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USING THE EITC TO HELP POOR FAMILIES: NEW EVIDENCE AND A COMPARISON WITH THE MINIMUM WAGE

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ABSTRACT

This paper evaluates the effects of the earned income tax credit (EITC) on poor families. Exploiting state-level variation in EITCs, we find that the EITC helps families rise above poverty-level earnings. This occurs by inducing labor market entry in families that initially do not have an adult in the workforce. Evidence based on the federal EITC is less supportive of a positive impact of the EITC on poor families. Finally, our results suggest that for the range of policy changes typical of recent history in the U.S., the EITC is more beneficial for poor families than is the minimum wage.

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Introduction

The United States has in recent years relied on three types of policies to boost the incomes of poor families: the Earned Income Tax Credit (EITC), the minimum wage, and welfare. Although welfare acts directly to provide income to many of the neediest families, and thus is perhaps the most immediate means of assisting the poor, concerns have been raised about the potential for longer-term dependency associated with pure income-support programs. Given these concerns, the principal attraction of both the EITC and the minimum wage is that they are intended to raise the *earned* income of the poor, a goal which is generally viewed as more desirable than making direct transfer payments to low-income families. However, most empirical research has focused on the impacts of the EITC and the minimum wage on other labor market behaviors–such as labor supply or labor demand–which although obviously related, provide only part of the overall picture.[1] The extensive body of research on the effects of the minimum wage on the employment outcomes of youths is, of course, well known. But, even with respect to the EITC, where the bulk of the literature is quite recent, previous studies have focused primarily on the effects of the program on labor force participation and hours worked for particular subgroups of the population (such as single mothers with children), rather than on the overall distributive effects among families participating in the program.

Moreover, those few studies that do examine the effects of the EITC (or the minimum wage) on family incomes have relied mainly on simulation methods to reach their conclusions, rather than on direct empirical estimates. Such studies, which tend to take parameter values from the empirical literature, might be adequate if there was a general consensus about the effects of these programs on labor market behavior. However, because much of the literature studying the behavioral influences of anti-poverty policies is contentious (e.g., the recent debate over the employment effects of minimum wages), the simulation results are likely to be viewed skeptically, especially by those with an alternative point of view regarding the assumptions underlying the simulation.

In this paper, we examine the empirical link between the EITC and earned pre-tax income using panel data on poor and low-income families that are the EITC's intended beneficiaries. Using data on federal and state parameters of the EITC program, we estimate the effects of the EITC on earned incomes of poor families and on transitions of families into and out of poverty. While the main contribution of the paper is this new evidence on the EITC, we also provide some contrasts with the effects of minimum wages on family incomes, building on some of our earlier work (Neumark, et al., 1998).

Although very much a reduced-form approach, the resulting estimates of the effects of these two alternative policies on earned income have three major advantages over the estimates from previous studies. First, our procedure allows the data to speak directly to the question of the efficacy of using either the EITC or the minimum wage as a redistributive tool, and circumvents the need to choose point estimates for behavioral parameters. Second, we can control for other influences on family income changes (including the EITC or the minimum wage) that might bias estimates based on parameter values from studies focusing solely on one or the other policies. Third, by examining both federal and state policy, we are able to use several types of variation in the data to help identify the effects on income.

Our results indicate that there are differences in how the EITC and minimum wage policy affect earnings. Both policies appear to have positive income effects on poor families with children. However, the EITC effects are larger, evaluated on the basis of the average policy changes of the past 15 years. Complementary evidence on employment and hours effects indicates that the benefits of the EITC come about mainly by inducing labor market entry for poor families without any adult workers in the year prior to the change in the EITC. On net, these results suggest that the EITC is the more effective anti-poverty tool, especially if one considers positive work incentives as a goal of anti-poverty programs.

By using pre-tax (and therefore pre-EITC) earnings rather than total income, we obviously ignore an important component of the redistributive effects of the EITC. In this respect, our estimates provide a very conservative test of the benefits of the EITC relative to the minimum wage. We choose to focus on pre-tax earnings for two reasons. First, it permits a direct comparison of our estimates for earnings with our complementary estimates of labor supply effects; the fact that we find consistent results in both sets of estimates increases our confidence that the income effects that we are estimating are related to the EITC. Second, in much of the discussion of potential anti-poverty policies, there is a stated preference for policies that encourage work and longer-run economic self-sufficiency; witness, for example, the recent emphasis on welfare reform and empowerment. The EITC is frequently praised along these same lines, reflecting an assumption that the credit encourages families to work more. Our aim is to provide some additional evidence with which to assess the accuracy of these claims, by looking at the EITC's effects on families' earned income.

Nevertheless, we should stress that because we use pre-tax earnings, our estimates imply an even greater effectiveness of the EITC in raising the income levels of poor families than is suggested by a literal reading of the coefficients in our family earnings equations. Either way, however, our results confirm the sense provided by previous simulation studies of the EITC and minimum wage that the former is the more effective policy for fighting poverty.[2]

Previous Research

Research on the EITC is quite sparse as compared with that on other anti-poverty programs, although the quantity of research in this area is growing rapidly. In large part, this is because the EITC is a relatively recent policy initiative. Although it was first implemented in 1975, the EITC was considered a relatively unimportant component of welfare policy until it was expanded sharply as part of the 1986 Tax Reform Act, effective in 1987; additional EITC expansions took place in each year from 1991 to 1996. Moreover, as noted above, most of the research thus far has focused on estimating behavioral responses for particular subgroups of the population rather than on evaluating the net effects of the program for the poor.

In earlier papers, such estimates were typically derived from simulations based on parameters taken from the negative income tax literature or from more general studies of labor supply (Hoffman and Seidman, 1990; Dickert, et al., 1995). More recently, Eissa and Liebman (1996) and Meyer and Rosenbaum (1999a and 1999b) have directly estimated the effects of the EITC on the labor supply of single women with children. Both papers find that the expansion of the EITC raised work activity among this group. In contrast, Eissa and Hoynes (1998) compute similar estimates for married couples and find that the EITC had a small positive effect on the labor supply of married men, but a large negative effect on married women, with the net result being a decline in family labor supply.

Comparisons of the EITC and minimum wage policy have also been conducted using a simulation approach. The best known study in this area is by Burkhauser, et al. (1996), who evaluate how well the minimum wage targets the poor and compare the amount of additional income received by such families with what would be provided by the EITC. The paper concludes that the 1990 and 1991 minimum wage increases stemming from the 1989 Amendments to the Fair Labor Standards Act benefited upper income families (income-to-needs over 3) more than poor families, because many minimum wage workers are in higher-income families. In contrast, the increases in the EITC between 1989 and 1992 went nearly exclusively to poor and near-poor families with children. However, these simulations ignore both labor supply effects of the EITC and labor demand effects of minimum wages. While informative about the targeting of benefits, they are unlikely to be definitive about the ultimate effects of the alternative policies on income.

Data

To conduct the analysis, we use data at the family level drawn from the March CPS annual demographic files for the years 1986 through 1995.[3] As each family is potentially in the March sample for two consecutive years, we attempt to match records across years in order to observe changes in income during the period of our sample. Overall, the match rates were above 80 percent, although families with younger heads and lower income-to-needs ratios were somewhat less likely to be successfully matched.[4]

For each family that could be matched across years, we extracted data on the amount and composition of family income, family size, and state of residence, as well as other variables introduced below. The income and family size data are then used to calculate an income-to-needs ratio for each family, based on the official poverty line for a given family size in each year. Note that the income data in the March CPS refer to the previous year, so that our sample period actually corresponds to the years 1985 to 1994.

To each of these family-year records, we appended the relevant data for the key parameters of the EITC program and the prevailing minimum wage in effect for the year in which the income data are reported. For the minimum wage, we used the higher of the federal or state minimum wage for each state and year, following existing practice in the minimum wage literature. For the EITC, we collected information on various parameters of the federal program applicable to each family based on the number of dependent children residing in the family; these parameters include the phase-in rate of the credit, the maximum income level to which the phase-in rate is applied, the income level at which the credit begins to be phased out, and the phase-out rate. In the empirical analysis, we use the phase-in rate as the primary measure of the generosity of the EITC.[5] The phase-in rate and the maximum credit are reported for the federal program in the first six columns of Table 1. The phase-in rate varies from zero prior to 1993 for families with no children to 30 percent in 1994 for families with two or more children; the maximum credit ranged from 0 to more than \$2,500 over the same period. In addition, there are currently 10 states that offer an EITC to low-income families. These state credits are, for the most part, defined as a simple percentage of the federal credit received by the family, and range from less than five percent to more than 60 percent, depending on the state, year, and number of children (Table 1). Finally, while the federal program provides for a refundable credit, certain states offer only a non-refundable credit, and in some specifications we exclude these states from the sample or try to make credit rates comparable in the two types of states.

In addition, in some specifications we include controls for business cycle conditions and for the presence of other changes in welfare programs. For the former, we use the unemployment rate for prime-age males in each state and year. For the latter, we use the maximum level of AFDC benefits (in real terms) available to a family of three, along with a variable measuring the fraction of months in each year for which a state received a waiver from federal AFDC requirements.[6]

Empirical Estimates

Our empirical strategy is to employ a specification that can be applied directly both to an analysis of the effects of the EITC on income-to-needs and to an analysis of the effects of a minimum wage policy. Partly for this reason, we chose a reduced-form specification that limits the number of explanatory variables to exclude those that might be considered endogenous to these specific policies. In addition, we estimate some specifications using income-to-needs categories to construct the dependent variable, and some using income-to-needs directly. In the former case, the dependent variable is defined as the probability that a family moves from having an earned income-to-needs ratio (I/N) in the X_1 to Y_1 range in year one to a ratio in the X_2 to Y_2 range in year two. Using a linear probability model, this specification can be expressed as:

$$I\{X_2<(I/N)_2<Y_2 \mid X_1<(I/N)_1<Y_1\}_{ist} = \alpha + P_{ist}\beta + \gamma\Delta U_{st} + Z_{ist}\pi + \delta_s + \lambda_t + \varepsilon_{ist} \ , \qquad (1)$$

where I{…} is an indicator variable for the specified outcome, 'i' indexes families, 's' states, and 't' years. P_{ist} is the policy under consideration (e.g., the state and/or federal EITC, or the real minimum wage).[7] ΔU_{st} is the change in the adult male unemployment rate, which we include to capture different patterns of changes in economic conditions across states; these could generate spurious results if, for example, good economic conditions positively impact poor families, and also lead to more generous state EITCs as a result of budget surpluses. Z_{ist} is a vector of other control variables, δ_s is a vector of state indicators, and λ_t is a vector of year indicators. The state indicators capture persistent differences across states in transition rates, for example different trends in poverty. The year indicators capture aggregate changes in transition rates that may be driven by other factors, including other policy changes at the national level.

The parameter β is of particular interest in this study as it provides an estimate of the effect of the policy variable on the transition rates or changes in income-to-needs. For example, if the policy variable is the change in the EITC credit rate and X_1, X_2, Y_1, and Y_2 are defined to measure the transition rate out of poverty, then β would tell us whether an increase in the credit rate results in an increase in the probability of families leaving poverty. Given that the state and year terms soak up some of the variation in the data—including some of that in the EITC variables, it is useful to point out how the estimates of policy effects are identified. In particular, the inclusion of the year indicators implies that the effect of the federal EITC is identified from variation in EITC parameters across families with different numbers of children. The inclusion of the state indicators implies that the effect of state EITCs is identified from within-state variation in EITC parameters.[8] We also include controls for the number of children under 18 (defined using separate indicator variables for families with one child, two children, and three or more children). In some specifications we interact these children indicators with the year dummy variables to account for other sources of change in family incomes that might differentially affect families with different numbers of children. When we do this, the effect of the federal EITC is no longer identified, since this varies only with year and number of children.

After looking at the evidence on the effects of the EITC on transitions into and out of poverty (in terms of earned income), we use similar specifications to examine the effects of changes in the EITC on income-to-needs ratios of poor families. In particular, we estimate specifications of the form:

$$\{(I/N)_2-(I/N)_1\}_{ist} = \alpha + P_{ist}\beta + \gamma\Delta U_{st} + Z_{ist}\pi + \delta_s + \lambda_t + \varepsilon_{ist} \ , \qquad (2)$$

for the change in income-to-needs ratios among families with earnings below the poverty line in year one. Finally, in some specifications we examine the direct influence of the policy variables on employment transitions or on changes in hours worked in an attempt to identify the channels through which the EITC affects income-to-needs.

Table 2 reports the means and standard deviations for the basic data used in the analysis. The first column shows the descriptive statistics for the sample of matched families in the CPS. Mean earned income is above \$30 thousand per year in our sample, and the average income-to-needs ratio is a bit above 3. However, 23 percent of families in our sample have an earned income-to-needs ratio below one, and another 8 percent are between one and 1.5. These poor and near-poor families are the intended targets of both the EITC and the minimum wage, and we would expect families with much higher income-to-needs ratios to be unaffected by the EITC. For this reason, we restrict the sample in the subsequent analysis to include only those families with an income-to-needs ratio between 0 and 3 in the first year they are in the sample. When the higher income families are dropped from the sample (column 2), mean earned income falls to less than \$13 thousand and the average earned income-to-needs ratio is only slightly above one. In addition, nearly half of the families in this subsample have an earned income-to-needs ratio below one and about 60 percent are below 1.5. About 55 percent have children below age 18, which for most of the sample period is necessary to be eligible for the EITC.

The EITC is targeted at families with children under age 18, and thus in many specifications, we further restrict the sample to such families.[9] Note that this choice, coupled with the information we have on state EITC supplements, leads to a different control group than that used by Eissa and Liebman (1996) and Eissa and Hoynes (1998) in their studies of the federal EITC. In particular, those studies typically identify families most likely to be eligible for the EITC and compare the labor supply response in those families to the changes in labor supply in families not eligible for the EITC, such as those without children. In contrast, our estimation procedure makes use of state variation in the EITC across families with the same number of children to identify EITC effects on earned income, which, as we show below, often leads to quite different estimates. Descriptive statistics for this subsample are shown in the third column of Table 2.

As can be seen in the middle of the table, transition rates across various parts of the earned income distribution are fairly high. In each of the samples, more than 20 percent of families move out of poverty (based on earnings) each year, while about one-quarter of families with an initial earned income-to-needs ratio between one and 1.5 fall into poverty in the following year. These numbers are suggestive of a sizable amount of idiosyncratic change in family incomes, and provide a baseline with which to compare the EITC or minimum wage effects we report later in the paper.

Finally, the average federal EITC credit rate in our sample period was 14.8 percent for families with children, with the average size of an increase equalling 4.0 percentage points per year (using the numbers in column (2) of Table 2). The average state supplement for those states with an EITC program was 4.8 percent, with the average increase equal to 1.5 percentage points per year. The average real minimum wage was a bit less than $3 over our sample period, with the average real increase equal to $.20 per year.

The Earned Income Tax Credit: effects on earned income

Table 3 presents estimates of the effects of the EITC from the basic specifications shown in equations (1) and (2). Each row of the table reports estimates from a single specification, with the first column showing the estimated effects of changes in the federal credit rate and the second column estimated effects of changes in the state supplements. For the federal program, the credit rate is defined as the proportion of earned income that can be applied as a credit to an eligible family's federal taxes over the phase-in range of the EITC; this variable thus varies across families based on the year in which they are in the sample and on the number of children in the family. For the state programs, the variable is the supplement used to augment the federal credit as specified in states that also have an EITC program;[10] this variable also varies across years and with the number of dependent children in the family, but varies across states as well.

There is perhaps some question as to whether the policy variable should be entered in levels or changes. The motivation behind using changes is that a change in the policy (like an increase in the credit rate) generates a contemporaneous increase in the probability that poor families escape poverty, as some families now decide that employment is worthwhile for some member or members. On the other hand, to the extent that there are always random shocks throwing some families into poverty, a higher level of the EITC could arguably be associated with faster transitions out of poverty.[11] As the choice of whether to use the level or change is readily testable in a nested framework, we let the data guide us in this respect. In particular, the specification using levels of the credit rate and the p-values for the test of the restrictions implied by the change specification are shown in Appendix Table A1. The statistical significance of the lagged value in many specifications argues against using only the contemporaneous level of the credit rate, and in no case were we able to reject the restriction implied by the specification using only the change in the credit rate; indeed for the state credit rate this restriction held remarkably closely.[12] Thus, in the remainder of the paper, we report results from specifications using changes in the EITC variables.[13]

Returning to Table 3, Panel I reports estimates of the effects of the EITC on the probability that a family with earned income below the poverty line in year one has an earned income level above the poverty line in year two. As can be seen in the top row of the panel, the effect of the federal credit rate is negative, suggesting that increases in the EITC credit rate are associated with reductions in the rate of transition out of poverty, but the estimate is not statistically significant. One possible interpretation of a negative coefficient is that the EITC reduces work among low-income families, reflecting a large income effect on labor supply. Note, however, that because year effects are included in this specification, identification of the coefficient comes primarily from the correlation of changes in the federal EITC with differences in transition rates for families with no children and families with children. And, because there are potentially other federal policies that have different effects on the incomes of families with and without children (e.g., AFDC or child care credits), this equation may be subject to specification bias. We can get around some of the more obvious misspecifications by limiting the sample to families with children, thus identifying the coefficient from the differences in transition rates for families with one child and families with more than one child. But as indicated in the second row, the coefficient for this subsample is more negative, although with a considerably larger standard error.

A potentially more fruitful approach is to use state-year variation in the EITC program as a means of identifying the effects of the EITC on earned income.[14] The estimated coefficients of the state EITC variables are shown in the second column. For the sample as a whole, the estimated state EITC effect on the transition rate out of poverty is positive but not statistically significant. However, when the sample is restricted to include only families with children, the estimated EITC effect is larger and becomes significant. This is, in fact, the sample for which one would expect to see sizable EITC effects, and, in our view, results in a cleaner control group than can be achieved with the entire sample.

In the third row, we include year-children interaction terms in the specification to control for changes in other policies or economic changes that vary with the number of children in each family. Upon including these interaction terms, only the effect of state EITCs is identified. Using this specification, the results are similar to those in the previous row, with the beneficial effects of the EITC on family earnings again showing up quite clearly. Moreover, the size of the coefficient estimate indicates that the effect of the EITC on poverty rates is not negligible. To make this readily apparent from the regression results, the EITC variables are standardized so that one-unit changes correspond to the average increases among observations for which an increase occurred (reported in Table 2).[15] These coefficients indicate that an average (.04) increase in the credit rate increases the probability that a poor family's earnings rise above the poverty threshold by about .06. Since the mean of this transition rate is .21 (Table 2), this is an increase of between one-quarter and one-third.

We delve into the EITC effects further in Panel II, focusing in particular on changes in income-to-needs for families with earnings below the poverty line in year one (equation (2) above). The results for this specification are quite similar to those in Panel I. Changes in the federal credit rate are estimated to reduce income-to-needs in both the full poor family sample and in the sample restricted to poor families with children, although the estimates are not statistically significant. With respect to the state credit, the EITC has a positive and significant effect on income-to-needs in both samples, with the size of the coefficients indicating that an average (.04) increase in the credit rate raises the income-to-needs ratio among poor families by around .07 on average.

In Panel III, we examine the possibility that the EITC increases or reduces the probability that a family's earnings drop below the poverty line using a similar set of specifications. In this set of results, we restrict the sample to families with earned income-to-needs ratios between one and 1.5. In our sample period, the income level at which families are first entitled to the maximum EITC benefit almost always occurs at an income level that is below the poverty line for families with one or more children.[16] Thus, for these families theory predicts that a higher EITC will reduce labor supply. Of course it is not the credit rate that is relevant to these families, but the maximum credit. Historically, however, the maximum credit and the credit rate have always moved in the same direction, so to keep things consistent with the earlier specifications, we continue to look at the credit rate as a proxy for the generosity of the EITC.[17]

In general, there is little evidence that the EITC has any effect on the rate of transition from near-poor to poor. For the federal credit, the coefficient estimates are in the direction of suggesting that the EITC increases the likelihood that families fall into poverty, although the estimated effects are again not statistically significant. In addition, the coefficients on the state credit rate show essentially zero effect of the EITC on this transition rate.

Panel IV of the table reports the effects of the EITC on changes in earned income-to-needs ratios for this same set of near-poor families. As can be seen in the first column, changes in the federal credit rate are estimated to have negative effect on the income-to-needs ratio of near-poor families, with the estimated effect statistically significant for the sample as a whole but not for the subsample of families with children. In contrast, and consistent with the results in Panel III, changes in the state credit rate are estimated to have little effect on income-to-needs for this group of families.

In sum, the evidence from the federal and state experiments reported in Table 3 points in quite different directions. For the federal credit rate, there is no evidence that the EITC increases earned income among poor families, a result that runs counter to the program's intent. In contrast, the coefficients on the state credit rate suggest strongly that the EITC raises earned income among poor families with children. And, perhaps more importantly, families eligible for the EITC are more likely to see their earnings rise above the poverty line when the credit increases in generosity.[18] In the other direction, there is some evidence from the federal experiment that a higher EITC leads to a reduction in earned income among near-poor families, although this evidence is relatively weak. For the state experiment, the evidence points to very little effect on families with earned income initially just above the poverty line. Of course, all of these estimated coefficients understate the overall positive effects on income associated with the EITC. That is, the increase in total resources would be more pronounced if one considered the additional income received from the credit itself, which is not taken into account in this analysis.[19]

To demonstrate the source of the differences in results more clearly, Figures 1 and 2 present an analysis of the residuals from regressions of the EITC credit rate and poverty status in year two (conditional on poverty in year one) on the auxiliary variables in the basic specification. In particular, the upper-left panel of Figure 1 shows the yearly mean residuals of the change in the federal EITC credit rate for families with one child, while the upper-middle panel shows the yearly mean residuals for the changes in the federal credit rate for families with two or more children. Comparing these two panels documents that the credit rate was increased by significantly different amounts in 1991 and 1994, with more minor differences in the intervening years (see Table 1); note that in all instances, the credit rate was increased by more for families with two children than for families with one child, resulting in the pattern of residuals shown in these panels.

As it is relative movements in the one- and two-child credit rates that identify the EITC effect in the federal experiment, the estimated effect can be derived from the correlation of these residuals with those in the bottom panel, which are the mean residuals from the poverty status regression. In particular, note that the relative movements in the credit rate in 1991 are negatively correlated with the transition rate residuals in that year, while the large relative change in the credit rates in 1994 are associated with residuals in the middle of the range. As can be seen in the upper-right panel, these patterns, combined with those for the remaining years, result in a negative and imprecisely estimated EITC effect from the federal experiments.

Figure 2, which shows similarly calculated residuals for the state treatment and control groups, indicates that there is significantly more variation in the relative movements in the credit rate in the state experiment, which probably helps to identify the EITC effect more accurately. As indicated in the upper-right panel, the mean residuals for changes in the credit rate and the transitions out of poverty are positively correlated in the state experiment, producing a positive estimate of the effect of the EITC on earned income-to-needs.

Labor supply effects

Although our sense is that using state variation in the EITC provides a cleaner experiment than does the federal credit, the differences in coefficient estimates associated with these two EITC variables raise important questions about what we have found. To attempt to shed additional light on the results for income, we performed a complementary analysis of the effects of the EITC on employment and hours worked for our subsample of poor families with children. In general, the specifications are similar to those in Table 3, with the exception that the dependent variable is either the probability that a family added an adult worker in the second year they were in the sample, or the change in total hours supplied by adult family members between the first and second years.

In Table 4, we estimate the effects of the EITC on the probability that a family adds an adult worker between year one and year two. In Panel I, we first (in sub-panel A) limit the sample to those families that had no adults working in year one. For both the federal and state EITC variables, the effects of the EITC show through quite clearly. In particular, an average increase in the federal EITC is estimated to raise the probability of adding a worker in year two by .11, and the coefficient is statistically significant at the five-percent level. The specifications using the state credit show a similarly-sized positive effect on employment, although it is estimated a bit less precisely. The fact that we find positive employment effects for both credit rates suggests that the earlier negative effects on income found for the federal EITC specifications may have been spurious, possibly owing to correlations between the EITC and other federal programs that affected the incomes of poor families in a way that masked the benefits of the EITC, but did not have a corresponding effect on labor supply behavior.

In sub-panel B, we restrict the sample to families that had one adult worker in year one to see if the employment effects extended to the working poor as well. In general, there is little evidence of any EITC effect for this subgroup. The estimated effect of the federal credit is positive, but imprecise. For the state credit rate, there is weak evidence of a negative effect of the EITC on the probability that such families add another worker to the labor market. Although this result is suggestive of a negative income effect on labor supply, the estimated coefficients are also statistically insignificant.

In Panel II, we examine the effects of the EITC on total hours worked by adult family members, splitting the sample into those families with no adult workers in year one and those families with at least one adult worker initially. As can be seen in sub-panel C, the federal EITC is estimated to have essentially no effect on hours worked by families with no adult workers initially. This result seems surprising given the positive employment effects reported in the top panel, and again points to the inconsistent and potentially spurious results obtained for the federal EITC experiment. In contrast, the estimated effects of the state credit rate show sizable positive effects on annual hours worked in families without an adult worker in the first year, which although only marginally significant, are consistent with the results for employment shown in the top panel. The size of the hours change seems large at first glance, but given that it is associated with entry into the work force, it is consistent with the lumpy nature of employment. For families with an adult worker in year one (sub-panel D of Panel II), there is a positive EITC effect using the federal credit rate as the explanatory variable, but the estimated coefficient is not statistically significant. The effect of increases in the state EITC credit rate, however, is to reduce hours worked for families that initially had at least one adult worker in year one. These results are the clearest indication we have of the negative effect of the EITC on labor supply found by some previous researchers (e.g., Eissa and Hoynes, 1998).

The results from Table 4 indicate more clearly the sources of the positive and negative effects from the EITC. Increases in earnings mainly come about by inducing adult workers in families without an adult in the labor market to enter the labor market in order to take advantage of the credit. In contrast, poor families that already have an adult working do not increase their hours and may reduce them in response to an increase in the credit rate. This suggests that such families are likely to be operating on the flat portion of the EITC schedule, so that the negative influence of higher income on labor supply is the dominant consideration. However, the evidence in Table 3 indicating no negative effects of increased generosity of the EITC on near-poor families differs somewhat in indicating that for these families there is little response to the EITC. This poses somewhat of a puzzle because the families for whom these effects are identified are more likely to be in the phase-out range, where the negative labor supply effects should be stronger.

In Table 5, we attempt to match up our estimated effects of the EITC on income with the estimated labor supply effects reported in Table 4. In particular, given that the effects on employment and hours worked are most pronounced for poor families with no adult workers in year one, we would expect to see the largest positive effects on earned income for these families. In contrast, the effects on earned income for poor families with an adult worker in year one should be minimal or even negative, as there is no evidence of a positive labor supply response for such families, and some evidence of an EITC-induced reduction in hours worked.

For poor families with no adult workers in year one, the results in Table 5 generally conform to these expectations, at least for the coefficients on the state credit rate. In particular, there are clear positive effects from the state credit rate on the probability that a poor family without a worker in year one becomes non-poor in year two (sub-panel A), as well as on the change in income-to-needs ratios (sub-panel C) for such families. In contrast, the estimates show little effect of the federal EITC on the earned income of poor families with no adult worker in the first year, either in terms of the transition rate out of poverty or the income-to-needs ratio, despite the strong positive employment effect reported in Table 4.

Turning to poor families with an adult worker in year one, the results in sub-panel B indicate little effect from either the federal or state EITC on the transition rate to above poverty-level earnings, consistent with the lack of positive labor supply influences reported in Table 4. As indicated in sub-panel D, when the dependent variable is specified as the change in income-to-needs, the coefficient estimates on the state EITC variable are positive, although not statistically significant. Nevertheless, these positive or non-negative earnings effects are difficult to square with the EITC-induced reduction in hours worked for these families estimated in Table 4. In part, it appears that some large changes in income-to-needs within this group account for the positive estimate in Panel D. When we use the probability that a family experienced an increase in income-to-needs between year one and year two (Panel III), as a means of reducing the influence of extreme observations, the EITC coefficient estimates for the sample of poor families with no adult worker in year one are similar to those in Panel II (although less precisely estimated), while the coefficient estimates for the sample of poor families with an adult worker in year one become small and negative; the failure to find stronger negative effects despite the negative labor supply effects for this group is nonetheless puzzling.

Robustness and validity checks

The next two tables assess the robustness and validity of the estimated effects of the EITC on income-to-needs by varying the specification and sample used in the analysis. In Table 6, we vary the threshold across which we measure transitions to test the sensitivity of our results to this rather arbitrary specification choice. There are two reasons to look at evidence for earned income-to-needs thresholds below one. First, given our findings that the EITC effects are largest for families without an adult worker (and thus little if any earned income) in year one, we might expect to see stronger effects when we set the threshold at a lower level. For example, a family in which a worker enters the labor market at a minimum wage job is unlikely to attain poverty-level earnings in the subsequent year (more so if there are children or the job is part-time). In this case, our choice of one for the earned income-to-needs cutoff in previous specifications would miss some of the beneficial effects of the EITC. Second, as already noted, we do not measure all components of income used to classify families as poor or non-poor, so that families attaining a lower fraction of the poverty threshold in terms of earnings alone might nonetheless escape poverty.

The results in the table are suggestive of some sensitivity to the choice of threshold, at least for families with no adult worker in year one (sub-panel B). In particular, the state EITC effects are strongest for this subsample when the threshold is set at .3, indicating that some families' earned income-to-needs are lifted by the EITC, although not to a ratio above the poverty line. This suggests that our previous estimates focusing on the transition out of poverty may understate the beneficial effects of the EITC on earnings of low-income families. Of course, some families seem to be helped even more by the EITC, as is indicated by the positive coefficients for all of the income thresholds in the table. Consistent with the results reported in Table 5, the estimated effect of the federal EITC is small and negative for poor families with no adult worker in year one, a result that is insensitive to the choice of threshold. Similarly, for poor families with an adult worker in year one, our earlier finding of essentially no effect of the EITC on poverty transitions is not affected by the choice of income-to-needs threshold.

In Table 7, we repeat the analysis in Table 3 for both the effects of the EITC on transition rates to above poverty-level earnings and on changes in income-to-needs, but introducing a variety of other differences in the sample or specification. In Panels A and B, we vary our treatment of states offering only non-refundable tax credits at the state level, on the grounds that refundability is often touted as a major feature contributing to the value of the program to low-income families. In particular, if refundability is important to a family's labor supply decisions, the estimated EITC effects should be larger for the lowest-income families once the credit for states without refundable credits is downgraded or when these states are dropped.[20]

When the states with non-refundable credits are excluded from the sample (Panel A), the estimated effects of the state EITC on transitions out of poverty for this subsample are smaller than the baseline results, rather than larger, and no longer statistically significant. However, the effect of the state EITC on the change in income-to-needs is still significant when these states are excluded and is nearly identical to the baseline results. As an alternative, we attempt to equate the effective rate of refundable and non-refundable credits, setting the credit rate for the latter equal to one-fifth of the statutory rate. The choice of one-fifth as the equivalent rate for non-refundable credits is based on the "trade-off" between refundable and non-refundable rates established in Maryland in 1998, when taxpayers were offered an option of a 50-percent non-refundable supplement or a 10-percent refundable supplement. However, we obtain essentially the same results when we do this, as reported in Panel B. In no case is there any meaningful change to the federal or state EITC coefficients.

In Panels C and D, we alter the set of control variables used in the analysis. In Panel C, we drop the change in the unemployment rate, on the grounds that it is potentially endogenous to changes in the EITC (if, for example, the EITC raises household spending). The drawback to this change in specification is the absence of controls for state labor market influences on poverty rates. In any event, there is little change in any of the EITC parameter estimates when the unemployment rate is omitted, perhaps because much of the variation in economic conditions is aggregate variation captured by the year effects. In Panel D, we add controls for changes in real AFDC benefits for a family of three, and in the extent to which state welfare laws could differ from federal regulations (federal waivers). The results are very similar when these controls are added, consistent with our earlier argument that the specifications using the state credit rate are less susceptible to biases associated with the omission of other state policies.

In Panel E, we use changes in the maximum credit rather than the phase-in rate as the EITC policy variable. The rationale for this alternative is that the change in the phase-in rate does not always provide a complete description of changes in the EITC program. In particular, using the maximum credit also captures changes in the phase-in range, and thus may pick up some variation omitted by the credit variable. As it turns out–perhaps not surprisingly given that the changes in the maximum credit and the phase-in rate are highly correlated in our sample–substituting the change in the maximum credit as the EITC policy variable has little qualitative effect on the results. In particular, for the federal EITC, the coefficients are still negative and insignificant. For the state EITC, the estimated effect is somewhat smaller than the baseline results (as before, for average policy changes), but the coefficients continue to indicate positive and statistically significant effects of the EITC on both the transition rate out of poverty and on the change in income-to-needs.

The next three panels investigate the effects of using alternative treatment and control groups in the analysis. In Panel F, we limit the control group to states that have a geographical border with the states that have their own EITC program. The idea here is that bordering states will be more similar in other ways to the EITC states and thus form a better control group. For the state EITC phase-in rate, there is little change in the coefficient estimates. For the federal

EITC, the estimated effect is still negative, but is substantially larger than in the baseline, and is statistically significant. In Panels G and H, we omit the states with the largest changes in credit rates from the sample to test the sensitivity of the estimated EITC effects to these policy outliers. In Panel G, we drop all observations on families from Wisconsin with three or more children, for whom the phase-in rate was very high beginning in 1989. In Panel H, we omit observations from Maryland, which introduced a large non-refundable credit in 1987. In neither case does the change in sample have much effect on either the federal or state EITC coefficient estimates. [21]

Finally, in Panels I and J, we examine the possibility that the results we report for the EITC in earlier tables are spurious, by looking for positive EITC effects in samples of families not eligible to participate in the program. In Panel I, we apply our methodology to families that have earned income-to-needs ratios between zero and three, but that do not have any children and thus would not be eligible for the EITC prior to 1994. In particular, we estimate specifications like those in Table 3 for this sample of currently childless families, but attach the EITC parameters that would have been relevant to them if they had three or more children. If the effects we have attributed to the EITC were actually associated with an omitted variable that also boosted earned income and that was correlated with the EITC, then we might expect to see spurious "EITC effects" for this ineligible population as well. For both the federal and state EITC variables, the estimated coefficients are very small and insignificant. In Panel J, we perform the same analysis on a sample of families with income-to-needs between three and 10, well above the qualifying cut-off for the EITC. Again, the resulting coefficient estimates are small and insignificant, indicating no effect of the EITC on this group of families. As we should not expect to see EITC effects for these particular samples, these results add credence to our interpretation of the results in Table 3 as reflecting causal effects of the EITC.

The EITC versus the minimum wage

In Table 8, we report estimates from specifications that include both the EITC variables and the minimum wage, to see if conditioning on minimum wage changes alters the estimated EITC effects on earned income, and to provide a more direct contrast of the effects of the alternative policies for the same population. As it turns out, there is a relatively low correlation coefficient (less than .15) between changes in minimum wages and changes in EITC parameters, and thus adding the minimum wage changes to the specification has little effect on the estimates of the EITC effects. As already noted, we scale the EITC variables to measure the effects of average policy increases in the federal credit rate over the sample period. Similarly, we scale the minimum wage variable to measure the effects of average real increases in the minimum wage over the same period.[22]

In Panel I, we report estimates of the effects of the two policies on transition rates from below poverty to above poverty-level earnings. Looking first at the results for the full sample (sub-panel A), as can be seen in the first row, for the entire sample of poor families we again find no evidence that the federal EITC raises earned income-to-needs, and only weak evidence that the state EITC has a positive effect. In addition, the minimum wage is estimated to have essentially no effect on the transition rate, consistent with the evidence reported in Neumark and Wascher (1997).[23] When the sample is restricted to families with children, however, both the state EITC and the minimum wage are estimated to have a positive influence on earnings. The EITC effect is more than twice as large as the minimum wage effect, pointing to the greater effectiveness of the EITC in raising earnings, at least for the types of changes in policy enacted during our sample period.[24] Moreover, the difference in minimum wage effects between families with and without children is suggestive of an offsetting negative effect on earnings of families without children.

The remainder of Panel I reports results conditioning on the presence of an adult worker in the family initially, which, as documented in earlier tables, was strongly associated with the effects of the EITC. In sub-panel B, we report results for families with no adult worker in year one. As before, we find stronger positive effects of the EITC on the probability of a transition to above poverty-level earnings. In contrast, the minimum wage effects are weaker, with the contemporaneous effect, in particular, becoming smaller and statistically insignificant. In sub-panel C, we report estimates for families with at least one adult worker in year one. Here there are no significant positive effects of the EITC, but the positive effects of the minimum wage are considerably larger. This evidence is consistent with the EITC doing much more to help families with no adult worker initially, while the benefits of the minimum wage fall more strongly on those families–with children–that have adult workers.[25]

In Panel II, we repeat the analysis using changes in income-to-needs as the dependent variable. Qualitatively, the results are similar to those in Panel I. The federal EITC has either a negative or no discernible effect on income-to-needs ratios among poor families; the state EITC has a positive effect on income-to-needs, especially for the subsample of poor families with children; and the minimum wage has a modest positive effect on earnings of poor families with children, but no effect for the sample of poor families as a whole. Similarly, disaggregating by the presence of adult workers in the initial year again reveals that the benefits of the EITC fall largely on families with no adult workers initially, while the minimum wage benefits families with adult workers.

Conclusions

This paper evaluates the effectiveness of the earned income tax credit in raising the earnings of poor and low-income families. In particular, we move beyond the simulation approach commonly used in the literature and provide reduced-form estimates using two-year panels of families derived from matched CPS files, focusing on the probability that an initially poor family sees its earned income rise above the poverty line, and on the changes in income-to-needs that such families experience, as a result of increased generosity of the EITC.

The results of our estimation strategy hinge on the nature of the experiment we perform. In particular, when we look at the federal EITC, identifying its effects from differences in the credit rate by family size, we either find that the EITC reduces the probability that a poor family escapes poverty or, in our preferred specifications, find no statistically significant effect. In contrast, when we focus on state credit rates, thus using state variation to identify the EITC effects, we find evidence that the EITC helps families rise above poverty-level earnings. Neither experiment points to any effect of the EITC on families initially above the poverty line, suggesting that the EITC is well targeted. We find a similar pattern of results when we use changes in income-to-needs as the dependent variable.

The difference between the estimated effects on earned income using the federal or state credit rate is explored further by examining the effects of the EITC on family labor supply. In general, the results based on the state credit rate form a more consistent whole, pointing to a positive labor supply effect for the same sets of families for which we estimate a positive effect on earned income. In contrast, the results based on the federal credit rate are difficult to interpret in a consistent fashion, raising the possibility that our federal EITC estimates are being contaminated by spurious influences, perhaps stemming from the small amount of variation that identifies the federal experiment, and/or underlying differences in trends for families of different sizes. In particular, one possibility is that other federal policies are changing over this period in a way that limits the extent to which variation in the number of children in a family can identify the EITC effect.

Meyer and Rosenbaum (1999a) provide a detailed discussion of a wide array of such policy changes, in particular those focused on increasing work incentives for single mothers. These include changes in income taxes, AFDC and Food Stamp benefits, Medicaid, training programs, child care programs, and private health insurance. They also provide rather compelling evidence that–as a whole–the time-series pattern of changes in work among single mothers is consistent with these policy changes having sharply boosted employment of single mothers. To the extent that such policy changes induce different patterns of employment change in families with and without children, as well as in families with different numbers of children, isolating the effects of the EITC may require using specifications that allow for different trends over time for families with different numbers of children (i.e., year $\times$ children interactions).[26] As only the state EITC experiment is identified in such specifications, this explanation is an argument in favor of our state experiment for estimating the effects of the EITC on earnings, and is consistent with our evidence that only the state EITCs yield a set of results broadly consistent with theoretical predictions.

The labor supply estimates we report also indicate more clearly the channels through which the EITC operates. In particular, there is strong evidence that the EITC raises earnings by inducing labor market entry among families that initially do not have any adults in the work force. In contrast, we find some evidence that increasing the EITC credit rate reduces hours among poor families that already have a working adult, thus offsetting the additional (post-tax) earnings associated with the more generous credit. Thus, our estimates are consistent with both alternatives found in the literature–notably the positive labor supply effect for single mothers (many of whom would not have been in the work force initially) and the negative overall labor supply effect for married couples (at least one of whom may initially have been an earner).

Finally, our results suggest that for the range of policy changes that have been typical in recent years, the EITC is more beneficial for poor families than is the minimum wage. The minimum wage appears to have some positive effect on the earnings of families with children. But our estimates of the effects of the EITC are much larger than for the minimum wage. Our evidence also highlights the different types of families that the alternative policies are likely to benefit, indicating that a higher minimum wage helps families with adults in the work force, while increased generosity of the EITC primarily benefits families that initially have no adult workers, an empirical result that follows quite expectedly from the theoretical prediction that the EITC will encourage labor market entry.

The fact that the EITC appears to increase earnings is a particularly strong statement in favor of such a policy. For one thing, our analysis does not include the income benefits of the tax credit itself, and thus we understate the overall effects of the EITC on the total income of eligible families. Moreover, if the EITC were simply to increase total income rather than earnings, it would be just another tax and transfer scheme, similar to those that have consistently disappointed policy makers in the past, presumably because the EITC would not provide longer-run beneficial effects stemming from increased attachment to the labor market. In this sense, evidence that the EITC raises labor force participation and earnings among the poorest families suggests that it may also have a positive influence by encouraging economic self-sufficiency among the poor, which would enhance its effectiveness as a policy for fighting poverty, and–we suspect–its political support.

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Footnotes

Neumark is Professor of Economics at Michigan State University and a Research Associate of the NBER. Wascher is Chief of the Wages, Prices, and Productivity Section at the Board of Governors of the Federal Reserve System. We thank Raven Saks and Yuri Soares for excellent research assistance, and Douglas Holtz-Eakin, Bruce Meyer, Chris Taber, David Wetzell, Jim Ziliak, and two anonymous referees for helpful comments. The views expressed are those of the authors only, and do not necessarily reflect those of the Federal Reserve Board. This paper was prepared for the Joint Center for Poverty Research conference "The Earned Income Tax Credit: Early Evidence," Northwestern University, Oct. 7-8, 1999.

[1] Blank, et al. (1999) suggest that policy makers also attempt to use such programs to encourage longer-run economic self-sufficiency. This stated objective might help to explain the interest in the labor market effects of income-support programs, although it does not explain the dearth of research on their effects on income. Of course, one could argue that if there are no effects of the EITC on wages (which theory predicts might fall), then studying labor supply effects is sufficient to gauge income effects. Strictly speaking, this is true, as long as we can look at labor supply of all family members, weighted by the wages of each person. Studying family income directly is less demanding of the data, and of course we should not necessarily rule out wage effects of the EITC.

[2] Another alternative is to focus on the effects of the EITC on consumption, as is done in Romich and Weisner (1999), Smeeding, et al. (1999), and Barrow and McGranahan (1999).

[3] We have chosen to cut off the sample at this point for two reasons. First, we wanted to avoid potential complications associated with welfare reform. In particular, the introduction of TANF in 1996 and the corresponding proliferation of state experiments would likely make it difficult to distinguish EITC effects from those of other policies. Second, because of CPS sample redesign, data from June '94-August '95 cannot be matched forward, so we cannot use the March 1995-March 1996 match.

[4] When reporting descriptive statistics we use weights designed to reduce bias associated with differential match rates. Specifically, we retained the family-specific sampling weight from the CPS and adjusted by an estimate of the probability of a successful match derived from a logistic regression of matching success on the age of the family head and the initial income-to-needs ratio. The resulting weight is an estimate of the inverse of the probability of being in our matched sample of families. Although the regression estimates we report are unweighted, the results were not qualitatively affected by weighting.

[5] In our sample, the correlation between the changes in the credit rate and the maximum credit is 0.98, which is not surprising since they are closely related by construction. When we estimated specifications using the change in the maximum credit in place of the change in the credit rate, the results were very similar to those shown here; limited results using this alternative measure are reported below.

[6] See Council of Economic Advisers (1997) for additional information on how these variables were constructed.

[7] P_{ist} is typically a vector, including, for example, changes in federal and state EITC parameters.

[8] In estimating equation (1), we found that the estimated standard errors of the state EITC variables are smaller when fixed state and year effects are included. This implies that the inclusion of these variables reduces the residual variance by an amount sufficient to outweigh the

loss of information that including such fixed effects usually entails. In such a case, the fixed effects should obviously be included.

[9] Indeed, prior to 1994, families with no children were not eligible for the EITC. Henceforth, we simply refer to families with and without children under 18 as those with and without children.

[10] With the federal rate denoted r_f, and the state rate denoted r_s (defined as zero in states without an EITC), the combined rate (r_c) is $r_c = r_f \cdot (1+r_s)$. The federal EITC is calculated as r_f multiplied by income (Y), over the phase-in range. The state EITC is then equal to r_s multiplied by the federal EITC, or $r_s \cdot (r_f \cdot Y)$ (see, e.g., Johnson and Lazere, 1999). We therefore use r_f as the federal rate, and $r_s \cdot r_f$ as the state rate.

[11] One potential drawback of using the level of the EITC in equation (1) is that, unless for some reason a higher EITC also increased the rate of transition into poverty, the eventual implication of a permanent increase in the transition rate out of poverty would be to eliminate poverty altogether.

[12] For the federal credit rate, the estimates of coefficients on the lagged level were very imprecise, as this variable has far less variation given that the sample period ends in 1994.

[13] We also estimated specifications in which we added the second lag (in levels) to the specification with current and lagged levels. The second lag was always insignificant, whether tested jointly (federal and state) or separately.

[14] This is, of course, subject to some of the same concerns as the federal credit, in that there may be state-specific policies coming into play at the same time that have differing income effects on families with different numbers of children. However, we suspect that this is less of a problem across states than it is for the federal EITC; moreover, the additional variation in the data allows us to include additional interactions to control for some of these possible effects. The drawback of this approach is that relatively few states have a separate EITC. This raises the possibility that the estimates are being driven by one or two states, an issue to which we return later in the paper.

[15] We use the average change in the federal EITC rate for both federal and state EITC variables since we are trying to estimate the effect of a single policy, although we have two different experiments with which to do this. The choice of scale has no effect on the statistical inferences, of course.

[16] For only a small handful of near-poor families (1.7 percent) did the poverty line climb above the maximum credit in year 2, by an average of only $160 (nominal).

[17] The phase-out rate and the length of the credit plateau (i.e., the range of earnings over which the maximum credit is earned) can also affect labor supply. Since 1987, increases in the credit rate have always been associated with increases in the phase-out rate, which should reduce labor supply, but the length of the plateau has also increased with the credit rate, which could exert a slight positive influence on labor supply as families move to a higher kink point. In any event, we verified that the empirical results for the near-poor are similar using the change in the maximum credit as the policy variable.

[18] For the specifications for transitions to above poverty-level earnings, and for changes in income-to-needs for families below poverty-level earnings, we tested the restriction that the state and federal effects were equal. This was rejected at the 5-percent level.

[19] Information on the actual credit received is not available in the CPS.

[20] For our sample period, excluding families only eligible for non-refundable credits means dropping all observations for Maryland, Iowa, and Rhode Island.

[21] There were also sizable expansions in Medicaid in Wisconsin in both 1988 and 1992, which may confound the estimated effects of the EITC. We verified that the results were qualitatively similar omitting the Wisconsin observations altogether, although remaining statistically significant only for the change in income-to-needs.

[22] Whether we use federal increases only or federal and state increases is irrelevant, as the former is .209, and the latter .204. (These are real increases in 1982-1984 dollars; see Table 2.)

[23] In that paper, we find that minimum wages have a slight positive effect on the probability of escaping poverty (defined in terms of total income), and a larger positive effect on the probability of slipping into poverty.

[24] One could contemplate policy changes of very different magnitudes, and in principle come up with a comparison as favorable to one policy or the other as one would like. However, the dangers of extrapolating regression results well beyond the range of changes in the exogenous variables that occur in the sample are well-known, and it is plausible that the effects of policy changes of very different magnitudes differ substantially. For example, many of those who argue that minimum wage increases do not reduce employment of low-wage workers qualify their conclusions to refer to small changes. It is because of these considerations that we express our results in terms of the sample average policy changes.

[25] The latter result is not surprising. While job loss from minimum wage increases may entail some "losers," the winners are relatively more likely to be those with jobs already. Of course, given relatively high turnover among low-wage workers, some of those without jobs initially may benefit by entering the labor market at higher wages.

[26] Meyer and Rosenbaum (1999b) carry out a multivariate analysis of the effects of the EITC (and income tax rates) on the labor supply of single women, in light of other accompanying policy changes. Echoing our concerns, they add interactions between years and number of children to their specifications. Because they look at the EITC and income taxes combined, use simulated tax/EITC payments, and focus on single women, the results are not directly comparable to ours.

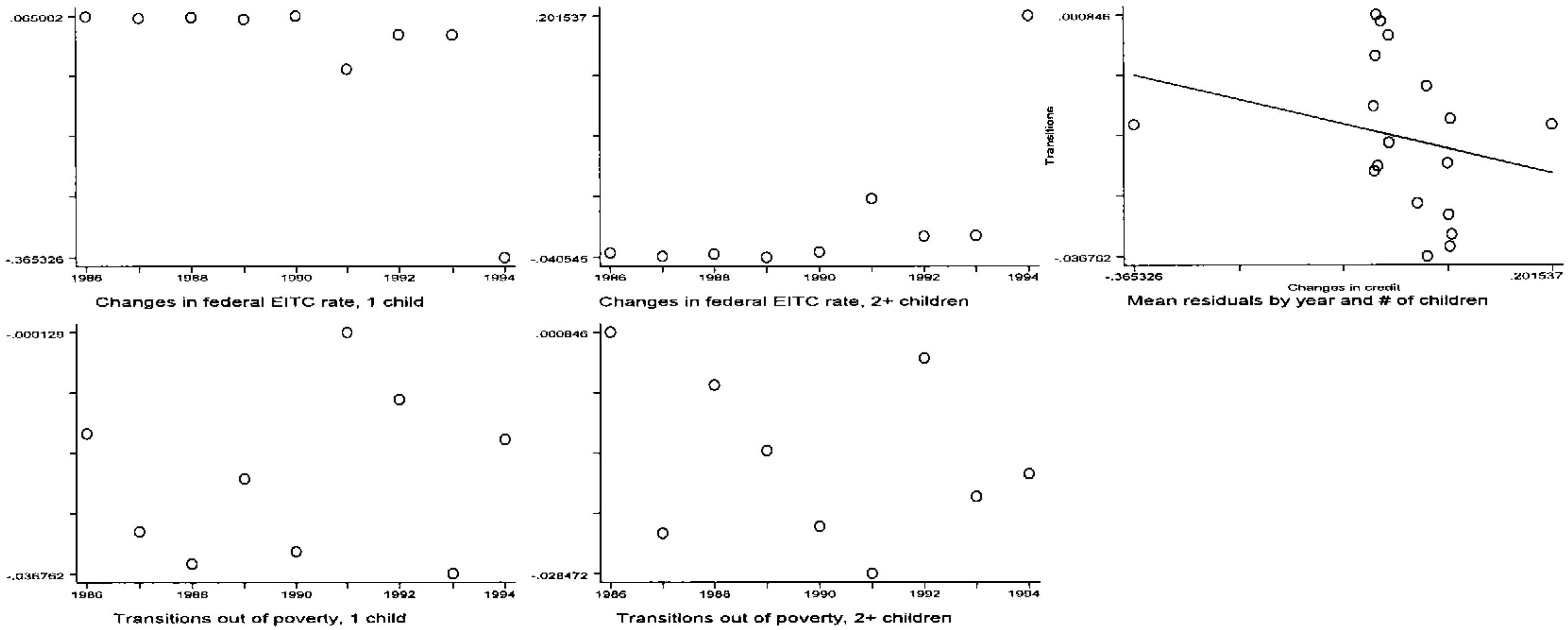

Figure 1: Analysis of residuals for federal experiment

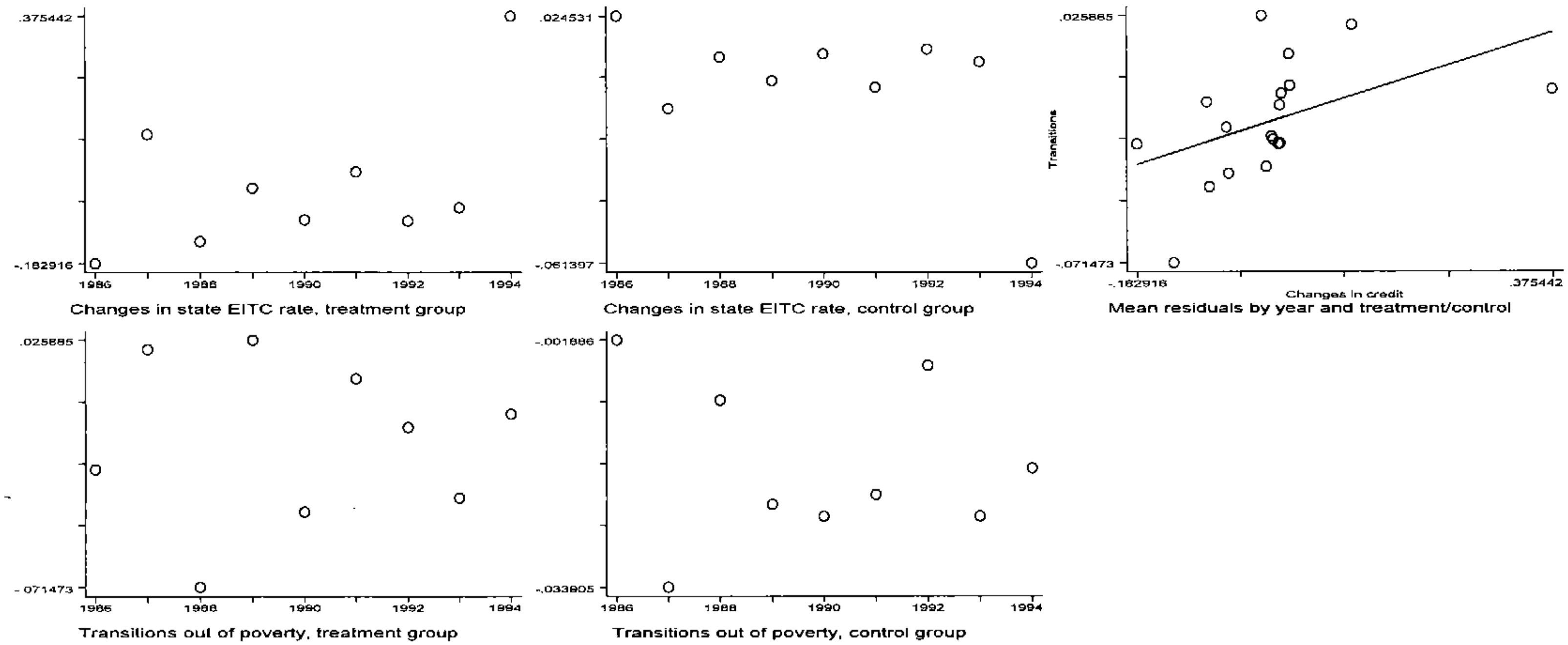

Figure 2: Analysis of residuals for state experiment

Table 1: Federal and State EITC Parameters

Jurisdiction:	Federal						Percentage supplement								
Parameter:	Credit rate			Maximum credit			Maryland	Minnesota	New York	Vermont	Wisconsin			Iowa	Rhode Island
# children	0	1	2+	0	1	2+					1	2	3+		
1984	...	10	10	...	500	500	...	...	...	...	30	30	30	...	...
1985	...	11	11	...	550	550	...	...	...	...	30	30	30	...	...
1986	...	11	11	...	550	550	...	...	...	...	...	...	...	...	22.21
1987	...	14	14	...	850.5	850.5	50	...	...	...	...	...	...	...	23.46
1988	...	14	14	...	871.5	871.5	50	...	...	23	...	...	...	...	22.96
1989	...	14	14	...	910	910	50	...	...	25	5	25	75	...	22.96
1990	...	14	14	...	953.4	953.4	50	...	...	28	5	25	75	5	22.96
1991	...	16.7	17.3	...	1192.4	1235.2	50	10	...	28	5	25	75	6.5	27.5
1992	...	17.6	18.4	...	1323.5	1383.7	50	10	...	28	5	25	75	6.5	27.5
1993	...	18.5	19.5	...	1433.8	1511.3	50	15	...	28	5	25	75	6.5	27.5
1994	7.65	26.3	30	306	2038.3	2527.5	50	15	7.5	28	4.4	20.8	62.5	6.5	27.5

The "percentage supplement" describes the state EITC credit rate as a percentage of the federal rate. Thus, for example, for Wisconsin in 1984 for a family with one child the effective rate is 13 percent ($10 \times (1+.3)$). The credits for Maryland, Iowa, and Rhode Island are non-refundable. For Rhode Island, in 1992 and 1993 the credit rate changes slightly for incomes over \$15,000; the table reports the initial credit rate, which is also used in the empirical analysis. We ignore two features of the federal EITC that prevailed from 1991-1993: the young child credit, because it applies only to those with children under one year old; and the health insurance credit, as the ability of taxpayers to use this credit depends on other expenses that we cannot observe.

Sources:

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Additional information on all states was also obtained from contacts with state income tax offices, and found in:
Johnson, Nicholas, and Ed Lazere, "Rising Number of States Offer Earned income Tax Credits." Center on Budget and Policy Priorities. September, 1998.

Table 2: Descriptive Statistics

		Families with income-to-needs between 0 and 3	
	Full sample	Full sample	Families with children under 18
	(1)	(2)	(3)
Earned income (nominal)	30,858.1	12,453.2	15,593.7
	(27,069.9)	(11,391.5)	(12,652.3)
Earned income-to-needs	3.13	1.17	1.21
	(2.71)	(.95)	(.92)
Change in earned income-to-needs	.03	.02	.04
	(1.81)	(.71)	(.92)
Income-to-needs based on earned income:			
Poor (<1)	.23	.46	.44
Near-poor (1-1.5)	.08	.15	.16
Transitions among income-to-needs categories, based on earned income:			
P(Non-poor in year 2|Poor in year 1)	.247	.207	.208
P(Poor in year 2|Near-poor in year 1)	.256	.284	.251
Real minimum wage	2.99	2.99	2.99
Change in real minimum wage, nominal change > 0	.20	.20	.20
	(.13)	(.13)	(.12)
	[N=39,681]	[N=16,441]	[N=8,725]
Federal EITC credit rate, rate > 0	.147	.148	.148
	[N=69,396]	[N=34,282]	[N=34,282]
Change in federal EITC credit rate, change > 0	.042	.040	.035
	(.033)	(.033)	(.032)
	[N=46,774]	[N=22,316]	[N=19,326]
State EITC credit rate, rate > 0	.048	.048	.048
	[N=2,866]	[N=1,279]	[N=1,279]
Change in state EITC credit rate, change > 0	.015	.015	.015
	(.016)	(.017)	(.017)
	[N=4,258]	[N=1,860]	[N=1,497]
One child	.20	.20	.37
Two children	.18	.20	.36
Three or more children	.10	.15	.28
N	149,506	63,791	34,282

The sample is restricted to families in which the age of the family head is under 65. Real values are in 1982-1984 dollars. Levels refer to year 1 in the matched CPS samples, and changes to the year 1 to year 2 change. The data cover matched March CPS files from 1986-87 to 1994-95. Standard deviations of some variables are reported in parentheses. Estimates are weighted by family weights adjusted for probability of being matched.

Table 3: Effects of Earned Income Tax Credit on Probability of Transitions to Above or Below Poverty-Level Earnings, and on Changes in Income-to-Needs

I. Transitions to Above Poverty-Level Earnings

	Changes in credit rate, year 1 to year 2	
	Federal	State
	(1)	(2)
P{Non-poor in year 2|Poor in year 1}, N=26,707		
EITC effect	-.022	.042
	(.016)	(.031)
<u>Families with children only (N=12,573)</u>		
EITC effect	-.053	.063**
	(.039)	(.029)
<u>Add year × children interactions, families with children only</u>		
EITC effect	...	.061**
		(.029)

II. Changes in Income-to-Needs, Families Initially Below Poverty-Level Earnings

	Federal	State
Change in income-to-needs|Poor in year 1, N=26,707		
EITC effect	-.019	.053*
	(.022)	(.032)
<u>Families with children only (N=12,573)</u>		
EITC effect	-.067	.067**
	(.055)	(.027)
<u>Add year × children interactions, families with children only</u>		
EITC effect	...	.068**
		(.027)

III. Transitions to Below Poverty-Level Earnings, from Near-Poverty-Level Earnings

	Federal	State
P{Poor in year 2|Near-poor (income-to-needs between 1 and 1.5) in year 1}, N=9,341		
EITC effect	.041	.019
	(.029)	(.041)
<u>Families with children only (N=5,559)</u>		
EITC effect	.042	-.015
	(.061)	(.048)
<u>Add year × children interactions, families with children only</u>		
EITC effect	...	-.018
		(.047)

IV. Changes in Income-to-Needs, Families Initially At Near-Poverty-Level Earnings

	Federal	State
Change in income-to-needs|Near-poor in year 1, N=9,341		
EITC effect	-.099**	-.050
	(.042)	(.062)
<u>Families with children only (N=5,559)</u>		
EITC effect	-.112	.020
	(.089)	(.066)
<u>Add year × children interactions, families with children only</u>		
EITC effect	...	.019
		(.066)

The income measure is total family earnings. Linear probability models are estimated using OLS. All specifications include fixed state and year effects, the change in the prime-age male unemployment rate, and indicators for one, two, and three or more children. Each row reports estimates from a single specification. Standard errors are robust to non-independence among state-year clusters, and to heteroscedasticity. Estimated effects are transformed to reflect the impact of the "average" change (.04) in the federal credit rate; see Table 2. A '**' superscript indicates statistical significance at the five-percent level in a two-sided test, and a '*' superscript at the ten-percent level.

Table 4: Effects of Earned Income Tax Credit on Employment and Hours Worked in Families with Initial Earnings Below Poverty Level, Families with Children

I. Employment

	Changes in credit rate, year 1 to year 2	
	Federal	State
	(1)	(2)
A. P{Add adult worker in year 2|No adult worker in year 1}, N=4,834		
EITC effect	.112**	.133*
	(.054)	(.077)
Add year × children interactions		
EITC effect	...	.133*
		(.076)
B. P{Add adult worker in year 2|One adult worker in year 1}, N=6,022		
EITC effect	.034	-.019
	(.032)	(.021)
Add year × children interactions		
EITC effect	...	-.022
		(.021)
II. Hours worked		
C. Change in total hours worked by adults|No adult worker in year 1, N=4,834		
EITC effect	2.63	180.98
	(120.25)	(121.13)
Add year × children interactions		
EITC effect	...	183.41
		(121.85)
D. Change in total hours worked by adults|At least one adult worker in year 1, N=7,739		
EITC effect	103.62	-154.66*
	(131.82)	(85.57)
Add year × children interactions		
EITC effect	...	-149.41*
		(85.04)

See notes to Table 3 for details.

Table 5: Effects of Earned Income Tax Credit on Probability of Transitions to Above Poverty-Level Earnings and on Changes in Income-to-Needs for Families with Initial Earnings Below Poverty Level, Families with Children

	Changes in credit rate, year 1 to year 2	
	Federal	State
	(1)	(2)
I. Transitions to Above Poverty-Level Earnings		
A. P{Non-poor in year 2|Poor in year 1 and no adult worker in year 1}, N=4,834		
EITC effect	-.023	.098**
	(.056)	(.025)
Add year × children interactions		
EITC effect	...	.098**
		(.025)
B. P{Non-poor in year 2|Poor in year 1 and at least one adult worker in year 1}, N=7,739		
EITC effect	-.036	.017
	(.059)	(.035)
Add year × children interactions		
EITC effect	...	.014
		(.036)
II. Changes in Income-to-Needs		
C. Change in income-to-needs|Poor in year 1 and no adult worker in year 1, N=4,834		
EITC effect	.014	.084**
	(.096)	(.038)
Add year × children interactions		
EITC effect	...	.088**
		(.039)
D. Change in income-to-needs|Poor in year 1 and at least one adult worker in year 1, N=7,739		
EITC effect	-.103	.052
	(.070)	(.038)
Add year × children interactions		
EITC effect	...	.051
		(.039)
III. Increases in Income-to-Needs		
E. P{Change in income-to-needs|Poor in year 1 and no adult worker in year 1}, N=4,834		
EITC effect	.074	.098
	(.049)	(.070)
Add year × children interactions		
EITC effect	...	.098
		(.068)
F. P{Change in income-to-needs|Poor in year 1 and at least one adult worker in year 1}, N=7,739		
EITC effect	-.050	-.008
	(.058)	(.046)
Add year × children interactions		
EITC effect	...	-.005
		(.047)

See notes to Table 3 for details.

Table 6: Robustness Analyses for Linear Probability Estimates of Effects of Earned Income Tax Credit on Probability of Transitions to Specified Ratios of Earned Income-to-Needs Ratios, Families with Children

*P{Income/needs > **threshold** in year 2|income/needs < **threshold** in year 1}*

	Threshold = .3		**Threshold = .5**		**Threshold = .7**		**Threshold = .9**	
	Federal	State	Federal	State	Federal	State	Federal	State
A. All families								
EITC effect	-.026	.026	-.042	.021	-.029	.017	-.049	.029
	(.055)	(.037)	(.057)	(.039)	(.051)	(.032)	(.043)	(.028)
Add year × children interactions								
EITC effect	...	.032	...	.022	...	.016	...	.026
		(.039)		(.040)		(.032)		(.028)
N	6,797		8,200		9,724		11,573	
B. Families with no worker in year 1								
EITC effect	-.003	.090**	-.016	.069**	-.030	.079**	-.025	.084**
	(.059)	(.038)	(.065)	(.034)	(.068)	(.038)	(.065)	(.029)
Add year × children interactions								
EITC effect	...	.093**	...	.070**	...	.081**	...	.084**
		(.038)		(.034)		(.038)		(.029)
N	4,629		4,712		4,762		4,809	
C. Families with at least one worker in year 1								
EITC effect	.045	-.103	.019	-.066	.018	-.054	-.033	-.038
	(.109)	(.061)	(.098)	(.072)	(.069)	(.039)	(.066)	(.038)
Add year × children interactions								
EITC effect	...	-.098	...	-.068	...	-.053	...	-.042
		(.063)		(.072)		(.041)		(.038)
N	2,168		3,488		4,962		6,764	

See notes to Table 3 for details. "Federal" and "state" refer to changes in phase-in rates.

Table 7: Robustness Analyses for Linear Probability Estimates of Effects of Earned Income Tax Credit on Probability of Transitions from Below to Above Income-to-Needs Thresholds, Families with Children in Poverty in Year 1

	Transition to above poverty-level earnings		**Change in income-to-needs**	
	Federal (1)	State (2)	Federal (3)	State (4)
Measurement of credit rate:				
A. Excluding states with non-refundable credits, N=12,122				
EITC effect	-.055	.038	-.060	.067**
	(.040)	(.032)	(.055)	(.034)
Add year × children interactions				
EITC effect	...	.038	...	.070**
		(.033)		(.034)
B. Setting credit rate in non-refundable states to one-fifth of statutory rate, N=12,573				
EITC effect	-.053	.048	-.067	.070**
	(.039)	(.034)	(.055)	(.035)
Add year × children interactions				
EITC effect	...	.047	...	.072**
		(.033)		(.035)
Control variables:				
C. Omit change in unemployment rate, N=12,573				
EITC effect	-.053	.063**	-.067	.068**
	(.039)	(.030)	(.055)	(.028)
Add year × children interactions				
EITC effect	...	.061**	...	.068**
		(.031)		(.028)
D. Add controls for changes in real AFDC benefits for family of three, and in federal waivers, N=12,573				
EITC effect	-.053	.063**	-.066	.069**
	(.039)	(.029)	(.055)	(.027)
Add year × children interactions				
EITC effect	...	.061**	...	.069**
		(.029)		(.027)
Alternative EITC parameterization:				
E. Using maximum credit instead of phase-in rate, N=12,573				
EITC effect	-.016	.038*	-.019	.038**
	(.013)	(.020)	(.019)	(.018)
Add year × children interactions				
EITC effect	...	.035*	...	.038**
		(.020)		(.018)

	Transition to above poverty-level earnings		**Change in income-to-needs**	
	Federal (5)	State (6)	Federal (7)	State (8)
Alternative treatment/control groups:				
F. States with EITC and bordering states only, N=5,277				
EITC effect	-.109**	.071**	-.181**	.063**
	(.056)	(.031)	(.084)	(.030)
Add year × children interactions				
EITC effect	...	.066**	...	.058*
		(.032)		(.031)
G. Omitting Wisconsin families with three or more children, N=12,539				
EITC effect	-.054	.069**	-.068	.076**
	(.039)	(.034)	(.055)	(.030)
Add year × children interactions				
EITC effect	...	.066**	...	.076**
		(.034)		(.030)
H. Omitting Maryland observations, N=12,453				
EITC effect	-.051	.051	-.063	.076**
	(.039)	(.032)	(.055)	(.036)
Add year × children interactions				
EITC effect	...	.050	...	.077**
		(.033)		(.036)
"Non-experiments":				
I. Original coding of credit, families with no children, EITC parameters for families with three children (prior to 1994), N=12,648				
EITC effect	-.010	.001	.012	-.012
	(.013)	(.026)	(.022)	(.040)
Add year × children interactions				
EITC effect	...	.001	...	-.012
		(.026)		(.040)
J. Families with income-to-needs between three and 10 in each year, N=54,427				
EITC effect	...	...	.018	-.015
			(.038)	(.051)
Add year × children interactions				
EITC effect	...	...	...	-.013
				(.052)

See notes to Tables 3 and 4 for details. The experiment in Panel B is based on the "tradeoff" between refundable and non-refundable rates established in Maryland in 1998, when taxpayers were offered an option of a 50 percent non-refundable supplement, or a 10 percent refundable supplement. In Panel D the waivers variable is defined based on the fraction of the year in which waivers from federal rules for state experimentation were in effect; the change is included. In Panel E, we use the average change in the federal maximum credit ($141.79, in real terms) in years when it changed, just as we treat the credit rate in the preceding tables. In Panel I, we restrict attention to 1993 and earlier because in that period there was no EITC for families with no children. In Panel J, the estimated coefficient for the federal experiment suggested a stronger positive effect when families without children were excluded, but the results for the state experiment were very similar.

Table 8: Effects of Earned Income Tax Credit on Probability of Transitions to Above or Below Poverty-Level Earnings

I. Transitions to Above Poverty-Level Earnings

	Changes in credit rate, year 1 to year 2		Change in real minimum wage		
	Federal	State	Current	Lagged	P-value, sum
	(1)	(2)	(3)	(4)	(5)
A. P{Non-poor in year 2|Poor in year 1}, N=26,707					
EITC effect	-.022	.042	.003	.006	.184
	(.016)	(.031)	(.004)	(.006)	
<u>Families with children only (N=12,573)</u>					
EITC effect	-.054	.065**	.015**	.013*	.004
	(.039)	(.029)	(.006)	(.008)	
<u>Add year × children interactions, families with children only</u>					
EITC effect	...	.063**	.014**	.013*	.004
		(.029)	(.006)	(.008)	
B. P{Non-poor in year 2|Poor in year 1 and no adult worker in year 1}, N=12,795					
EITC effect	-.020	.061**	.007**	.002	.105
	(.014)	(.031)	(.003)	(.004)	
<u>Families with children only (N=4,834)</u>					
EITC effect	-.024	.099**	.007	.014**	.002
	(.056)	(.025)	(.004)	(.004)	
<u>Add year × children interactions, families with children only</u>					
EITC effect	...	.099**	.006	.013**	.003
		(.025)	(.004)	(.004)	
C. P{Non-poor in year 2|Poor in year 1 and at least one adult worker in year 1}, N=13,912					
EITC effect	-.021	.017	.001	.006	.491
	(.024)	(.032)	(.006)	(.010)	
<u>Families with children only (N=7,739)</u>					
EITC effect	-.037	.019	.022**	.014	.007
	(.059)	(.035)	(.008)	(.011)	
<u>Add year × children interactions, families with children only</u>					
EITC effect	...	.017	.021**	.014	.008
		(.036)	(.009)	(.011)	

Table 8 (continued)

II. Changes in Income-to-Needs, Families Initially Below Poverty-Level Earnings

	Changes in credit rate, year 1 to year 2		Change in real minimum wage		
	Federal	State	Current	Lagged	P-value, sum
	(1)	(2)	(3)	(4)	(5)
D. Change in income-to-needs|Poor in year 1, N=26,707					
EITC effect	-.019	.054*	.006	.007	.302
	(.022)	(.032)	(.007)	(.007)	
Families with children only (N=12,573)					
EITC effect	-.067	.070**	.020**	.019	.011
	(.055)	(.027)	(.009)	(.013)	
Add year × children interactions, families with children only					
EITC effect	...	.070**	.020**	.018	.012
		(.027)	(.009)	(.013)	
E. Change in income-to-needs|Poor in year 1 and no adult worker in year 1, N=12,795					
EITC effect	-.032	.048	.000	-.001	.954
	(.027)	(.043)	(.007)	(.009)	
Families with children only (N=4,834)					
EITC effect	.013	.085**	-.006	.017*	.467
	(.096)	(.038)	(.010)	(.010)	
Add year × children interactions, families with children only					
EITC effect	...	.089**	-.005	.017*	.483
		(.039)	(.010)	(.010)	
F. Change in income-to-needs|Poor in year 1 and at least one adult worker in year 1, N=13,912					
EITC effect	-.005	.054	.012	.011	.201
	(.036)	(.036)	(.011)	(.018)	
Families with children only (N=7,739)					
EITC effect	-.104	.055	.039**	.019	.003
	(.070)	(.038)	(.012)	(.018)	
Add year × children interactions, families with children only					
EITC effect	...	.055	.037**	.020	.004
		(.039)	(.012)	(.018)	

See notes to Table 3. Estimated effects are transformed to reflect the impact of the "average" increase in the federal credit rate (.040) or the real minimum wage in 1982-1984 dollars (.204); see Table 2. The p-values reported in column (5) are for the hypothesis that the minimum wage coefficients add to zero.

Appendix Table A1: Examination of Levels vs. Change Specifications

	Federal, contemporaneous (1)	Federal, lagged (2)	State, contemporaneous (3)	State, lagged (4)	P-value for restriction(s): Federal cont.=federal lagged, state cont.=state lagged (5)	P-value for restriction(s): State cont.= state lagged (6)
Table 3, Panel I:						
P{Non-poor in year 2|Poor in year 1}, N=12,573						
EITC effect	-.149	.220	.142**	-.218**	.427	.211
	(.116)	(.310)	(.068)	(.068)		
Table 3, Panel II:						
Change in income-to-needs|Poor in year 1, N=12,573						
EITC effect	-.239	.487	.162**	-.164**	.758	.970
	(.166)	(.454)	(.066)	(.072)		
Table 3, Panel III:						
P{Poor in year 2|Near-poor in year 1}, N=5,559						
EITC effect	.046	.126	-.035	.052	.895	.835
	(.199)	(.574)	(.116)	(.133)		

See notes to Table 3. Estimates are reported for families with children only, corresponding to the second specification in each sub-panel of Table 3. The restricted specifications are the second specifications in each sub-panel of Table 3. In levels specifications, estimated effects are transformed to reflect the impact of the "average" level (.148) of the federal credit rate; see Table 2. A '**' superscript indicates statistical significance at the five-percent level in a two-sided test, and a '*' superscript at the ten-percent level.