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HELPING WITHOUT HURTING

The Long-run Effects of Anti-Poverty Policies on Disadvantaged Neighborhoods

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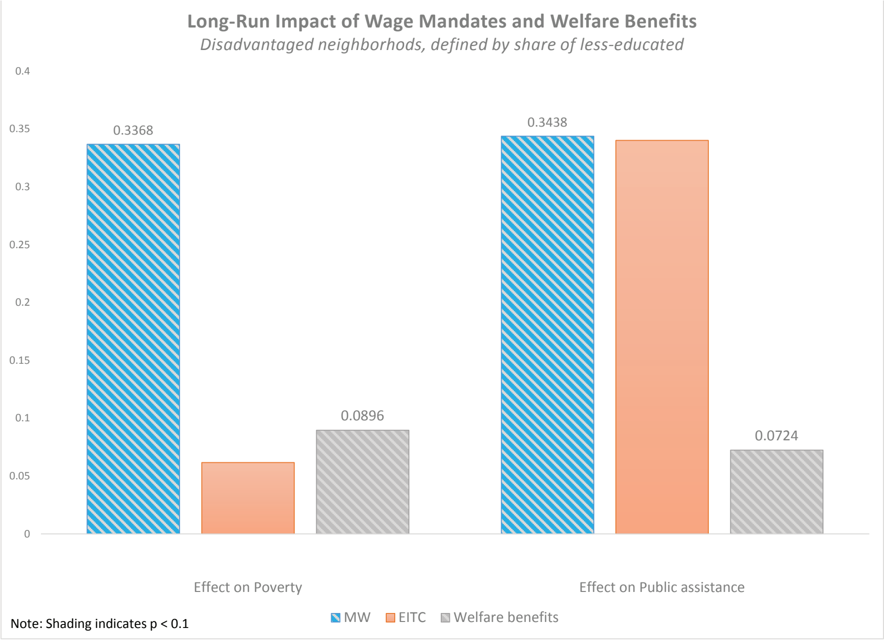
SUMMARY

Decades of economic research and policymaking have focused on minimum wages, tax credits, and welfare programs as essential tools to improve the lives of struggling and disadvantaged Americans.

In this new study, economists David Neumark and Brittany Bass of the University of California, Irvine, and Brian Asquith of the National Bureau of Economic research, measure the long-run effects of minimum wages, the Earned Income Tax Credit (EITC), and welfare programs on reducing poverty in disadvantaged areas.



This research is necessary because, according to Neumark et al., debates about these anti-poverty policies focus on short-term effects of individuals and their families instead of disadvantaged areas more broadly. The researchers simultaneously study four effects of minimum wages, EITC, and welfare programs: earnings, employment, poverty, and reliance on public assistance. The resulting data are used to estimate the effects of these policies on disadvantaged neighborhoods over the past three decades, which offers insight into how policy influences their long-run success or failure.

The researchers' most notable conclusion is that neither a higher minimum wage nor more-generous welfare benefits have reduced poverty rates in the country's most-disadvantaged neighborhoods. In fact, the authors find some evidence that poverty rates and the share of residents on public assistance have increased alongside a rising minimum wage. (They also find some evidence that, for disadvantaged neighborhoods, the long-run effect of more-generous welfare benefits has been to increase poverty and receipt of public assistance.)

To put this in practical terms, it means that each $1 increase in the minimum wage has, in disadvantaged neighborhoods over the past three decades, increased poverty rates and the receipt of public assistance by roughly three percent.

These results call into question one of the most oft-cited talking points in favor of raising the minimum wage—that it will reduce poverty and in turn reduce public assistance. The net effect of a rising minimum wage could be to further reduce workplace opportunities for those workers who need them most.

Ultimately, these data offer critical insight to policymakers who genuinely seek to reduce poverty. These findings cast serious doubt on whether the normal poverty-reduction policies--minimum wages and welfare programs--actually contribute to increased employment, reduced poverty, and higher household earnings. Indeed, this study should give pause to any level of government interested continuing or expanding such policies.

I. INTRODUCTION

The long-running research record and policy debates about anti-poverty policies have two important shortcomings that we seek to begin to rectify in this paper. First, they 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. Second, they have largely ignored "place," focusing on program effects on individuals and their families, without asking whether these policies have succeeded in lifting the economic fortunes of particularly disadvantaged areas.

We counter these shortcomings with respect to the main anti-poverty policies in the United States that attempt to increase income from work, or that might strongly affect work incentives – minimum wages, the Earned Income Tax Credit, and welfare (and welfare reform).[1] We estimate the longer-run effects of these policies on measures of economic self-sufficiency – most importantly, poverty and receipt of public assistance – and focus on their effects in neighborhoods that are initially disadvantaged. 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.

Turning first to the issue of longer-run effects, research on minimum wages has focused almost exclusively on the short-term employment effects of minimum wages. By far the most common focus of minimum wage research is the employment impact on teenagers (see the review in Neumark and Wascher, 2007), and more recently on other low-wage workers like restaurant workers (e.g., Dube et al., 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 be broached more in recent research (e.g., Sabia and Burkhauser, 2010). Moreover, virtually no work has studied the longer-run effects of minimum wages, with three exceptions: indirect evidence on training (or education), which could affect earnings in the longer term (e.g., Acemoglu and Pischke, 2003); Neumark and Nizalova's (2007) paper that finds adverse longer-run effects on adult earnings of exposure to a higher minimum wage as a teenager; and more recent work by Clemens and Wither (2014) reporting that binding minimum wage increases during the Great Recession period lowered the income growth of affected workers.

Research on the Earned Income Tax Credit (EITC) has also focused on employment (e.g., Meyer, 2010), although some work studies the effects of the EITC on poverty (e.g., Neumark and Wascher, 2011). However, the only study of which we are aware that examines longer-term effects of the EITC via work incentives is the Dahl et al. (2009) study of impacts on individual women's earnings up to five years after a major federal expansion of the EITC.[2] The EITC is sometimes viewed as a more effective policy than the minimum wage regarding raising 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, the literature on welfare is extensive, and has focused on both employment effects (e.g., Grogger, 2003) and distributional effects (e.g., Bitler et al., 2006). There is far less work on longer-run effects, although Grogger (2009), and Hotz et al. (2006) 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, 1983).

The existing research has also focused nearly exclusively on effects of anti-poverty policies on individuals or families, and not on effects in areas of concentrated disadvantage or poverty. Perhaps the only exception is Thompson (2009), who shows that federal minimum wage increases in the mid-1990s had more adverse effects on teen employment in counties where minimum wages were more binding because of lower market wages. Our paper goes well beyond Thompson's analysis – studying a much longer time horizon (1970-2010), including outcomes across all age ranges, and using a more disaggregated level of geography (the Census tract). Thus, existing research has not addressed whether, and to what extent, anti-poverty policies are beneficial or detrimental in helping to lift the economic fortunes of particularly disadvantaged areas. This is a potentially important question 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.[3]

Geographically-concentrated poverty poses its own challenges above and beyond individual poverty, perhaps most importantly for minorities, given that minorities tend to cluster residentially in poor areas.[4] Moreover, research suggests that living in impoverished 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.[5] Thus, if anti-poverty policies lead to greater poverty in areas of concentrated poverty, their adverse consequences may be exacerbated, extending beyond those who are directly affected. Conversely, policies that are particularly beneficial in disadvantaged areas may have important short- and long-term positive spillovers, as disadvantaged neighborhoods can have lasting impacts on the next generation (Chetty et al., 2014).

This paper is distinguished by a number of features. First, we simultaneously examine the effects of multiple anti-poverty policies, which provides direct comparisons of their effects and ensures that we do not spuriously attribute the effects of one policy to the effects of others. Second, we estimate policy effects on disadvantaged areas. And third, we look at longer-run effects, with a sample covering many decades.

To briefly summarize the results, we find evidence that higher minimum wages lead, in the longer run, to increases in poverty and the share of families on public assistance. We find some evidence that the EITC has positive longer-run employment effects. We do not generally find significant evidence of longer-run effects of the EITC on poverty or public assistance in our standard difference-in-difference-in-differences specification. But in some specifications, especially when we allow the national changes in the EITC to influence the estimates, we find evidence that the more generous EITC reduced poverty and the share on public assistance. Finally, we find evidence that more generous welfare benefits lead to higher poverty and public assistance in the longer-run. Perhaps the most robust important conclusion is that a higher minimum wage and more generous welfare benefits do not *reduce* poverty and reliance on public assistance in the longer-term.

II. RESEARCH STRATEGY

Our econometric strategy is to use a panel data, triple-difference (DDD) approach to estimate the longer-run effects of minimum wages and other anti-poverty policies on economic outcomes of Census tracts that are initially disadvantaged, relative to other tracts.[6] To explain the approach, denote tracts by c, states by s, and years by t. Denote by Y_{cst} an outcome variable; we will focus most strongly on the poverty rate and the share of households on public assistance, but will study other outcomes as well.[7] Denote by P_{cst} a vector of policies that can vary by state and year. Finally, denote by DIS^b_c a measure of initial disadvantage defined at the tract level; DIS^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).

We use the Neighborhood Change Database (NCDB), which provides measures of Y for 1980, 1990, 2000, and 2010, and measures of DIS^b for 1970.[8] We specify our model to estimate the longer-run impacts of the anti-poverty policies in P

on initially disadvantaged tracts. The standard version of the DDD approach applied to these data – simply treating tracts as a set of observations from the state in which they are located – is the following:

$$(1)\quad Y_{cst} = \alpha + \{DIS^b{}_c \cdot P_{cst}\} \cdot \beta + \{DIS^b{}_c \cdot P_{cs,t-10}\} \cdot \beta^L + P_{cst} \cdot \gamma + P_{cs,t-10} \cdot \gamma^L$$
$$+ \delta DIS^b{}_c + YR_t\tau + \{DIS^b{}_c \cdot YR_t\} \cdot \lambda + ST_s \cdot \pi + \{DIS^b{}_c \cdot ST_s\} \cdot \psi + \theta \cdot \ln(PSE_{cst}) + \varepsilon_{cst}\ .$$

In equation (1), the parameters β and β^L capture the contemporaneous and 10-year lag effect of the policies in *P*; these are the DDD estimates. The other variables (except for *PSE*, a control variable discussed below) are the main effects and two-way interactions between the indicator for disadvantaged tracts (DIS^b), year fixed effects (*YR*), and state fixed effects (*ST*). With these included, 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*.

Using this estimator, the effects of policy are identified only from state-level variation. Thus, for example, differential effects of federal EITC variation in disadvantaged relative to advantaged areas, common to all states, is absorbed in the $DIS^b \cdot YR$ interactions. It is conceivable that this eliminates an important dimension by which the EITC influences the outcomes we study. On the other hand, this approach avoids attributing to the EITC the effects of other sources of national-level changes in disadvantaged tracts.[9]

Simplifying, and thinking about *P* as containing only one policy, if we were to interpret the estimate of γ^L as a causal effect in the non-disadvantaged tracts, then the estimate of $\beta^L + \gamma^L$ measures the effect of the policy in the most-disadvantaged tracts. However, typically in the DDD strategy, the main policy effects variables (P_{cst} and $P_{cs,t-10}$) are interpreted as control variables for other shocks that are correlated with the policy variation, and the estimates of γ and γ^L are not given a causal interpretation. Indeed, it is quite common to control for these shocks more flexibly by saturating the model with area-by-period fixed effects.

With policy variation at the state level, this would entail adding state-by-year effects. These effects will subsume all state-level policy variation over time, and hence the main policy effects for state-level policy drop out. In this case, the model takes the form:

$$(2)\quad Y_{cst} = \alpha + \{DIS^b{}_c \cdot P_{cst}\} \cdot \beta + \{DIS^b{}_c \cdot P_{cs,t-10}\} \cdot \beta^L$$
$$+ \delta DIS^b{}_c + YR_t\tau + \{DIS^b{}_c \cdot YR_t\} \cdot \lambda + ST_s \cdot \pi + \{DIS^b{}_c \cdot ST_s\} \cdot \psi + \{ST_s \cdot YR_t\} \cdot \omega$$
$$+ \theta \cdot \ln(PSE_{cst}) + \varepsilon_{cst}\ .$$ [10,11]

Finally, we also add tract fixed effects (*CT*) to account for time-invariant heterogeneity across Census tracts. These fixed effects subsume the DIS^b main effect in the prior equations, as well as the state fixed effects and the $DIS^b \cdot ST$ interactions, leading to the model we estimate:

$$(3)\quad Y_{cst} = \alpha + \{DIS^b{}_c \cdot P_{cst}\} \cdot \beta + \{DIS^b{}_c \cdot P_{cs,t-10}\} \cdot \beta^L$$
$$+ CT_c \cdot \delta' + YR_t\tau + \{DIS^b{}_c \cdot YR_t\} \cdot \lambda + \{ST_s \cdot YR_t\} \cdot \omega + \theta \cdot \ln(PSE_{cst}) + \varepsilon_{cst}\ .$$

We also always report alternative results when we drop observations in the third quartile of the observations used to define DIS^b. In this case, the "control" or "untreated" tracts are more sharply delineated from the tracts in the top quartile of disadvantage, because we omit observations for which DIS^b is between the median and the 75th percentile. This is a potentially useful analysis because the DDD approach estimates the relative effects of policies on the most-disadvantaged areas, rather than absolute effects. It is possible, for example, that a higher minimum wage reduces poverty more in somewhat less-disadvantaged areas than in the most-disadvantaged areas, by, for example, delivering more wage gains in somewhat more-advantaged areas because of higher employment rates, and generating weaker job losses because workers are higher skilled. In this case, the DDD estimator using the three more-advantaged quartiles as controls could mask an overall poverty reduction induced by the higher minimum wage. We suspected this was unlikely, since the anti-poverty policies we study target disadvantaged people and families, and there is considerable residential segregation by income across tracts (e.g., Watson, 2009). Indeed, our DDD estimates are very robust to omitting the observations in the third quartile of our measures of disadvantage. We explore this issue in more detail below.

One variable we always include, which is potentially important given the long sample period used, is a control for the effects of long-term changes in the structure of jobs in the aggregate economy on specific subareas within a state. For example, it is widely agreed that declines in manufacturing hit subareas of states where manufacturing was concentrated (think the South Side of Chicago, or Flint, Michigan), as highlighted in the seminal work of Wilson (1990) or in Autor et al. (2013). To address this issue, we use the approach, which originated with Bartik (1991), of applying national time-series changes in aggregated industry employment to the tract or other subareas, based on the tract's or subarea's industry composition in the baseline period of stable aggregate economic growth. While it is most natural to think of this in terms of industry, in the NCDB data we can only do this for occupation.[12]

Let subscript k index occupations. Denote by SE_{cskb} total employment in tract c, state s, occupation k, and baseline period b, denote by AE_{kt} aggregate (national) employment in each period t in occupation k, and denote by AE_{kb} aggregate employment in occupation k in the baseline period b. Then tract (or subarea) employment based solely on aggregate developments is predicted in each period after b by applying the national changes to the baseline composition, as in

$$(4) \qquad PSE_{cst} = \sum_k SE_{cskb} \times \left(\frac{AE_{kt} - AE_{kb}}{AE_{kb}}\right).$$

Equation (4) predicts tract employment in each period by applying the national growth rate of employment in each occupation between the baseline period and that period to the baseline employment level in the corresponding occupation in the tract, and then aggregating, weighting by the baseline occupational distribution of employment in the tract. This control is entered in logs, since the level can differ so much across tracts.

Aside from the controls we have discussed, the model is parsimonious. It does not control for other characteristics of the population that may have changed over time (such as educational levels), because skill-related compositional shifts may be endogenous. For example, people may respond to a higher wage by acquiring more education (although the theoretical prediction is ambiguous; see, e.g., Agell and Lommerud, 1997). Thus, the exclusion of these compositional changes is appropriate in estimating policy impacts.[13]

Since the individuals more likely to benefit from anti-poverty policies are more concentrated in disadvantaged areas, we might expect some similarities to findings from evidence focused on low-skill or low-income individuals or families. However, because our estimates focus on place we may also pick up more general effects on disadvantaged locations stemming from direct income effects of anti-poverty policies, or spillovers that occur through other channels. The place-based approach of our analysis may lead to different results from an approach based on individuals or families for other reasons. For example, in the disadvantaged areas we study, there may be a dearth of labor demand, and a large share of jobs may be at the minimum wage. In this case, a policy like the EITC, which increases labor supply, may do relatively little to increase employment, suggesting we may find weaker effects than, for example, analyses based on families without regard to where they live.

III. DATA

Neighborhood Change Database

Our data on economic outcomes and other measures by tract come from the NCDB.[14] The NCDB provides tract-level aggregates on many of the key outcomes that are relevant to our inquiry – in particular, earnings, employment, poverty, and public assistance. Importantly, the NCDB provides consistent tract definitions over time. In particular, it includes historical tract populations, demographic, and socioeconomic characteristics in 2010 Census tract geography, providing consistent longitudinal measures of these variables.[15] However, because we estimate effects over many decades, based on characteristics of tract residents in a much earlier period (using 1970 as our baseline period to define DIS_b), we are restricted in the set of tracts we can use.[16]

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").[17] 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 dataset for this analysis.[18]

The longer-run perspective of our project 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 U.S. as a whole, but to date, the 1960 Census is not included in the NCDB.

With regard to policy, 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 – federal variation after 1960, and state variation 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 limitation of starting our analysis is 1980 (with DIS^b measured in 1970) is not too limiting.

Minimum Wages

We also constructed data series by state on minimum wages, the EITC, and welfare.[21] Information on state minimum wages from 1983-2014 was taken from the panel described in Neumark et al. (2014). We extended the data back to 1960 relying on Quester (1980) and Sutch (2010),[22] also cross-referencing dates and numbers against state and federal sources.[23] We code the minimum wage as the higher of the state or federal minimum wage, as is standard, since lower state minimum wages, if they exist, apply to a tiny fraction of workers. In the analysis, we lag the minimum wage one 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.

Earned Income Tax Credit (EITC)

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 $F2\% \cdot (1 + S\%)$.[26]

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

Welfare

We include two measures of welfare generosity or stringency. From 1962-1996, the United States 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 two dependent children.[27] Second, for the post-welfare reform period, we include a dummy variable for whether 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 21-48 months, but generally about two 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] For states where the maximum welfare payment could not be determined from these sources, the average across other states in the state's Census Division was used.[32] 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.[33] 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. The most notable feature is the substantial variation across states. Figure 4 displays information on time limits.

Measures of Disadvantage

We use two measures of disadvantage: the share of the population with a high school degree or less, and the share of the population that is black.[34] We could measure disadvantage based on income-related measures, like poverty. However, such measures seem more likely to be endogenously related to the policies we study – either contemporaneously, or because policy may respond to them over the longer-run.

Figures 5 and 6 provide some information on the geographic distribution of tracted areas as of these years, and of our disadvantage measures. Figure 5 shows areas tracted in 1970, with differential shading for tracts in the four quartiles of the share disadvantaged – based on the share with a high school degree or less. (The darkest shading is for the highest quartile of this share – i.e., the most disadvantaged tracts.) As the figure shows, a small geographic area was tracted, but the tracted areas include most of the population.[35] Figure 6 provides similar information for the share black. The tracted areas are lighter, because the share black is quite a bit lower, on average.

IV. CORE RESULTS

Descriptive Statistics

Table 1 reports descriptive statistics. Recall that these are means across tracts, not individual units, and hence may not line up with the latter kinds of estimates. The top panel reports means (and standard deviations) for the outcomes we study. The earnings variable is 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 earnings data are in nominal terms, which is why they rise sharply. 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.[36]

The share on public assistance is lower (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 NCDB data. This change should not influence our results materially, since the definitional change should be captured in the year effects that are included in the model, and their interactions with DIS^b.[37]

The middle two panels report descriptive statistics for our four outcome measures for the disadvantaged tracts, based on our two measures classifying tracts in the top quartile of disadvantage. As we would expect, earnings and employment are lower, and poverty and the share on public assistance higher. For earnings, employment, and public assistance, the differences are larger for tracts defined as disadvantaged based on low education.

The bottom panel reports the disadvantage measures for 1970. We also report the 75th percentiles of these measures – the cutoff for defining DIS^b.
Regression results

We report our results in four main tables: earnings (Table 2), employment (Table 3), poverty (Table 5), and the share on public assistance (Table 6). These tables report estimates of equation (3), reporting only the estimates of the DDD coefficients β and β^L, on the variables $DIS^b_c \cdot P_{cst}$ and $DIS^b_c \cdot P_{cs,t-10}$, respectively.

Earnings

The baseline earnings estimates appear in column (1), defining disadvantaged tracts in terms of the share with low education, and in column (3), using instead the share black. There is no statistically significant evidence of longer-run (or contemporaneous) effects of minimum wages on average household earnings in disadvantaged tracts, and indeed the estimates are as likely to be negative as positive for the longer-run effects.

Because both 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 tracts (per the estimator we use, relative to the effect in other tracts). This way of specifying the model and interpreting the magnitudes allows comparisons with minimum wage-earnings elasticities reported in other studies – although typically other studies estimate these for wages of low-skilled individuals, and focus only on short-term, contemporaneous effects. In the standard minimum wage literature, contemporaneous wage elasticities in the 0.1 to 0.2 range are not uncommon (e.g., Allegretto et al., 2011). Aside from the differences just noted, the elasticities we estimate include both hours and employment effects, and not just effects on wages, suggesting they should be lower and could be negative.[38]

For both the low education and the share black specifications, we find positive longer-run effects of the EITC. The estimated effect is larger, and statistically significant, for disadvantaged tracts defined based on the share black, with an elasticity around 0.41. The contemporaneous effects are negative; this finding may reflect the fact that the EITC increases labor supply, which can depress market wages (Leigh, 2000),

There is no evidence of statistically significant longer-run effects of either welfare benefit levels or time limits on average household earnings. The contemporaneous effects of welfare benefits are positive (with elasticities a bit above 0.1), and the contemporaneous effects of time limits are negative and significant at the 10-percent level for the share black specifications. If anything, we might expect higher benefits to reduce labor supply and time limits to encourage work. There could be offsetting substantial wage effects if labor demand is relatively inelastic. However, the employment effects (discussed below) are in the same direction, so we suspect it is more plausible that the contemporaneous effects may in part reflect endogenous policy choices such that welfare is more generous when labor market outcomes imply lower recipiency or benefits.

Columns (2) and (4) report results when we drop observations in the third quartile of the observations used to define DIS^b. The results are very robust to this change, suggesting that we are isolating effects in the most-disadvantaged tracts, by our measures.

Employment

Table 3 turns to the employment rate. The baseline estimates in columns (1) and (3) are very similar to those dropping the third quartile of the observations used to define DIS^b, so we do not discuss the latter estimates separately.[39]

There is consistent evidence of positive effects of minimum wages in the short run, with elasticities mainly in the 0.13 to 0.17 range, always significant at the 5- or 10-percent level. These positive short-run estimates contrast with much evidence of negative employment effects for the least-skilled workers, but the estimates in Table 3 are identified from different groups – residents of tracts that were disadvantaged many decades back. The longer-run employment estimates are negative in three out of four cases, but never statistically significant (and very near zero for the share black specifications).

For both the low education and the share black specifications, we find positive longer-run effects of the EITC, with elasticities in the 0.15 to 0.24 range, statistically significant at the 5- or 10-percent level. Curiously, the contemporaneous effects are negative, which differs from the EITC literature focused on low-skilled, often single, mothers. We come back to this point later.

We find significant positive longer-run effects of welfare benefit on employment, although only for the low education specifications, and with very small elasticities (about 0.02).[40] The estimated longer-run effects of time limits are negative – the opposite of what we might anticipate – but not statistically significant.

As noted above, the EITC estimates point to positive employment effects in the longer-run. The existing literature focuses on women, and usually on low-skilled women defined in terms of education.[41] In Table 4, we report estimates of our baseline employment specification by sex, and find that there is stronger evidence of positive longer-run employment effects of the EITC for women. Across the two specifications, the elasticity is larger by about 0.08, and it is only strongly statistically significant for women (and is insignificant for men in the low education specification). A limitation of the NCDB data is that we cannot test for differences by marital status or number of children. The same considerations might explain why the estimated effects of welfare policy do not appear to be sharply different between men and women.

Poverty

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, we are interested in the longer-run effects of anti-poverty policies on economic self-sufficiency, and reductions in poverty and dependence on public assistance would be consistent with increases in 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.

The evidence on minimum wages is unambiguously in one direction. The estimated longer-run effects are positive, indicating that minimum wage increases raise poverty in disadvantaged areas in the longer-run. The evidence is stronger statistically for the specification based on the share with low education, which may be because education is more closely related to skill levels than race. The contemporaneous effects are also positive, although the evidence is weaker statistically. Note that this is not inconsistent with past research on the effects of minimum wages on poverty (although again, the existing research does not focus on disadvantaged locations, or longer-run effects). There is little evidence suggesting that minimum wages reduce poverty (e.g., Sabia and Burkhauser, 2010; an exception is Dube, 2014), and some evidence suggesting the opposite (Neumark et al., 2005). There is also some evidence of positive contemporaneous effects of the minimum wage, which is surprising, and a result we come back to later.

There is no clear evidence that, in the longer run, the EITC reduces poverty in disadvantaged areas. The estimates using either measure of disadvantage are relatively small, statistically insignificant, and not consistently of one sign. It is hard to compare this evidence with prior results focusing on women with children (e.g., Neumark and Wascher, 2011) because, as already noted, with the NCDB data we cannot estimate separate effects for families with children – let alone zoom in further on low-skilled, single mothers for whom extensive margin employment effects are most likely. We did (as reported in Table 4) detect a longer-run positive employment effect on women. However, the results on poverty can be more complicated, if, for example, some individuals or families experience lower earnings because of labor supply increases. There is, again, some anomalous evidence for the EITC, for the specifications defining disadvantage in terms of the low education share, for which we find a positive short-run effect of the EITC on poverty. We return to this point below.

The estimates in Table 5 indicate that more generous welfare benefits increase poverty in the longer-run, with elasticities of around 0.1. There are negative contemporaneous effects, which could reflect the direct effects of higher benefits prior to any longer-run effects influencing labor supply and human capital accumulation; however, we have been cautious about overinterpreting the contemporaneous effects, and some of these have been harder to rationalize in the prior tables. For time limits, the longer-run estimates are all negative, consistent with poverty reductions, but none of the estimates are statistically significant. Thus, the main result is that more generous welfare benefits appear to increase poverty in the longer run.

Share on Public Assistance

We might expect qualitatively similar results for the share on public assistance as for poverty, since the two are related, and this is very much the case. First, a higher minimum wage increases the share of households receiving public assistance in the longer-run. Second, there is no statistically significant evidence of a longer-run effect of the EITC. And third, the welfare effects are similar: there is a positive effect of welfare benefits on the share receiving public assistance, although this evidence is weaker for the share black specifications; and there are negative but insignificant estimates of the effects of time limits.

V. UNDERSTANDING THE SOURCES OF IDENTIFICATION

Our evidence to this point suggests that higher minimum wages lead, in the longer run, to increases in poverty and the share of families on public assistance. We find some evidence that the EITC has positive longer-run employment effects, but we do not find significant evidence of longer-run effects on poverty or public assistance. Finally, we find evidence that more generous welfare benefits lead to higher poverty and public assistance in the longer-run. In this section, we present additional evidence that addresses three potential limitations of our analysis. First, it is difficult to capture the effects of welfare reform in a limited set of variables. Second, it is difficult to identify long-term effects of policy – let alone of multiple policies. Third – and potentially related to the prior point, there are some anomalous estimates in some of our previous tables – in particular, the negative contemporaneous employment effects of the EITC (Table 3), the positive contemporaneous effects of the EITC on poverty, for the low education specifications (Table 5), and the rather large positive contemporaneous effects of the minimum wage on poverty. With regard to the second and third limitations, we explore additional evidence to understand the sources of identification for our longer-run policy effects.

Classifying/Coding Welfare Policy

Coding the generosity of welfare, especially post-welfare reform, is not nearly as clear-cut as, for example, coding the minimum wage.[42] One concern is that the effects of welfare benefit pre- and post-reform can be quite different, because it became much more difficult 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 they flag potentially more stringent rules in the post-reform period. As another alternative, we modified the welfare benefits variable to also always include an interaction with a post-1996 dummy variable, to allow the effects of benefits to change after welfare reform. (This variable was included in the same way as the main effect in the preceding specifications – i.e., contemporaneous and lagged, with both also interacted with DIS.)

The results, reported in Table 7, change substantively for the specifications defining disadvantage based on the share black (but not the share with low education). In particular, we still find that more generous welfare benefits increased poverty, in the longer run, in the pre-welfare reform period. However, this effect is no longer present for the effect of welfare benefits in the post-reform period. The estimated longer-run effect of the welfare benefits-post-reform interaction is negative (-0.22), and the summed effect is negative, although not statistically significant. This evidence provides some hint that, at least for disadvantaged black neighborhoods, welfare reform may have eliminated the adverse longer-run effects of more generous welfare benefits on poverty. However, note that the longer-run effect of welfare benefits in the post-reform period is identified only from the 2006-2010 data.

One other finding of note in Table 7 is that, once we allow more flexibly for the effects of welfare reform, we find – again, when we define disadvantage based on the share black – statistically significant evidence that the longer-run effect of the EITC is to reduce poverty. (The corresponding estimates was negative, but not significant, in Table 5.)

Endogenous Policy?

Our identification relies on the differential effects on disadvantaged tracts of state-level variation in policy, and we focus on 10-year lags. Nonetheless, it is possible, in principle, that policy variation is driven by prior changes in disadvantaged relative to more-advantaged tracts in the outcomes we study. If, in addition, there is some persistence in these outcomes, then our estimated policy effects could be biased by this policy endogeneity. We address this question by asking whether policy changes can be predicted by past outcomes.[43] This of course parallels asking whether there are leading effects of policy that – barring anticipation effects – could point to endogenous policy responses that bias estimates of causal effects.

To take a specific example of potential bias, consider our evidence that more generous welfare benefits increase the share in poverty in disadvantaged tracts. Suppose that state governments adopt more generous benefits when poverty is high, to try to help poor families. In that case, some persistence in the share in poverty would generate evidence that adoption of more generous welfare benefits is associated with a high share in poverty. Our evidence that a higher minimum wage increases the share in poverty or on public assistance could have a similar interpretation.

We would expect the relationships just described to be stronger for our estimated contemporaneous effects than for our estimated longer-term effects. For the estimated effects of minimum wages, the contemporaneous estimates are, in fact, often about the same magnitude (although not larger). However, for the estimated effects of welfare benefits on poverty or public assistance, the estimated contemporaneous effects are not even the same sign.

We present more systematic evidence in Table 8, where we report estimates of regressions of our four policy variables (the minimum wage, the EITC, welfare benefits, and time limits) on the 10-year lag of either the poverty rate or the share on public assistance, and the interaction of this lag with the indicator for the most-disadvantaged tracts. We also include the 10-year lag of the policy. These regressions tell us whether there is a relationship between policy changes and past variation in poverty or the share on public assistance in the disadvantaged tracts in a state.[44]

With respect to the welfare benefits – per the example just discussed – we find a negative relationship between welfare benefits and the lagged share on public assistance in disadvantaged tracts. Assuming some persistence in this share, this would suggest if anything a bias towards finding that welfare benefits are associated with a lower share on public assistance, which is for the most part the opposite of what we find (i.e., the bias appears to be against finding what we do). For minimum wages, there is consistent evidence that the 10-year lag of the shares in poverty or on public assistance in disadvantaged tracts are associated with a lower minimum wage. Again, if there is persistence in these shares, the bias is against what our results show, which is that a higher minimum wage increases the share in poverty or on public assistance in the longer run. Hence, again, the results are most likely not driven by endogenous policy.[45] Finally, there is a positive relationship between the EITC and the lagged share in poverty in disadvantaged tracts, implying that the EITC may have been raised when poverty rates were high in these tracts. If there is persistence in the share in poverty, this would generate a bias against finding that a higher EITC lowers poverty in the longer-run.[46] Thus, this is the one case where it is possible that policy endogeneity affects our conclusions – in this case, the absence of evidence that a more generous EITC reduced poverty.

Effects Identified from Treated vs. Control Tracts

The DDD estimator identifies the relative rather than the absolute effects on more-disadvantaged areas of the policies we study. We suggested that it was unlikely that the relative effects would be misleading – although this could conceivably happen if a particular policy reduces poverty, for example, more in advantaged areas than disadvantaged areas; in this case an adverse relative effect could mask a positive absolute effect. However, given some of the potentially anomalous contemporaneous effects we find, in this subsection we provide additional evidence on this question, by estimating simpler specifications that drop the state-by-year interactions, so that we can instead estimate main "effects" (which we put in quotes because these main effects are interpreted as controls for other state-level changes correlated with variation in the policies we study).[47] These results are reported in Tables 9A and 9B.

Table 9A reports the results for earnings and employment, corresponding to Tables 2 and 3. For both earnings and employment, the estimated main effects of the 10-year lags are generally small and insignificant. For the contemporaneous main effects, however, there are larger and significant positive estimates on earnings for the minimum wage and for welfare benefits. We noted earlier that we might have expected a positive effect of minimum wages on earnings in disadvantaged tracts (although not necessarily), and we noted that the positive DDD estimate of the contemporaneous effect of welfare benefits on earnings was surprising. The estimated contemporaneous minimum wage effects in Table 9A suggest that minimum wage increases are associated with earnings increases in the more-advantaged tracts. This association could be causal, given that many minimum wage workers are in higher-income families (Sabia and Burkhauser, 2010), or it could be spurious. Regardless, the positive association of contemporaneous minimum wage increases with earnings in more-advantaged tracts may explain why we do not find a positive relative effect on earnings in disadvantaged tracts.

Table 9B reports the results for poverty and the share on public assistance. Regarding longer-term effects, one finding of interest is the negative main effects on both outcomes of the 10-year lag of the maximum welfare benefit (significant in three out of four cases). This might plausibly lower our confidence in a causal interpretation of the estimated positive effect of the maximum welfare benefit on poverty and the receipt of public assistance in disadvantaged tracts (Tables 5 and 6). In addition, the evidence of adverse longer-run effects of minimum wages becomes a good deal weaker, and is positive and statistically significant (at the 10-percent level) in only one specification.

One anomalous contemporaneous effect we noted earlier was the large positive effect of the minimum wage on poverty. In Table 9B we see that the estimated main effect is large and negative, implying that – based on these specifications – there is little evidence of an absolute contemporaneous effect of minimum wages on poverty in disadvantaged tracts.

Overall, the evidence from Tables 9A and 9B sheds some light on some of the potentially peculiar contemporaneous effects we have estimated, suggesting that they are driven in part by changes in more-advantaged tracts.

More importantly, with respect to longer-run effects, the evidence in Table 9B suggests caution in interpreting our main DDD estimates of the longer-run effects of policy on poverty and the receipt of public assistance as causal. In particular, there is less clear evidence that higher welfare benefits increase poverty and the share on public assistance (a result already called into question from our exploration of effects post-welfare reform). And the evidence of adverse longer-run effects of minimum wages is weaker – although three of the four point estimates are positive. Still, a robust and important conclusion across all of these estimates is that a higher minimum wage and more generous welfare benefits do not *reduce* poverty and reliance on public assistance in the longer-term.

In general, these results indicate that what one concludes depends in part on taking a stand on whether the estimated main effects – the associations between policy and outcomes in the more-advantaged tracts – are causal, or if they simply pick up correlations between other state-level changes and these policies. The DDD estimation strategy relies on the latter interpretation. But, to be sure, we would be more confident in a causal interpretation if the estimates were driven by the tracts where the policies have the largest impact.

National Policy Variation

Finally, our DDD estimator relies solely on within-state variation in policy. In particular, the relative effect of national time-series variation on disadvantaged tracts is subsumed in the DISb·YR interactions in equation (3). While it is standard to rely on state-level variation in policy when studying the United States – precisely because there is a good deal of state-level variation – for the EITC, in particular, the national-level creation and expansion of the program suggests that we might at least like to know what estimates we obtain if we also allow the national-level variation to identify effects on disadvantaged tracts. To this end, Table 10 reports estimates of equation (3) dropping the $DIS^b{\cdot}YR$ interactions. To be clear, these estimates should be interpreted as causal only if one is willing to assume that the other sources of aggregate (i.e., national) shocks to disadvantaged tracts are common to the other tracts, so that aggregate relative changes in disadvantaged tracts can be attributed to policy.

The evidence regarding the EITC is quite different in Table 10, relative to the earlier estimates. First, we no longer find the anomalous negative and significant contemporaneous employment effects reported in Table 3. More important with respect to our key outcomes of poverty and public assistance, the estimates in Table 10 point to longer-run beneficial effects of a higher EITC – for poverty, in the specification using the share black to define disadvantage, and in both specifications for public assistance (for which the elasticities are around -0.10). These findings are consistent with positive longer-run effects on employment. Another difference, however, is that the evidence on the longer-run effects of minimum wages on poverty and the share on public assistance changes, with the estimates pointing to reductions in both – sometimes significant. In this case, however, neither the longer-run employment effects nor the longer-run earnings effects are positive, so these results are harder to explain. They may reflect the influence of other factors that are accounted for in the full DDD specifications, suggesting caution in interpreting the estimates that rely on the national policy variation to identify effects – whether of the EITC or of the minimum wage.

VI. CONCLUSIONS AND DISCUSSION

Our goal in this paper is to estimate the longer-run effects of anti-poverty policies on key socioeconomic outcomes in disadvantaged areas. We study three policies – minimum wages, the Earned Income Tax Credit, and welfare (and 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 estimate 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. However, our focus on disadvantaged areas is unique, as is our consideration of multiple policies simultaneously.

We identify tracts that are initially disadvantaged in terms of either a high share with low education, or a high share black. We then estimate the longer-run effects of these alternative policies on key economic indicators of economic self-sufficiency – in particular, poverty and the receipt of public assistance. Our identification strategy largely relies on state-level policy variation that has differential impacts on more- vs. less-advantaged tracts within a state, which allows us to flexibly allow for national- and state-level shocks or changes – including national-level changes that differentially affect disadvantaged areas – that are potentially correlated with policy changes, although we also consider the effects on the estimates of varying the sources of identification of policy effects.

Figures 7 and 8 provide a graphical summary of our key results based on our difference-in-difference-in-differences (DDD) strategy (Tables 2-6). Using our baseline estimates for both measures of disadvantage, the figures report the estimated elasticities of our earnings, employment, poverty, and public assistance measures with respect to the minimum wage, the EITC phase-in rate for two children, and welfare benefits.[48] Some findings differ across the two disadvantage measures, while some are more similar. However, in our view, a few key general results emerge.

First, there is no evidence that higher minimum wages reduce poverty or receipt of public assistance. The evidence is in the opposite direction; all the estimates indicate that the longer-run effect of higher minimum wages is to increase poverty and reliance on public assistance, and three of the four estimates are statistically significant.

Second, we do not find strong evidence of longer-term beneficial effects of the EITC, despite some evidence of positive employment effects of the EITC. The point estimates for employment are always positive – and larger and statistically significant only for the share black specification. For this latter specification, the point estimates indicate reductions in poverty and public assistance, but neither estimate is statistically significant. However, in other specifications, especially when we allow national changes in the EITC to influence the estimates of relative effects on disadvantaged tracts, we find evidence that the more generous EITC reduced poverty for the share black disadvantage measure, and the share on public assistance for both measures of disadvantage.

Third, the longer-run effects of higher welfare benefits are to increase poverty and the share on public assistance. The effects on the share on public assistance could simply reflect a take-up effect of higher benefits. However, the fact that the estimated effects on poverty are similar suggests, instead, that they reflect a behavioral response. We do note, though, that we found some evidence that the adverse longer-run effects of more generous welfare benefits may have been mitigated in the post-welfare reform period.

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 pro-work incentives of the EITC established in other literature, the absence of strong evidence of positive longer-run effects of the EITC in our DDD analysis is perhaps surprising, especially given that other research has found beneficial effects of the EITC using only the state-level variation (e.g., Neumark and Wascher, 2011). One difference may be that we are studying disadvantaged neighborhoods, and a dearth of job opportunities in these neighborhoods may lessen the ability of positive extensive labor supply shifts to increase overall employment (especially when the minimum wage is more binding in the local labor market). Another possibility is that the neighborhood-level effects also reflect negative effects on wages owing to the labor supply increase induced by the EITC (Leigh, 2000), in contrast to poverty-reduction effects of the EITC for those eligible for generous EITC payments, who face strong incentives to increase employment. Regardless, one possible conclusion is that people-based policies to encourage employment (Ladd, 1993), like the EITC, may not be effective at improving economic conditions in highly-disadvantaged areas, which is unfortunate given that the track record is not good for place-based policies explicitly targeting job growth in such areas. Then again, we remind the reader that estimates that also rely on national-level variation in the EITC point to beneficial effects in reducing poverty and the share on public assistance in disadvantaged neighborhoods, although identification of causal effects from the national variation is more tenuous.

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-parenthood are associated with poorer future outcomes for children (Chetty et al., 2014). Thus, for example, if there are beneficial longer-run effects of the EITC in reducing poverty, they could also lead to positive intergenerational effects.[49]

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 outcomes of individuals, families, and neighborhoods.[50] 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 variation in some of our findings depending on the sources of policy variation used to identify the effects we estimate highlights the challenge of estimating longer-run policy effects.

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ENDNOTES

[1]We do not study the Supplemental Nutrition Assistance Program (SNAP, previously Food Stamps), or the Child and Dependent Care Tax Credit (CCTC). Using SNAP/Food Stamp benefit levels is impractical, because only Alaska and Hawaii have amounts differing 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 et al., 2011), which again 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. It has no spatial variation.

[2]Card and Hyslop (2005) study longer-term effects of similar program in Canada. There is also some research tying the EITC to longer-term outcomes via effects on children (e.g., Hoynes et al., 2015). For a review of related work, see Neumark (2016).

[3]See, e.g., Neumark and Simpson (2015) and Neumark and Young (2017). 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).

[4]American Community Survey (ACS) data from 2010 indicate that 50.4 percent of blacks, 44.1 percent of Hispanics, but only 20.3 percent of whites, reside in areas where the poverty rate is 20 percent 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). Thus, a far greater share of non-poor blacks appears to live in poverty areas than do non-poor whites.

[5]See the summary of the evidence in Federal Reserve System and Brookings Institution, 2008.

[6]It would clearly also be interesting to estimate longer-run effects of anti-poverty policies on people or families. There are not many data sets with which to do this, however, since the most compelling analyses requires long-term longitudinal data, and data covering many cohorts, so that there is policy variation (ruling out the National Longitudinal Surveys). In very recent work, Neumark and Shirley (2017) estimate the long-term effects of exposure to a more generous EITC, using the PSID. With the Census data we use in this present paper, we can track areas over time (indeed, many decades), but not people.

[7]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 current work using microdata (Neumark et al., in progress), we can do more to unpack the effects on these outcomes, and to explore other hypotheses raised in this paper related to effects on different subsets of individuals or families.

[8]DIS can be measured for later years, but we work with 1970 as our baseline. As explained in the data section below, the 2010 measures are actually 2006-2010 measures based on the ACS.

[9]Among the many reasons we may want to avoid this is that that the definition of public assistance changes over time in our data source (discussed below), which can differentially impact disadvantaged tracts.

[10]In fact, one of our policy variables (welfare benefits) has a small degree of within-state variation, by county, so the main effects remain. However, these are identified from a very small number of observations, and hence are not reported.

[11]The inclusion of the state-by-year interactions and the focus on estimating the effects on most-disadvantaged tracts is potentially important in light of recent debates over the identification of minimum wage effects and the selection of the appropriate comparison groups (Dube et al., 2010; Allegretto et al., 2011; Neumark et al., 2014). This work highlights the question of whether state-specific economic shocks are correlated with minimum wage changes. However, when the state-by-year interactions are included, identification of ğ and ğL comes solely from within-state and year variation, and the bias from potential correlations between state-level economic conditions and the (possibly endogenous) variation in minimum wages becomes moot. That is, a correlation

between state-specific economic shocks and state minimum wages (or other policy) does not bias the estimated effects of minimum wages on lower- versus higher-skilled areas within a state once the state-year interactions are included. Indeed, as noted in Neumark and Wascher (2017), much of the most recent minimum wage research adopts a DDD approach that uses unaffected (or less affected) and affected (or more affected) groups to control for common shocks potentially associated with minimum wages.

[12]The NCDB data 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 in forestry and fishing.

[13]With the microdata we are studying (Neumark et al., in progress), we can parse effects by subgroup versus compositional shifts (in the latter case, we can also obtain some information on the role of in- and out-migration).

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

[15]The NCDB-reported counts are reallocations of the Census' reported counts that use 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 occurred at finer geographic levels and was then reconciled across Census waves can be found in Tatian et al. (2003).

[16]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).

[17]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.

[18]One key advantage of using the NCDB data 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. Thus, we did not simply want to do this research using confidential individual-level data with tracts identified – especially given that we are estimating and reporting on very different types of analyses than what has been done in past research.

[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]Recall that welfare benefits occasionally vary by county.

[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 (1980), to maintain consistency when constructing the panel.

[24]We use real (2014$) minimum wages, although 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 slightly understate the effects of refundable credits.

[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 three. 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, although the results were not affected. These and other results we discuss below are available upon request.

[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, (Moffitt, 2007) and that they were responsible for decreasing welfare caseloads (Grogger, 2004; Grogger and Michalopoulos, 2003; 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 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. U.S. Department of Health, Education, and Welfare (various years) publications on selected state maximum welfare payments were used where available. For 1962-1968, we extrapolated backwards from the 1969 maximums by using a publication from U. S. Health and Human Services (2001). This publication has no information on average maximum benefits for 1962-1968, but it does report

the average monthly benefit per family assistance unit in nominal dollars. We used the rate of growth in this measure between the missing year and 1969 and assumed that average maximum benefits grew at the same rate to infill the missing data.

[32]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.

[33]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 are observed and are the same, then year t is 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 infill the missing amounts.

[34]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.

[35]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, 2002), or 73 percent of the U.S. population. Using the same sources, the figures are 80 percent and 99.99 percent of the population for 1980 and 1990, respectively.

[36]See, e.g., 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).

[37]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).

[38]Indeed, minimum wage studies have found negative effects on earnings. For recent evidence, see Jardim et al. (2017).

[39]They are often slightly stronger in magnitude than the effect calculated across all quartiles, as expected.

[40]The positive employment effects of higher welfare benefits in the longer-run, for the specification using education to define disadvantage, are a bit hard to explain. One possibility is that welfare allows some earnings, and could help cover fixed costs of working, so we could get some positive response on the extensive margin, even though other women/families reduce hours/earnings in response.

[41]The standard difference-in-differences estimates, like Eissa and Liebman (1996), focus on the short-run. The analysis in Meyer and Rosenbaum (2001) looks at the relative employment of affected women in the years after the EITC became more generous, and hence can detect longer-run effects. Another important difference is that these studies rely on federal variation in the EITC, whereas our estimator focuses on state-level variation only.

[42]See, for example, the complex coding of welfare reform variables in Fang and Keane (2004).

[43]For an example of this approach in the context of the effects of unemployment benefits, see Hagedorn et al. (2016); in the context of minimum wages, see Allegretto et al. (2011); in the context of health economics, see Kessler and McClellan (2002). In some panel data contexts, the policy endogeneity problem is couched as a correlation between policies and prior trends, leading some researchers to control for linear trends specific to the jurisdictions in question. However, the linear restriction is typically unjustified, and linear trends imposed over long periods can lead to nonsensical results (like outcomes that must be positive becoming negative).

[44]The regressions also include tract and year fixed effects. We cannot include the state-by-year interactions, as these would fully explain the state-level policy variation.

[45]If there were regression to the mean in the share on public assistance or in poverty, rather than persistence, then this could potentially drive the welfare benefit and minimum wage results. However, there is no clear reason to expect regression to the mean, especially because the tract-level results are based on the large sample of all Long-Form respondents to the Census (or the large ACS samples).

[46]Note that this positive bias is a potential explanation of the positive contemporaneous effects of the EITC on poverty in the specifications in Table 5 using the low education share to define disadvantaged tracts.

[47]Note that in addition to providing estimates of the main effects, these specifications are less saturated because they include interactions between year dummy variables and DISb (the indicator for disadvantaged tracts), rather than a full set of state-by-year interactions.

[48]We omit the results for welfare time limits. They are never statistically significant, and the effects cannot be summarized using this metric.

[49]In the microdata we are examining in Neumark et al. (in progress), we can partially examine this by estimating effects on households headed by adults of different ages.

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

FIGURE 1: STATE LEVEL MINIMUM WAGE VARIATION (NOMINAL)

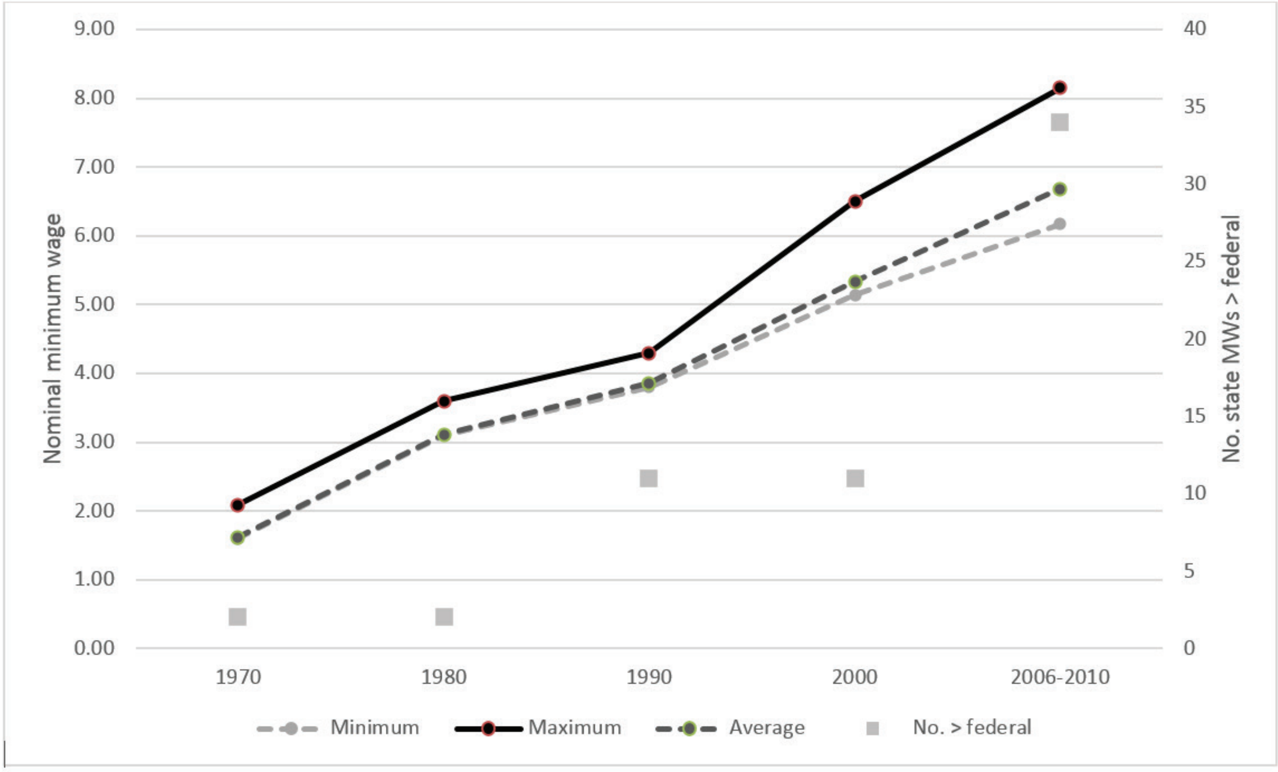


FIGURE 2: STATE LEVEL EITC VARIATION (PHASE-IN RATE, 2 CHILDREN)

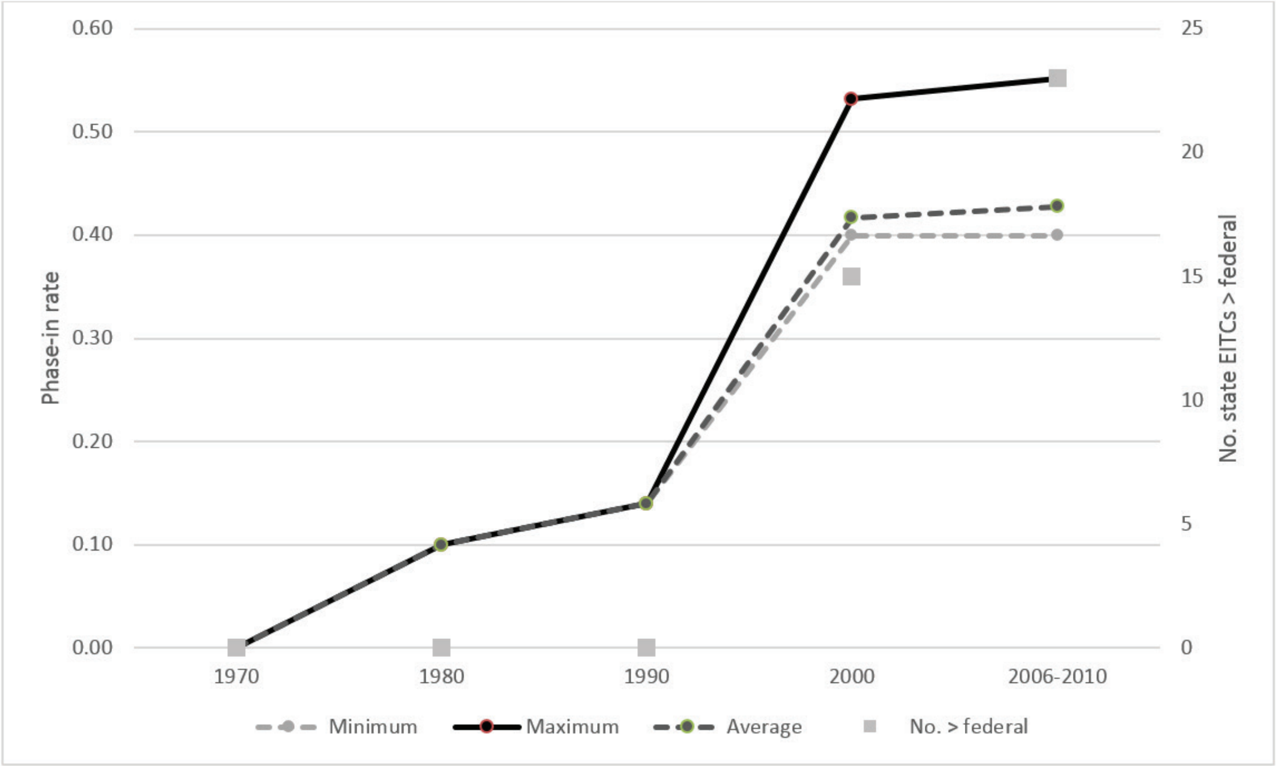


FIGURE 3: WELFARE BENEFITS (NOMINAL) FOR FAMILY OF THREE

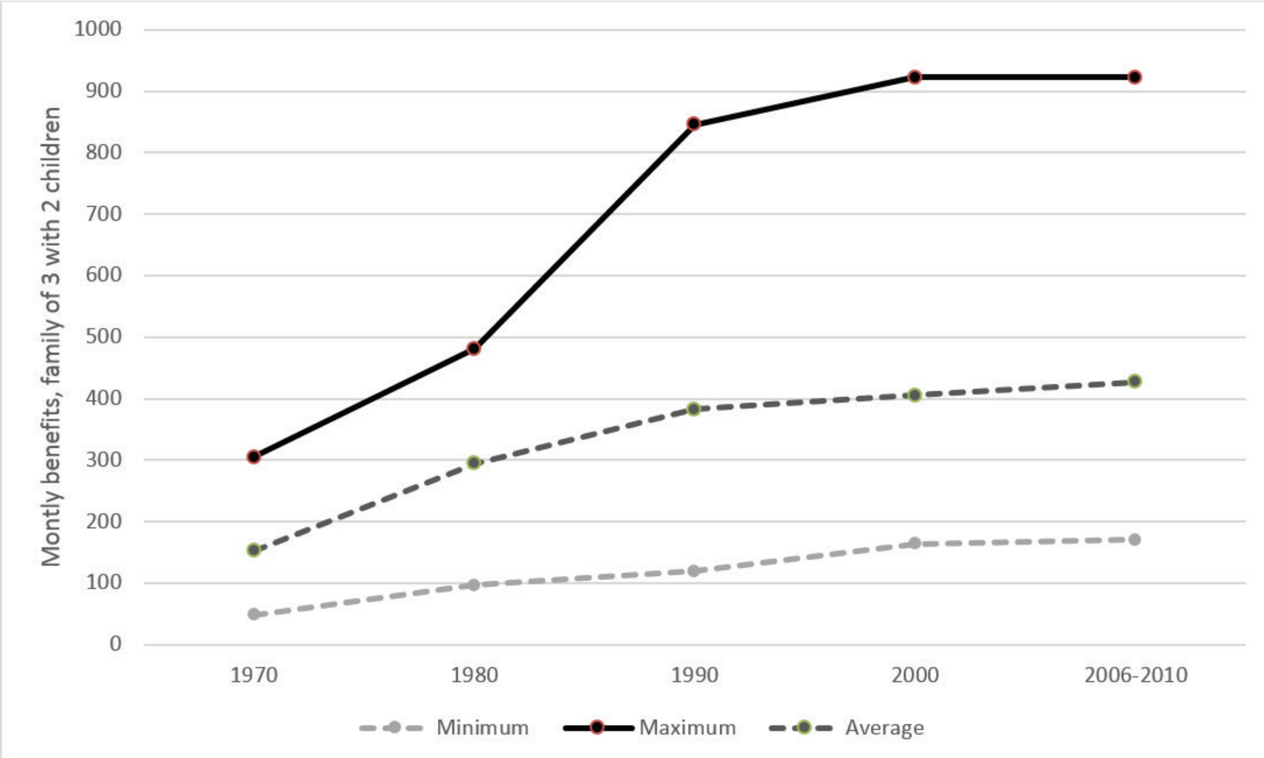


FIGURE 4: WELFARE TIME LIMITS

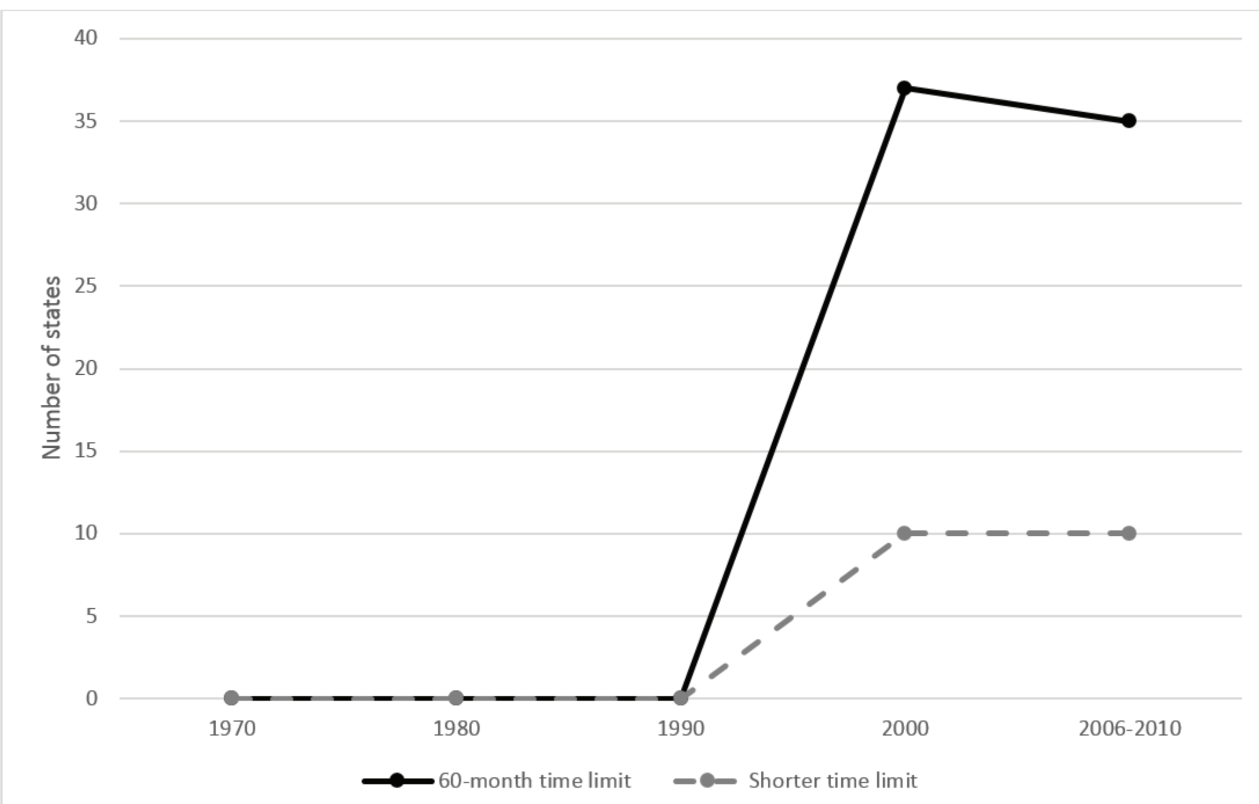


FIGURE 5: 1970 DISADVANTAGE BY TRACT, BASED ON SHARE WITH HIGH SCHOOL DEGREE OR LESS, FOR AREAS TRACTED IN 1970

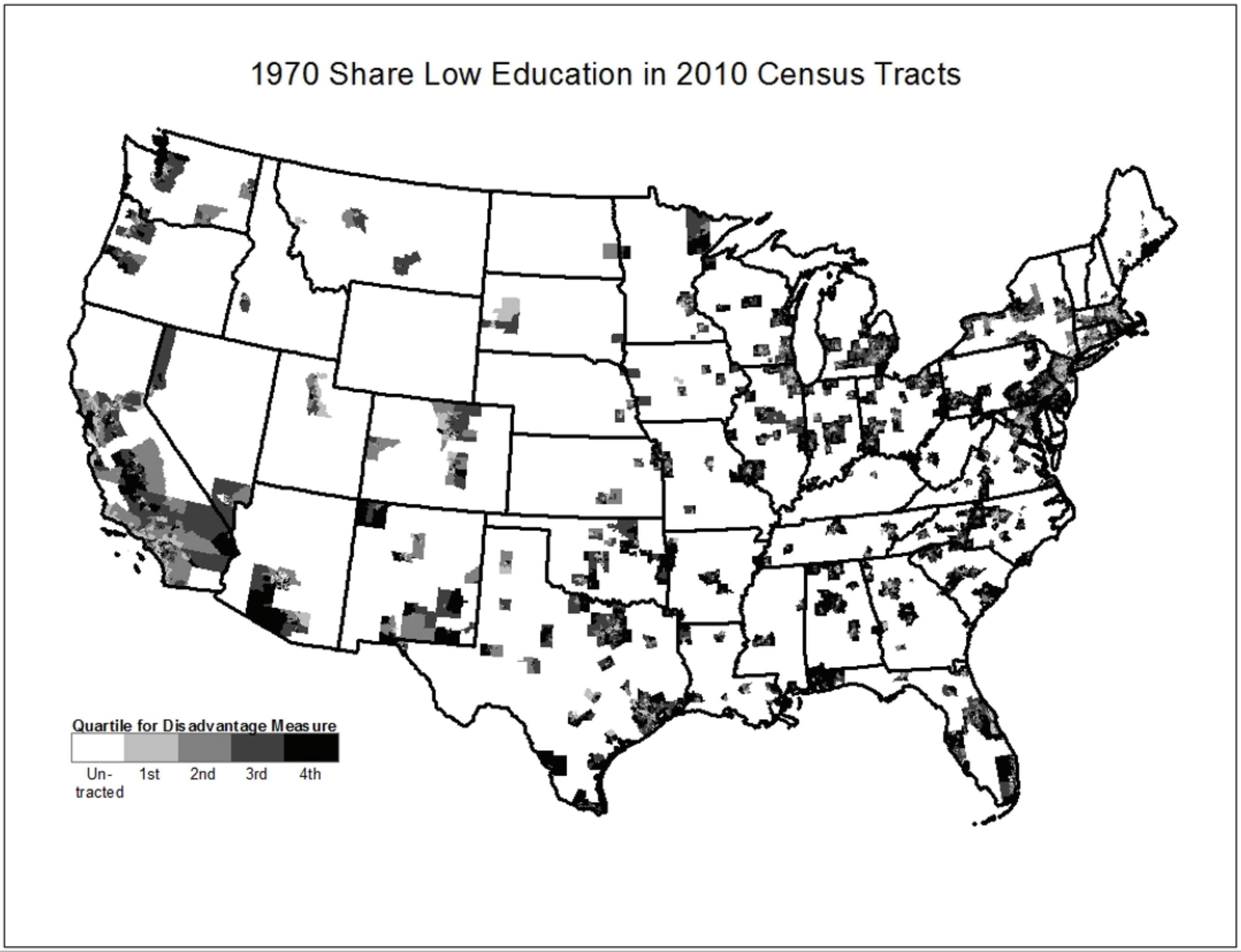


FIGURE 6: 1970 DISADVANTAGE BY TRACT, BASED ON SHARE BLACK, FOR AREAS TRACTED IN 1970

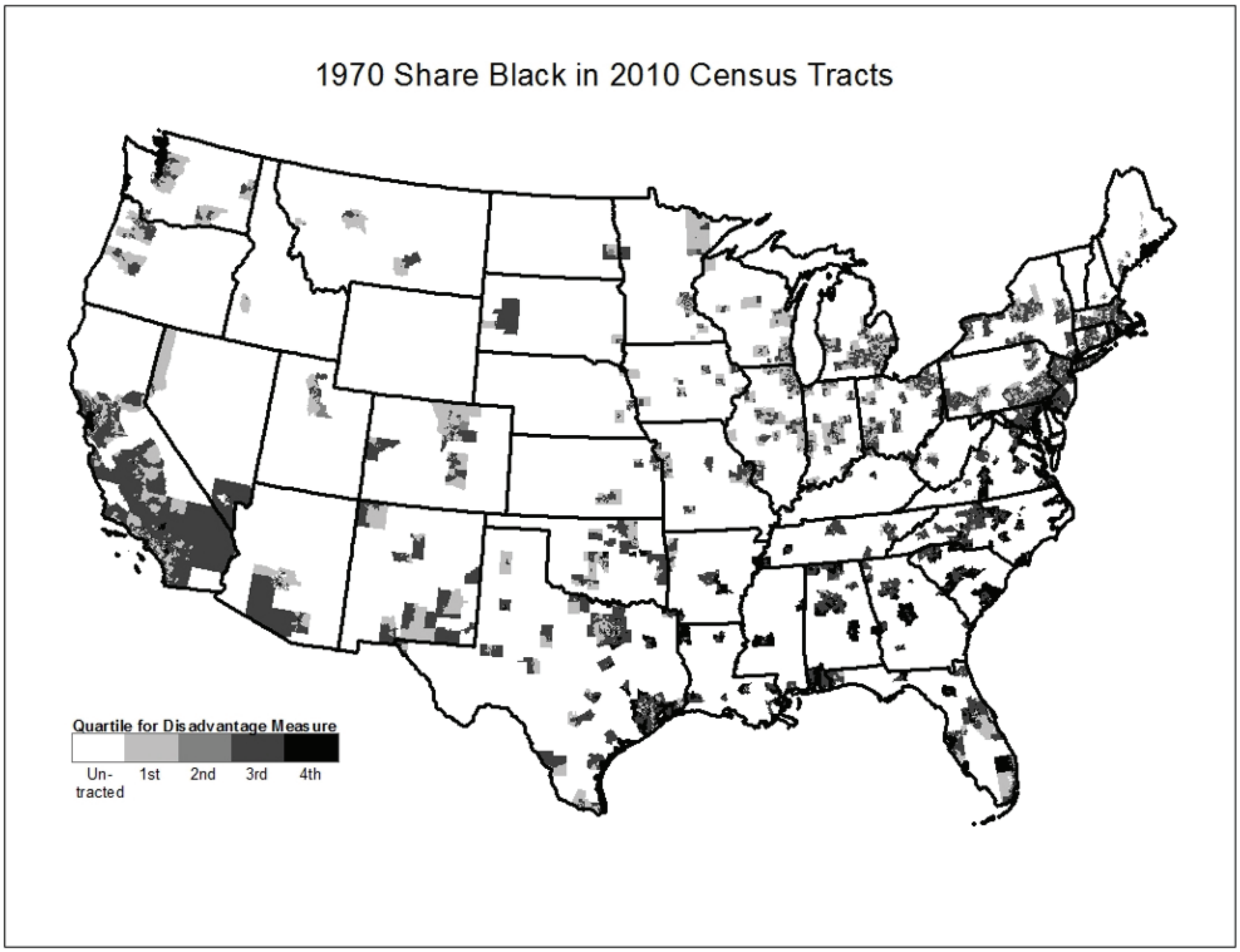
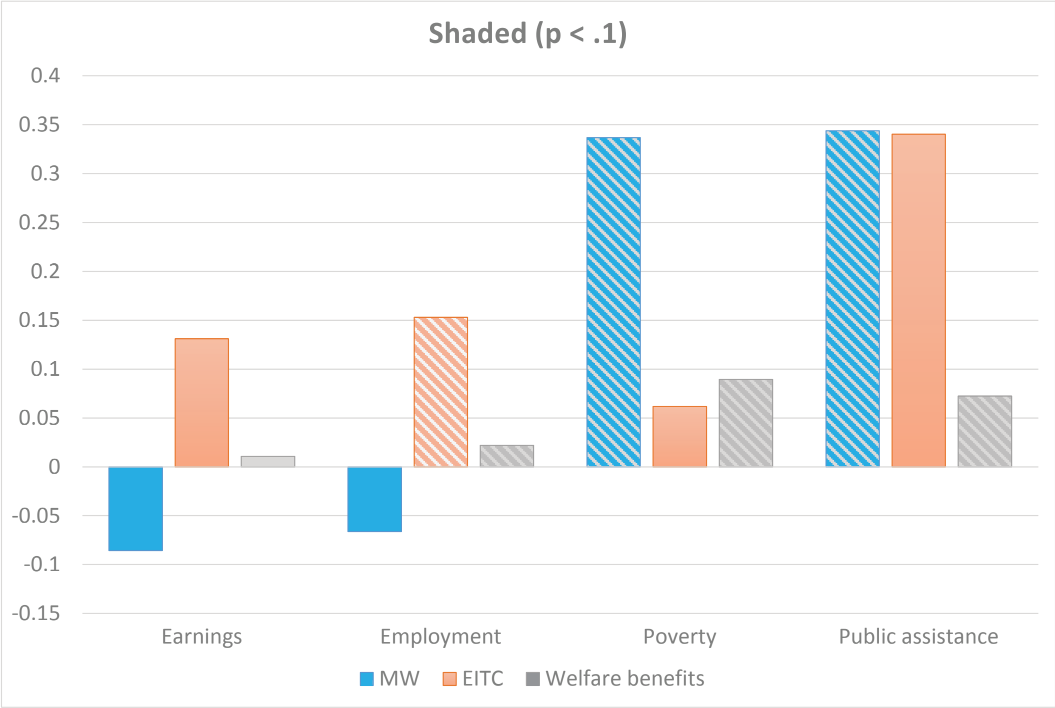


FIGURE 7: ESTIMATED ELASTICITIES OF OUTCOMES WITH RESPECT TO POLICIES, BASED ON SHARE WITH HIGH SCHOOL DEGREE OR LESS



Notes: Estimates are based on Tables 2, 3, 5, and 6, column (1).

FIGURE 8: ESTIMATED ELASTICITIES OF OUTCOMES WITH RESPECT TO POLICIES, BASED ON SHARE BLACK

Notes: Estimates are based on Tables 2, 3, 5, and 6, column (3).

Table 1: Descriptive Statistics on Outcomes and Tract Characteristics				
	1980	1990	2000	2006-2010 (average)
	(1)	(2)	(3)	(4)
Outcomes				
Average earnings per household (nominal)	18,595.1 (7,508.9)	34,495.1 (16,825.0)	48,760.1 (24,479.1)	60,074.1 (32,630.8)
Employment rate, male and female civilians aged 16+	59.6 (10.6)	62.4 (11.4)	60.5 (11.2)	61.9 (11.0)
Employment rate, female civilians aged 16+	48.6 (10.3)	54.8 (11.3)	54.6 (10.8)	56.6 (11.2)
Employment rate, male civilians aged 16+	71.8 (12.2)	70.8 (12.4)	67.2 (12.6)	67.9 (13.2)
Share of population in poverty	11.1 (10.5)	12.4 (12.4)	12.7 (11.7)	14.6 (13.1)
Share of households on public assistance	7.52 (8.19)	7.56 (8.51)	8.29 (8.18)	2.83 (3.68)
Outcomes: most-disadvantaged tracts (share low education, 1970)				
Average earnings per household (nominal)	13,764.8 (5,044.9)	24,665.6 (10,444.2)	36,177.5 (15,937.6)	44,174.2 (22,209.8)
Employment rate, male and female civilians aged 16+	53.1 (10.7)	55.7 (12.4)	54.8 (12.2)	57.1 (12.0)
Share of population in poverty	18.7 (14.0)	20.8 (16.1)	19.8 (14.4)	21.8 (15.5)
Share of households on public assistance	13.9 (11.8)	13.7 (12.1)	13.7 (10.9)	4.59 (5.10)
Outcomes: most-disadvantaged tracts (share black, 1970)				
Average earnings per household (nominal)	14,923.0 (6,530.1)	28,132.9 (14,522.3)	40,515.4 (21,267.2)	49,870.2 (29,731.9)
Employment rate, male and female civilians aged 16+	54.8 (12.5)	57.3 (14.0)	55.4 (13.4)	57.5 (13.3)
Share of population in poverty	19.3 (14.0)	20.5 (16.3)	19.8 (14.9)	21.4 (16.0)
Share of households on public assistance	13.6 (12.1)	13.1 (12.4)	13.0 (11.2)	4.14 (5.03)
Measures of disadvantage	***1970***			
Initial share HSG or less	76.2 (16.9)			
75th percentile	87.7			
Initial share black	8.98 (20.9)			
75th percentile	5.10			

Notes: Table reports means for tract-level measures, not individual-level measures. Standard deviations are shown in parentheses. Estimates for the outcomes are for the samples used for the corresponding regressions based on using low education to measure disadvantage. The estimates for the shares and percentiles of the disadvantage variables are for the samples used for the earnings regressions. Samples vary slightly across the different outcomes studied, and whether low education or high black shares are used to measure disadvantage; sample sizes are reported in the following tables. The public assistance definition excludes SSI for 2006-2010.

Table 2: Effects of Anti-Poverty Policies on Average Earnings per Household in Areas with Low Education or High Share Black at Baseline (1970), 1980-2010				
	Share ≤ HSG		Share black	
	(1)	(2)	(3)	(4)
3rd quartile of share disadvantaged omitted?	No	Yes	No	Yes
Log minimum wage	-0.0504 (0.0969)	-0.0533 (0.0984)	-0.0394 (0.0971)	-0.0693 (0.1231)
10-year lag of log minimum wage	**-0.0857 (0.0801)**	**-0.0816 (0.0932)**	**0.0519 (0.0817)**	**0.0018 (0.0979)**
Log EITC phase-in rate	-0.1303* (0.0656)	-0.1951** (0.0770)	-0.1586* (0.0878)	-0.1857* (0.1017)
10-year lag of log EITC phase-in rate	**0.1310 (0.1251)**	**0.0935 (0.1200)**	**0.4105*** (0.1507)**	**0.4419** (0.1700)**
Log maximum welfare benefit	0.1303*** (0.0375)	0.1095** (0.0434)	0.1016* (0.0579)	0.1427** (0.0630)
10-year lag of log maximum welfare benefit	**0.0107 (0.0179)**	**-0.0073 (0.0200)**	**0.0023 (0.0280)**	**0.0175 (0.0345)**
Welfare time limits (< 60 months)	-0.0102 (0.0173)	-0.0199 (0.0201)	-0.0535* (0.0315)	-0.0590* (0.0321)
10-year lag of welfare time limits (< 60 months)	**-0.0180 (0.0253)**	**-0.0191 (0.0271)**	**-0.0066 (0.0321)**	**-0.0084 (0.0340)**
Adjusted R^2	0.68	0.69	0.68	0.69
N	206,617	154,881	206,675	154,947
Tract fixed effects	Yes	Yes	Yes	Yes
State x year interactions	Yes	Yes	Yes	Yes

Notes: The specification corresponds to equation (3) in the text; only the coefficients of $DIS^b_c \cdot P_{cst}$ and $DIS^b_c \cdot P_{cs,t-10}$ are reported. Earnings are defined as average household earned income per household. 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. HSG = high-school graduate. ***, **, or * indicates statistically significantly different from zero at the 1-, 5-, or 10-percent level. Standard errors are clustered by state.

Table 3: Effects of Anti-Poverty Policies on Employment Rate in Areas with Low Education or High Share Black at Baseline (1970), 1980-2010

	Share ≤ HSG		Share black	
	(1)	(2)	(3)	(4)
3rd quartile of share disadvantaged omitted?	No	Yes	No	Yes
Log minimum wage	0.1367* (0.0703)	0.1667** (0.0719)	0.1324** (0.0657)	0.1502** (0.0647)
10-year lag of log minimum wage	**-0.0662 (0.0623)**	**-0.0717 (0.0814)**	**0.0026 (0.0763)**	**-0.0067 (0.0848)**
Log EITC phase-in rate	-0.0982** (0.0392)	-0.1239** (0.0518)	-0.1379** (0.0517)	-0.1656*** (0.0537)
10-year lag of log EITC phase-in rate	**0.1530* (0.0794)**	**0.1758* (0.0999)**	**0.2385** (0.1005)**	**0.2388*** (0.0887)**
Log maximum welfare benefit	-0.0095 (0.0147)	-0.0207 (0.0185)	0.0286 (0.0239)	0.0374 (0.0230)
10-year lag of log maximum welfare benefit	**0.0220** (0.0103)**	**0.0237** (0.0116)**	**-0.0063 (0.0140)**	**-0.0026 (0.0144)**
Welfare time limits (< 60 months)	-0.0301** (0.0116)	-0.0393*** (0.0123)	-0.0283** (0.0106)	-0.0360*** (0.0121)
10-year lag of welfare time limits (< 60 months)	**-0.0136 (0.0148)**	**-0.0157 (0.0157)**	**-0.0094 (0.0140)**	**-0.0122 (0.0141)**
Adjusted R^2	0.73	0.74	0.73	0.73
N	206,776	155,020	206,836	155,055
Tract fixed effects	Yes	Yes	Yes	Yes
State x year interactions	Yes	Yes	Yes	Yes

Notes: See notes to Table 2. The only difference is the dependent variable. Employed is defined as the number of employed civilian males and females aged 16 or over, relative to the population defined the same way.

Table 4: Effects of Anti-Poverty Policies on Employment Rate in Areas with Low Education or High Share Black at Baseline (1970), 1980-2010, Women vs. Men				
	Share ≤ HSG		Share black	
	(1)	(2)	(7)	(8)
Woman/Men	Women	Men	Women	Men
3rd quartile of share disadvantaged omitted?	No	No	No	No
Log minimum wage	0.0746 (0.0843)	0.1621** (0.0619)	0.0650 (0.0854)	0.1411* (0.0818)
10-year lag of log minimum wage	**-0.0667 (0.0898)**	**-0.0156 (0.0549)**	**-0.0269 (0.0941)**	**0.0831 (0.0822)**
Log EITC phase-in rate	-0.1129** (0.0479)	-0.1034** (0.0411)	-0.1809*** (0.0576)	-0.1219** (0.0547)
10-year lag of log EITC phase-in rate	**0.2120*** (0.0724)**	**0.1289 (0.0897)**	**0.2999*** (0.1015)**	**0.2239* (0.1210)**
Log maximum welfare benefit	0.0018 (0.0191)	-0.0161 (0.0153)	0.0565* (0.0312)	0.0239 (0.0226)
10-year lag of log maximum welfare benefit	**0.0209* (0.0112)**	**0.0200* (0.0109)**	**-0.0165 (0.0133)**	**-0.0014 (0.0151)**
Welfare time limits (< 60 months)	-0.0290* (0.0157)	-0.0284** (0.0121)	-0.0222 (0.0165)	-0.0297** (0.0116)
10-year lag of welfare time limits (< 60 months)	**-0.0114 (0.0166)**	**-0.0140 (0.0146)**	**0.0066 (0.0153)**	**-0.0178 (0.0165)**
Adjusted R^2	0.64	0.70	0.64	0.70
N	206,731	206,763	206,791	206,823
Tract fixed effects	Yes	Yes	Yes	Yes
State x year interactions	Yes	Yes	Yes	Yes

Notes: See notes to Table 2. The only difference is the dependent variable. Employed is defined as the number of employed civilian males or females aged 16 or over, relative to the same-gender population defined the same way.

Table 5: Effects of Anti-Poverty Policies on Share in Poverty in Areas with Low Education or High Share Black at Baseline (1970), 1980-2010				
	Share ≤ HSG		Share black	
	(1)	(2)	(3)	(4)
3rd quartile of share disadvantaged omitted?	No	Yes	No	Yes
Log minimum wage	0.2566 (0.1629)	0.3956** (0.1661)	0.2212 (0.1609)	0.3426* (0.1953)
10-year lag of log minimum wage	**0.3368***** **(0.1096)**	**0.4321***** **(0.1168)**	**0.1543** **(0.0993)**	**0.2299*** **(0.1147)**
Log EITC phase-in rate	0.3084** (0.1158)	0.4788*** (0.1484)	-0.0261 (0.1022)	-0.1235 (0.1070)
10-year lag of log EITC phase-in rate	**0.0617** **(0.1318)**	**0.0083** **(0.1644)**	**-0.1534** **(0.1337)**	**-0.0708** **(0.1620)**
Log maximum welfare benefit	-0.2182*** (0.0607)	-0.1893** (0.0779)	-0.1053 (0.0667)	-0.2162*** (0.0797)
10-year lag of log maximum welfare benefit	**0.0896***** **(0.0259)**	**0.1293***** **(0.0265)**	**0.1027***** **(0.0334)**	**0.1005**** **(0.0381)**
Welfare time limits (< 60 months)	-0.0228 (0.0190)	-0.0123 (0.0220)	-0.0278 (0.0261)	-0.0385 (0.0328)
10-year lag of welfare time limits (< 60 months)	**-0.0148** **(0.0289)**	**-0.0049** **(0.0350)**	**-0.0194** **(0.0212)**	**-0.0325** **(0.0220)**
Adjusted R^2	0.78	0.79	0.78	0.79
N	206,652	154,912	206,710	154,970
Tract fixed effects	Yes	Yes	Yes	Yes
State x year interactions	Yes	Yes	Yes	Yes

Notes: See notes to Table 2. The only difference is the dependent variable. Poverty is defined as the share of the population below the poverty level in the year.

Table 6: Effects of Anti-Poverty Policies on Share on Public Assistance in Areas with Low Education or High Share Black at Baseline (1970), 1980-2010				
	Share ≤ HSG		Share black	
	(1)	(2)	(3)	(4)
3rd quartile of share disadvantaged omitted?	No	Yes	No	Yes
Log minimum wage	0.3022 (0.2504)	0.3884 (0.2753)	-0.0993 (0.3687)	-0.1286 (0.3957)
10-year lag of log minimum wage	**0.3438***** **(0.1072)**	**0.3430**** **(0.1346)**	**0.2557**** **(0.0968)**	**0.2883**** **(0.1103)**
Log EITC phase-in rate	0.1893 (0.1544)	0.3314 (0.2041)	0.0475 (0.1777)	-0.0567 (0.1949)
10-year lag of log EITC phase-in rate	**0.3402** **(0.2582)**	**0.2435** **(0.3261)**	**-0.0912** **(0.3590)**	**0.1383** **(0.4157)**
Log maximum welfare benefit	-0.3088*** (0.0836)	-0.3283*** (0.0993)	-0.0513 (0.0830)	-0.0629 (0.0965)
10-year lag of log maximum welfare benefit	**0.0724***** **(0.0234)**	**0.1015***** **(0.0268)**	**0.0593*** **(0.0308)**	**0.0611** **(0.0369)**
Welfare time limits (< 60 months)	-0.0340 (0.0268)	-0.0309 (0.0315)	0.0144 (0.0335)	0.0282 (0.0346)
10-year lag of welfare time limits (< 60 months)	**-0.0151** **(0.0551)**	**-0.0090** **(0.0630)**	**-0.0371** **(0.0691)**	**-0.0111** **(0.0722)**
Adjusted R^2	0.75	0.76	0.75	0.76
N	206,617	154,881	206,675	154,946
Tract fixed effects	Yes	Yes	Yes	Yes
State x year interactions	Yes	Yes	Yes	Yes

Notes: See notes to Table 2. The only difference is the dependent variable, which is the share of households on public assistance.

Table 7: Effects of Anti-Poverty Policies on Share in Poverty or on Public Assistance in Areas with Low Education or High Share Black at Baseline (1970), Separate Effects of Welfare Benefits Post-Welfare Reform, 1980-2010

	Share ≤ HSG		Share black	
	(1)	(2)	(3)	(4)
Outcomes	Poverty	Public assistance	Poverty	Public assistance
Log minimum wage	0.2032 (0.1489)	0.2280 (0.2362)	0.2374 (0.1478)	-0.1426 (0.3379)
10-year lag of log minimum wage	**0.1054 (0.1496)**	**0.0191 (0.1939)**	**0.0639 (0.1430)**	**0.0310 (0.1772)**
Log EITC phase-in rate	0.1665 (0.1450)	-0.0107 (0.1807)	-0.1454 (0.1404)	-0.1178 (0.2282)
10-year lag of log EITC phase-in rate	**-0.0567 (0.1145)**	**0.1733 (0.2536)**	**-0.2468* (0.1257)**	**-0.1664 (0.3598)**
Log maximum welfare benefit	-0.2040*** (0.0594)	-0.2898*** (0.0791)	-0.1719** (0.0708)	-0.0780 (0.0820)
10-year lag of log maximum welfare benefit	**0.0657*** (0.0237)**	**0.0387** (0.0192)**	**0.0842** (0.0323)**	**0.0364 (0.0302)**
Log maximum welfare benefit x post-welfare reform	0.1013 (0.0752)	0.1456 (0.1671)	0.2811** (0.1090)	0.1826 (0.1629)
10-year lag of log maximum welfare benefit x post-welfare reform	**0.0098 (0.0801)**	**0.0109 (0.1628)**	**-0.2156* (0.1146)**	**-0.0876 (0.1644)**
Welfare time limits (< 60 months)	-0.0200 (0.0165)	-0.0301 (0.0250)	-0.0256 (0.0255)	0.0167 (0.0323)
10-year lag of welfare time limits (< 60 months)	**-0.0118 (0.0284)**	**-0.0107 (0.0484)**	**-0.0016 (0.0244)**	**-0.0268 (0.0680)**
Adjusted R^2	0.78	0.75	0.78	0.75
N	206,652	206,617	206,710	206,675
Tract fixed effects	Yes	Yes	Yes	Yes
State x year interactions	Yes	Yes	Yes	Yes

Notes: See notes to Tables 2, 5, and 6. The only difference is the additional set of welfare benefit variables interacted with the post-welfare reform (year > 1996) variable.

Table 8: Relationship between Policy Changes and Lagged Shares in Poverty or on Public Assistance in Disadvantaged Tracts, Based on Low Education or High Share Black at 1970 Baseline, 1980-2010

Policy:	Log minimum wage		Log EITC phase-in rate		Log maximum welfare benefit		Welfare limit < 60 months	
Disadvantage measure	Share ≤ HSG (1)	Share black (2)	Share ≤ HSG (3)	Share black (4)	Share ≤ HSG (5)	Share black (6)	Share ≤ HSG (7)	Share black (8)
Results for share in poverty								
10-year lag of policy variable	0.6243*** (0.0391)	0.6227*** (0.0397)	1.1870*** (0.1070)	1.1891*** (0.1070)	0.8512*** (0.0471)	0.8447*** (0.0480)	0.5857*** (0.2098)	0.5857*** (0.2101)
10-year lag of share in poverty	0.0038 (0.0041)	0.0050 (0.0035)	-0.0038*** (0.0014)	-0.0036*** (0.0013)	-0.0275** (0.0136)	-0.0135 (0.0119)	0.0016 (0.0105)	-0.0008 (0.0087)
10-year lag of share in poverty x disadvantaged tract	**-0.0040 (0.0030)**	**-0.0053** (0.0022)**	**0.0021* (0.0011)**	**0.0017* (0.0009)**	**0.0035 (0.0073)**	**-0.0134 (0.0083)**	**-0.0027 (0.0064)**	**0.0002 (0.0058)**
Adjusted R^2 N	0.74 206,626	0.74 206,684	0.99 206,626	0.99 206,684	0.70 206,626	0.70 206,684	0.26 206,626	0.26 206,684
Results for share on public assistance								
10-year lag of policy variable	0.6002*** (0.0386)	0.6019*** (0.0379)	1.1883*** (0.1061)	1.1898*** (0.1061)	0.8422*** (0.0439)	0.8333*** (0.0470)	0.5848*** (0.2099)	0.5847*** (0.2095)
10-year lag of share on public assistance	0.0136** (0.0058)	0.0139*** (0.0047)	-0.0012 (0.0021)	-0.0009 (0.0021)	0.0338*** (0.0116)	0.0427*** (0.0122)	-0.0096 (0.0089)	-0.0116 (0.0094)
10-year lag of share on public assistance x disadvantaged tract	**-0.0084** (0.0041)**	**-0.0091*** (0.0028)**	**0.0019 (0.0014)**	**0.0015 (0.0013)**	**-0.0149** (0.0071)**	**-0.0269*** (0.0092)**	**0.0011 (0.0069)**	**0.0038 (0.0073)**
Adjusted R^2	0.74	0.74	0.99	0.99	0.70	0.70	0.26	0.26
N	206,544	206,587	206,544	206,587	206,544	206,587	206,544	206,587

Notes: All specifications include tract and year effects. Disadvantaged tracts are based on the top quartile of share ≤ HSG / share black in 1970, as in Tables 5 and 6.

Table 9A: Effects of Minimum Wages and Other Anti-Poverty Policies on Share in Poverty and Share on Public Assistance in Areas with Low Education or High Share Black at 1970 Baseline, 1980-2010, with Main Effects Included – Average Earnings per Household and Employment Rate

	Average earnings per household				Employment rate			
	Share ≤ HSG		Share black		Share ≤ HSG		Share black	
	Interaction (1)	Main effect (2)	Interaction (1)	Main effect (2)	Interaction (1)	Main effect (2)	Interaction (1)	Main effect (2)
Log minimum wage	-0.1105 (0.1120)	0.3570*** (0.1209)	-0.0655 (0.1306)	0.3496*** (0.1214)	0.0917 (0.0734)	0.0868 (0.0570)	0.0812 (0.0923)	0.0881* (0.0497)
10-year lag of log minimum wage	**-0.1208 (0.1184)**	**0.0839 (0.1003)**	**0.1826 (0.1094)**	**0.0130 (0.0962)**	**-0.0261 (0.0751)**	**-0.1204 (0.1067)**	**0.0214 (0.1081)**	**-0.1732 (0.1231)**
EITC phase-in rate	-0.1316 (0.0903)	0.1678 (0.1756)	0.0907 (0.1330)	0.1117 (0.1718)	-0.1875*** (0.0394)	0.0051 (0.0903)	-0.1464* (0.0780)	-0.0085 (0.1028)
10-year lag of EITC phase-in rate	**0.1765 (0.1788)**	**0.0115 (0.2363)**	**0.1823 (0.2021)**	**0.0310 (0.2356)**	**0.2973*** (0.0731)**	**0.0680 (0.1185)**	**0.3725*** (0.1175)**	**0.0703 (0.1304)**
Log maximum welfare benefit	0.1242** (0.0489)	0.1577** (0.0670)	0.0065 (0.0748)	0.1848*** (0.0637)	-0.0277 (0.0205)	-0.0048 (0.0278)	0.0011 (0.0272)	-0.0124 (0.0294)
10-year lag of log maximum welfare benefit	**0.0390 (0.0271)**	**0.0777 (0.0539)**	**-0.0124 (0.0378)**	**0.0956** (0.0458)**	**0.0294** (0.0119)**	**0.0111 (0.0182)**	**-0.0139 (0.0188)**	**0.0211 (0.0177)**
Welfare limit 60+ months	-0.0228 (0.0181)	-0.0009 (0.0468)	-0.0406 (0.0292)	0.0034 (0.0446)	-0.0403*** (0.0126)	0.0039 (0.0214)	-0.0243*** (0.0078)	0.0007 (0.0232)
10-year lag of welfare limit 60+ months	**-0.0426 (0.0328)**	**-0.0049 (0.0468)**	**-0.0152 (0.0346)**	**-0.0082 (0.0452)**	**-0.0170 (0.0139)**	**0.0175 (0.0194)**	**-0.0113 (0.0114)**	**0.0164 (0.0199)**
Adjusted R^2	0.67		0.67		0.72		0.72	
N	206,617		206,675		206,776		206,836	
Tract fixed effects	Yes		Yes		Yes		Yes	
State x year interactions	No		No		No		No	

Notes: See notes to Tables, 2, 3, 5, and 6. Interaction refers to interaction with indicator for tract in top quartile of share ≤ HSG / share black, 1970. Because these specifications drop the state-by-year interactions, the main effects corresponding to the terms $P_{cst}\cdot\gamma + P_{cs,t-10}\cdot\gamma^{L}$ are identified.

Table 9B: Effects of Minimum Wages and Other Anti-Poverty Policies on Share in Poverty and Share on Public Assistance in Areas with Low Education or High Share Black at 1970 Baseline, 1980-2010, with Main Effects Included – Share in Poverty and Share on Public Assistance

	Share in poverty				Share on public assistance			
	Share ≤ HSG		Share black		Share ≤ HSG		Share black	
	Interaction (1)	Main effect (2)	Interaction (3)	Main effect (4)	Interaction (5)	Main effect (6)	Interaction (7)	Main effect (8)
Log minimum wage	0.2770 (0.1700)	-0.2734 (0.1835)	0.1998 (0.1468)	-0.3341** (0.1560)	0.3873 (0.3199)	-0.0743 (0.1815)	0.0540 (0.3977)	-0.1240 (0.1609)
10-year lag of log minimum wage	**0.2342* (0.1239)**	**0.1778 (0.1757)**	**-0.0449 (0.1743)**	**0.2353 (0.1960)**	**0.1406 (0.2509)**	**-0.0304 (0.1915)**	**0.1337 (0.1910)**	**-0.0300 (0.2145)**
EITC phase-in rate	0.3647** (0.1791)	-0.1266 (0.3347)	-0.1511 (0.2177)	0.0684 (0.3491)	0.2449 (0.2648)	0.0352 (0.2209)	0.2361 (0.2813)	0.1073 (0.2307)
10-year lag of EITC phase-in rate	**-0.3482 (0.2316)**	**0.0622 (0.5718)**	**-0.4849 (0.3123)**	**-0.0324 (0.5548)**	**-0.2339 (0.3740)**	**-0.0380 (0.6242)**	**-0.6060 (0.5866)**	**-0.1361 (0.6089)**
Log maximum welfare benefit	-0.2015*** (0.0731)	-0.0866 (0.0959)	0.0245 (0.0900)	-0.1118 (0.0680)	-0.2996*** (0.1028)	-0.2075** (0.0871)	0.0519 (0.1078)	-0.2361*** (0.0665)
10-year lag of log maximum welfare benefit	**0.0898** (0.0349)**	**-0.0905 (0.0835)**	**0.1644*** (0.0533)**	**-0.1207* (0.0612)**	**0.0893** (0.0431)**	**-0.1593** (0.0705)**	**0.1339*** (0.0449)**	**-0.1801*** (0.0587)**
Welfare time limits (< 60 months)	-0.0068 (0.0285)	-0.0123 (0.0587)	-0.0141 (0.0474)	-0.0097 (0.0632)	-0.0464 (0.0382)	0.0167 (0.0563)	0.0235 (0.0464)	0.0039 (0.0567)
10-year lag of welfare limit 60+ months	**0.0094 (0.0419)**	**0.0768 (0.0703)**	**-0.0033 (0.0421)**	**0.0759 (0.0662)**	**0.0074 (0.0914)**	**0.0238 (0.1114)**	**-0.0798 (0.0843)**	**0.0420 (0.1027)**
Adjusted R^2	0.77		0.77		0.74		0.74	
N	206,652		206,710		206,617		206,675	
Tract fixed effects	Yes		Yes		Yes		Yes	
State x year interactions	No		No		No		No	

Notes: See notes to Table 8A.

Table 10: 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 or High Share Black at Baseline (1970), 1980-2010, Using National Policy Variation

	Average earnings per household		Employment rate		Share in poverty		Share on public assistance	
	Share ≤ HSG	Share black	Share ≤ HSG	Share black	Share ≤ HSG	Share black	Share ≤ HSG	Share black
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log minimum wage	0.0620 (0.0775)	-0.0548 (0.0644)	0.2192*** (0.0456)	0.1823*** (0.0564)	-0.1197 (0.1262)	-0.0322 (0.1017)	-0.2940 (0.1978)	-0.4514 (0.2697)
10-year lag of log minimum wage	**-0.0146 (0.0611)**	**-0.0386 (0.0552)**	**-0.1016* (0.0564)**	**-0.0253 (0.0803)**	**-0.0264 (0.0895)**	**-0.1728*** (0.0596)**	**-0.3782** (0.1667)**	**-0.2210 (0.1371)**
Log EITC phase-in rate	0.0663*** (0.0246)	0.0559** (0.0259)	-0.0208 (0.0186)	-0.0123 (0.0265)	-0.1170*** (0.0436)	-0.1547*** (0.0287)	-0.1317* (0.0660)	-0.0863 (0.0725)
10-year lag of log EITC phase-in rate	**-0.0253** (0.0116)**	**-0.0359** (0.0172)**	**0.0222*** (0.0062)**	**0.0127* (0.0072)**	**-0.0096 (0.0179)**	**-0.0347*** (0.0123)**	**-0.0809*** (0.0279)**	**-0.1141*** (0.0337)**
Log maximum welfare benefit	0.1064*** (0.0372)	0.1277** (0.0584)	-0.0137 (0.0167)	0.0385 (0.0270)	-0.1144 (0.0809)	-0.0511 (0.0734)	-0.1361 (0.1075)	0.0334 (0.0995)
10-year lag of log maximum welfare benefit	**-0.0081 (0.0164)**	**0.0113 (0.0226)**	**0.0150* (0.0087)**	**-0.0079 (0.0118)**	**0.1626*** (0.0309)**	**0.1395*** (0.0397)**	**0.1853*** (0.0312)**	**0.1145*** (0.0407)**
Welfare time limits (< 60 months)	-0.0071 (0.0193)	-0.0504 (0.0336)	-0.0306** (0.0125)	-0.0266** (0.0126)	-0.0301 (0.0233)	-0.0310 (0.0273)	-0.0386 (0.0331)	0.0100 (0.0363)
10-year lag of welfare time limits (< 60 months)	**-0.0081 (0.0262)**	**-0.0112 (0.0336)**	**-0.0137 (0.0153)**	**-0.0137 (0.0155)**	**-0.0590** (0.0246)**	**-0.0332* (0.0168)**	**-0.0768 (0.0483)**	**-0.0605 (0.0686)**
Adjusted R^2	0.68	0.68	0.73	0.73	0.78	0.78	0.75	0.75
N	206,617	206,675	206,776	206,836	206,652	206,710	206,617	206,675
Tract fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State x year interactions	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: See notes to Tables 2, 3, 5, and 6. Only the specifications using all four quartiles of disadvantage are reported. The specifications differ from those tables in omitting the $DIS^b \cdot YR$ interactions.

NOTES