | 0.071 | | | 0.022 | 0.208 |
| Hansen J Statistic | | | 4.765 | 12.380 | | | 10.220 | 6.763 | | | 5.077 | 15.110 |
| p-value (J Statistic) | | | 0.574 | 0.054 | | | 0.116 | 0.343 | | | 0.534 | 0.019 |
| Number of Daughters | 2623 | 2623 | 2623 | 2572 | 2763 | 2763 | 2763 | 2684 | 1965 | 1965 | 1965 | 1965 |
| Observations | 44218 | 44218 | 44218 | 44167 | 36875 | 36875 | 36875 | 36796 | 36498 | 36498 | 36498 | 36498 |

Robust standard errors with state clustering are shown in parentheses. All specifications control for state and year effects in addition to daughter's time-varying controls including age, age squared, and indicator variables for number of children equal to 1, 2, 3, or at least 4. The weak IV test statistic is a Kleibergen-Paap (2006) rank statistic. ** p<0.01, * p<0.05, + p<0.1.

## Table 8. Identifying AFDC/TANF Effects Through Welfare Reform Aggressiveness

| | (1) | (2) | (3) |
|---|---|---|---|
| Mother's Participation | 0.215** | 0.122** | 0.536** |
| | (0.021) | (0.028) | (0.122) |
| After Welfare Reform | 0.071** | 0.101** | 0.204** |
| | (0.011) | (0.015) | (0.060) |
| Mother's Participation × After Welfare Reform | -0.168** | -0.169** | -0.424** |
| | (0.019) | (0.023) | (0.117) |
| Grogger-Karoly Instrument 3 (GK3) | -0.011 | -0.027 | 0.119* |
| | (0.015) | (0.017) | (0.050) |
| GK3 × Mother's Participation | -0.043 | -0.015 | -0.335** |
| | (0.033) | (0.042) | (0.116) |
| GK3 × After Welfare Reform | 0.018+ | 0.016+ | -0.095* |
| | (0.010) | (0.008) | (0.046) |
| GK3 × Mother's Participation × After Welfare Reform | 0.024 | 0.019 | 0.273* |
| | (0.031) | (0.022) | (0.110) |
| Daughter Fixed Effects | No | Yes | No |
| Instrumental Variables | No | No | Yes |
| Weak IV Test Statistic | | | 24.880 |
| p-value (Weak IV) | | | 0.024 |
| Hansen J Statistic | | | 8.060 |
| p-value (J Statistic) | | | 0.780 |
| Number of Daughters | 2967 | 2967 | 2967 |
| Observations | 56206 | 56206 | 56206 |

Robust standard errors with state clustering are shown in parentheses. All specifications control for state and year effects in addition to daughter's time-varying controls including age, age squared, and indicator variables for number of children equal to 1, 2, 3, or at least 4. The weak IV test statistic is a Kleibergen-Paap (2006) rank statistic. ** p<0.01, * p<0.05, + p<0.1.

**Table 9. Intergenerational Transmission of AFDC/TANF, Food Stamps/SNAP, and SSI Participation**

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Mother's Participation | 0.269** | 0.098** | 0.401** | 0.258 |
| | (0.016) | (0.025) | (0.080) | (0.462) |
| After Welfare Reform | 0.018 | 0.055** | 0.052 | 0.078** |
| | (0.013) | (0.013) | (0.033) | (0.027) |
| Mother's Participation × | -0.053** | -0.086** | -0.095 | -0.142* |
| After Welfare Reform | (0.019) | (0.018) | (0.061) | (0.056) |
| Daughter Fixed Effects | No | Yes | No | Yes |
| Instrumental Variables | No | No | Yes | Yes |
| Daughter Controls | Yes | Yes | Yes | Yes |
| Weak IV Test Statistic | | | 21.330 | 11.450 |
| p-value (Weak IV) | | | 0.003 | 0.120 |
| Hansen J Statistic | | | 10.770 | 6.942 |
| p-value (J Statistic) | | | 0.096 | 0.326 |
| Number of Daughters | 2967 | 2967 | 2967 | 2967 |
| Observations | 56206 | 56206 | 56206 | 56206 |

Robust standard errors with state clustering are shown in parentheses. All specifications control for state and year effects in addition to daughter's time-varying controls including age, age squared, and indicator variables for number of children equal to 1, 2, 3, or at least 4. The weak IV test statistic is a Kleibergen-Paap (2006) rank statistic. ** p<0.01, * p<0.05, + p<0.1.

**Appendix Table 1. AFDC/TANF Reporting Rates for PSID Taken as Given for Misclassification Bias Correction Estimates**

| | Meyer et al. (NBER 2015) | | Estimation Parameter |
|---|---|---|---|
| Year | Amount | Cases | $\tau_1$ |
| 1975 | 0.669 | | 0.241 |
| 1976 | 0.664 | | 0.246 |
| 1977 | 0.626 | | 0.289 |
| 1978 | 0.653 | | 0.259 |
| 1979 | 0.647 | | 0.266 |
| 1980 | 0.703 | | 0.202 |
| 1981 | 0.720 | | 0.183 |
| 1982 | 0.687 | | 0.220 |
| 1983 | 0.714 | | 0.190 |
| 1984 | 0.658 | | 0.253 |
| 1985 | 0.597 | | 0.322 |
| 1986 | 0.579 | | 0.343 |
| 1987 | 0.562 | | 0.362 |
| 1988 | 0.603 | | 0.316 |
| 1989 | 0.577 | | 0.345 |
| 1990 | 0.571 | | 0.352 |
| 1991 | 0.624 | | 0.292 |
| 1992 | 0.563 | | 0.361 |
| 1993 | 0.520 | 0.621 | 0.379 |
| 1994 | 0.456 | 0.567 | 0.433 |
| 1995 | 0.481 | 0.546 | 0.454 |
| 1996 | 0.515 | 0.532 | 0.468 |
| 1997 | | | 0.534 |
| 1998 | 0.389 | 0.400 | 0.600 |
| 1999 | | | 0.547 |
| 2000 | 0.385 | 0.507 | 0.493 |
| 2001 | | | 0.573 |
| 2002 | 0.319 | 0.347 | 0.653 |
| 2003 | 0.408 | 0.680 | 0.320 |
| 2004 | 0.512 | 0.573 | 0.427 |
| 2005 | | | 0.548 |
| 2006 | | | 0.560 |
| 2007 | | | 0.571 |
| 2008 | | | 0.583 |
| 2009 | | | 0.595 |
| 2010 | | | 0.607 |
| 2011 | | | 0.618 |
| 2012 | | | 0.630 |

PSID reporting rates for dollar amount of AFDC/TANF received as well as number of cases are estimated in Meyer et al. (NBER 2015). The estimation parameter $\tau_1$ is 1 minus the reporting rate and assumed to be the false-negative rate for misclassification bias-corrected estimates in Table 9. Column 3 is calculated as 1 minus column 2 when available. For years before 1993, $\tau_1$ is imputed by inflating column 1 by the median ratio of columns 1 and 2. For years 1997, 1999, and 2001, $\tau_1$ is imputed by the interpolation of the years before and after. For years after 2004, $\tau_1$ is imputed by predicted values based on a trend regression of $\tau_1$ in all earlier years in Meyer et al. Only values from years 1975-2012 are used in our estimation.

## Appendix Table 2. Alternative Instrument Specifications for Intergenerational Transmission of AFDC/TANF Participation with Fixed Effects

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Mother's Participation | 0.458 | 0.388 | 0.344 | 0.369 | 0.229 | -0.063 |
| | (0.594) | (0.593) | (0.547) | (0.583) | (0.506) | (0.496) |
| After Welfare Reform | 0.199** | 0.198** | 0.218** | 0.197** | 0.216** | 0.141** |
| | (0.045) | (0.044) | (0.048) | (0.043) | (0.047) | (0.031) |
| Mother's Participation × | -0.388** | -0.386** | -0.434** | -0.384** | -0.428** | -0.247** |
| After Welfare Reform | (0.092) | (0.091) | (0.097) | (0.089) | (0.097) | (0.068) |
| *Instrumental Variables:* | | | | | | |
| Avg. Unemployment Rate | X | X | X | X | X | X |
| Max. Unemployment Rate | | X | | X | X | X |
| Avg. AFDC/TANF Benefit | X | X | X | X | X | X |
| Max. AFDC/TANF Benefit | | | | X | X | X |
| Reform × Avg. Unemployment | X | X | X | X | X | X |
| Reform × Max. Unemployment | X | X | | X | | X |
| Reform × Avg. AFDC/TANF | | | X | | X | X |
| Reform × Max. AFDC/TANF | | | | | | X |
| Weak IV Test Statistic | 6.023 | 6.235 | 5.428 | 7.283 | 6.004 | 11.450 |
| p-value (Weak IV) | 0.111 | 0.182 | 0.143 | 0.200 | 0.306 | 0.120 |
| Hansen J Statistic | 1.222 | 2.498 | 1.114 | 2.529 | 2.593 | 9.686 |
| p-value (J Statistic) | 0.543 | 0.476 | 0.573 | 0.639 | 0.628 | 0.139 |
| Number of Daughters | 2967 | 2967 | 2967 | 2967 | 2967 | 2967 |
| Observations | 56206 | 56206 | 56206 | 56206 | 56206 | 56206 |

Robust standard errors are shown in parentheses where clustering by state is applied for all cases except bias-corrected Probit. All specifications control for state and year effects. Daughter's time-varying controls include age, age squared, and indicator variables for number of children equal to 1, 2, 3, or at least 4. ** p<0.01, * p<0.05, + p<0.1.