Some Evidence on Race, Welfare Reform, and Household Income

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In 1996, federal welfare-reform legislation eliminated Aid to Families with Dependent Children (AFDC) and replaced it with Temporary Assistance for Needy Families (TANF). Numerous studies have estimated impacts of reform on welfare caseloads, employment, earnings, family structure, income, and poverty.[1] Two principal challenges to identifying TANF's impact have been discussed in the literature. First, factors other than welfare reform should have increased household income. It is well known that reform occurred during a period of strong economic performance. While the unemployment rate for blacks fell to the lowest level ever recorded, wages for low-skill groups rose for the first time since the 1970's. Further, other policy changes in the second half of the 1990's focused on improving the economic status of the disadvantaged. Examples include expansions in the Earned Income Tax Credit (EITC), minimum wages, and public health insurance (Medicaid and the Children's Health Insurance Program). Second, TANF was implemented in all states over just 16 months (between September 1996 and January 1998), leaving only limited scope for identifying impacts of TANF through timing across states.[2]

While these challenges are well known, their implications for interpreting estimated TANF impacts in nonexperimental studies are not. In this paper, we do four things. First, we discuss the identification of TANF effects in a prototypical nonexperimental model. We show that if TANF effects are the same in every year, then the lack of time variation in TANF implementation is not problematic. However, if TANF and trend effects are allowed to vary over time in an unrestricted fashion, then TANF effects for later years are unidentified. Second, we propose a method for bounding impacts in light of this identification problem. Third, we apply this method to analyze the impact of TANF on household income for a sample of children in the Current Population Survey (CPS) covering calendar years 1988–1999. Fourth, we document significant heterogeneity in the association between household income and both TANF and residual factors across white, Hispanic, and black children.

I. Background

Recent welfare reforms began in the early 1990's with implementation of state waivers from federal AFDC rules. Federal law changed greatly in 1996, with the enactment of the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA). TANF replaced AFDC, changing many features of the welfare system. Theoretically, welfare reform could increase or decrease income. For some groups, reform could increase earnings, while for others, lost welfare income might go unreplaced by earnings. Total household income, our focus, could also be affected by changes in other assistance programs, income of other household members, and household and family structure.

Figure 1 presents basic trends in real household income for a sample of white non-Hispanic (henceforth, white), Hispanic, and black non-Hispanic (henceforth, black) children from the March CPS. After a decline during the early 1990's recession, the figure shows that the late

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[1] For example, see recent reviews by Rebecca Blank (2002); Jeffrey Grogger et al. (2002); and Robert Moffitt (2002). Due to space limitations, we discuss specific studies sparingly. We refer interested readers to these excellent reviews.

[2] By contrast, there was considerable variation across states and over time in the implementation of pre-TANF waivers.

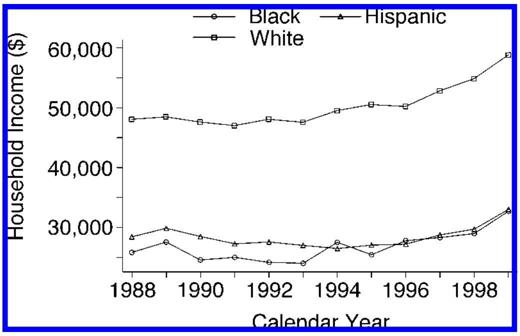

FIGURE 1. MEAN REAL HOUSEHOLD INCOME AMONG ALL CHILDREN

1990's generated tremendous gains in average household income for all three groups. The steep increases in income for 1997–1999 are particularly notable.

Given the identification challenges discussed above, most studies of the impact of TANF have used one of four estimation strategies: experimental data; nonexperimental data using regression techniques to control for state policy, economic conditions, and state and year fixed effects, relying on the available variation in TANF; comparing changes before and after TANF implementation across "treatment" and control groups (for example, less-educated vs. highly educated women); and adding cross-sectional variation via cross-state variation in detailed TANF policy characteristics (such as the length of the time limit or the severity of sanctions). Our discussion in this paper adopts the second approach, using nonexperimental data and estimating regression models within treatment groups (and without detailed reforms). Our goal is not to advocate any particular estimation strategy, but rather to investigate how much can be learned using this approach. In particular, we propose a method that appeals only to plausible assumptions in order to bound effects within "treatment" groups.

II. Empirical Modeling and an Identification Problem

Our framework assumes ordinary least-squares (OLS) estimation of a linear regression function relating the natural log of household income to individual demographic covariates, state-level variables, and unrestricted year effects using pooled cross-sections from the March CPS. A typical specification has the form

$$y_{ist} = \beta_0 + \mathbf{R}_{st}\boldsymbol{\beta}_r + \mathbf{X}_{ist}\boldsymbol{\beta}_x + \mathbf{D}_t\boldsymbol{\beta}_t + \mathbf{D}_s\boldsymbol{\beta}_s + \varepsilon_{ist}. \tag{1}$$

The row vector, $\mathbf{X}_{ist}$, contains characteristics of person i living in state s at time t, including demographic and state-level policy and labor-market variables. The vectors $\mathbf{D}_t$ and $\mathbf{D}_s$ are sets of indicator variables, with $\boldsymbol{\beta}_t$ and $\boldsymbol{\beta}_s$ being the associated year and state fixed effects. The $\boldsymbol{\beta}_r$ coefficients are reform effects, and ε_{ist} represents unobservables. Our primary focus is $\mathbf{R}_{st}$, which contains two welfare reform variables: for state s at time t, whether a waiver is in place and whether TANF has been implemented. Because income in the CPS refers to the prior calendar year, these variables represent the share of the last year (months divided by 12) for which the given reform is in place in a state.

Some observers object to an empirical model like (1) on the grounds that it apparently constrains reform effects to occur instantaneously at implementation. However, detailed aspects of state reforms and economic conditions can be difficult to observe, and in any case there is no reason to think that different people will respond identically to the same reforms. Thus, there is no way around viewing the estimated treatment effects as averages of heterogeneous effects, an issue we now discuss in some detail.

III. Treatment-Effect Heterogeneity and TANF

It will be helpful to simplify the discussion and briefly review some concepts on estimating program effects in the presence of treatment-effect heterogeneity; a more detailed discussion may be found in James Heckman and Richard Robb (1985). Suppose that the dummy variable R indicates whether or not a person is in the treatment group, (in the present context, subject to a policy change). Let the counterfactual outcome value for person i be $y_{i0} \equiv \mu_i$ when not treated and $y_{i1} \equiv \mu_i + \alpha_i$ when treated. The treatment effect for i is thus α_i, and the observed outcome value is $y_i \equiv (1 - R_i)y_{0i} + R_i y_{1i}$.

Consider the decomposition $\mu_i = \bar{\mu} + u_i$, where $\bar{\mu}$ is defined as the population average value of y_i and $E[u_i = 0]$ (again by definition). The average effect of treatment on the treated is $\bar{\alpha} \equiv E[\alpha_i|R = 1]$, which can be estimated by the *observed mean difference* $E[y_i|R_i = 1] - E[y_i|R_i = 0]$ when assignment to the treatment group is as good as random. Given that identification comes from variation across states in the timing of reform's implementation, "as good as random" means that reform is not systematically more likely to be implemented earlier in states where income is trending up (or down). We will simply assume this to be true, as our focus lies elsewhere.

Now suppose we want to evaluate a policy change affecting all sample members beginning at time t^* (e.g., there is no variation in the timing of the treatment). If we assume that the policy change has the constant impact α on each person i in every year, then one plausible estimator would be $E[y_i|t < t^*] - E[y_i|t \geq t^*] = E[\alpha_i] = \alpha$. To be more concrete, suppose that we have data on the years 0, 1, and 2, with the policy change in effect for years 1 and 2 but not in 0 (so $t^* = 1$). Now, allow the possibility that α_i varies over time. For example, due to an expanding labor market, the availability of jobs over time increases, and the impact of welfare reform on a household's income depends on the ease of finding employment. Our model must be modified to add a time index; for example, we can write $y_{i0t} = \bar{\mu} + \delta_t + u_{it}$ and $y_{i1t} = \bar{\mu} + \alpha_{it} + \delta_t + u_{it}$, with $y_i^t = (1 - R_{it})y_{i0t} + R_{it}y_{i1t}$ the observed value. This model has a population-wide "year effect" δ_t, year-specific individual heterogeneity terms u_{it} (which satisfy $E[u_{it}] = 0$ by construction), and year-specific treatment effects for each i, α_{it}.

Taking the observed mean difference for each of years 1 and 2 relative to the pre-reform baseline for year 0 yields two distinct estimates: $E[y_i^1] - E[y_i^0] = \delta_1 - \delta_0 + \bar{\alpha}_1$ and $E[y_i^2] - E[y_i^0] = \delta_2 - \delta_0 + \bar{\alpha}_2$. For $t \in \{1, 2\}$, observed mean differences cannot distinguish between $\delta_t - \delta_0$ and $\bar{\alpha}_t$: we cannot tell whether changes in household income are due to welfare reform effects per se (the term $\bar{\alpha}_t$) or to the fact that easier job search would raise income even without welfare reform (the term $\delta_t - \delta_0$).

It is for this reason that researchers using models like (1) have focused attention on the variation across states in the *timing* of waiver and TANF implementation. Implementation dates for waivers varied considerably across states, so both the impact of reform and the time effects are well identified. However, since all states implemented TANF between September 1996 and January 1998, there is no variation in TANF implementation for calendar-year observations before 1996 or after 1998. For observations from calendar year 1996, the average value of the TANF variable is about 0.05 (i.e., about 5 percent of person-months were exposed to TANF). For observations from calendar-year 1997, this figure is about 0.70; for 1998, it is about 0.99. Thus, the TANF coefficient is identified almost entirely by cross-state variation in calendar-year 1997 implementation status (early vs. late implementers). Thus, interpretation of the TANF coefficient is complicated by the facts that (i) all states ultimately implement TANF and (ii) we want to allow for treatment-effect heterogeneity across years.

IV. What Does the TANF Coefficient Estimate?

To gain some intuition about what the estimated TANF effect actually represents, it will be helpful to consider the following stripped-down version of (1). Suppose that there is variation in TANF implementation only for 1997, and ignore the demographic ($\mathbf{X}_{ist}$) variables, the waiver dummy, and the state fixed effects. We thus have the simple model

$$y_{ist} = \beta_0 + \mathrm{TANF}_{st}\tau + \mathbf{D}_t\boldsymbol{\beta}_t + \varepsilon_{ist} \tag{2}$$

where we note that for the moment no time heterogeneity is allowed in the TANF effect. In this case, expected income for someone in a state having TANF implemented for the fraction λ of 1997 is $E[y_{is97}|\lambda] = \beta_0 + \lambda\tau + \beta_{97}$. The difference in expected income given implementation of the reform for λ_0 and λ_1 is $\Delta y(\lambda_0, \lambda_1) = (\lambda_1 - \lambda_0)\tau$. Assuming for exposition that λ_0 and λ_1 are the only fractions observed in the data, the estimated TANF effect is $\tau = \Delta y(\lambda_0, \lambda_1)/(\lambda_1 - \lambda_0)$. Before 1997, expected income in all states is $E[y_{ist}] = \beta_0 + \beta_t$, while after 1997, expected income in all states is

$E[y_{ist}] = \beta_0 + \tau + \beta_t$. One implication of this fact is that there is no identification problem when the treatment effect is constant over time (i.e., τ does not vary by year). We simply estimate the TANF effect for 1997, taking advantage of the observed variation in TANF in 1997. This (well-identified) effect is then, by assumption, the effect that holds for all years.

However, this discussion yields an unsettling conclusion: years other than 1997 contribute no information to the identification of the TANF effect. If the TANF effect is allowed to differ across years, then without appealing to some set of assumptions, the *only* identified TANF effect is the one for 1997. One interpretation of work using the approach outlined in (1), is as follows: subject to the usual list of caveats associated with difference-in-difference models, this approach cleanly identifies the effect of TANF for 1997 and 1997 only. It does *not* identify an average impact over the post-TANF period, as would be the case if some states had never implemented TANF. An interesting and perhaps surprising corollary is that, if the partial relationship between other covariates and income is stable over the period of study, adding additional years of data will not change the estimated TANF effect beyond sampling variation. In fact, the only argument for adding additional years of data is to reduce variance by increasing sample size. To our knowledge, this fact has not previously been noted in the literature.

This is clearly a vexing problem for researchers who want to investigate the impact of TANF in nationally representative samples. As mentioned above, the nonexperimental literature either stops here (and does not point out what the estimated TANF effect is measuring), adds control groups, or uses detailed characteristics of reform.[3] Rather than take one of those approaches, we focus here on constructing bounds for τ_{98} and τ_{99}. From the discussion above, expected income for years after 1997 is $E[y_{ist}] = \beta_0 + \tau_{97} + G_t$, where $G_t \equiv \Delta\tau_t + \beta_t$, $\Delta\tau_t \equiv \tau_t - \tau_{97}$ is the net-of-1997 TANF effect for year t, and β_t is the trend effect. Note that G_t is itself identified; the coefficient on the year-t dummy reported by our computer software will be a consistent estimate of this gross effect. The identification problem is that, without assumptions, we do not have any way to separate the gross effect for t into its net-of-1997 TANF effect and its "true" trend effect.

To form bounds, we start with the well identified τ_{97}. We then make the plausible assumption that the true trend effect for the years 1998 and 1999 relative to the baseline period 1994–1996 was nonnegative. That is, we assume that, in the absence of welfare reform, income would not have fallen in 1997–1999 relative to 1994–1996. This assumption seems reasonable given the extremely strong economy in the 1997–1999 period. In our model's terms, the assumption is $\beta_t \geq 0$ for $t > 1997$. Thus, the net-of-1997 TANF effect for $t > 1997$ is $\Delta\tau_t \leq G_t$. We can estimate an upper bound on the 1998 and 1999 TANF effects as $\tau^u_{98} \equiv \tau_{97} + G_{98}$ and $\tau^u_{99} \equiv \tau_{97} + G_{99}$. Since we have estimates of all parameters on the right-hand side, we can bound the TANF effects for 1998 and 1999 from above.

V. Empirical Findings

To illustrate the bounding method, Table 1 presents estimates of the model in (1). Our prior research (Bitler et al., 2002) and vast differences in baseline characteristics suggest that impacts of welfare reform may vary by race and ethnicity. Therefore, we estimate separate models for whites, Hispanics, and blacks.[4] The table's first several rows provide estimated year

[3] Robert Schoeni and Blank (2000) compare estimated pre- and post-TANF year fixed effects (by education) in a model excluding reform variables. This approach differs from our bounding method in excluding reform variables with identified coefficients and in interpreting year differences as treatment effects, rather than bounds on these effects.

[4] Our CPS sample includes all children younger than 16 in the first four interview months for survey years 1989–2000. Given changes in CPS top-coding, we drop observations with income in the top 5 percent for each race/ethnicity. Control variables are the age of child and its square; MSA and central-city location dummies; the log of the poverty level applicable to the child's household (an implicit equivalence scale); current and one-year lags of state unemployment and employment growth rates; real state AFDC/TANF guarantee levels for a family of three; and measures of state Medicaid generosity. We date state reforms using information from the web site for the Assistant Secretary for Planning and Evaluation. Further details can be found in Bitler et al. (2002).

TABLE 1—OLS REGRESSIONS OF CHILDREN'S LOG(HOUSEHOLD INCOME) BY RACE AND ETHNICITY

Variable	White	Hispanic	Black
Calendar-Year Dummies:			
Average, 1994–1996	0.017	−0.115	0.151
	(0.038)	(0.075)	(0.108)
1997	0.034	−0.099†	0.277**
	(0.031)	(0.054)	(0.102)
1998	0.051	−0.068	0.366**
	(0.042)	(0.074)	(0.139)
1999	0.127**	−0.040	0.426**
	(0.044)	(0.077)	(0.143)
TANF Effects:			
1997	0.017	0.049	−0.168
	(0.032)	(0.043)	(0.104)
1998 upper bound	0.051	0.096	0.047
	(0.068)	(0.100)	(0.223)
1999 upper bound	0.127	0.123	0.107
	(0.080)	(0.133)	(0.257)
Pre-reform mean	10.575	9.963	9.790
Pre-reform welfare-participation rate	0.072	0.201	0.333
N:	132,290	31,799	25,228

Notes: Standard errors (reported in parentheses) are robust to heteroscedasticity and dependence within state-by-year cells. All specifications are weighted using March CPS psupwgt variable. Economic and welfare reform variables refer to the year preceding the CPS survey year. See footnote 4 for a list of other variables included.

† Statistically significant at the 10-percent level.

** Statistically significant at the 1-percent level.

effects for 1994–1996, 1997, 1998, and 1999 (the excluded year is 1992, the trough of the recession). The next three rows present the 1997 TANF effect and the upper bounds for the 1998 and 1999 TANF effects. To construct the terms G_{98} and G_{99}, we subtract the average 1994–1996 effects from the year effects for 1998 and 1999. The 1998 and 1999 upper bounds are obtained by adding G_{98} and G_{99} to the estimated 1997 TANF effect. For example, the 1998 upper bound for the TANF effect for white children is equal to the 1997 TANF effect plus the 1998 year effect, less the 1994–1996 average year effect: $0.017 + (0.051 - 0.017) = 0.051$.

Before discussing the main results, consider first the degree to which these groups of children are "at risk" of being impacted by welfare reform. At the bottom of the table, we provide the pre-reform (e.g., before implementation of any waivers or TANF) mean of log household income and welfare participation for each group. There are striking differences across group: about one in three black children lived in households with some welfare income, compared to about one in five Hispanic children and only 7 percent of white children. There are three main points to draw from these results. First and most striking are the incredibly large and statistically significant coefficients on the year effects for black children. These year effects suggest that, relative to 1992, household income was 28 percent higher in 1997, 37 percent higher in 1998, and 43 percent higher in 1999. These tremendous, unexplained gains are not present for the other subgroups. White children show positive, though smaller, gains (3 percent in 1997, 5 percent in 1998, and 13 percent in 1999), while Hispanics show declines relative to 1992 (but increases relative to our baseline period of 1994–1996).

The second point is that in the presence of these substantial trends in the post-reform period, the upper bounds for the TANF treatment effects for 1998 and 1999 are quite large. For white children, the 1997 TANF impact is a modest and insignificant 1.7-percent increase in household income. The upper bounds for 1998 and 1999 are larger (5.1 percent and 12.7 percent), though they are estimated imprecisely. The story for Hispanics is similar: a 1997 TANF effect of 4.9 percent and upper bounds of 9.6 percent and 12.3 percent for 1998 and 1999, respectively. The most dramatic results in terms of point estimates are clearly those for black children. The 1997 TANF effect shows a reduction in household income of 16.8 percent. However, using the year effects to create the upper bound for blacks suggests the possibility of a very different story for later years: reform may have led to increases in household income of up to 4.7 percent and 10.7 percent for 1998 and 1999, respectively.

VI. Conclusion

In this paper we point out an identification problem confronting nonexperimental estimates of TANF effects. We develop a method to identify upper bounds on TANF effects for 1998 and 1999, illustrating it with OLS estimates of log household income regressions for a CPS sample of children covering calendar years 1988–1999. Our results suggest that for 1997, TANF is associated with (insignificant) increases in household income for whites and Hispanic children and (again insignificant) reductions in household income for black children. The point estimates for 1998 and 1999 bounds suggest that the impact may have been positive for all three groups. We are currently extending these methods to analyze changes in the income distribution using quantile regression techniques.

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