CONTEMPORARY ECONOMIC POLICY

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LONGER-RUN EFFECTS OF ANTI-POVERTY POLICIES ON DISADVANTAGED NEIGHBORHOODS

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We assess evidence on the longer-run effects of minimum wages, the Earned Income Tax Credit, and welfare on key economic indicators of economic self-sufficiency in disadvantaged neighborhoods. The evidence suggests that the longer-run effects of the Earned Income Tax Credit are to increase employment and to reduce poverty and public assistance. We also find some evidence consistent with higher welfare benefits having longer-run adverse effects, and stronger evidence that tighter welfare time limits reduce poverty and public assistance in the longer-run. The evidence on the longer-run effects of the minimum wage on poverty and public assistance is not robust. (*JEL* J22, J23, J38)

I. INTRODUCTION

The long-running research and policy debates about anti-poverty policies have tended to focus on short-term effects, rather than asking how these policies have affected income, and economic self-sufficiency more generally, in the longer run. In this paper, we attempt to counter this shortcoming. We study the effects of the main anti-poverty policies in the United States that attempt to increase income from work, or that substitute for income from work and hence might strongly affect work incentives—minimum wages, the Earned Income Tax Credit (EITC), and welfare. We estimate the longer-run relationships between these policies and measures of economic self-sufficiency—most importantly, poverty and receipt of public assistance. The underlying potential mechanism we have in mind for differing longer-run effects of these policies is that policies that encourage more work over time will lead to greater accumulation of human capital, and hence higher wages and earnings.

Most research on minimum wages focuses on the short-term employment effects of minimum wages—typically for teenagers (see the review in Neumark and Wascher 2007) and more recently for other low-wage workers, such as restaurant workers (e.g., Dube, Lester, and Reich 2010). This evidence tells us little or nothing about whether minimum wages reduce poverty even in the short term, although that question has begun to get more attention (e.g., Dube 2017; Sabia and Burkhauser 2010). Virtually, no work has studied the longer-run effects of minimum wages, with

*We thank the Laura and John Arnold Foundation and the Employment Policies Institute for support for this research. B.A. gratefully acknowledges funding from the Alfred P. Sloan Foundation (grant G-2017-9813) and the National Bureau of Economic Research (NBER). Any opinions or conclusions expressed are the authors' own and do not necessarily reflect those of the Laura and John Arnold Foundation, the Employment Policies Institute, the Sloan Foundation, or the NBER. The funders have had no control over the content or conclusions of this research. We received helpful comments from Matt Freedman, Hilary Hoynes, Joe Sabia, and from seminar/conference participants at Bar Ilan University, DIW-Berlin, Hebrew University, IZA, the Melbourne Institute, Nanyang Technical University, the National Tax Association Annual Conference, the National University of Singapore, the Norwegian School of Economics, Singapore Management University, the Tinbergen Institute, Tulane, UC-Berkeley, UCI, Université Catholique de Louvain, and the University of Sydney. We are grateful for helpful research assistance from Luis Munguia Corella.

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ABBREVIATIONS

ACS: American Community Survey
AFDC: Aid to Families with Dependent Children
CCTC: Child and Dependent Care Tax Credit
EITC: Earned Income Tax Credit
NCDB: Neighborhood Change Database
SSI: Supplemental Security Income
TANF: Temporary Assistance for Needy Families

Contemporary Economic Policy (ISSN 1465-7287)
Vol. 38, No. 3, July 2020, 409–434
Online Early publication September 29, 2019

doi:10.1111/coep.12445

three exceptions: indirect evidence on training (or education), which could affect earnings in the longer term (e.g., Acemoglu and Pischke 2003); research directly estimating the effects on adult earnings of exposure to a higher minimum wage as a teenager (Neumark and Nizalova 2007); and more recent work by Clemens and Wither (2019) reporting that binding minimum wage increases during the Great Recession period lowered the income growth of affected workers.

Research on the EITC has also focused on short-run employment effects (e.g., Meyer 2010), and some work studies short-run effects on poverty (e.g., Neumark and Wascher 2011). There are just a few, mostly recent, exceptions to the short-run focus. Dahl et al. (2009) examine longer-term effects of the EITC via work incentives, estimating the impacts of a major federal expansion of the EITC on individual women's earnings up to 5 years later.[1] More recently, Neumark and Shirley (2017) study the effects of exposure to a more generous EITC over women's 20s and 30s on subsequent wages and earnings. And taking an intergenerational perspective, Bastian and Michelmore (2018) estimate the effect of exposure in childhood on adult outcomes, finding positive employment and earnings effects; they suggest that these results are driven by labor supply (and hence earnings) impacts on parents.

The EITC is sometimes viewed as a more effective policy than the minimum wage to increase income from work, in large part because it incentivizes work. This question can be revisited in the longer-run perspective we adopt in this paper, recognizing the possibility that the EITC could also have limited effectiveness in economically disadvantaged areas if there are not employment opportunities to be taken advantage of by those induced to look for work by a more generous EITC.

Finally, although the literature on welfare is extensive and has studied both employment effects (e.g., Grogger 2003) and distributional effects (e.g., Bitler et al. 2006), there is very little work on longer-run effects. Two exceptions are Grogger (2009) and Hotz et al. (2006), who study whether welfare programs that encouraged employment (and in the latter case, training) boosted longer-run earnings. Moreover, the question has been raised of whether welfare generates longer-run dependency on government programs (e.g., Murray 1984).

To obtain evidence on the longer-run effects of the anti-poverty policies we study, it would be ideal to have very long-term longitudinal data on many cohorts of individuals and families. However, such data are not available in the United States (except, in principle, if one could link tax data over many decades).[2] We do, however, have long-term longitudinal data on small geographic areas—Census tracts—which can be consistently observed over time in the U.S. Census and in later years in the American Community Survey (ACS). We study the longer-run effects of anti-poverty policies on the disadvantaged by estimating the longer-run relationships between these policies and socioeconomic outcomes in the most-disadvantaged tracts relative to other tracts.

Our working definition of "longer run" in this study is 10 years.[3] This is a significant difference from the contemporaneous effects emphasized in most research on anti-poverty policies. Relationships over periods even longer than 10 years are of potential interest, but the timing of much of the variation in the policies we study precludes reliably estimating these relationships over two or three decades. In addition to looking at longer-run relationships over many decades, our paper is distinguished by simultaneously examining multiple anti-poverty policies, which provides direct comparisons of their relationships with longer-run outcomes and helps ensure that we do not spuriously attribute the potential effects of one policy to the effects of others.

To briefly summarize the results, our strongest findings are twofold. First, the evidence suggests that the longer-run effects of the EITC are to increase employment and to reduce poverty and public assistance, as long as we rely on national and state variation in EITC policy. Second, tighter welfare time limits also appear to reduce poverty and public assistance in the longer run; while the relationship with public assistance result may be mechanically related to loss of benefits, the relationship with poverty is more

1. Card and Hyslop (2005) study longer-term effects of a similar program in Canada. There is also some research tying the EITC to longer-term outcomes via effects on children. For a review of related work, see Neumark (2016).

2. The one exception is the Panel Study of Income Dynamics, which Neumark and Shirley (2017) use to estimate the long-term effects of exposure to a more generous EITC. However, this yields quite small long-term longitudinal samples, and has other limitations.

3. Some of the papers discussed above define different windows for longer-run effects, usually dictated by the data. The same is true for us, as the data we use provide decadal measurements.

likely behavioral. We also find some evidence that higher minimum wages, in the longer run, may lead to declines in poverty and the share of families on public assistance, whereas higher welfare benefits appear to have adverse longer-run effects, although the evidence on welfare benefits and especially on minimum wages is not always robust to using only more recent data, nor to some other changes we consider.[4]

We want to be clear, at the outset, that identifying longer-run effects of policy—especially of multiple policies—is a challenge. We may rarely, if ever, have the kinds of compelling identification strategies sometimes available to study the short-term effects of a single policy. In contrast, the most feasible and convincing approach is likely what we have carried out in this paper—combining differencing strategies with a detailed look at potential threats to identification, and at the sensitivity of the conclusions to sensible alterations in the sample, the specification, etc. Nonetheless, even being cautious about a causal interpretation of our findings, we believe that the longer-term relationships we document provide interesting suggestive evidence about the likely longer-run effects of alternative anti-poverty policies.

II. RESEARCH STRATEGY

Our econometric strategy is to use long-term panel data to obtain evidence on the longer-run relationships between anti-poverty policies and economic outcomes in Census tracts that are initially disadvantaged, relative to other tracts. To explain the approach, denote tracts by c, states by s, counties by j, and years by t. Denote by Y_{cst} our economic outcomes; we focus on the poverty rate and the share of households on public assistance, but study other outcomes as well.[5] Denote by P_{cst} a vector of policies that can vary by state and year (and very modestly by county). And denote by DIS_c^b a measure of initial disadvantage defined at the tract level; DIS_c^b is a dummy variable indicating that a tract was in the top quartile of a measure of socioeconomic disadvantage (i.e., the most-disadvantaged quartile) in the baseline period (b). For most of our analysis, we focus on disadvantage defined by the share of the population in poverty, although we also explore different measures of disadvantage (based on education, race, and single motherhood).

As control variables, we also include Census tract fixed effects (CT_c), to account for time-invariant heterogeneity across Census tracts. We also include a full set of interactions between county dummy variables (CO_j) and year fixed effects (YR_t). These interactions control very flexibly for local shocks to economic outcomes that could potentially be correlated with the policy variables. We cannot, of course, include tract-by-year interactions because these would capture all of the variation in the dependent variables.

We also always include a control for the potential effects on tracts of long-term changes in the structure of jobs in the aggregate economy.[6] We use the approach, originating with Bartik (1991), of applying national time-series changes in aggregated sectoral employment to the tract or other subareas, based on the tract's or subarea's sectoral composition in the baseline period. While it is most natural to think of this in terms of industry, in the Neighborhood Change Database (NCDB) we use, we can only do this for occupation.[7] To define this control variable, let subscript k index occupations. Denote by SE_{cskl} total employment in tract c, state s, occupation k, and baseline period l (which for each year t is the 10-year lag), denote by AE_{kt} aggregate (national) employment in each period t in occupation k, and denote by AE_{kl} aggregate employment in occupation k in the baseline period l. Then tract (or subarea) employment based solely on aggregate developments is predicted in each period after l

4. Note that the evidence on welfare benefits does not imply that more generous welfare benefits do not help recipients, but rather that more generous benefits may reduce the extent to which these recipients become economically self-sufficient.

5. Aside from policy concerns, our focus on poverty and public assistance is motivated by data limitations. As discussed below, the data we use provide tract-level aggregates. Although we also estimate effects on average earnings and employment rate measures, we cannot, for example, estimate effects on earnings and employment of separate groups (such as single mothers) to better understand the estimated effects on family-level outcomes such as poverty. In future work using microdata we may do more to unpack the effects on these outcomes for subgroups.

6. For example, it is widely agreed that declines in manufacturing hit narrow areas where manufacturing was concentrated (think the South Side of Chicago, or Flint, Michigan), as highlighted in the seminal work of Wilson (1990) or Autor, Dorn, and Hanson (2013).

7. The NCDB does not provide tract-level information on the number of persons working in a specific industry. Instead, it includes employment in nine categories of occupations (for persons 16+): professional and technical occupations; executives, managers, and administrators; sales; administrative support and clerical; precision production, craft, and repair; operators, assemblers, transportation, and materials; nonfarm laborers; service; and farm workers or forestry and fishing.

by applying the national changes to the baseline composition, as in

$$(1)\ PSE_{cst} = \sum_k SE_{cskl} \times \left(\frac{AE_{kt} - AE_{kl}}{AE_{kl}} \right). \quad [8]$$

For our baseline analyses, we extract from the NCDB measures of Y for 1980, 1990, 2000, and 2010, and measures of DIS^b for 1970.[9] We specify our model to estimate the longer-run impacts of the anti-poverty policies in P on initially disadvantaged tracts relative to other tracts. This is a "substitute" for directly estimating effects on initially disadvantaged individuals and families, which the data precludes; the assumption underlying the strategy, of course, is that initially disadvantaged tracts have much higher concentrations of initially disadvantaged individuals and families. The model we estimate is

$$\begin{aligned}(2)\qquad Y_{cst} = {} & \alpha + \{DIS_c^b \cdot P_{cst}\} \cdot \beta \\ & + \{DIS_c^b \cdot P_{cs,t-10}\} \cdot \beta^L \\ & + CT_c \cdot \delta' + \{CO_j \cdot YR_t\} \cdot \omega \\ & + \theta \cdot \ln(PSE_{cst}) + \varepsilon_{cst}.\end{aligned}$$

In Equation (2), the parameters β and β^L capture the contemporaneous and 10-year lag relationship between the outcomes we study and the policies in P. The main effects of policy variation on non-disadvantaged tracts (with $DIS^b = 0$) are subsumed in the county-by-year fixed effects, because policy generally varies at the state level (and very slightly at the county level, which we have coded to the tract level, and which is why P has a c subscript).[10,11] The estimates of β and β^L capture the relative change in Y in disadvantaged tracts, versus more-advantaged tracts, that are associated with the policy variation P.

Considered this way, β and β^L are akin to triple-difference parameters. A basic triple-difference specification (ignoring the minor county variation in welfare benefits) would include main effects of the policy variables (not interacted with the indicator for disadvantaged tracts), year fixed effects, state fixed effects, the indicator for disadvantaged tracts, and interactions between this indicator and the year and state fixed effects. Many of these are subsumed in the richer control variables in Equation (2). Specifically, including the county-by-year fixed effects subsumes main policy effects, and the tract fixed effects subsume the indicator for disadvantaged tracts as well as the interactions between this indicator and state fixed effects. The model would still include the year-by-disadvantaged tract interactions, which would imply that the effects of policy would be identified only from state-level variation; for example, differential effects of federal EITC variation in disadvantaged relative to advantaged areas, common to all states, would be absorbed in these interactions. It turns out, however—as we show later—that the estimated relationships with the EITC are extremely imprecise when we include the year-by-disadvantaged tract interactions, owing to relatively little state variation. Hence, our main estimates do not include these interactions, and rely on federal as well as state EITC variation.[12] However, the inclusion of the county-by-year

8. This control is entered in logs because the level can differ so much across tracts. Although not reported in the tables that follow, the estimated coefficients of this control variable on earnings, employment, and poverty are of the expected sign (increasing employment and earnings, and reducing poverty) and statistically significant. The estimated coefficient on public assistance receipt is positive, rather than negative—not in the most obvious expected direction, although to be fair we are not aware of studies that use a Bartik control except to look at employment-related outcomes. One possible reason for the public assistance result is that the Bartik control does not necessarily imply that higher-wage jobs grew, and there may have been changes in public assistance that made it more likely to get public assistance while working at low-wage jobs (most notably, welfare reform). This is consistent with what we find in separate models by sex, where the positive effect of the Bartik variable on employment is nearly twice as large for women as for men. We also verified that the results are very robust to excluding this control.

9. DIS^b can be measured for later years, but we work with 1970 as our baseline in almost all of our analyses. As explained in the data section below, the 2010 measures are actually 2006–2010 measures based on the ACS.

10. Welfare benefits have a limited degree of within-state, cross-county variation in a handful of states (as discussed below).

11. The inclusion of the county-by-year interactions and the focus on estimating the effects on most-disadvantaged tracts is related to recent work on the identification of minimum wage effects (Allegretto, Dube, and Reich 2011; Dube et al. 2010; Neumark, Salas, and Wascher 2014). This work highlights the question of whether region-specific economic shocks are correlated with minimum wage changes. However, when the county-by-year interactions are included, identification of β and β^L comes solely from within-county and year variation, and the bias from potential correlations between state-level or county-level economic conditions and the (possibly endogenous) variation in minimum wages or other policies at the state or county level is eliminated.

12. The key papers in the EITC literature—establishing positive employment effects for low-skilled mothers—also use federal variation (Eissa and Liebman 1996; Meyer and Rosenbaum 2001). The same is true of the longer-term analyses discussed in the Introduction.

interactions controls for geographic heterogeneity in shocks at a very granular level.

Aside from the controls we have discussed, the main model we use is parsimonious. It may be most appropriate not to control for other characteristics of the population that may have changed over time (such as educational levels), because skill levels may be endogenous (e.g., Agell and Lommerud 1997). On the other hand, there may be differential migration responses to policy in more- versus less-disadvantaged areas, and insofar as our strategy is to obtain evidence on effects on individuals and families from variation in outcomes across geographic areas, it might be important to control for these compositional changes. We present estimates of models that allow for compositional shifts, and find that the results are robust.

Naturally, to assess the robustness of our results and to gauge potential sources of bias, we also estimate other variants of our specification. We discuss these modifications of our analysis in the empirical section of the paper.

III. DATA

A. *Neighborhood Change Database*

Our data on economic outcomes and other measures by tract come from the NCDB,[13] which provides tract-level aggregates on the key outcomes we study—earnings, employment, poverty, and public assistance.[14] Importantly, the NCDB provides consistent tract definitions over time. In particular, it includes historical tract populations, demographic characteristics, and socioeconomic characteristics in 2010 Census tract geography, providing consistent longitudinal measures of these variables.[15]

The NCDB includes data from the 1970, 1980, 1990, and 2000 Census, and from the five-year rollups of the 2006–2010 ACS (which we sometimes refer to as "2010").[16] The NCDB excludes some variables otherwise publicly available from the Census (in the "Summary Files"), such as crosstabs on education by employment status by age group. However, these crosstabs were not published by the Census for 1970, and the age ranges that are reported change in each Census wave, making reconciling them longitudinally difficult. Thus, the NCDB remains the best public data set for this analysis.[17]

Because we estimate models covering many decades, based on characteristics of tract residents in a much earlier period (using 1970 as our baseline period to define DIS^b, for our baseline analysis), we are restricted in the set of tracts we can use. The 1970 NCDB data set is comprised exclusively of counties surrounding population centers, because Census tracts were still in the process of growing out of their original usage for measuring health and sanitary conditions at the neighborhood level in urban areas (Krieger 2006).[18] The practical implication is that when we use 1970 as our base year for measuring disadvantage, we drop mostly rural and suburban

13. For a description of the data, see http://www.geolytics.com/USCensus,Neighborhood-Change-Database-1970-2000,Products.asp (viewed February 13, 2017).

14. From 1970 to 2000, the measure of public assistance in the NCDB (which comes from the decennial Census) includes Supplemental Security Income (SSI), AFDC/TANF, and General Assistance (sometimes called General Relief)—which usually refers to programs that provide income support to adults without dependents. For the period ending in 2010, the NCDB data come from the ACS (as explained more below); the ACS definition of public assistance includes TANF and General Relief.

15. The NCDB counts are reallocations of the Census' reported counts, using a combined area and population approach. Areal weights are determined from publicly available maps for all recent Census geography so that it is possible to calculate the area overlay between tracts in different Census years. To account for the uneven distribution of population within a tract, the NCDB exploits sub-tract geographic units, called Census blocks, which first exist nationwide in the 1990 Census. Census blocks are not standardized by population, but their decennial population counts are known and Census block boundaries never cross tract boundaries within the same Census year. These Census blocks form the basis for more precisely mapping populations across Census years and then aggregating the results to the tract level. More details on how the population reapportionment occurs at finer geographic levels and is reconciled across Census waves can be found in Tatian, Hayes, and Zhang (2003).

16. Specifically, the 1970 data come from the Fourth Count Summary Tape for Population and Housing; the 1980 and 1990 data come from the Summary Tape Files 3A of their respective years; the 2000 data come from the Summary File 3A and Summary File 1; and the 2010 data come from the Summary File 1.

17. One key advantage of using the NCDB is that the data are publicly available, and the analysis therefore can be replicated and explored further by other researchers. The minimum wage literature, in particular, is replete with exchanges, comments, and replications of the work of others, and in our view these exchanges and sharing of data have been a critical part of the research endeavor and central to the high level of transparency to which researchers on all sides of the minimum wage debate have contributed.

18. The Census first fully tracted the nation in 2000 (Krieger 2006). In 1990, the Census had tracts in all 50 states plus Puerto Rico and U.S. outlying territories, but had only fully tracted six states: California, Connecticut, Delaware, Hawaii, New Jersey, and Rhode Island. Prior to that, Census tracts were only drawn for large cities (U.S. Department of Commerce, Bureau of the Census 1994).

tracts. Appendix Table A1 shows that the NCDB sample is still reasonably representative of 1970 national averages. The shares black, in poverty, employed, and receiving public assistance are virtually indistinguishable between the NCDB and the decennial Census data. The share of families headed by a single mother is about twice as high in the NCDB sample, and earnings are about 20% higher, both of which are attributable to economic and cultural differences between urban and rural areas. Thus, although the data from 1970 Census tracts are representative of many features of the U.S. population, these differences suggest that the results may not fully generalize to rural areas (and there could be other reasons policies have different impacts in rural areas). We also present a sensitivity analysis where we start with 1980 as the baseline year, thus incorporating more tracts into our analysis.

The longer-run perspective of our research makes it useful to have data covering many decades, and our specifications include 10-year lags, so that the first sample year we can use with the NCDB is 1980. Although some of the relevant policy variation goes back to before 1940 (the minimum wage was created by the Fair Labor Standards Act in 1938), Census tracts can only be identified in a small subset of areas for 1940 and 1950.[19] Thus, only beginning in 1960 can one use any Census data at the tract level to obtain a comprehensive look at the United States as a whole, but to date, the 1960 Census is not included in the NCDB.

With regard to the policies we study, the inability to use the earlier years is not much of a disadvantage. Most of the variation in the federal minimum wage, and all of the variation in state minimum wages, occurred much later—most federal variation after 1960, and state variation beginning in the late 1980s, and coverage of workers by the federal minimum wage was not very broad until the beginning of the 1960s.[20] The other policies we study arise and begin to vary later—welfare in the 1960s and again with welfare reform in the 1990s, and the EITC at the federal level in the 1970s and at the state level in the 1980s. Thus, the constraint of starting our analysis in 1980 (with DIS^b measured in 1970) is not too limiting.

B. Anti-Poverty Policies

We study the potential effects of minimum wages, the EITC, and welfare. We view these three policies as the intersection of the policies most central to anti-poverty efforts, and the policies most likely to affect income and work.[21]

Information on state minimum wages from 1983 to 2014 was taken from the data used in Neumark et al. (2014). We extended the data back to 1960 relying on Quester (1981) and Sutch (2010),[22] also cross-referencing dates and levels against state and federal sources.[23] We code the minimum wage as the higher of the state or federal minimum wage, as is standard, because

19. Census tract coverage and publicly available information prior to 1960 is limited. Only 45 cities were consistently given Census tracts before 1960 (Bogue 2000a, 2000b, 2000c).

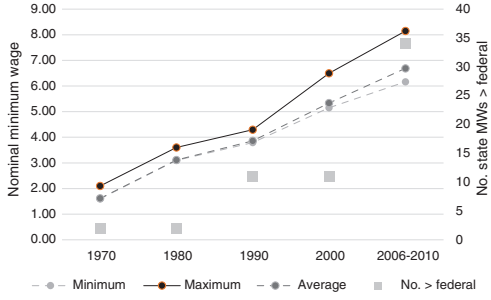
20. See http://www.dol.gov/whd/minwage/coverage.htm (viewed February 13, 2015) and http://www.dol.gov/whd/minwage/chart.htm (viewed February 13, 2015).

21. Four substantial programs we do not consider are the Supplemental Nutrition Assistance Program (SNAP, previously Food Stamps), the Child and Dependent Care Tax Credit (CCTC), child support, and Medicaid. In general, we view these programs as less directly related to work incentives (and even less so to affecting income from work), and some have less variation. For SNAP/Food Stamp benefit levels, only Alaska and Hawaii differ from federal guidelines. Prior to July 1, 1974, there was some spatial variation through rollouts of when Food Stamps became active, although most urbanized, predominantly low-income, and high black share counties had the program by 1970 (Almond, Hoynes, and Schanzenbach 2011), which severely limits variation in SNAP/Food Stamp benefits (in this case based on rollout). Since welfare reform in the 1990s, SNAP/Food Stamps has had work, search, or training requirements, and hence potentially affects work incentives through those mechanisms, although limited research suggests it does not, but is instead largely a supplement to wages (for those able to work); see Rosenbaum (2013) and Moffitt (2015). The CCTC is a non-refundable credit, unlike the EITC, and hence is thought to provide weak benefits to low-income families for whom the question of economic self-sufficiency is most salient. Child support is the financial support paid by parents to support a child or children of whom they do not have full custody. The Child Support Enforcement Amendments of 1984 required states to adopt numerical child-support guidelines. States can set their own child support guidelines, and decide what type of formula to use in determining income to establish the amount of the child support owed. While child support could affect labor supply decisions, this is likely a secondary impact. Moreover, child support is not targeted on the disadvantaged. Medicaid has provided health coverage for low-income populations since 1965. It is a federal mandate, with program parameters chosen by the states. Medicaid may influence labor force participation, since eligibility is graduated by income, but there is not strong evidence that Medicaid disincentivizes work (Baicker et al. 2014; Meyer and Rosenbaum 2001; Strumpf 2011).

22. The main information in the latter is in the appendix of the working paper, at http://www.nber.org/data-appendix/w16355/Appendix%20A%20State%20Laws.pdf (viewed February 15, 2017).

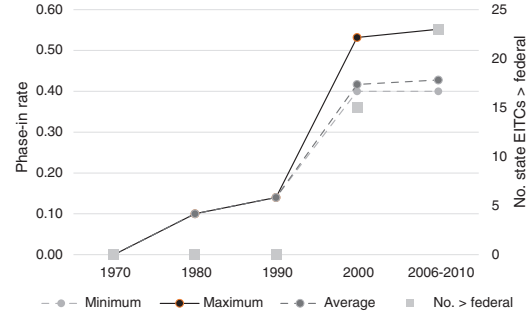
23. If there was a conflict between sources, we chose the information in Quester (1981), to maintain consistency when constructing the panel.

FIGURE 1
State-Level Minimum Wage Variation (Nominal)



Note: The minimum, average, and maximum minimum wage (measured on the left-hand axis) are shown for the decennial Census years and the average over the 5 years corresponding to the ACS data. The minimum values measure the federal minimum wage. The gray boxes indicate the number of states with a minimum wage above the federal level (measured on the right-hand axis). Data on state-level minimum wage variation come from Neumark et al. (2014), Quester (1981), and Sutch (2010).

FIGURE 2
State-Level EITC Variation (Phase-in Rate, Two Children)



Note: The minimum, average, and maximum EITC phase-in rate (measured on the left-hand axis) are shown for the decennial Census years and the average over the 5 years corresponding to the ACS data. The minimum value measures the federal EITC. There was no EITC in 1970, and no state variation until after 1990. Information on the EITC comes from a database of historical parameters maintained by the Tax Policy Center.

lower state minimum wages, if they exist, apply to a tiny fraction of workers. In the analysis, we lag the minimum wage 1 year for all outcomes except employment, because in the Census data these outcomes are measured in the previous year; we do the same for the other policies, for the same reason. Finally, we use the log of the minimum wage.[24]

Figure 1 shows the minimum, average, and maximum minimum wage (measured on the left-hand axis); the minimum values measure the federal minimum wage. The gray boxes indicate the number of states with a minimum wage above the federal level (measured on the right-hand axis). As the figure indicates, this latter number is trivial early in the sample, but the number of states with higher minimum wages rises sharply in the 2000s, to over 30.

Information on the EITC comes from a database of historical parameters maintained by the Tax Policy Center.[25] We use the percentage supplement in the federal EITC for a family with two children on the phase-in range (*F2%*), which can be amplified by the state EITC, usually specified as a percentage supplement to the federal EITC (*S%*). Thus, our combined variable is $F2\% \cdot (1 + S\%)$, where *F2%* and *S%* are measured on a scale from zero to one.[26]

Figure 2 shows the EITC variation, displayed in a similar way as Figure 1. There was no EITC in 1970, and no state variation until after 1990. By the end of the sample period 23 states had an EITC supplement, and the maximum supplements increase the phase-in rate by over 15 percentage points.

We include two measures of welfare generosity or stringency. From 1962 to 1996, the U.S. joint federal and state social assistance program was known as Aid to Families with Dependent Children (AFDC). The program was reformed by Congress in 1996 and rebranded as Temporary Assistance for Needy Families (TANF). Our first measure is the maximum payment for a family of three, usually held to be one adult and

24. We use real ($2014) minimum wages (and do the same for earnings), although in the regression model, with the log transformation and year effects, the deflator is irrelevant. Historically, there has been some debate in the research literature over whether to define the minimum wage relative to an average wage measure. In recent work, this approach has fallen out of favor, and the log of the minimum wage is used instead. The data on minimum wages can be accessed at http://www.socsci.uci.edu/~dneumark/datasets.html (viewed February 15, 2017).

25. See http://www.taxpolicycenter.org/sites/default/files/legacy/taxfacts/content/PDF/historical_eitc_parameters.pdf (viewed October 11, 2016).

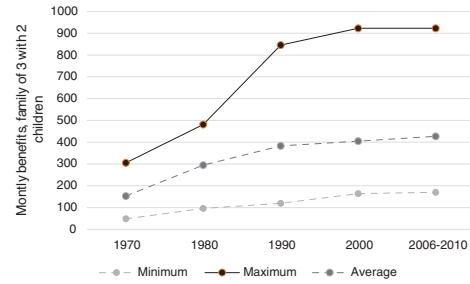
26. State credits are fully refundable (as is the federal credit), except for Delaware, Maine, Rhode Island, and Virginia. This would suggest that our estimates could slightly understate the effects of refundable credits.

two dependent children.[27] Second, for the post-welfare reform period, we include a dummy variable for whether tight time limits were imposed. There were no time limits until welfare reform in 1996, after which 10 states adopted limits of less than 60 months (in 2000, ranging from 21 to 48 months, but generally about 2 years), and most of the remaining states adopted time limits of 60 months. We use a time limit dummy variable that is equal to zero for all states before welfare reform, and, after welfare reform, switches to one for states that imposed tight time limits (less than 60 months), to capture states that more substantially tightened eligibility for welfare.[28,29]

All information on TANF comes from the Urban Institute's Welfare Rules Database.[30] For AFDC, various sources were utilized.[31] Some states had benefit amounts that varied by subarea. Only for Illinois, Louisiana, Vermont, and Virginia were the regional benefit levels and geographies reported with enough consistency to reconstruct their longitudinal series, and even then, we had to fill in missing years.[32] For the remaining states with region-specific benefit amounts, in most cases the publications reported the highest payment amount across regions, and this is what we used. However, in a few cases the publications did not consistently state which region or amount they were reporting, so we could be overstating or understating the benefit amount in certain years.

Figures 3 and 4 display information on the two welfare measures we use. Figure 3 graphs nominal benefit levels. There is substantial variation across states. Figure 4 displays information on time limits.

FIGURE 3
Welfare Benefits (Nominal) for Family of Three



Note: The minimum, maximum, and average monthly nominal welfare benefits for a family of three are displayed for the decennial Census years and the average over the 5 years corresponding to the ACS data. All information on TANF comes from the Urban Institute's Welfare Rules Database. For AFDC, various sources were utilized.

C. *Measuring Disadvantage*

For our main analyses, we measure disadvantage as the share of the population living in poverty in the baseline year. However, we also show key results for three alternative measures of disadvantage: the share of the population with a high school degree or less; the share of the population that is black; and the share of families headed by single mothers.[33] The results are generally robust.

27. We are typically able to measure benefits this way, but in some cases, we can only determine the level of benefits for a family of two. We always use the former when possible.

28. We also explored distinguishing between states that imposed tighter time limits and those that imposed limits of 60 months (versus none), although the results were not affected.

29. To be sure, there are many possible measures of welfare reform one could use (Fang and Keane 2004). However, including many measures would be problematic because of multicollinearity, perhaps especially in our framework. Time limits seem like a good choice to capture the effects of welfare reform. A small but consistent literature has shown that welfare time limits were a significant element of welfare reform distinguishing TANF from AFDC (Moffit 2007), and that they were responsible for decreasing welfare caseloads (e.g., Grogger 2009).

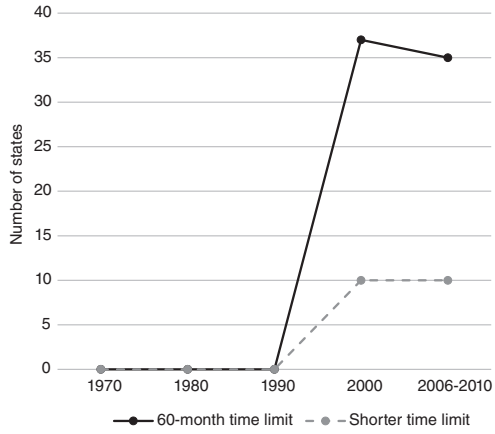
30. See http://wrd.urban.org/wrd/Query/query.cfm (viewed February 16, 2017).

31. U.S. Department of Health and Human Services (1973) publications (Characteristics of State Plans [various years]) provided program parameters for 1973–1976, 1978–1985, and 1988–1990. For 1994 and 1996, program parameters came from U.S. House of Representatives publications (Green Book [various years]). For 1969 and 1970, publicly available information was incomplete. The U.S. Department of Health, Education, and Welfare (n.d.) publications on selected state maximum welfare payments were used where available. For program parameters for years with missing data, the annualized growth rate between the two observed years that bracketed the missing year or years was calculated, and the benefit amount for the missing year or years was assumed to equal the previous year's amount times one plus the annualized growth rate.

32. For these states, in years where the publications indicated that there was regional variation in benefit amounts but did not report them, we used the following method to estimate the missing amounts. First, if for a year t with missing data, years t-1 and $t + 1$ were observed and were the same, then year t was assumed to be the same as those years. If only one region's amount was reported, we assumed the yearly growth rate was the same across regions, and extrapolated to the missing year/region on that basis. For years where no region-specific amounts were reported or specified, we used documents from the next year forward and used implied growth rates between known years to interpolate the missing amounts.

33. This is the share of families and subfamilies. While tracts with large Hispanic populations are also of interest and likely, on average, to be disadvantaged, Hispanic ethnicity has not been measured consistently over the long time span we study.

FIGURE 4
Welfare Time Limits



Note: The number of states with a 60-month welfare time limit and the number of states with less than a 60-month welfare time limit (shorter time limit) are displayed for the decennial Census years and the average over the 5 years corresponding to the ACS data. There were no time limits until welfare reform in 1996. All information on TANF comes from the Urban Institute's Welfare Rules Database.

Figure 5 provides information on the geographic distribution of tracted areas as of 1970, and of our disadvantage measure. The figure shows areas tracted in 1970, with differential shading for tracts in the four quartiles of the share disadvantaged—based on the share in poverty. The darkest shading is for the highest quartile of this share—that is, the most disadvantaged tracts. As the figure shows, a small geographic area was tracted; however, the tracted areas include most of the U.S. population.[34]

IV. RESULTS

A. Descriptive Statistics

Table 1 reports descriptive statistics. Recall that these are means across tracts, not individuals or households. The top panel reports means (and standard deviations) for the outcomes we study. The earnings variable is average earnings per household, which we construct in the NCDB from data on earnings per household with workers, and the computed share of households with earnings. The employment rate is simply the employment-to-population ratio at the tract level. The poverty rate measure is on a per person rather than per household or per family basis. Both track U.S. statistics closely, despite being tract-level observations.[35]

The share on public assistance is lower than the poverty rate (although it is a per household measure, and the poverty rate is lower at the family or household than at the individual level). It drops sharply in the final years of the sample (the 2006–2010 period covered by the ACS) because SSI is excluded in the ACS data. This change should not influence our results materially, because the definitional change should be captured in the year effects (including interactions) that are included in the model;[36] moreover, we find that results are very similar for poverty and for public assistance—and the change in the data affects only the latter.

The second panel reports descriptive statistics for our four outcome measures for the disadvantaged tracts. As we would expect, earnings and employment are lower, and poverty and the share on public assistance are higher. The bottom panel reports the disadvantage measure for 1970. We report the mean as well as the 75th percentile of this measure; the latter is the cutoff for defining DIS^b.

B. Baseline Regression Results

We report baseline results for earnings, employment, poverty, and public assistance in Table 2.[37] The table reports estimates of Equation (2), reporting the estimates of the

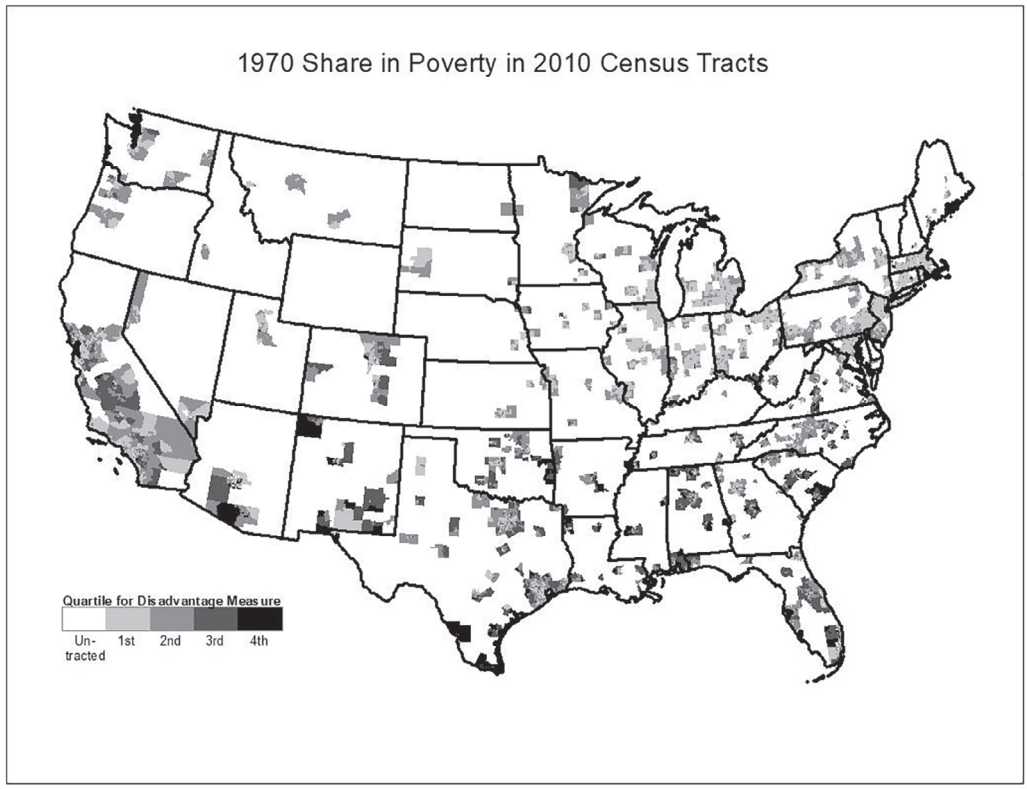
34. The sum of the tracted population in 1970 was 148,456,474 (found from the NCDB) against a total U.S. 1970 population of 203,302,031 (U.S. Department of Commerce, Bureau of the Census 2002), or 73% of the U.S. population. Using the same sources, the figures are 80% and 99.99% of the population for 1980 and 1990, respectively.

35. See, for example, https://data.bls.gov/timeseries/LNS12300000 (viewed February 16, 2017) and http://www.census.gov/data/tables/time-series/demo/income-poverty/historical-poverty-people.html (viewed February 16, 2017).

36. The table does not show a decline from 1990 to 2000. While AFDC/TANF rolls declined over this period, participation in SSI grew by an amount that offsets a large share of this decline (see, e.g., Figure IND 4, https://aspe.hhs.gov/report/welfare-indicators-and-risk-factors-fourteenth-report-congress, viewed November 29, 2017).

37. There are small numbers of tracts or years with missing data on some outcomes; for example, a tract may have missing employment information in 1980, but complete earnings, poverty, and public assistance information in that year. Rather than restrict to a balanced panel, we use all the data we can. If we used a balanced panel across outcomes, we would have 204,740 tract-by-year observations (for 51,185 tracts) for each outcome, about 2,000 fewer than in each of the columns in Table 2. Results are very robust to using the balanced panel (results available upon request).

FIGURE 5
1970 Disadvantage by Tract, Based on Share in Poverty, for Areas Tracted in 1970



Note: Figure shows the geographic distribution of tracted areas as of 1970 for the share in poverty with differential shading for tracts in the four quartiles of the share in poverty, based on the NCDB data. The darkest shading is for the highest quartile of this share—that is, the most disadvantaged tracts. Information on tracted areas in 1970 for the share in poverty comes from the NCDB.

coefficients β, the contemporaneous relationship, and β^L, the longer-run relationship, on the variables $DIS_c^b \cdot P_{cst}$ and $DIS_c^b \cdot P_{cs,t-10}$, respectively. The estimates of the longer-run relationships answer the question, "Conditional on the current policy, how different are contemporaneous outcomes when the current policy has been in place longer?" That is, what is the potential longer-run effect of the policy?

The estimates for average household earnings are in column (1). There is a positive but statistically insignificant contemporaneous relationship between minimum wages and earnings, but the 10-year lag relationship is negative and statistically significant (and of offsetting magnitude). Because earnings and the minimum wage are measured in logs (as are the EITC and welfare benefit variables), the estimated coefficients can be interpreted as the elasticities with respect to the minimum wage in the most-disadvantaged (high-poverty) tracts, relative to other tracts. This way of specifying the model allows comparisons with minimum wage-earnings elasticities reported in other studies—although typically these other estimates are for low-skilled individuals and focus only on short-term, contemporaneous effects. Contemporaneous wage elasticities in the 0.1–0.2 range are not uncommon (e.g., Allegretto et al. 2011).[38] The 0.12 estimate in column (1) is within this range.

There is statistically significant evidence of a positive short-run relationship of the EITC with earnings, but no evidence of a longer-run effect.

38. There are exceptions. In a recent study of the Seattle minimum wage, Jardim et al. (2017) find negative effects on earnings.

TABLE 1
Descriptive Statistics on Outcomes and Tract Characteristics

	1980 (1)	1990 (2)	2000 (3)	2006–2010 (average) (4)
Panel I: Outcomes				
Average earnings per household (2014 dollars)	53,392.6	62,475.5	67,079.5	67,332.7
	(21,574.8)	(30,470.8)	(33,678.9)	(36,582.2)
	[51,643]	[51,762]	[51,798]	[51,449]
Employment rate, male and female civilians aged 16+	59.5	62.4	60.5	61.9
	(10.7)	(11.4)	(11.2)	(11.2)
	[51,660]	[51,792]	[51,820]	[51,529]
Employment rate, female civilians aged 16+	48.6	54.8	54.6	56.5
	(10.4)	(11.3)	(10.8)	(11.4)
	[51,655]	[51,788]	[51,813]	[51,497]
Employment rate, male civilians aged 16+	71.7	70.8	67.2	67.8
	(12.3)	(12.5)	(12.7)	(13.4)
	[51,658]	[51,790]	[51,819]	[51,520]
Share of population in poverty	11.2	12.4	12.7	14.6
	(10.5)	(12.4)	(11.7)	(13.0)
	[51,648]	[51,772]	[51.801]	[51,463]
Share of households on public assistance	7.52	7.56	8.28	2.82
	(8.19)	(8.51)	(8.17)	(3.67)
	[51,643]	[51,762]	[51,798]	[51,449]
Panel II: Outcomes: most-disadvantaged tracts (share in poverty, 1970)				
Average earnings per household (2014 dollars)	36,862.3	43,264.4	48,935.5	50,186.3
	(15,404.8)	(21,745.5)	(25,220.8)	(29,227.3)
	[12,969]	[12,974]	[12,971]	[12,846]
Employment rate, male and female civilians aged 16+	52.4	54.9	53.8	56.6
	(11.0)	(12.9)	(12.8)	(12.9)
	[12,970]	[12,978]	[12,980]	[12,826]
Share of population in poverty	22.4	24.6	23.0	24.2
	(13.5)	(16.0)	(14.8)	(16.1)
	[12,970]	[12,978]	[12,974]	[12,848]
Share of households on public assistance	14.8	14.7	14.5	4.49
	(11.8)	(12.2)	(11.1)	(5.22)
	[12,968]	[12,974]	[12,971]	[12,846]
Panel III: Measures of disadvantage, 1970				
	Mean (SD)		75th percentile	
Initial share in poverty	10.9		13.7	
	(9.72)			
	[51,923]			

Notes: Table reports means for tract-level measures. Standard deviations are shown in parentheses. Sample sizes are shown in brackets. The statistics in Panel I are for the samples used for the respective outcome's regression. The public assistance definition excludes SSI for 2006–2010.

As discussed below, there is also evidence of a positive longer-run relationship of the EITC with employment. The absence of an earnings impact may reflect the fact that the EITC increases labor supply, which can depress market wages (Leigh 2010). Finally, there is no evidence of statistically significant longer-run (or contemporaneous) relationships of either welfare benefit levels or time limits with average household earnings.

Results for the employment rate are reported in column (2). There is evidence of a positive relationship with minimum wages in the short run, with an implied elasticity of 0.23. This result contrasts with a good deal of evidence in the broader literature on the employment effects of minimum wages. But the estimates here are for different groups—residents of tracts that were disadvantaged many decades back—as opposed to the low-skilled and usually very young workers considered in most of the minimum wage literature. The longer-run estimated employment relationship is negative but not significant (with an elasticity of −0.11).

We find a positive longer-run relationship of the EITC with employment, with an implied elasticity of 0.026. We find no statistical evidence of a contemporaneous relationship. The point estimate of the contemporaneous impact is

TABLE 2

Effects of Anti-Poverty Policies on Average Earnings per Household, Employment Rate, Share in Poverty, and Share on Public Assistance in Areas with High Share Poverty at Baseline (1970), 1980–2010

Outcomes	Earnings (1)	Employment (2)	Poverty (3)	Public Assistance (4)
Log minimum wage	0.1235	0.2261***	−0.0261	−0.3187*
	(0.0892)	(0.0517)	(0.1297)	(0.1745)
10-year lag of log minimum wage	**−0.1249***	**−0.1064**	**−0.0997**	**−0.4015****
	(0.0657)	**(0.0744)**	**(0.0908)**	**(0.1647)**
Log EITC phase-in rate	0.0443*	−0.0224	−0.2143***	−0.2344***
	(0.0252)	(0.0195)	(0.0328)	(0.0534)
10-year lag of log EITC phase-in rate	**0.0129**	**0.0261*****	**−0.0465*****	**−0.0923*****
	(0.0113)	**(0.0061)**	**(0.0153)**	**(0.0213)**
Log maximum welfare benefit	0.0276	−0.0025	0.0483	0.0065
	(0.0364)	(0.0174)	(0.0744)	(0.0841)
10-year lag of log maximum welfare benefit	**−0.0206**	**−0.0022**	**0.1654*****	**0.1583*****
	(0.0184)	**(0.0108)**	**(0.0330)**	**(0.0400)**
Welfare time limits (< 60 months)	−0.0053	−0.0278**	−0.0263	0.0103
	(0.0233)	(0.0113)	(0.0251)	(0.0299)
10-year lag of welfare time limits (< 60 months)	**−0.0070**	**−0.0160**	**−0.0845*****	**−0.1024****
	(0.0263)	**(0.0135)**	**(0.0275)**	**(0.0434)**
Adjusted R^2	0.69	0.74	0.80	0.77
N	206,652	206,801	206,684	206,652
Tract fixed effects	Yes	Yes	Yes	Yes
County x year interactions	Yes	Yes	Yes	Yes

Notes: The specification corresponds to Equation (2) in the text; only the coefficients of $DIS_c^b \cdot P_{cst}$ and $DIS_c^b \cdot P_{cs,t-10}$ are reported. The longer-run effects are highlighted in boldface. Earnings are defined as average earned income per household (in $2014). All outcomes, and the minimum wage, EITC, and welfare benefits variables, are in logs. (The EITC phase-in rate is scaled from zero to 100, with one replacing zero, prior to taking logs.) Thus, the estimates of the minimum wage, EITC, and welfare benefits coefficients can be interpreted as elasticities for the disadvantaged tracts (i.e., those in the top quartile of disadvantage), relative to other tracts. The welfare time limits variable is a dummy variable, so its estimated effect approximates the percentage change in the outcome in disadvantaged tracts when welfare time limits are shorter. ***, **, or * indicates statistically significantly different from zero at the 1, 5, or 10% level. Standard errors are clustered by state.

negative, which differs from the EITC literature focused on low-skilled, often single mothers. Again, this difference may reflect the fact that results for disadvantaged places can differ from what has been found using individual- or household-level data on the most affected groups.

For the welfare variables, we find no statistically significant evidence of longer-run relationships with employment. This contrasts with the view that more generous welfare (higher benefits, or no or longer time limits) creates work disincentives. There is evidence of a significant negative short-run relationship with tighter time limits, again contrary to the intended effect.

For two reasons, our most important results are for poverty and the share on public assistance. First, these are the direct "targets" of anti-poverty policies. And second, evidence on the longer-run effects of anti-poverty policies on poverty and receipt of public assistance tells us more about the effects of these policies on economic self-sufficiency. It is important to keep in mind that because both poverty and receipt of public assistance depend on the value of family income relative to thresholds, and because effects on family income depend on who is affected by the policies we consider, we should not necessarily expect a tight correspondence between effects on these outcomes and the prior earnings and employment results.

Nonetheless, we should expect similar results for poverty and public assistance, so we discuss these results together. The evidence on minimum wages in column (3) suggests that higher minimum wages reduce poverty in the longer run, with an elasticity of −0.10, although the estimate is not statistically significant. There is also evidence of a longer-run reduction in the share of households on public assistance, coupled with evidence of a contemporaneous relationship in the same direction. Most past research on the short-run effects of the minimum wage on poverty has not found a significant effect (e.g., Sabia and Burkhauser 2010), although the point estimates tend to be in the direction of reducing poverty, and more recently Dube (2017) finds evidence of significant poverty reductions. Recent research has not found that

higher minimum wages reduce participation in public assistance programs in the short-term (Sabia and Nguyen 2017).

The estimates also suggest significant short-run and longer-run effects of the EITC in reducing both poverty and the receipt of public assistance. The longer-run implied elasticities are −0.05 and −0.09, respectively. Note that the public assistance measure does not include the EITC, and EITC payments do not count in the poverty measure, so that these results indicate that the EITC reduces poverty and the receipt of other public assistance via behavioral changes—*before* taking account of the EITC (paralleling findings in Neumark and Wascher 2011).

Finally, the estimates indicate that more generous welfare benefits increase poverty and public assistance receipt in the longer run, with both elasticities around 0.16. The result for receipt of public assistance may be somewhat mechanically linked to the level of benefits, given benefit formulas as well as take-up effects, and the same is potentially true of time limits. However, the fact that the results are so similar for poverty makes it more likely that we are detecting behavioral effects. We also find that tighter time limits appear to reduce poverty and public assistance in the longer run. Note that these potential effects of our two welfare measures are not reflected in positive relationships with earnings or employment. This is not necessarily a contradiction, however; as noted earlier, because these outcomes are based on threshold definitions, there can be changes in the distribution but not the level of earnings, and these distributional changes can change the share poor or receiving public assistance. Moreover, time limits could reduce public assistance receipt without necessarily increasing earnings or employment. However, one might be a bit more cautious about the estimated longer-run estimates for welfare benefits and time limits because of the absence of positive longer-run relationships with earnings or employment.

C. Additional Analyses

We now turn to a number of additional analyses that hew closely to our preferred specification, but assess the robustness or sensitivity of the results using that specification, including to important variations in the "treated" and "control" tracts that potentially help identify effects on the disadvantaged. In Appendix B, we discuss three other sets of results that help establish the specification in Table 2 (and close variants thereof) as our preferred specification. In our view, there are number of choices about precisely how to specify the model or sample. Our preferred choices are reflected in the baseline estimates we just discussed. But knowing how sensitive the results are to some of these choices naturally affects the confidence with which one should view our findings.

Results for Women Versus Men. Our analysis of the effects of the EITC is quite different from the more typical analysis that focuses on the short-run effects of the EITC on the employment of disadvantaged women (generally proxied by low skill).[39] We can use the NCDB data to estimate separate longer-run coefficients on the EITC for women and men. (We cannot do this for other outcomes, and indeed this disaggregation would make less sense for a household-level measure.) As reported in Table 3, we find that the estimated employment coefficient is larger for women than for men. This is what the prior literature—focusing on employment of less-skilled women—would lead us to expect.

The estimated difference between men and women is not large, but most evidence of positive employment effects of the EITC for women focuses on unmarried women with children, whereas in the NCDB data we cannot measure employment rates for women distinguished by marital status and number of children.[40] The fact that we find a positive coefficient for men is interesting. The standard EITC literature ignores men, focusing mostly on single mothers for whom incomes should be lowest and thus the positive extensive-margin effects of the EITC strongest. However, men in low-income families—especially with spouses who do not work—can still, in principle, respond positively to the positive employment incentives created by the EITC.

Alternative Definitions of Disadvantage. We explored using alternative definitions of disadvantage for classifying tracts as disadvantaged at baseline: the share of the population with a high

39. See, for example, Eissa and Liebman (1996) and Meyer and Rosenbaum (2001).

40. We have plans to do additional work on the longer-run effects of anti-poverty policies using confidential microdata from the Census and ACS. However, even then we will not be able to focus sharply on unmarried versus married women, or women with or without children, since both of these can change for an individual over time.

TABLE 3

Effects of Anti-Poverty Policies on Male and Female Employment Rates in Areas with High Poverty Rate at Baseline (1970), 1980–2010

	Male employment (1)	Female employment (2)
Log minimum wage	0.1732***	0.2460***
	(0.0554)	(0.0597)
10-year lag of log minimum wage	**−0.0260**	**−0.1507**
	(0.0545)	**(0.0908)**
Log EITC phase-in rate	−0.0158	−0.0161
	(0.0172)	(0.0224)
10-year lag of log EITC phase-in rate	**0.0204****	**0.0252*****
	(0.0081)	**(0.0063)**
Log maximum welfare benefit	−0.0020	0.0102
	(0.0167)	(0.0206)
10-year lag of log maximum welfare benefit	**0.0088**	**−0.0200**
	(0.0108)	**(0.0126)**
Welfare time limits (< 60 months)	−0.0236**	−0.0322**
	(0.0115)	(0.0127)
10-year lag of welfare time limits (< 60 months)	**−0.0257***	**0.0021**
	(0.0140)	**(0.0150)**
Adjusted R^2	0.71	0.66
N	206,787	206,753
Tract fixed effects	Yes	Yes
County x year interactions	Yes	Yes

Notes: See notes to Table 2. The only difference is the dependent variables.

school degree or less; the share of the population that is black; and the share of families headed by single mothers.[41] These results are reported in Table 4. Focusing on our key results—the longer-run relationships with poverty and public assistance—the estimates are quite robust. We always find negative estimated relationships between both the minimum wage and the EITC and poverty, although the estimates are statistically significant only for the share black disadvantage measure (and more strongly so for the EITC). The evidence (including statistical significance) is more robust for the potential effects of the minimum wage and the EITC in reducing receipt of public assistance—especially for the EITC. The estimated impacts of welfare benefits and time limits in reducing poverty and public assistance are very robust; the magnitudes are similar across the three alternative measures of disadvantage, and most of the estimated coefficients are statistically significant.[42]

Compositional Changes. As noted above, our estimates using geographic variation might better identify effects on individuals and families if we control for compositional changes. We do this, in Table 5, by adding controls for migration—capturing the proportion that moved into the tract from the same county, or from anywhere else, in the previous 5 years. To capture additional detail on the composition of migration, we add controls for the composition of the tract population by race, education, single motherhood, and age.[43]

Comparing Tables 2 and 5, the qualitative conclusions are generally very similar. However, one noteworthy finding is that the estimated coefficients for poverty and public assistance are typically smaller in magnitude in Table 5. This is true of the estimated longer-run coefficients on the minimum wage and the EITC in reducing poverty and public assistance, and the estimated longer-run impacts of less generous welfare benefits and welfare time limits in reducing poverty and public assistance. Taken at face value, this implies that part of the evidence suggesting beneficial effects found in Table 2 is attributable to changes in the composition of disadvantaged tracts, consistent with policies that reduce poverty in the most-disadvantaged tracts also leading to in-migration (in relative terms, at least) of residents less likely to be poor and on public assistance.

The question of whether anti-poverty policies are beneficial or detrimental in helping to lift the economic fortunes of particularly disadvantaged areas is important, given that there is scant evidence that explicit place-based anti-poverty programs, such as enterprise zones, increase jobs or reduce poverty in disadvantaged neighborhoods.[44] Geographically-concentrated poverty poses its own challenges above and beyond individual poverty, perhaps most importantly for minorities, who tend to cluster

41. The correlations of the indicators for the most-disadvantaged quartiles are as follows: share poor and share black: 0.44; share poor and share single mothers: 0.38; and share poor and share low-education: 0.25. The corresponding correlations for the actual shares, rather than the dummy variables for the top quartile of disadvantage, are 0.55, 0.62, and 0.26.

42. We also find robust evidence of positive longer-run employment effects of the EITC, although also of modest reductions in average earnings.

43. We are somewhat limited in what compositional controls we can use over time because of the data available in the NCDB. Thus, for example, for age we can only measure the proportion aged 16–34 over our entire sample period.

44. See, for example, Neumark and Simpson (2015) and Neumark and Young (forthcoming). The latter paper does not examine longer-run effects of explicit place-based policies, although research on this topic is in progress (Neumark and Young, in progress).

TABLE 4

Effects of Anti-Poverty Policies on Average Earnings per Household, the Employment Rate, the Share in Poverty, and the Share on Public Assistance, in Areas with Low Education, High Share Black, or High Share of Single Mothers at Baseline (1970), 1980–2010

	Earnings			Employment			Share in Poverty			Share on Public Assistance		
	Share ≤ HSG (1)	**Share Black (2)**	**Share SM (3)**	**Share ≤ HSG (4)**	**Share Black (5)**	**Share SM (6)**	**Share ≤ HSG (7)**	**Share Black (8)**	**Share SM (9)**	**Share ≤ HSG (10)**	**Share Black (11)**	**Share SM (12)**
Log minimum wage	0.0247	−0.0730	0.0013	0.1556***	0.1495***	0.2320***	−0.0291	0.0081	−0.0325	−0.2269	−0.3022	−0.3089
	(0.0759)	(0.0627)	(0.0651)	(0.0437)	(0.0451)	(0.0426)	(0.1339)	(0.1009)	(0.1179)	(0.1895)	(0.2479)	(0.1879)
10-year lag of log minimum wage	**−0.0355**	**−0.0872**	**−0.0309**	**−0.0657**	**−0.0387**	**−0.0600**	**−0.0316**	**−0.1111***	**−0.1322**	**−0.2896***	**−0.3109****	**−0.3435***
	(0.0632)	**(0.0613)**	**(0.0639)**	**(0.0516)**	**(0.0779)**	**(0.0586)**	**(0.0946)**	**(0.0647)**	**(0.1160)**	**(0.1450)**	**(0.1484)**	**(0.1740)**
Log EITC phase-in rate	0.0602**	0.0403*	0.0737***	−0.0152	−0.0137	−0.0170	−0.1343***	−0.1557***	−0.1646***	−0.1591**	−0.1669**	−0.1795***
	(0.0253)	(0.0237)	(0.0196)	(0.0153)	(0.0228)	(0.0157)	(0.0421)	(0.0237)	(0.0419)	(0.0609)	(0.0642)	(0.0538)
10-year lag of log EITC phase-in rate	**−0.0230***	**−0.0322***	**−0.0177**	**0.0169*****	**0.0110**	**0.0185*****	**−0.0115**	**−0.0409*****	**−0.0199**	**−0.0597****	**−0.0868*****	**−0.0776*****
	(0.0122)	**(0.0164)**	**(0.0123)**	**(0.0060)**	**(0.0066)**	**(0.0069)**	**(0.0166)**	**(0.0135)**	**(0.0170)**	**(0.0235)**	**(0.0309)**	**(0.0225)**
Log maximum welfare benefit	0.1161***	0.1068**	0.1159***	−0.0162	0.0149	0.0245	−0.1109*	0.0229	0.0257	−0.1551*	0.0078	0.0195
	(0.0337)	(0.0494)	(0.0422)	(0.0143)	(0.0256)	(0.0203)	(0.0578)	(0.0572)	(0.0892)	(0.0795)	(0.0873)	(0.0925)
10-year lag of log maximum welfare benefit	**−0.0195**	**−0.0063**	**−0.0253**	**0.0139***	**−0.0025**	**0.0064**	**0.1395*****	**0.1175*****	**0.1160****	**0.1469*****	**0.1136*****	**0.1441*****
	(0.0139)	**(0.0127)**	**(0.0159)**	**(0.0077)**	**(0.0137)**	**(0.0137)**	**(0.0218)**	**(0.0302)**	**(0.0450)**	**(0.0261)**	**(0.0369)**	**(0.0406)**
Welfare time limits <60 months	−0.0014	−0.0243	0.0006	−0.0239**	−0.0217*	−0.0138	−0.0254	−0.0289	−0.0062	−0.0340	0.0063	0.0063
	(0.0189)	(0.0325)	(0.0316)	(0.0109)	(0.0115)	(0.0107)	(0.0199)	(0.0224)	(0.0266)	(0.0255)	(0.0292)	(0.0269)
10-year lag of welfare time limits < 60 months	**−0.0099**	**0.0030**	**0.0178**	**−0.0121**	**−0.0076**	**0.0064**	**−0.0414***	**−0.0517****	**−0.0729****	**−0.0922****	**−0.0871***	**−0.1209****
	(0.0241)	**(0.0280)**	**(0.0312)**	**(0.0140)**	**(0.0141)**	**(0.0135)**	**(0.0238)**	**(0.0215)**	**(0.0277)**	**(0.0441)**	**(0.0518)**	**(0.0561)**
Adjusted R^2	0.69	0.69	0.69	0.74	0.74	0.74	0.79	0.79	0.79	0.76	0.76	0.76
N	206,590	206,652	206,652	206,737	206,801	206,801	206,622	206,684	206,684	206,590	206,652	206,652
Tract fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County x year interactions	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: See notes to Table 2. The specifications differ from those in Table 2 in the measures of disadvantage used. HSG, high school graduate; SM, single mothers.

TABLE 5

Effects of Anti-Poverty Policies on Average Earnings per Household, Employment Rate, Share in Poverty, and Share on Public Assistance in Areas with High Share Poverty at Baseline (1970), 1980–2010, with Migration and Demographic Controls

Outcomes	Earnings (1)	Employment (2)	Poverty (3)	Public Assistance (4)
Log minimum wage	0.0492	0.1919***	0.1082	−0.2425*
	(0.0484)	(0.0499)	(0.0768)	(0.1283)
10-year lag of log minimum wage	**−0.1656*** **	**−0.1042***	**0.0011**	**−0.2978****
	(0.0427)	**(0.0575)**	**(0.0629)**	**(0.1330)**
Log EITC phase-in rate	0.0040	−0.0298*	−0.1312***	−0.1429***
	(0.0145)	(0.0171)	(0.0206)	(0.0416)
10-year lag of log EITC phase-in rate	**−0.0079**	**0.0206*** **	**−0.0165**	**−0.0710*** **
	(0.0072)	**(0.0073)**	**(0.0112)**	**(0.0165)**
Log maximum welfare benefit	0.0624**	−0.0100	0.0153	0.0078
	(0.0270)	(0.0199)	(0.0465)	(0.0564)
10-year lag of log maximum welfare benefit	**0.0330*** **	**0.0151***	**0.0917*** **	**0.0772*** **
	(0.0111)	**(0.0081)**	**(0.0172)**	**(0.0280)**
Welfare time limits (< 60 months)	−0.0108	−0.0299**	−0.0287	0.0064
	(0.0198)	(0.0118)	(0.0192)	(0.0237)
10-year lag of welfare time limits (< 60 months)	**−0.0147**	**−0.0213***	**−0.0541***	**−0.0663***
	(0.0179)	**(0.0118)**	**(0.0292)**	**(0.0337)**
Proportion moved from within county	−0.1387***	0.1322***	0.4570***	0.2032***
	(0.0442)	(0.0393)	(0.0376)	(0.0326)
Proportion moved from somewhere else	−0.1573***	−0.0976***	0.5622***	−0.2334***
	(0.0334)	(0.0243)	(0.0245)	(0.0530)
Proportion low education	−1.2202***	−0.4774***	1.6638***	1.2507***
	(0.0489)	(0.0215)	(0.0701)	(0.0352)
Proportion black	−0.1673***	−0.0957***	0.6841***	0.7383***
	(0.0465)	(0.0290)	(0.0508)	(0.0518)
Proportion single mothers	−0.4159***	−0.0439***	0.8236***	0.6001***
	(0.0270)	(0.0142)	(0.0209)	(0.0429)
Proportion ages 16–34	−0.0114	0.0151	0.0069	0.0006
	(0.0208)	(0.0093)	(0.0115)	(0.0023)
Adjusted R^2	0.83	0.84	0.79	0.78
N	206,378	206,395	206,378	206,445
Tract fixed effects	Yes	Yes	Yes	Yes
County x year interactions	Yes	Yes	Yes	Yes

Notes: See notes to Table 2. The only difference is the addition of migration and demographic controls. The migration variables refer to the previous 5 years. Only the proportion aged 16–34 is included as a control due to a lack of consistent age categories available across years in the NCDB.

residentially in poor areas.[45] Moreover, research suggests that living in poverty areas creates extra hardships for the poor and also for the non-poor residing in those areas, owing to less private-sector investment, higher crime, weaker labor market networks, poor health, etc.[46] Thus, if anti-poverty policies reduce poverty in disadvantaged areas, there may be positive spillover effects, as effects from disadvantaged neighborhoods can have lasting impacts on the next generation (Chetty et al. 2014). And the in-migration of the somewhat less-disadvantaged, as suggested by Table 5, may provide positive spillovers on the most-disadvantaged living in these areas, although we also need to be concerned that the latter are simply being displaced to other areas—a potential with respect to explicit place-based policies as well, although one on which the evidence is limited (see, e.g., Reynolds and Rohlin 2015).

Varying the Comparison Tracts. Next, we vary the comparison or control tracts used to try to identify the longer-run effects of anti-poverty policies on the most-disadvantaged tracts. First, we drop observations in the third quartile of the observations used to define DIS^b. In this case, the

45. American Community Survey (ACS) data from 2010 indicate that 50.4% of blacks, 44.1% of Hispanics, but only 20.3% of whites, reside in areas where the poverty rate is 20% or higher (see Bishaw 2014, for more descriptive evidence). At the same time, poverty rate differences between these groups are much smaller (see https://www.census.gov/prod/2013pubs/acsbr11-17.pdf, viewed March 31, 2017).

46. See the summary of the evidence in Federal Reserve System and Brookings Institution (2008).

TABLE 6

Effects of Anti-Poverty Policies on Average Earnings per Household, Employment Rate, Share in Poverty, and Share on Public Assistance in Areas with High Poverty Rate at Baseline (1970), 1980–2010, Omitting 3rd Quartile of Disadvantage

	Earnings (1)	Employment (2)	Poverty (3)	Public Assistance (4)
Log minimum wage	0.1424	0.2642***	−0.0417	−0.4019*
	(0.1107)	(0.0628)	(0.1657)	(0.2185)
10-year lag of log minimum wage	**−0.1571***	**−0.1314**	**−0.1393**	**−0.4591****
	(0.0790)	**(0.0974)**	**(0.1129)**	**(0.1854)**
Log EITC phase-in rate	0.0533*	−0.0291	−0.2606***	−0.2705***
	(0.0291)	(0.0247)	(0.0392)	(0.0599)
10-year lag of log EITC phase-in rate	**0.0126**	**0.0307*****	**−0.0595*****	**−0.1110*****
	(0.0156)	**(0.0090)**	**(0.0206)**	**(0.0263)**
Log maximum welfare benefit	0.0611	0.0041	0.0464	−0.0382
	(0.0523)	(0.0277)	(0.1036)	(0.1141)
10-year lag of log maximum welfare benefit	**−0.0346**	**−0.0032**	**0.2241*****	**0.2069*****
	(0.0238)	**(0.0138)**	**(0.0422)**	**(0.0490)**
Welfare time limits (< 60 months)	−0.0056	−0.0275**	−0.0473	0.0098
	(0.0296)	(0.0123)	(0.0325)	(0.0328)
10-year lag of welfare time limits (<60 months)	**−0.0140**	**−0.0146**	**−0.0878****	**−0.1238*****
	(0.0362)	**(0.0164)**	**(0.0328)**	**(0.0437)**
Adjusted R^2	0.71	0.75	0.81	0.78
N	154,905	155,038	154,935	154,905
Tract fixed effects	Yes	Yes	Yes	Yes
County x year interactions	Yes	Yes	Yes	Yes

Notes: See notes to Table 2. The only difference is omitting observations in the third quartile of the disadvantage measure.

"control" or "untreated" tracts are more sharply delineated from the tracts in the top quartile of disadvantage because we omit observations for which the share poor at the tract level is between the median and the 75th percentile. Given that the policies we study would also be expected to affect low-wage earners or low-income families in other tracts, the inclusion of tracts in the third quartile of the distribution of disadvantage could bias our findings towards zero. Consistent with this expectation, nearly every estimate in Table 6 is larger in absolute value than the corresponding baseline estimate in Table 2; and this is true for *every* statistically significant longer-run estimate in Table 2. However, the estimates are not very different between Tables 2 and 6, suggesting that our evidence is driven by differences in outcomes that are strongly concentrated in the tracts in the top quartile of disadvantage.

We also show, in Table 7, that we find quite similar results if we estimate our model only for the top two quartiles of disadvantage, estimating the potential longer-run effects of policy from changes in the most-disadvantaged tracts relative to tracts in the third quartile. All of the longer-run coefficients for the minimum wage, the EITC, and our two welfare variables that were significant in Table 2 remain statistically significant, while the point estimates for the EITC and the welfare variables are smaller in absolute value. In our view, the evidence in Table 7 bolsters a causal interpretation of our findings, as it seems much less likely that there are different shocks hitting tracts in the top (fourth) and third quartiles of the distribution of disadvantage, in contrast to the comparison between the top quartiles and other quartiles. And regardless of whether one subscribes to a causal interpretation, the results in Tables 6 and 7 show that our evidence on the associations between anti-poverty policies and economic outcomes in the most-disadvantaged tracts is robust to the choice of comparison tracts.

Robustness to Baseline/Starting Year. We next present a robustness analysis in which we move up the baseline year in which we measure disadvantage by one decade—to 1980—and estimate the models for 1990–2010 instead of 1980–2010. If the composition of tracts changed much from 1970 to 1980, then a good part of our identification of effects for "disadvantaged" tracts may not reflect tracts that are as likely to be disadvantaged in later years, given that we use data over four decades for our main analysis. Although there is considerable persistence in our measure of disadvantage, the classification of tracts as disadvantaged in 1980 is more persistent than the classification in 1970. The shares of the tracts in the top quartile of the poverty

TABLE 7

Effects of Anti-Poverty Policies on Average Earnings per Household, Employment Rate, Share in Poverty, and Share on Public Assistance in Areas with High Poverty Rate at Baseline (1970), 1980–2010, Top Versus Third Quartile

	Earnings (1)	Employment (2)	Poverty (3)	Public Assistance (4)
Log minimum wage	0.1054	0.1804***	0.0416	−0.1298
	(0.0732)	(0.0413)	(0.0753)	(0.0911)
10-year lag of log minimum wage	**−0.0687**	**−0.0589**	**−0.1145**	**−0.3796****
	(0.0512)	**(0.0618)**	**(0.0727)**	**(0.1429)**
Log EITC phase-in rate	0.0314	−0.0088	−0.1584***	−0.1953***
	(0.0223)	(0.0157)	(0.0257)	(0.0431)
10-year lag of log EITC phase-in rate	**0.0103**	**0.0185*****	**−0.0239****	**−0.0526*****
	(0.0077)	**(0.0039)**	**(0.0104)**	**(0.0150)**
Log maximum welfare benefit	0.0015	−0.0046	0.0364	0.0130
	(0.0247)	(0.0127)	(0.0656)	(0.0727)
10-year lag of log maximum welfare benefit	**−0.0090**	**−0.0030**	**0.0940*****	**0.0954*****
	(0.0143)	**(0.0092)**	**(0.0292)**	**(0.0313)**
Welfare time limits (< 60 months)	0.0055	−0.0261**	−0.0219	−0.0183
	(0.0145)	(0.0127)	(0.0187)	(0.0261)
10-year lag of welfare time limits (< 60 months)	**−0.0004**	**−0.0138**	**−0.0817*****	**−0.0714***
	(0.0161)	**(0.0105)**	**(0.0202)**	**(0.0395)**
Adjusted R^2	0.65	0.70	0.79	0.79
N	103,454	103,495	103,461	103,454
Tract fixed effects	Yes	Yes	Yes	Yes
County x year interactions	Yes	Yes	Yes	Yes

Notes: See notes to Table 2. The only difference is that the 1st and 2nd quartiles of the disadvantage measure are omitted.

distribution in 1970 that are in the top quartile one, two, and three decades later are 67.6% (1980), 59.2% (1990), and 58.8% (2000). The corresponding shares based on the 1980 data are 71.9% (1990), 71.8% (2000), and 61.7% (2010).

The results are reported in Table 8. The results for the EITC, and for welfare time limits, are robust to this change. We still find statistically significant evidence suggesting that the longer-run effect of the EITC is to reduce poverty and public assistance (and to increase employment, but significant only for the longer-run coefficient). And we find similar evidence for welfare time limits. In contrast, however, the results for minimum wages flip sign, as do the results for welfare benefits. Especially insofar as policymakers should be more interested in results based on more current data, this evidence suggests that we should not draw strong conclusions about the beneficial longer-run effects of either minimum wages or less generous welfare benefits.[47]

Two of the prior analyses accounted for changes in the composition of tracts due to migration (Table 5), or due to the passage of time (Table 8). Our next analysis explores a hybrid of these two issues. Rather than fixing the classification of disadvantaged tracts in a single baseline period (1970 in most tables, and 1980 in Table 8), we allow the classification of tracts to evolve over time. (That is, we modify Equation (2), changing the dummy variable for disadvantaged tracts from DIS_c^b to $DIS_{c,t-10}$).[48] This specification allows the composition of tracts to change over time, which implies that we are more certain that we are estimating 10-year lags of policy effects for tracts that were disadvantaged 10 years prior. At the same time, the composition changes could reflect long-term effects of policy, which is why we prefer the specification using a fixed baseline.

The results are reported in Table 9. The results are generally quite similar for the EITC and welfare, with two exceptions. First, the estimated longer-run coefficients on welfare benefits are smaller; and second, the evidence that the EITC may reduce poverty in the longer run is weaker, although the evidence that it reduces public assistance does not change. Evidence persists suggesting that the longer-run effect of the EITC is to increase employment. More substantial is

47. There is some important minimum wage variation in the very early part of the sample, which could explain why the minimum wage effects are so different when we move up the baseline period and the starting year for the analysis.

48. The second and third terms in Equation (2) become $\{DIS_{c,t-10} \cdot P_{cst}\}\beta + \{DIS_{c,t-10} \cdot P_{cs,t-10}\}\beta^L$.

TABLE 8

Effects of Anti-Poverty Policies on Average Earnings per Household, Employment Rate, Share in Poverty, and Share on Public Assistance in Areas with High Poverty Rate at Baseline (1980), 1990–2010

	Earnings (1)	Employment (2)	Poverty (3)	Public Assistance (4)
Log minimum wage	0.0724	0.0794	0.0976	0.2786
	(0.1543)	(0.0897)	(0.3068)	(0.5431)
10-year lag of log minimum wage	**−0.1398**	**−0.1006**	**0.2567***	**0.2894**
	(0.1119)	**(0.0700)**	**(0.1405)**	**(0.2383)**
Log EITC phase-in rate	−0.0063	−0.0226	−0.0003	0.0575
	(0.0332)	(0.0207)	(0.0401)	(0.0680)
10-year lag of log EITC phase-in rate	**−0.0170**	**0.0269*****	**−0.0993*****	**−0.2030*****
	(0.0179)	**(0.0098)**	**(0.0217)**	**(0.0532)**
Log maximum welfare benefit	0.1304**	0.0606	−0.1961**	−0.3967**
	(0.0537)	(0.0451)	(0.0908)	(0.1713)
10-year lag of log maximum welfare benefit	**0.0996****	**−0.0267**	**−0.0953**	**−0.1642***
	(0.0434)	**(0.0341)**	**(0.0635)**	**(0.0969)**
Welfare time limits (< 60 months)	0.0041	−0.0257*	−0.0179	−0.0146
	(0.0258)	(0.0134)	(0.0213)	(0.0242)
10-year lag of welfare time limits (<60 months)	**−0.0041**	**−0.0135**	**−0.0620****	**−0.1151****
	(0.0276)	**(0.0142)**	**(0.0266)**	**(0.0470)**
Adjusted R^2	0.68	0.76	0.81	0.76
N	175,072	175,211	175,106	175,072
Tract fixed effects	Yes	Yes	Yes	Yes
County x year interactions	Yes	Yes	Yes	Yes

Notes: See notes to Table 2. The only differences are the sample period and the baseline year for defining disadvantage.

TABLE 9

Effects of Anti-Poverty Policies on Average Earnings per Household, Employment Rate, Share in Poverty, and Share on Public Assistance in Areas with High Poverty Rate (10 Years Prior), 1980–2010

	Earnings (1)	Employment (2)	Poverty (3)	Public Assistance (4)
Log minimum wage	−0.1184*	0.1690***	−0.0019	−0.3496
	(0.0634)	(0.0529)	(0.1249)	(0.2538)
10-year lag of log minimum wage	**−0.1218*****	**−0.1295*****	**0.1982*****	**0.2261**
	(0.0368)	**(0.0355)**	**(0.0720)**	**(0.1359)**
Log EITC phase-in rate	0.0541**	−0.0415**	−0.0812**	−0.0157
	(0.0224)	(0.0182)	(0.0322)	(0.0732)
10-year lag of log EITC phase-in rate	**−0.0281****	**0.0223****	**−0.0177**	**−0.0975****
	(0.0111)	**(0.0088)**	**(0.0203)**	**(0.0379)**
Log maximum welfare benefit	0.0469**	−0.0067	−0.0810***	−0.0042
	(0.0202)	(0.0090)	(0.0227)	(0.0356)
10-year lag of log maximum welfare benefit	**0.0075**	**0.0048**	**0.0811*****	**0.1153*****
	(0.0173)	**(0.0141)**	**(0.0241)**	**(0.0360)**
Welfare time limits (< 60 months)	−0.0011	−0.0321**	0.0182	0.0348**
	(0.0284)	(0.0153)	(0.0236)	(0.0173)
10-year lag of welfare time limits (<60 months)	**−0.0040**	**−0.0002**	**−0.0853*****	**−0.1955*****
	(0.0256)	**(0.0170)**	**(0.0225)**	**(0.0466)**
Adjusted R^2	0.67	0.74	0.79	0.76
N	254,827	254,994	254,870	254,827
Tract fixed effects	Yes	Yes	Yes	Yes
County x year interactions	Yes	Yes	Yes	Yes

Notes: See notes to Table 2. The only difference is that the "baseline" disadvantage dummy variable DIS^b is defined as of 10 years prior to the observation rather than in a fixed baseline year (1970 in most of our analyses)—that is, $DIS_{c,t-10}$ instead of DIS_c^b.

TABLE 10

Effects of Anti-Poverty Policies on Average Earnings per Household, Employment Rate, Share in Poverty, and Share on Public Assistance in Areas with High Poverty Rate at Baseline (1970), 1980–2010, with 10-Year Policy Leads

	Earnings (1)	Employment (2)	Poverty (3)	Public Assistance (4)
10-year lead of log minimum wage	*0.0804*	*0.0130*	*−0.0495*	*0.1340*
	(0.0843)	*(0.0537)*	*(0.1579)*	*(0.1501)*
Log minimum wage	0.1015	0.2324***	0.0667	−0.2240
	(0.0708)	(0.0459)	(0.1066)	(0.1339)
10-year lag of log minimum wage	**−0.1820***	**−0.1264**	**0.2066***	**−0.0438**
	(0.0939)	**(0.0806)**	**(0.1061)**	**(0.1570)**
10-year lead of log EITC phase-in rate	*−0.0510*	*0.0105*	*0.2523****	*0.3288****
	(0.0371)	*(0.0161)*	*(0.0592)*	*(0.0783)*
Log EITC phase-in rate	0.0238	−0.0281	−0.1009**	−0.0835
	(0.0332)	(0.0215)	(0.0388)	(0.0554)
10-year lag of log EITC phase-in rate	**0.0231**	**0.0217*****	**−0.1107*****	**−0.1987*****
	(0.0157)	**(0.0054)**	**(0.0223)**	**(0.0309)**
10-year lead of log maximum welfare benefit	*−0.0042*	*0.0234*	*−0.1239**	*−0.1929***
	(0.0394)	*(0.0272)*	*(0.0729)*	*(0.0750)*
Log maximum welfare benefit	0.0481	−0.0114	−0.0156	−0.0580
	(0.0388)	(0.0238)	(0.0636)	(0.0747)
10-year lag of log maximum welfare benefit	**−0.0094**	**−0.0023**	**0.0950*****	**0.0575**
	(0.0222)	**(0.0118)**	**(0.0309)**	**(0.0375)**
10-year lead of welfare time limits (< 60 months)	*−0.0132*	*0.0103*	*−0.0375**	*−0.0103*
	(0.0131)	*(0.0098)*	*(0.0211)*	*(0.0286)*
Welfare time limits (< 60 months)	−0.0081	−0.0300**	−0.0023	0.0205
	(0.0252)	(0.0118)	(0.0225)	(0.0320)
10-year lag of welfare time limits (< 60 months)	**−0.0171**	**−0.0112**	**−0.0563***	**−0.0560**
	(0.0292)	**(0.0142)**	**(0.0300)**	**(0.0400)**
Adjusted R^2	0.69	0.74	0.80	0.77
N	206,652	206,801	206,684	206,652
Tract fixed effects	Yes	Yes	Yes	Yes
County x year interactions	Yes	Yes	Yes	Yes

Notes: See notes to Table 2. The only difference is the addition of 10-year leads of the policy variables. The leading effects are highlighted in italics.

the change in results for the minimum wage. The evidence in Table 9 suggests longer-run effects that increase poverty and public assistance, with the effect on poverty statistically significant. Coupled with the evidence in Table 8, this is another indication that we cannot draw robust conclusions that higher minimum wages reduce poverty or public assistance in the longer run.

Allowing for Leading Effects (Pre-Trends). Our final analysis addresses additional evidence—in addition to the analysis using different quartiles of disadvantage for comparisons—related to a causal interpretation of the findings. We add 10-year leads of our policy variables to our models, to see whether policy changes were correlated with prior changes in outcomes, which could lead to biased estimates of policy effects. For the leads corresponding to the most recent data in our sample, we use 2016 (for welfare) or 2017 values (for the EITC and minimum wage)—depending on data availability—which nearly correspond to 10 years after the midrange of the 2006–2010 period.

The results, reported in Table 10, indicate that there are some significant partial correlations between our outcome variables and future policy changes. For the EITC, there are positive partial correlations for poverty and the share on public assistance. If both EITC generosity and poverty/public assistance are quite persistent over time, then this leading relationship creates a bias *against* finding beneficial effects of the EITC. This is what we find, as controlling for the leads generates stronger beneficial longer-run effects of the EITC on these outcomes (compare to Table 2)—thus strengthening our conclusions for the EITC. In contrast, this analysis somewhat weakens the evidence that more generous welfare benefits increase poverty and public assistance, and that tighter time limits reduce them. Still, these estimated effects of welfare policy on poverty remain statistically significant

(only at the 10% level for time limits).[49,50] Finally, although the leading effects of minimum wages are not statistically significant, including these leading effects eliminates the evidence of beneficial longer-run effects of minimum wages in reducing poverty and public assistance receipt, and in fact leads to evidence consistent with higher minimum wages increasing poverty in the longer run.

V. CONCLUSIONS

Our goal in this paper is to generate evidence on the longer-run effects of anti-poverty policies on key socioeconomic outcomes in disadvantaged areas. We study three policies—minimum wages, the EITC, and welfare (including a key aspect of welfare reform)—and estimate how these policies influence earnings and employment, and most important, poverty and public assistance, in the most disadvantaged areas. The kinds of longer-run effects we study differ substantially from almost all research on the effects of these policies, although there are a few exceptions that focus on longer-run effects of a single one of these policies.

We identify tracts that are initially disadvantaged in terms of a high share of residents who are poor. We then estimate the longer-run relationships between these alternative policies and key economic indicators of economic self-sufficiency—in particular, poverty and the receipt of public assistance, but also employment and earnings.

We have two main results that emerge across the many analyses we report. First, the evidence suggests that longer-run effects of the EITC are to increase employment and to reduce poverty and public assistance, as long as we rely on national as well as state variation in EITC policy. Second, tighter welfare time limits also appear to reduce poverty and public assistance in the longer run. We also find some evidence that higher minimum wages, in the longer run, may lead to declines in poverty and the share of families on public assistance, whereas higher welfare benefits appear to have adverse longer-run effects, although the evidence on minimum wages and welfare benefits—and especially the evidence on minimum wages—is not robust to using only more recent data, nor to other changes. In our view, the most robust relationships we find are consistent with the EITC having beneficial longer-run impacts in terms of reducing poverty and public assistance, whereas there is essentially no evidence that more generous welfare delivers such longer-run benefits, and some evidence that more generous welfare has adverse longer-run effects on poverty and reliance on public assistance—especially with regard to time limits.

The comparison across anti-poverty policies is perhaps the most important evidence we provide. In our view, we have captured the main anti-poverty policies that target working-age adults and that can affect both their work incentives and their income from work. Given the strong, short-term pro-work incentives of the EITC established in other research, the evidence we find suggesting beneficial longer-run effects of the EITC might seem like a natural conclusion. But it is by no means a foregone conclusion, as there is little evidence on longer-run effects, nor evidence that simultaneously looks at these key anti-poverty policies. And we caution that more work is needed to pin down EITC effects, given that our results depend on using national policy variation, while at least one paper has found beneficial (short-run) effects of the EITC using only the state-level variation (Neumark and Wascher 2011).

Our evidence on how anti-poverty policies change economic outcomes in disadvantaged neighborhoods could connect in important ways to the intergenerational mobility literature, which emphasizes the importance of place in longer-run economic outcomes. Moreover, it may be possible to draw some specific policy links. For example, one key finding in this research is that neighborhoods with larger fractions of single parents are associated with poorer future outcomes for children (Chetty et al. 2014). This

49. The pattern of bias for time limits is the same as for the EITC. In column (3), there is an estimated negative lead for tighter welfare time limits (significant at the 10% level). Assuming the same type of persistence, this implies a bias towards finding that tighter time limits reduce poverty, consistent with the smaller estimate of the longer-run effect of tighter time limits once we include the leading effect (in the last row of column (3)). However, the negative leading effect for welfare benefits—assuming the same persistence—should imply a bias against finding that more generous welfare benefits increase poverty and public assistance receipt. But including the leading effects has the opposite effect. This may be related to differences in the persistence of welfare benefits, especially coupled with the post-welfare reform issues discussed earlier.

50. One might wonder whether the evidence of leading effects we find is driven in part by defining disadvantage in terms of the poverty rate, which is related to poverty and public assistance outcomes (although our outcomes are measured in later decades). However, the estimated leading effects and the implications for the estimates were similar using the other measures of disadvantage from Table 4 (results available upon request).

suggests that beneficial longer-run effects of the EITC in reducing poverty could also lead to positive intergenerational effects.

Finally, we have focused on the longer-run effects of three key policies chosen because they are most likely to affect work incentives. In principle, of course, a whole set of policies, going back to early childhood interventions, could have longer-run effects on labor market and socioeconomic outcomes of individuals, families, and neighborhoods.[51] Most work, even on short-term policy effects on labor market outcomes, has focused on policies in isolation, and the same is true of the much more miniscule literature on longer-run policy effects. We readily acknowledge, however, that there is potentially a great deal more to be learned from simultaneously considering the effects of more policies, including their interactions, although the empirical challenges are likely to be severe. Moreover, the fact that some of our findings depend on the sources of policy variation we use highlights that there are further challenges in estimating longer-run policy effects.

APPENDIX A

TABLE A1
1970 NCDB Versus 1970 Decennial Census Selected Characteristics

	NCDB	Census
Share in urbanized areas	N.A.	73.5%
Share black	11.8%	11.1%
Share high school graduate or less	76.9%	78.6%
Share in poverty	11.1%	13.7%
Share of families headed by single mothers	13.4%	5.9%
Share employed	56.4%	54.3%
Share receiving public assistance[a]	5.4%	5.3%
Average household earnings[a]	$59,967	$50,106

Notes: Table reports NCDB and Census estimates of several sample characteristics and outcomes, drawn from the NCDB and National Historical GIS (NHGIS) organization for the Decennial Census data (Manson et al. 2018). NCDB statistics are weighted by tract population. Residence in urbanized areas was not reported in the NCDB in 1970, but given how areas were selected for being tracted, it is reasonable to assume the figure is close to 100%.

[a]For public assistance and earnings, the NCDB reports household-level data. In the NHGIS data, the numerator and denominator for public assistance are families instead of households. The NHGIS reports aggregate earnings by sex instead of by household, but once summed, the estimates are directly comparable.

51. See the extensive inventory of such policies, and research summaries, in Neumark (2016).

APPENDIX B

In this appendix, we discuss three other sets of results that help establish the specification in Table 2 (and close variants thereof) as our preferred specification. First, in Appendix Table B1, we report results paralleling those in Table 2, but including the minimum wage, the EITC, and the welfare variables in separate specifications, rather than simultaneously. The minimum wage results are not at all robust to this alternative analysis, suggesting a strong negative longer-run effect on employment, and strong positive longer-run effects on poverty and public assistance. The EITC results, and more so the welfare time limit results, are robust. The minimum wage results are particularly interesting, because it is very much the norm in the minimum wage literature to exclude other policies from the regression models estimated (as it is, indeed, in many similar panel data analyses of the effects of other policies). In our context, at least, given that precision of the estimates does not decline much from including the three policies simultaneously (compare Table 2 and Appendix Table B1), it is preferable to include the three policies simultaneously.

Second, in Appendix Table B2 we report the results (noted earlier) that we obtain estimating a more saturated model including interactions between the indicator for disadvantaged tracts (DIS^b) and year fixed effects. As the table shows, our estimated EITC coefficients become far less precise, especially for the longer-run effects, with standard errors that increase by a factor of as much as 15 and lead to uninformative confidence intervals. Presumably reflecting this, the estimated EITC coefficients diverge quite strongly from Table 2, with opposite signs for all of the estimated longer-run effects (all insignificant). The precision of the estimated coefficients for the other policies does not change much, but given that we saw the importance of controlling simultaneously for all three policies for pinning down the effects of minimum wages (in Appendix Table B1), it is not surprising that the estimated relationships with minimum wages change dramatically in Appendix Table B2, pointing to large positive relationships with poverty and public assistance. Because of the imprecision of the estimated EITC coefficients, we do not use this more saturated specification. As noted in the main text, we already control for rich geographic heterogeneity in shocks to our outcomes.

Third, coding the generosity of welfare, especially post-welfare reform, is not as clear-cut as, for example, coding the minimum wage. It is difficult to capture the effects of welfare reform in a limited set of variables, as we noted earlier. One concern is that the effects of welfare benefits pre- and post-reform can be quite different, because it became much harder to get benefits in the latter period (e.g., Haveman et al. 2015). Our inclusion of the (tight) time limits variable should help on this score, as it flags states with potentially more stringent rules in the post-reform period. As another alternative, we modified the welfare benefits variable to also include an interaction with a post-1996 dummy variable, to allow the potential effects of benefits to change post-reform. (This variable was included in the same way as the main effects in the preceding specifications—contemporaneous and lagged, with both also interacted with DIS^b.)

The results, reported in Appendix Table B3, are quite robust. The key question concerns the effects of the welfare variables on poverty and public assistance. The interactions between the welfare benefit variables and the post-reform indicator are not significant. The estimated coefficients of benefits for poverty and public assistance in the pre-reform period remain positive and statistically significant, and the

TABLE B1
Effects of Anti-Poverty Policies on Average Earnings per Household, Employment Rate, Share in Poverty, and Share on Public Assistance in Areas with High Poverty Rate at Baseline (1970), 1980–2010, One Policy at a Time

	Earnings (1)	Employment (2)	Poverty (3)	Public Assistance (4)
I. Minimum wages				
Log minimum wage	0.1162**	0.0820***	−0.1854***	−0.2951***
	(0.0450)	(0.0267)	(0.0688)	(0.0990)
10-year lag of log minimum wage	**−0.3407*** **	**−0.1277*** **	**0.6576*** **	**0.7314*** **
	(0.0299)	**(0.0294)**	**(0.0363)**	**(0.0528)**
II. EITC				
Log EITC phase-in rate	0.0846***	0.0191**	−0.1525***	−0.1492***
	(0.0118)	(0.0081)	(0.0101)	(0.0130)
10-year lag of log EITC phase-in rate	**0.0010**	**0.0045**	**−0.0167** **	**−0.0409*** **
	(0.0047)	**(0.0043)**	**(0.0071)**	**(0.0052)**
III. Welfare				
Log maximum welfare benefit	0.1056***	0.0321*	−0.2516**	−0.3394***
	(0.0338)	(0.0180)	(0.1163)	(0.1126)
10-year lag of log maximum welfare benefit	**0.0625*** **	**0.0270*** **	**−0.0808*** **	**−0.1118*** **
	(0.0146)	**(0.0077)**	**(0.0293)**	**(0.0340)**
Welfare time limits (< 60 months)	0.0374**	−0.0176	−0.0967***	−0.0398
	(0.0179)	(0.0128)	(0.0280)	(0.0304)
10-year lag of welfare time limits (< 60 months)	**0.0024**	**0.0094**	**−0.1511*** **	**−0.2427*** **
	(0.0172)	**(0.0086)**	**(0.0299)**	**(0.0330)**
Adjusted R^2	0.69	0.74	0.80	0.76
N	206,652	206,801	206,684	206,652
Tract fixed effects	Yes	Yes	Yes	Yes
County x year interactions	Yes	Yes	Yes	Yes

Notes: See notes to Table 2. The difference in this table is that each panel reports estimates of the specification using minimum wage, EITC, and welfare variables separately, rather than simultaneously.

TABLE B2
Effects of Anti-Poverty Policies on Average Earnings per Household, Employment Rate, Share in Poverty, and Share on Public Assistance in Areas with High Poverty Rate at Baseline (1970), 1980–2010, Saturated Model Absorbing Federal Variation across Disadvantaged Versus Advantaged Tracts

	Earnings (1)	Employment (2)	Poverty (3)	Public Assistance (4)
Log minimum wage	0.0502	0.2093**	0.3936**	0.2537
	(0.1030)	(0.0785)	(0.1559)	(0.1888)
10-year lag of log minimum wage	**−0.1927** **	**−0.1000**	**0.4022*** **	**0.2586** **
	(0.0788)	**(0.0784)**	**(0.0963)**	**(0.1273)**
Log EITC phase-in rate	−0.0867	−0.0409	−0.1742	0.0253
	(0.1021)	(0.0449)	(0.1245)	(0.1700)
10-year lag of log EITC phase-in rate	**−0.0246**	**0.0400**	**0.2085**	**0.3997**
	(0.1631)	**(0.0537)**	**(0.2423)**	**(0.3041)**
Log maximum welfare benefit	0.0552	0.0004	−0.0995**	−0.1963***
	(0.0392)	(0.0173)	(0.0447)	(0.0594)
10-year lag of log maximum welfare benefit	**−0.0033**	**0.0001**	**0.0825*** **	**0.0373**
	(0.0239)	**(0.0135)**	**(0.0247)**	**(0.0327)**
Welfare time limits (< 60 months)	−0.0088	−0.0281**	−0.0196	0.0227
	(0.0226)	(0.0114)	(0.0223)	(0.0236)
10-year lag of welfare time limits (< 60 months)	**−0.0170**	**−0.0166**	**−0.0557**	**−0.0511**
	(0.0276)	**(0.0139)**	**(0.0336)**	**(0.0515)**
Adjusted R^2	0.69	0.74	0.80	0.77
N	206,652	206,801	206,684	206,652
Tract fixed effects	Yes	Yes	Yes	Yes
County x year interactions	Yes	Yes	Yes	Yes

Notes: See notes to Table 2. The only difference is the inclusion of the $DIS^b \cdot YR$ interactions.

TABLE B3
Effects of Anti-Poverty Policies on Average Earnings per Household, Employment Rate, Share in Poverty, and Share on Public Assistance in Areas with High Share Poverty at Baseline (1970), Separate Effects of Welfare Benefits Post-Welfare Reform, 1980–2010

	Earnings (1)	**Employment (2)**	**Poverty (3)**	**Public Assistance (4)**
Log minimum wage	0.1031	0.1879***	0.0069	−0.2396
	(0.0930)	(0.0519)	(0.1421)	(0.1808)
10-year lag of log minimum wage	**−0.1475**	**−0.0994**	**−0.1469**	**−0.3892****
	(0.0917)	**(0.0841)**	**(0.1088)**	**(0.1648)**
Log EITC phase-in rate	−0.0238	−0.0527	−0.2707*	−0.1578
	(0.0946)	(0.0502)	(0.1507)	(0.2649)
10-year lag of log EITC phase-in rate	**0.0168**	**0.0262*****	**−0.0371***	**−0.0916*****
	(0.0144)	**(0.0060)**	**(0.0211)**	**(0.0339)**
Log maximum welfare benefit	0.0314	−0.0117	0.0678	0.0168
	(0.0380)	(0.0219)	(0.0731)	(0.0813)
10-year lag of log maximum welfare benefit	**−0.0193**	**0.0016**	**0.1563*****	**0.1456*****
	(0.0204)	**(0.0103)**	**(0.0291)**	**(0.0400)**
Log maximum welfare benefit	0.0293	0.0636*	−0.1418	−0.1805
x post-welfare reform	(0.0505)	(0.0324)	(0.0956)	(0.1643)
10-year lag of log maximum welfare benefit	**−0.0180**	**−0.0579***	**0.1503**	**0.1669**
x post-welfare reform	**(0.0542)**	**(0.0335)**	**(0.1020)**	**(0.1591)**
Welfare time limits (< 60 months)	−0.0062	−0.0276**	−0.0280	0.0097
	(0.0235)	(0.0114)	(0.0248)	(0.0307)
10-year lag of welfare time limits (< 60 months)	**−0.0073**	**−0.0114**	**−0.0941*****	**−0.1114****
	(0.0279)	**(0.0130)**	**(0.0307)**	**(0.0459)**
Adjusted R^2	0.69	0.74	0.80	0.77
N	206,652	206,801	206,684	206,652
Tract fixed effects	Yes	Yes	Yes	Yes
County x year interactions	Yes	Yes	Yes	Yes

Notes: See notes to Table 2. The only difference is the additional set of welfare benefit variables interacted with the post-welfare reform (year >1996) variable.

estimated coefficients for tighter time limits remain negative and statistically significant. Thus, nothing in this analysis indicates we need to distinguish the effects of welfare benefits pre- and post-reform.

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