

**USING THE EITC TO INCREASE FAMILY EARNINGS:
NEW EVIDENCE AND A COMPARISON WITH 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 minimum wage, welfare, and the Earned Income Tax Credit (EITC). The minimum wage may well be the earliest, and most studied, of these policies. But at the same time that welfare reform is attempting to reduce the dependency that has plagued many previous income-support programs, the EITC has increasingly gained support among economists as a viable alternative, and perhaps as a result, interest in expanding this program has increased as well.

Indeed, the stated intent of both the minimum wage and the EITC is 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. In contrast, 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, as is the considerable amount of research on the effects of welfare payments on labor supply. 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 rather than on the influence on incomes among families participating in the program.

Moreover, those few studies that do examine the effects of income-support programs 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 such programs on labor market behavior. However, because much of the literature studying the behavioral influences of labor market and welfare policy 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 anti-poverty policies and earned pre-tax income using panel data on families likely to be affected by such programs. More specifically, we extend our previous research on the effects of minimum wages on the distribution of total family incomes (Neumark, et al., 1998) to the effects of the minimum wage and EITC on earned income. Using data on federal and state changes in both the minimum wage and in parameters of the EITC program, we estimate the effects of each policy on earned income and on transitions of families across different parts of the earned income-to-needs distribution.

Although very much a reduced-form approach, the resulting parametric estimates of the effects of the two 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 minimum wage or the EITC 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 minimum wage or the EITC) that might bias

¹ Blank, Card, and Robins (1999) suggest that policy makers also attempt to use such programs to encourage 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.

estimates based on parameter values from studies focusing solely on one or the other policies. Third, by using both federal and state variation in policy as indicators, we are able to use several types of variation in the data to help identify the specific effects on income.

Our results indicate that there are clear differences in how minimum wage policy and the EITC affect earnings. Both policies appear to have positive income effects on poor families with children. However, the EITC effects seem larger given the average policy changes of the past 15 years, and the EITC seems especially effective in raising the earned income of the poorest families in our sample—those with an initial ratio of earned income-to-needs below the .4 to .7 range. Complementary evidence on employment and hours effects indicates that these benefits come about mainly by inducing labor market entry for poor families without any workers in the year prior to the change in the EITC. In contrast, the beneficial minimum wage effects show through primarily for families who are just below the poverty line, presumably because of the higher wage received by a member of the family already in the work force. In addition, the minimum wage appears to have a negative effect on the earned income of families without children, and increases the rate of transition into poverty among this group. 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 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 because the minimum wage does, in a sense, entail transfers from many employers and perhaps most consumers to low-wage workers, albeit not through the tax system. We choose to focus on pre-tax earnings for two reasons. First, it permits a comparison between our estimates for earnings and 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 and to work harder. Given the paucity of actual empirical studies of this assumption, our aim is to provide some additional evidence with which to assess the accuracy of these claims.

Nevertheless, we should stress that 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 minimum wage and the EITC that the latter is the more effective policy for fighting poverty.²

Previous Research

As noted above, previous research on the economic effects of anti-poverty programs has typically been limited in two ways: first, by estimating particular behavioral or labor market parameters that do not necessarily directly address the policy goals; and second, by considering a given policy in isolation. For example, there is a voluminous literature examining the effects of the minimum wage on employment outcomes, but relatively little emphasis on the link between

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).

wage floors and the income distribution or on the relationship between the minimum wage and other income-support policies. Moreover, of those studies that have addressed the link between minimum wages and income, many have relied on simulation methods that make assumptions about employment elasticities and other relevant parameters. In particular, Gramlich (1976) and Horrigan and Mincy (1993) focus on identifying those workers most likely to be affected by minimum wage increases, while Burkhauser, et al. (1996) use parametric assumptions to conclude that the minimum wage is a very inefficient tool for raising the incomes of poor families.

A few recent studies have, however, considered the empirical link between the minimum wage and poverty. Addison and Blackburn (1999), for example, estimate the effects of minimum wages on state poverty rates and find poverty-reducing effects for some narrowly-defined subgroups (older junior high school dropouts and to a lesser extent teenagers), and only in the 1990s, but not the 1980s. Thus, we do not view this evidence as particularly strong, and certainly not generalizable to the poor population as a whole. Card and Krueger (1995) examine the effect of the 1990-91 increases in the minimum wage on earnings at specific centiles of the income distribution and find a somewhat positive effect. In two earlier papers, we conducted more comprehensive analyses that looked at the effects of changes in the minimum wage on changes in the family income distribution (Neumark and Wascher, 1997; Neumark, et al., 1998) using both parametric and nonparametric approaches. The results in these papers indicated that minimum wages tend to increase the net number of poor families because the number of families pushed into poor or near-poor categories (presumably associated with employment losses) tends to exceed the number lifted out of poverty by the higher minimum.

Research on the EITC, by contrast, is quite sparse as compared with that on the minimum wage, 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 with the minimum wage literature, 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 (1998 and 1999) 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.

Finally, comparisons of minimum wage policy and the EITC have 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

employment effects of minimum wages and labor supply effects of the EITC. 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 (total, earned, etc.), 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 prevailing minimum wage and key parameters for the EITC program 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 2 or more children; the maximum credit ranged from 0 to more than \$2,500 over the same period. In addition, there are currently ten 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

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 that 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 adjust this weight 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.

than 5 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 the 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 minimum wage policy on income-to-needs and to an analysis of the effects of the EITC. 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 particular, we consider linear probability models of the form:

$$P\{X_2 < (I/N)_2 < Y_2 \mid X_1 < (I/N)_1 < Y_1\}_{it} = \alpha + P_{it}\beta + \gamma U_{it} + X_{it}\pi + \delta_i + \lambda_t + \varepsilon_{it} \quad (1)$$

where P_{it} is the policy under consideration (e.g., a change in the real minimum wage, or a change in an EITC parameter),⁷ U_{it} is the change in the adult male unemployment rate, X_{it} is a vector of other control variables, δ_i is a vector of state indicators, and λ_t is a vector of year indicators. Including the year effects controls for other aggregate influences or trends—including, but not limited to, other policy changes—that are not reflected in unemployment rates (or other control variables discussed below), but that affect transitions in the income-to-needs distribution. The fixed state effects allow for state-specific differences in transition probabilities or the other outcomes we study, attributable to persistent differences in policies, markets, etc., not captured in the control variables. 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 EITC's is identified from within-state variation in EITC parameters.⁸ We also include controls for the number of children under 18, which, in some specifications, we interact 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.

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 1 to a ratio in the X_2 to Y_2 range

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

⁷ P_{it} is typically a vector, including, for example, federal and state EITC parameters or current and lagged values.

⁸ In the case of the baseline specifications for the effects of the EITC, we find that the estimated standard errors of the state EITC variables, which for reasons discussed below we regard as our best experiment, are actually lowered by including the fixed state or year effects. 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.

in year 2. Thus, in this form the model estimates the transition probabilities between pre-specified parts of the income-to-needs distribution; we chose poor to nonpoor, nonpoor to poor, and subsets of each of these broader categories as relevant ranges for this analysis. In addition, in some specifications we examine the direct influence of the policy variables on employment transitions or on changes in hours worked. In these specifications, we often restrict the sample to a particular income-to-needs category in order to estimate different effects of the EITC at different points in the income-to-needs distribution.

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 \$30,000 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 1, and another 8 percent are between 1 and 1.5. These poor and near-poor families are the intended targets of both the minimum wage and the EITC, 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 \$13,000 and the average earned income-to-needs ratio is only slightly above 1. In addition, nearly half of the families in this sample have an earned income-to-needs ratio below 1 and about 60 percent are below 1.5. About 55 percent have children less than 18, which potentially makes them 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 by families not eligible for the EITC, such as those without children. We view such a control group as potentially problematic because the labor supply responses of families eligible for the EITC may have indirect effects on the hours, wages, and earnings of workers and families that are not eligible for the EITC. 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.¹⁰ 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 each year, with about half raising their earned income-to-needs ratio above 1.5. Similarly, about 13 percent of families with an initial earned income-to-needs ratio between 1 and 3 fall into poverty in the following year. These numbers are suggestive of a sizable amount of idiosyncratic changes in family income, 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 an average increase of 3.5 percentage points per year. The average

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 A particular problem with using families with no children or different numbers of children as the control group is that child bearing decisions may be endogenous.

state supplement for those states with an EITC program was 4.8 percent, with an average increase of 1.5 percentage points per year. The average real minimum wage was a bit less than \$3 over our sample period, with an average increase of \$.20 per year.

The Earned Income Tax Credit: Effects on Earned Income

Table 3 presents estimates of the effect of the EITC on earned income from the basic specification shown in equation (1). Each row of the table reports estimates from a single specification, with columns (1) to (3) reporting the estimated effects of changes in the federal credit rate, and columns (4) to (6) reporting estimated effects of changes in the state supplements. For the federal program, this 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;¹¹ this variable also varies across years and with the number of dependent children in the family, but also varies across states. In most specifications, we include both contemporaneous and lagged values of the EITC parameters to allow possible income effects to occur over time.

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 1 has an earned income level above the poverty line in year 2. As can be seen in the top row of the panel, the effect of the federal credit rate is negative and statistically significant (both the individual coefficients and the sum), suggesting that increases in the EITC credit rate are associated with reductions in the rate of transition out of poverty. One interpretation of this result 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. 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), however, this equation may be subject to considerable 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. As indicated in the second row, the coefficient is still negative for this subsample, although it is smaller and no longer statistically significant.¹²

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.¹³ The estimated coefficients of

¹¹ 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 $r_c = r_f(1+r_s)$. 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 $r_s \cdot (r_f Y)$ (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.

¹² As noted in the table, once we restrict the sample to families with children, the estimated coefficient of the lagged change in the federal rate is extremely imprecise, and excluding it had no qualitative effect on the contemporaneous coefficient estimate.

¹³ This is, of course, subject to some of the same concerns as the federal credit, in that there may be state-specific policies coming in to 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 a study of 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

the state EITC variables are shown in columns (4)-(6). 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 the contemporaneous coefficient has a p-value of .03. 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. The effect of the inclusion of these interaction terms is to identify the EITC effect solely off of the variation in state EITC supplements. Using this specification, the results are similar to those in the previous row. In particular, the beneficial effects of the EITC on family earnings again show up quite clearly; the summed effect is positive and the contemporaneous effect is statistically significant.¹⁴ Moreover, the size of the coefficient 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 1-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 .07. Since the mean of this transition rate is .21 (Table 2), this is an increase of approximately one-third.

In the bottom panel, 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 general, there is little evidence that the EITC has any effect on such transitions. Using the federal credit, the coefficient estimates are small and have varying signs. For the specifications using the state credit rate (at least once we restrict the sample to families with children), the coefficient estimates are in the direction of suggesting that the EITC reduces the likelihood that families fall into poverty, although the estimated effects are not statistically significant.

We delve into the EITC effects further in Table 4, using the same set of specifications, but focusing more narrowly on where in the earned income distribution the transitions occur. In Panel I, we again look at transitions out of poverty. Estimates of the effects of the EITC on transitions from below the poverty line to an earned income-to-needs ratio of between 1 and 1.5 (near-poor) are shown in subpanel A, while EITC effects on transition probabilities from below the poverty line to an income-to-needs ratio of 1.5 to 3 are shown in subpanel B. For the specifications using the federal credit, the negative effects of the EITC on the rate at which families leave poverty are found in both of the transition probabilities shown here. The effect is considerably stronger for transitions into the 1.5 to 3 income-to-needs category, but is nearly

few states have a separate EITC. This raises the possibility that the estimates are being driven by one or two states, an issue we come back to later in the paper.

¹⁴ As noted in the table, excluding the lagged variable had essentially no effect on the estimate or standard error of the contemporaneous variable. In other specifications reported below, the lagged effect is significant but the contemporaneous effect is not, and again dropping the contemporaneous variable has essentially no effect on the estimates of the lagged effects. We therefore show both coefficients for completeness, along with the p-value for the summed contemporaneous and lagged effects, but focus on the individual coefficients (whether contemporaneous or lagged) in describing the results.

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.

significant for the poor to near-poor transition as well. As in Table 3, however, the results weaken considerably when the sample is restricted to families with children, raising concerns about potential specification errors.

For the specifications using the state credit, the EITC initially appears to push poor families into the 1.5 to 3 income-to-needs category. However, this contemporaneous effect is offset by a negative lagged effect. One possible explanation of this pattern is that an initial positive labor supply response by one individual in the household is subsequently offset by a negative labor supply response, perhaps by another member of the household. Alternatively, there may be positive labor supply effects in some families, but negative labor supply effects in others, with somewhat different timing.¹⁶ Thus, once lagged effects are taken into account as well, there is little overall EITC effect on transitions from below the poverty line to the 1.5 to 3 income-to-needs category. In contrast, increases in the generosity of the state EITC credit rates are associated with a higher probability of a transition from poverty to the near-poor (1 to 1.5) income-to-needs category. The effect appears to occur with a lag, with the coefficient on the lagged EITC variable strongly significant.

Panel II of the table examines transitions into poverty to see whether the apparent absence of an effect of the EITC on these transitions masks effects in particular parts of the earned income distribution. In particular, this panel focuses on the transition rate into poverty among families with earned income initially only slightly above the poverty line. However, the results in this panel are consistent with what was reported in Table 3. For the federal credit, the estimated coefficients are small and not particularly informative. For the state credit, the coefficient estimates are negative, in the direction of suggesting that the EITC reduces transitions into poverty for near-poor families, but the effects are never statistically significant.

Thus, the evidence from the two experiments reported in Table 3 and Table 4 points in quite different directions. For the federal credit rate, the estimated coefficients suggest that the EITC reduces earned income among poor families, a result that would be consistent with the program discouraging work. However, this result hinges on differences between families with and without children; once we restrict the analysis to families with children, the effects of the federal EITC are not statistically significant. In contrast, the coefficients on the state credit rate suggest strongly that the EITC raises earned income among poor families with children. That is, families eligible for the EITC are more likely to see their earnings rise above the poverty line when the credit increases in generosity. In both cases, the evidence points to very little effect on families with earned income initially above the poverty line, suggesting that the EITC is successful in targeting poor families. Moreover, in both cases, the estimated coefficients understate the overall income effects of the EITC. That is, the positive effects on total resources would be more pronounced if one considered the additional income received from the credit itself, which is not taken into account in these estimates.¹⁷

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 raises important questions about what we have found. To attempt to clarify the

¹⁶ Evidence reported in Tables 5 and 6 below is consistent with this, as we find that increases in state EITC's are associated with contemporaneous increases in hours worked for families that are initially poor and have no workers (i.e., adding a worker), but labor supply declines for families that initially poor but have at least one worker (which, although insignificant, are cumulatively larger with a one-year lag).

¹⁷ Information on the actual credit received is not available in the CPS.

results for income, we performed a complementary analysis of the effects of the EITC on employment and hours worked for the families in our sample. In general, the specifications are similar to those in earlier tables, with the exception that the dependent variable is either the probability that a family added a worker in the second year they were in the sample, or the change in total hours supplied by family members between the first and second years.

In Table 5, we estimate the effects of the EITC on the probability that a family adds a worker between year 1 and year 2. In panel A, we limit the sample to those families that initially did not have any workers. For the entire sample, there is little evidence of an EITC effect for either the state or federal credit rate variable. Once the sample is limited to the more relevant subsample of families with children, however, the effects of the EITC begin to show through more clearly. In particular, the federal EITC is estimated to raise the probability of adding a worker in year 2, and the coefficient is statistically significant at the 10-percent level. The specifications using the state credit also show a positive effect on employment, although it is not estimated very precisely.

The effects on employment become more pronounced when the focus is on adult workers (Panel B), a result that is not particularly surprising given that entry of teens into the workforce is likely to be much more random and unrelated to policy changes. In this case, the coefficient on both the federal and state EITC variables is positive and statistically significant at the 5- or 10-percent level, and the coefficients are of the same magnitude. 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 were spurious, possibly owing to correlations between the EITC and other federal programs that affected the income 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 Panel C, we limit the sample to families that had at least one worker in year 1 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 Table 6, we examine the effects of the EITC on total hours worked by family members, once again splitting the sample into those families with no workers in year 1 and those families with at least one worker initially. As can be seen in the top two panels, for the sample as a whole, the federal EITC is estimated to have a negative effect on hours worked by families with initially no workers; in addition, there is little estimated effect on hours for the subsample of families with children. This results seems surprising given the positive employment effects reported in Table 5, and again points to spurious and inconsistent results using the federal EITC experiment. For families with a worker in year 1, there is little evidence of any EITC effect using the federal credit rate as the explanatory variable.

In contrast, the estimated effects of the state credit rate show strong positive effects on annual hours worked in families without a worker in the first year. 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. Turning to the lower panels, the effect of changes in the state EITC credit rate is to reduce hours worked for families that initially had at least one worker in year 1. These results are the clearest indication we have of the negative effect on labor supply from the EITC found by some previous researchers (e.g., Eissa and Hoynes, 1998).

In Table 7, we look for effects of the EITC on labor supply for families with earnings initially above the poverty line. In our sample period, the income level at which families are first entitled to the maximum EITC benefit always occurs at an income level that is below the poverty line for families with one or more children. Thus, for these families theory predicts that a higher EITC will unambiguously reduce labor supply. Of course it is not the credit rate that is relevant to these families, but most importantly, probably, 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.¹⁸

As in the case of the regressions on earned income-to-needs, for this sample there is no statistically significant evidence of EITC effects for near-poor families, while for those with earned income-to-needs ratios of between 1.5 and 3, there are negative and statistically significant contemporaneous effects for total hours, and opposite-signed, insignificant lagged effects. Overall, though, this evidence suggests that the EITC is not very relevant for families above the poverty line.

The results from Tables 5, 6, and 7 indicate more clearly the sources of the positive and negative effects from the EITC. Increases in earnings mainly come about by inducing workers in families without anyone in the labor market to enter the labor force in order to take advantage of the credit. In contrast, poor families that already have someone 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.

In Table 8, we attempt to match up our estimated effects of the EITC on income with the estimates of labor supply effects reported in Tables 5 and 6. In particular, given that the effects on employment and hours worked were most pronounced for families with no workers in year 1, we would expect to see the largest effects on earned income for these families as well. In contrast, the effects on earned income for families with a worker in year 1 should be minimal, as the additional income received from the EITC is offset by a reduction in hours worked. The results 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 1 becomes non-poor in year 2. The effect is quite strong in the subsamples that exclude families without children and is highly significant. In contrast, there is little effect from the EITC on the transition probabilities for poor families with a worker in the first year, consistent with the negative labor supply effect found for this subset of families.

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. These tables report only specifications that include the year \times children interactions, and only use information on state EITC's.

¹⁸ 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 Table 9, 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 1. First, given our findings that the EITC effects are largest for families without a worker (and thus little if any earned income) in year 1, 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 1 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 might nonetheless escape poverty.

The results in the table are suggestive of some sensitivity to the choice of threshold. In particular, the EITC effects are strongest when the threshold is set between .4 to .7, 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 of the transition rate out of poverty, which is replicated here in the final row of Table 9, understates the effects of the EITC on earnings of low-income families. Rather, it appears that the largest effect of the EITC is for families with an income-to-needs ratio below .4, and that the EITC may be effective in raising the ratio to above .7. Of course, some families do appear to be helped even more by the EITC, as is indicated by the positive contemporaneous coefficient when the original poverty threshold is used.

In Table 10, we continue to report results for selected below-poverty thresholds in the estimation of the effects of the EITC on transition probabilities, but also introduce some 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.¹⁹ 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.

When the states with non-refundable credits are excluded from the sample, the overall estimated effects of the EITC on transitions out of poverty for this subsample are a bit weaker than the baseline results, rather than stronger, although they generally tell the same story. In particular, the EITC appears to have a positive effect on the probability that a family's earned income rises above the poverty line. For the other thresholds, there are some instances in which the effects are larger (notably with a threshold of .6), but just as many others with a smaller effect. In general, the results do not seem markedly different dropping these states. The results are a bit more favorable to this explanation when the credit rate for these states is set to one-fifth of the statutory rate, but the differences are not striking. In particular, the summed coefficients for all three of the specifications using a lower threshold are more positive than in Table 9, although the differences are often not large.

In Panel C, we add controls for real changes in AFDC benefits for a family of three and 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.

¹⁹For our sample period, excluding families only eligible for non-refundable credits means dropping all observations for Maryland, Iowa, and Rhode Island.

In Panel D, 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. Again, the results are little changed using this alternative sample.

In Panel E, we omit the largest changes in the state credit rate from the sample to test the sensitivity of the estimated EITC effects to these policy outliers. This involves dropping all observations that are influenced by the introduction of the EITC in Maryland in 1987 and in Wisconsin in 1989 (for families with three or more children). Depending on the choice of threshold level, the results change somewhat. In particular, the estimated effects of the EITC are larger and more precisely estimated when the threshold is set to .4, but become somewhat weaker for higher threshold levels. These findings suggest some sensitivity of the results to these rather extreme “treatment” observations.²⁰

Finally, in Panel F, we examine the possibility that the results we report for the EITC in earlier tables are spurious, by applying our methodology to families that have earned income-to-needs ratios between 0 and 3, 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 9 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. When the threshold is set to .4, the estimated effects are positive and statistically significant, although much smaller than in Table 9. However, the EITC coefficients are of the opposite sign for each of the other threshold specifications. As we should not expect to see EITC effects for this particular sample, these results add credence to our interpretation of the results in Table 9 as reflecting causal effects of the EITC.

The EITC versus the minimum wage

In Table 11, we report estimates that include both the EITC parameters and the minimum wage in the specification to see if conditioning on minimum wage changes alters the EITC effect 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 variable to measure the effects of average policy increases in

²⁰ Another way to document this point is to bootstrap the standard errors of the estimates, bootstrapping on the state-year clusters, rather than the individual observations. In a case in which particular clusters are highly influential, the bootstrapped standard errors will be higher, as the estimates vary depending on whether these particular clusters are oversampled or undersampled. When we did this (based on 1000 repetitions for some selected specifications), we found that the bootstrapped standard errors on the EITC coefficient estimates were higher by about 25-35 percent. The consequences of this for our statistical inferences regarding the EITC is that many of the effects significant at the 5-percent level become significant only at the 10-percent level, and some of the latter become insignificant. Of course, one can also view the bootstrapped standard errors as “testing” the asymptotic approximations on which the standard error calculations rely when correcting for heteroscedasticity and clustering.

the federal credit rate over this period. Similarly, we scale the minimum wage variable to measure the effects of average policy real increases in the minimum wage over this period.²¹

In Panel I, the point estimates of the EITC effects are quite close to those in Table 9, and suggest a positive effect of the EITC on the earned income-to-needs ratios.²² In Panel II, we report the effects of the EITC on transitions from above to below particular income-to-needs thresholds. As before, we find no effect of the EITC on the probability that a non-poor family in year 1 becomes poor in year 2, although there is evidence in this panel that the EITC reduces transitions into the lowest part of the earnings distribution, perhaps by reducing quit rates among families with one worker.

However, there are interesting differences between the effects of the minimum wage and those of the EITC. Most strikingly, for families with children, the positive effect of the minimum wage on earnings appears largest for transitions just above and below the poverty line. This is consistent with minimum wages having their largest influence on families that already have someone in the work force. In contrast, the EITC appears to have its strongest effects at the lowest part of the earnings distribution, apparently as a result of inducing labor force participation in families with no workers initially. Moreover, given the average size of EITC changes and minimum wage changes in recent years, the effects of the EITC on income appear to be much larger than are those for the minimum wage.²³

In addition, the effects of the minimum wage on poor families with children differ from the effects on families without children (and thus generally are not eligible for the EITC). Estimates of the impact of the minimum wage on families without children are shown in the right-hand columns of the table. In particular, there is no evidence that the minimum wage raises the earnings of initially poor families; the coefficients in the top panel are close to zero and not statistically significant. Moreover, there is some evidence in the bottom panel that families initially above the poverty line are more likely to fall below poverty following an increase in the wage floor, presumably reflecting an adverse employment effect. These results are not estimated precisely, but they do suggest that there are winners and losers from the minimum wage, and leave open the question as to its overall effects on the earnings distribution.

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

²¹ 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.)

²² Note that we include some results for earned income-to-needs thresholds where we did not find effects of the EITC, to see whether we nonetheless find minimum wage effects at these thresholds.

²³ 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.

poor family sees its earned income rise above the poverty line, and on the probability that an initially non-poor family sees its earned income fall below the poverty line.

The results of our estimation strategy depend critically on the nature of the experiment we perform. In particular, when we use the federal EITC as the explanatory variable and identify 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 use state credit rates as the explanatory variable, thus using state variation to identify the EITC effects, we find evidence that the EITC raises earned income-to-needs ratios. Neither experiment points to any effect of the EITC on families initially above the poverty line, suggesting that the EITC is well targeted.

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 using 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 using the federal credit rate are difficult to interpret in a consistent fashion, which leads us to believe that the experiment is flawed. In particular, we suspect 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 (1999) provide a detailed discussion of 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 are inducing 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 changes over time for families with different numbers of children (i.e., year \times children interactions). 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 EITC's yield a set of predictions 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 force participation among families that initially do not have anyone in the work force. In contrast, we find some evidence that increasing the EITC credit rate reduces hours among poor families that already have someone working, thus offsetting the additional 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). And, as a result, the EITC appears to have its most substantial effect on raising the earnings of the lowest-income families, with the impact largest on those families having no working members in the first year they appear in the sample.

Finally, our results suggest that for the range of policy changes that have been typical in recent history in the U.S., the EITC is much more likely to raise the earnings of poor families than is the minimum wage. Although the minimum wage appears to have some positive effect on the earnings of families with children, this is offset, at least to some degree, by adverse

consequences for families with no children. Moreover, even for families with children, our estimates of the effects of the EITC are much larger than for the minimum wage.

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. Rather, evidence that the EITC also 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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