

Giving Mom a Break: The Impact of Higher EITC Payments on Maternal Health

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October 2011

Abstract

The 1993 expansions of the Earned Income Tax Credit created the first meaningful separation in benefits between families containing two or more children and those with only one child. If income is protective of health, we should see improvements over time in the health for mothers eligible for these higher EITC benefits. Using data from the Behavioral Risk Factors Surveillance Survey, we find improvements in self reported health for affected mothers. Using data from the National Health and Nutrition Examination Survey, we find reductions in the probability of having risky levels of biomarkers for these same women.

The authors thank the Russell Sage Foundation for financial support, Ron Mariutto and Andrew Kenney for excellent research assistance, and seminar participants at Dartmouth College, the University of Illinois-Chicago, and the University of Oregon for their helpful comments.

I. Introduction

The Earned Income Tax Credit (EITC) is a refundable tax credit that provides cash payments to poor families and individuals with the most generous payments for families with children. In 2008, the program distributed \$49 billion in payments to 24 million people,¹ roughly the same level of spending for Temporary Assistance for Needy Families (TANF) and the Supplemental Nutritional Assistance Program (SNAP) programs combined.² Families earning the maximum credit could see their adjusted gross income increased by as much as 15 percent.

The 1993 expansions of the EITC created the first meaningful separation in benefit levels for families based on the number of children, with families containing two or more children receiving substantially more in payments. We exploit this change to examine the impact of income on health for low income mothers. If income is protective of health, we should see improvements over time in the health for mothers eligible for the EITC with two or more children compared to those with only one child. This empirical methodology has been used by Hotz and Scholz (2006) and Adireksombat (2010) in their analysis of the 1993 expansions on employment.

Using data from the Behavioral Risk Factor Surveillance System (BRFSS), we replicate earlier findings that EITC benefits expansion increased the labor supply of mothers. We also find evidence that the higher EITC payments increased self-reported health and reduced the number of poor mental health days reported by eligible women with two children compared to similar women with only one child. We also use data from the National Health and Nutrition Examination Survey (NHANES) to estimate the effect of the EITC expansion on health indicators that are measured by blood and medical tests. We utilize data on eight biomarkers that indicate whether the respondent has problems associated with cardiovascular diseases, metabolic disorders, and inflammation. The expansion of the EITC is associated with a large and statistically significant decrease in the counts of risky biomarkers associated with higher stress levels and are predictive of a wide range of conditions.

The results and methods in this paper contribute to two distinct literatures. The first is the literature examining the economic consequences of the EITC. As we outline below, analysts have examined the impact of the EITC on outcomes as diverse as labor supply, fertility, marriage, living arrangements, poverty, educational attainment, and spending patterns. Little

¹ <http://www.eitc.irs.gov/central/press/>

² SNAP payments total \$34.6 billion in calendar year 2008 (<http://www.fns.usda.gov/FSP/faqs.htm>) while assistance payments under TANF totaled \$14.5 billion in fiscal year 2008 (<http://www.acf.hhs.gov/programs/ofs/data/2008/overview.html>).

attention has been paid to the effect of the EITC on health, despite the fact that improving the living conditions of low-income families was an explicit objective of the EITC.³

Research on the health effects of government programs has traditionally centered on those programs that directly affect the provision of medical services such as Medicaid (Currie and Gruber, 1996a and 1996b) and Medicare (Card et al., 2009), policies that influence the ability to obtain health insurance coverage or care (Bitler, Gelbach, and Hoynes, 2005), or increase access to food and nutrition through the Women, Infants and Children program (Hoynes, Page, and Stevens, 2009). Adler and Newman (2002, p. 63) noted that there is “...little research in the United States examining how redistributive policies or other income distributions changes affect health outcomes.” Only recently have authors begun to more carefully consider the role of income support and transfer programs on health. Milligan and Stabile (forthcoming) examined the impacts of the Canada Child Tax Benefit and the National Child Benefit Program on a variety of outcomes for the child and mother. The authors found that higher tax benefits generated statistically significant higher test scores, height and self reported health among children. They also found that higher tax benefits reduced mother’s depression score but there is no impact on the mother’s self-reported health. In contrast, we find improvements in the self-reported health of affected mothers. Furthermore, Milligan and Stabile, were unable to examine the effect of these Canadian programs on the medically measured biomarkers utilized in our analysis.

A group of authors have considered whether US government support programs impact the health of newborns. Baker (2008) examined the effect of the 1993 EITC expansions on birth outcomes using a methodology similar to that in this paper. He found that the EITC expansions generated a small but statistically significant increase in birth weight among mothers with two or more children. The channel for this effect was not a change in the utilization of prenatal medical services. Similarly, Strully et al. (2010) found that increases in state-level EITCs increased birth weights and reduced maternal smoking. Finally, Almond, Hoynes, and Schanzenbach (2011) found that higher incomes from the food stamp program led to improved birth outcomes, with the largest increases accruing to African-Americans.

This current work also advances the understanding of the link between income and health. A large literature with contributions from a variety of disciplines has established that

³ Then-First Lady Hillary Clinton commented on the program, “a small investment in working parents [through the EITC], even just several hundred dollars a year, means stronger families, healthier children, more dependable employees, and a more stable future for America.”

health outcomes are much better among individuals with higher socioeconomic status. Despite the robust correlations, the literature has failed to definitively answer whether the income/health gradient represents a causal relationship. Those with more income are not a random sample of people and the factors that lead one to have higher socioeconomic status (patience, persistence, parents with resources, etc.) may also play a role in improving health outcomes. Likewise, health shocks reduce both health status and income so poor health may cause lower income rather than the other way around (Smith, 1999). Given this possibility of reverse causation and the lack of an obvious causal pathway from income to health, Deaton (2003, p. 118) notes that “...much of the economics literature has been skeptical about *any* causal link from income to health, and instead tends to emphasize causality in the opposite direction...”.

Economists have attempted to identify whether income impacts health by exploiting exogenous variation in income such as winning the lottery (Lindahl, 2005), German reunification (Frijters, Haisken-DeNew and Shields, 2005), receipt of an inheritance (Meer, Miller and Rosen, 2003), a drop in income due to crop damage (Banerjee et al., 2007), a rise in South African pensions (Case, 2004), changes in Social Security payments (Snyder and Evans, 2005), and permanent changes in cohort earnings brought about by technological shocks (Adda et al., 2009). The results from these papers are however incredibly varied.

The conflicting evidence from these previous studies is due to at least two factors. The first is that many of the papers listed above exploit unusual shocks to income that are not replicable and the results might therefore have limited external validity. In contrast, the source of variation in this paper is a change in income affecting tens of millions of low income Americans every year. The second is the primary focus on self-reported health and mortality as the outcome of interest. Mortality is rare among many demographic groups (including the one we consider here) and therefore, failing to detect a causal effect of income on mortality could be a Type-II error. Self reported health outcomes are subject to a variety of well known biases. This paper represents one of the first efforts in the economics literature to utilize medically documented biomarkers as the outcomes of interest. As we document below, these indicators of health are well measured, objective, and predictive of future health events. Our results suggest that biomarkers are a promising avenue for answering this question and many others. Given the decreasing costs of collecting these data, they are now being incorporated into many national and cross-national datasets.

To the extent that the results of this analysis of the health effects of the 1993 EITC expansion can be generalized to individuals on similar income support programs, they could provide valuable information regarding optimal policy decisions regarding redistributive programs. As Lindahl (2005) stated “if income causally determines health, an evaluation of a policy affecting people’s income should take into account its effect on their health.”

II. The Earned Income Tax Credit and the Omnibus Reconciliation Act of 1993

The EITC is a refundable tax credit available only to individuals with positive earnings. Since its creation in 1975, there have been several large expansions of the EITC including the Omnibus Reconciliation Act of 1993 (OBRA93)⁴ which increased the typical benefit and dramatically increased the differences between the maximum benefit available to families with two or more children as compared to families with only one child.⁵

The impact of this expansion on families with two or more children is illustrated in Figure 1 where the horizontal axis represents adjusted gross income and the vertical axis is the size of the credit. As a result of the OBRA93 expansion, the subsidy during the phase-in range for these families increased from 19.5 percent to 40 percent, and the maximum benefit increased from \$1,511 to \$3,556. The effect of the expansion on families with only one child is detailed in Figure 2. In this case, OBRA93 increased the size of the credit in the phase in range from 18.5 percent to 34 percent, increased the maximum benefit from \$1,434 to \$2,152, and decreased the phase-out rate from 21 to just under 14 percent, which extended the maximum AGI that will receive the credit from \$23,000 to roughly \$25,000.

Of particular interest to this analysis are the differences in the size of the credit between families with one versus two or more children that were generated by the expansion. In Figure 3, we note the difference in the EITC between 1993 and 1996 at various levels of AGI for one and two plus children families. Following OBRA93, families with two or more children had an 18 percent greater subsidy rate and were eligible for 65 percent more in maximum benefits. As a result, between \$8,900 and \$23,050 in AGI, the OBRA93 expansions increased the maximum benefit by between \$800 and \$1,327. The difference in the maximum benefit for individuals earning \$8,900 is nearly 15 percent of family income.

III. Existing Literature on the Earned Income Tax Credit

⁴ Public Law 103-66, <http://thomas.loc.gov/cgi-bin/query/z?c103:H.R.2264.ENR>:

⁵ Prior to 1993, there were only small differences in the size of the benefit by family size.

There is a large literature that examines the effects of the EITC and its expansions on a wide variety of economic outcomes and this literature is reviewed in Hotz and Scholz (2003). The most studied outcome is the effect of the EITC on labor supply. In many of these papers, authors utilize difference-in-difference models and exploit changes in the structure of the program over time. To isolate the EITC effects from secular changes, the authors typically use data from a comparison sample that is composed of people unlikely impacted by the reform. For example, Eissa and Liebman (1996) and Meyer and Rosenbaum (2001) examine the impact of the EITC on the labor supply of single mothers by comparing the time series changes in labor supply for women with and without children. Eissa and Hoynes (2004) used a similar methodology to examine the effect of the EITC on the labor supply of married mothers. This work suggests that the EITC raises the labor supply of single mothers but reduces the labor supply of married mothers. The results tend to be larger for women with lower years of education and the estimates in Meyer and Rosenbaum suggest that the EITC expansions were responsible for a 10.7 percentage point increase in the probability of working for single women over the 1984-1996 period.

The evidence on whether the EITC alters hours worked is less clear with Liebman (1998) and Eissa and Leibman (1996) finding little evidence that EITC expansions altered this measure of labor supply while Dickert, Houser, and Scholz (1995), Keane and Moffitt (1998), and Meyer and Rosenbaum, (2001), finding modest impacts of EITC expansions on hours of work.

The most salient article for our purposes is Hotz and Scholz (2006) who used administrative data from California to estimate the labor supply effects of the 1993 EITC expansions of mothers on welfare. The authors compared the changes in labor supply of women with two or more children to those of women with one child—two groups that have arguably more similar pre-expansion trends in labor force participation than the typical comparisons which are women with and without children. These authors found large, positive effects of the EITC expansions on employment. Similarly, Adireksombat (2010) used data from the Current Population Survey (CPS) and implemented a similar identification strategy comparing the labor supply of women with two or more children compare to those with only one child. This analysis found large and statistically significant increases in labor supply for unmarried women with two children following the expansion of the EITC compared to similar women with only one child.

Since the amount of credit is based on family income and size, it is possible that EITC expansions impacted other family outcomes, but in general, there is little empirical evidence that

the EITC has altered marriage or family formation rates (Dickert-Conlin 2002; Eissa and Hoynes, 1998; Ellwood, 2000) or fertility (Baughman and Dickert-Conlin, 2003 and 2009).

The EITC has been demonstrated to have reduced poverty for working families (Scholz, 1994; Neumark and Wascher, 2001). EITC benefits are usually paid as a lump sum when recipients receive their tax refund checks. Barrow and McGranahan (2000) estimated that one-fifth of the EITC benefits are spent during the month of receipt and in a sample of EITC recipients from Chicago, Smeeding et al. (2000) found that 50 percent of the EITC is spent on investments in social mobility, such as transportation or a residential move.

IV. Identifying the Income/Health Gradient in the Behavioral Risk Factor Surveillance Survey Samples

In many of the papers utilizing quasi-experimental variation in income or education to assess the causal impact of socioeconomic status on health, the primary outcome of interest has been mortality. Since most beneficiaries of the EITC are relatively young, mortality rates are low and as a result, there is little hope of finding an impact of income on mortality even for large changes in income.⁶ As these results suggest, identifying a relationship between income and health for a younger population requires thinking more broadly about the set of health outcomes. Existing research examining correlations in health disparities by socioeconomic status provides some guide as to where to look for such outcomes. Most of this literature to date has demonstrated that some of the likely mechanisms (e.g., poor health habits, environmental conditions, health insurance) explain only a small fraction of the SES/health gradient (Lantz et al., 1998; Cutler and Lleras-Muney, 2008). A more promising line of research has focused on the potential physiological linkages between SES and health. This line of literature notes that stress has been demonstrated to produce dysfunction in the body's regulatory systems such as fight-or-flight, metabolic, immune and the hypothalamic-pituitary-adrenal systems (Sterling and Eyer 1988; McEwan and Stellar, 1993) and this stress may accelerate cell aging (Epel et al.,

⁶ For example, using data from the National Health Interview Survey Multiple Cause of Death data for the 1997-1999 period for women aged 21-50, we find a one-year mortality rate for this group of 0.184 percent. In a regression where the dependent variable is a dummy that equals one if a person died within one year of the survey and the covariates include controls for age, race/ethnicity status and marital status plus the natural log of family income, the coefficient (standard error) on this last variable is -0.00064 (0.00024). Consider an experiment that would increase income by 20 percent for a randomly selected group of N people with an equally large control group. If the OLS estimate above were a 'causal' impact of income on mortality, the reduced-form regression of one-year mortality on treatment assignment would generate a difference in mortality between the two groups of only 0.000128 and a simple power calculation indicates that one would need a sample of 836,000 in the treatment group (and a total sample of 1.672 million observations) to detect a statistically significant ($\alpha=0.05$) reduced-form difference in mortality between the two groups.

2004; Cherkas et al., 2006). Research has also demonstrated that those in lower socioeconomic groups have higher levels of biochemicals associated with stress such as cortisol, C-reactive protein, fibrinogen, low density lipoproteins and blood pressure (Steptoe et al., 2001 and 2005; Seeman et al., 2008). This work is therefore suggestive that stress-induced physiological responses may partly explain the health/SES gradient. As a result, we focus on outcomes that are pre-cursors for later negative health events such as self-reported health, mental health status, as well as biomarkers that measure stress and other physiological characteristics.

Initially, we utilize data from the BRFSS, which is an annual, state-based telephone survey designed to measure the health and health habits of the U.S. population. The survey is administered by individual states and data is then aggregated into a single annual file by the Centers for Disease Control (CDC). It is a very large annual survey with the survey size increasing from 102,263 in 1994 to 212,510 in 2001 and 414,509 observations in 2004. BRFSS is an excellent survey for our purposes because it has detailed demographic data including the number of children in the household plus a host of health outcomes and health habits such as self reported health status and the number of bad physical and mental health days in the past month.

The econometric model we utilize is similar to that employed by Hotz and Scholz (2006) in their analysis of the EITC on female labor supply in California. Specifically, as we note in Figure 3, the 1993 expansions increased in absolute and relative terms the size of the benefit for low income families with two or more children compared to families with one child. Therefore, if income is protective of health, we should find an increase in the health of families with two or more children over time relative to the same time series change for families with one child.

A key question within this research framework is how to restrict the sample to include people *likely* to be eligible for the EITC? Although the EITC is an income-based benefit, the literature summarized above indicates that there are important labor supply consequences of the program so an income-based criterion would select the sample based on an outcome that would potentially contaminate results due to a sample selection bias. A strategy used in the past is to select likely recipients by level of education and this is the method employed here.

The other consideration concerns the age range of the mothers in the sample. According to the enabling legislation, qualifying children for the EITC must be under age 19 or under 24 for full time students. It was not until 1993 that BRFSS first asked respondents to identify the number of children in the household less than 18 years of age, meaning this is the first pre-

treatment year in the analysis sample.⁷ As we increase the maximum age of the mothers in the sample, we increase the likelihood of including families that have children older than 18 but potentially qualifying children and hence misplacing mothers in one versus two or more children families. At the same time, reducing the maximum age eliminates women potentially “treated” by the EITC and increases the chance of a Type II error. To balance these two interests, we restrict the sample to women 21 to 40 years of age with reported children in the household.⁸

Table 1 reports data from the Annual Demographic file from the 1994-1996 and 1999-2002 March Current Population Survey (CPS) regarding the percentage of women aged 21-40 who received the EITC, categorized by their education status and number of children, and pre and post-1996 time periods.^{9,10} The estimated amount of the credit received by each CPS respondent is generated by the United States Census Bureau tax model and the calculation assumes that all those eligible for the credit actually applied.¹¹ The results in Table 1 demonstrate that the probability of receiving any EITC benefit is decreasing in education, holding the number of children constant. Furthermore, the group that received the largest increase in benefits between the tax years 1993-1995 and the years 1998-2001 were women with two or more children and a high school diploma or less in education. The fraction of women who were eligible for any EITC benefit increased by approximately 20 percent during the two time periods. At the other end of the spectrum, women with a college degree received little benefit from the program regardless of the number of children in their family.

Table 1 also contains information about the estimated amount of the benefit received, again assuming all eligible women applied. The numbers in these tables are in nominal terms. As would be expected given the structure of the OBRA93 expansion, women with two or more children received much larger increases in their estimated EITC payment. For example, in the last two rows of Table 1, women with two or more children and a high school diploma or less experienced an increase in their estimated EITC payment of roughly \$820 (59%). On the other

⁷ Prior to that year, the survey asks respondents the number of children in grades K-8 and the age of the youngest child, eliminating any pre-1993 surveys from use.

⁸ This sample reduces the chance of having families with qualifying older children in the sample. In the 2000 Census One-Percent Public Use Micro Sample, the fraction of mothers aged 21-40 with a high school degree or below in families with older qualifying children (e.g., children aged 19-24 and in school) was only 1.8 percent.

⁹ The March CPS data was downloaded from www.ipums.org, King et al. (2010).

¹⁰ Because the March CPS asks about income earned in the previous year, data from the 1994-1996 CPS presents data for the 1993-1995 tax years.

¹¹ This is an assumption that previous research has established is clearly wrong. Data from the 1996 tax year suggest that the between 12.8 and 17.8 of those eligible for the program never applied (Tax Policy Center, 2002). At the same time, the IRS (2000) estimates that approximately 30 percent of the benefits paid out by the EITC in 1998 went to individuals who were not eligible for the benefit.

hand, women of a similar educational background but with only one child had an increase of only approximately \$300 (25%). The numbers in Tables 1 indicate that among those with children, the most likely recipients of the EITC are low-educated women and this group will represent the population eligible for the program in our econometric models. While the average dollar amount of benefits among all women may appear to be relatively small, it is important to note that the benefit amounts in Table 1 represent a mixture of people receiving large increases in annual income and those receiving relatively small changes in benefit levels.

In order to estimate the simple difference-in-difference model outlined above, at a minimum, we need information on mother's age, education and the number of children in the household. Because the first checks under the new EITC schedule for families with two or more children are distributed in 1996 (for tax year 1995), we look at data from the 1993 through 2001, giving us three years pre and six years post-EITC expansion.¹²

Sample means from the BRFSS data set for the pre-EITC expansion period are reported in Table 2. In the first two columns, we report estimates for women, age 21-40 with a high school education or less with one and two plus kids respectively. In the next column, we report the p-value on the test of the null hypothesis that the means are the same across the two columns allowing for observations within a state to be correlated. In the final three columns of the table, we repeat the same basic structure but for mothers with a college degree.¹³

Although our primary interest in this paper is to examine the impact of higher EITC payments on health outcomes of mothers, as we noted above, the bulk of the empirical work on the EITC in the past has examined the impact of the program on female labor supply. To place our estimates in this broader literature, we are interested in estimating some models with labor supply measures as outcomes. Unfortunately, information on labor supply in the BRFSS is limited to a single question that identifies whether someone is currently working for a wage, self-employed, out of work for less than a year or more than a year, a homemaker, a student, retired, or unable to work. Because the out of work questions do not identify whether a person is currently looking for work, the only measure of labor supply we can construct from the BRFSS

¹² The EITC was passed in 1993 and became effective in tax year 1995. The Advance EITC (AEITC) allows taxpayers to collect their EITC throughout the year in the form of lower tax withholdings in their paycheck (<http://www.irs.gov/individuals/article/0,,id=96515,00.html>). The GAO (2007), however, estimates that only 3 percent of eligible taxpayers in the 2002-2004 period collected the AEITC. Therefore, calendar year 1996 is the first year of increased benefits from the "1993 expansion."

¹³ We utilize this final group in a difference-in-difference-in-difference model and for completeness, report basic sample means for this group as well.

is whether a respondent is “currently employed.” As is discussed below, this variable is similar to a measure of labor supply that can be generated from other standard datasets.

In the sample with high EITC eligibility, there are noticeable differences in the observed characteristics of the mothers with one versus two plus children. Women with two plus children tend to be slightly older, have higher fraction minority, are more likely to be married and have lower incomes. Not surprisingly, women with more children are less attached to the labor force as well. Most of these differences are statistically significant.

In the bottom of the table, we report sample means for the measures of health status and health habits. The first outcome is a dummy that equals 1 if a person self-reports they are in excellent or very good health.¹⁴ The second and third variables are, respectively, the number of bad mental and physical health days reported in the past 30 days. Mothers with two or more children are less likely to report excellent or very good health and report more bad mental health days, but they report fewer bad physical days. Interestingly, unlike the demographic variables, there are much smaller differences in the reported health characteristics between women with only one child and those with two children. For women with a high school degree or less there are statistically significant but small differences in the number of bad mental health days in the past month (95% confidence level).

V. Econometric Models

Our econometric model exploits the fact that after tax year 1995, low income mothers with two or more children received a substantial rise in income relative to similar women with only one child due to the EITC expansions. As we outline below, the model is a straightforward difference-in-difference (DD) specification. Later, we also outline a difference-in-difference-in-difference (DDD) specification where women likely ineligible for the EITC form a comparison sample. We construct notation that will incorporate both of these specifications.

At its most basic level, the DD specification can be expressed as a comparison of pre and post treatment means for the affected groups versus the comparison sample. We can enhance the explanatory power of the model by adding a set of covariates to control for individual characteristics, state level fixed effects, and time effects. We begin by letting Y_{ji} be an outcome of interest for mothers i from group j . There are two groups of people: those likely eligible for the EITC ($j=e$) and those not typically eligible ($j=n$). We then add a set of explanatory variables

¹⁴ The original question in the survey is the standard one where respondents report whether their current health is excellent, very good, good, fair or poor.

(represented by the vector X_{ei}), a set of year effects that allows a more flexible time series pattern, and allow for persistent differences in outcomes across states by adding in a set of state dummy variables. These last two sets of variables are represented by the dummy variables $T(t)$ that equals 1 if an observation is from year t and $S(m)$ that equals 1 if the observation is from state m . The year and state effects are important in this context because this is a time period of rapidly changing labor supply for low skilled women, especially low educated single mothers. Welfare reform efforts and the robust economy of the 1990s could have potentially altered outcomes for women in our EITC eligible sample. Women with two or more children are identified by the indicator variable TWO_{ei} and the post-expansion time period is identified by the indicator variable $EXPAND_{ei}$ which is equal to 1 for the years after 1995. We can obtain a DD estimate with the equation:

$$(1) \quad y_{ei} = \alpha + TWO_{ei}\phi + \sum_{t=1993}^{2000} T(t)\pi_t + X_{ei}\gamma + \sum_{m=1}^{50} S(m)\lambda_m + (Two_{ei}Expand_{ei})\delta_{dd} + \varepsilon_{ei}$$

where ε_{ei} is an idiosyncratic error and the reduced-form impact of additional income generated by the EITC is captured by δ_{dd} . In our results, we call the estimates from the comparison of means as the “simple” DD estimates and the results from equation (1) as the regression-adjusted DD estimates. In these models we allow for an arbitrary correlation in errors for observations within a state over time.

As in any DD model, the key identifying assumption is that the trends in the comparison sample provide an estimate of the time path of outcomes that would have occurred in the treatment group had there been no intervention. If there are unmeasured factors that differentially impacted low educated mothers with two kids compared to mothers with one child then the estimate δ_{dd} will be biased.

We can potentially reduce this bias by increasing the dimensions of the problem and exploit data on a group of mothers with similar fertility experiences but not subject to the EITC shocks in a DDD framework. Specifically, noting the results in Table 1 that few college educated mothers are EITC recipients, differential trends in health outcomes for college-educated mothers with two plus children versus one child can be used to control for parity-specific trends in the lower educated and higher EITC eligible populations. In this case, we use data for both EITC eligible ($j=e$) and not eligible ($j=n$) households and therefore the dependent variable is Y_{ji} . Enrollment in the eligible group is defined by the dummy variable $Elig_{ji}$ which equals 1 if mothers are in the lower education group. In this case, the DDD model requires controls for

group eligibility ($Elig_{ji}$), time period ($Expand_{ji}$) and treatment group (Two_{ji}), the three unique cross terms for all these variables, and the final third-order term that identifies potentially treated mothers ($Two=1$) who are eligible for EITC ($Elig=1$) in the post treatment period ($Expand=1$).

The equation of interest is therefore:

$$(2) \quad Y_{ji} = \beta_0 + Two_{ji}\beta_1 + Elig_{ji}\beta_2 + \sum_{t=1993}^{2000} T(t)\pi_{1t} + (Two_{ji}Elig_{ji})\beta_4 + \sum_{t=1993}^{2000} T(t)Elig_{ji}\pi_{2t} + \sum_{t=1993}^{2000} T(t)Two_{ji}\pi_{3t} \\ + X_{ei}\gamma + \sum_{m=1}^{50} S(m)\lambda_m + (Two_{ji}Expand_{ji}Elig_{ji})\delta_{ddd} + \varepsilon_{ji}$$

where X_{ji} is a vector of covariates, $S(m)$ are state effects, and $T(t)$ are year effects.

The DDD estimate is the parameter δ_{ddd} . Under the assumptions that the health status of mothers with a college degree has a similar pre-treatment trend as those for women with a high school degree or less and that this group will react similarly to post-expansion shocks, the DDD estimate will provide an unbiased estimate of the effect of the EITC on health outcomes. A tradeoff is that these models use less variation in the data to identify parameter estimates and as a result, standard errors tend to rise considerably.

VI. Labor Supply Results From the CPS and the BRFSS

The bulk of the empirical literature concerning the EITC has, to date, examined the impact of the program on the labor supply of low educated women. As we noted above, from the BRFSS we can only determine whether a mother is currently employed. In this section, we briefly outline a companion sample constructed from the March Current Population Survey (CPS) where we estimate the impact of the EITC on labor supply. These results are presented for two primary reasons. First, obtaining similar labor supply responses to the EITC expansions in the CPS to the existing literature on these outcomes will provide validation of our proposed empirical strategy. Second, obtaining similar estimates for labor supply between respondents to the CPS and our BRFSS sample confirms that our sample is representative of those used in the existing literature estimating more traditionally studied outcomes.

The CPS is a monthly survey of about 50,000 households and it is the primary data set for labor force characteristics of the US civilian non-institutionalized population. For this section, we construct a sample of women aged 21-40 with 12 or fewer years of education and children in the home from the 1993 through 2001 March CPS, a sample that parallels the BRFSS data

outlined in the previous section. The CPS asks respondents the number of your own children (biological, foster or step children) of any age living in this house which is similar in scope to the question used to identify eligible mothers in the BRFSS.

We use the “employment status” variable from the regular CPS¹⁵ survey to construct two labor supply variables: one that measures whether the respondent is currently in the labor force, and another that measures whether they are currently employed. The variable for currently employed is most directly comparable to the labor market outcome from the BRFSS.

Unweighted descriptive statistics from the full 1993-2001 samples from both the March CPS and the BRFSS are reported in the first two columns of Table 3 with the final column being the p-value for the null hypothesis that the means are the same across the two samples.¹⁶ The samples look similar on many dimensions and very different on others. The average age and fraction of respondents with two or more children are very similar across the two samples. The BRFSS sample contains a smaller fraction lower-educated, a lower fraction married and much lower fraction Hispanic mothers than the CPS. This last number is expected given that BRFSS is a telephone based survey and the CPS is an in-home survey. Note that the fraction of mothers in the sample that are currently employed is 4.5 percentage points higher in the BRFSS compared to the March CPS.

In Table 4, we report the DD estimates of the impact of the EITC expansions on labor supply outcomes. Following the previous literature on the labor supply effects of the EITC, we produce estimates for three samples: single women, married women and then a pooled sample that includes both groups. For the CPS sample, we estimate models for the outcomes “currently in the labor force” and “currently employed” and these estimates are reported in the first two columns. In the final column, we report estimates from the BRFSS using the “currently employed” outcome. For each outcome, we estimate two models: a simple difference-in-difference model (equation 1) and a regression-adjusted version (equation 2). In the regression-adjusted models, we include indicator variables for all unique values of age, education, marital status, race, the number of children, year and state of residence. In the BRFSS model we also include survey month effects. We estimate models as linear probability equations and estimate standard errors that allow for an arbitrary correlation in errors within a state.

¹⁵ Each month, the CPS asks respondents a fixed set of questions and in some months, households are asked to complete a supplemental survey. Each March, respondents complete the Annual Demographic File which has detailed data about labor supply, earnings, and insurance status from the previous year. In our models, we use data from the basic March CPS survey and not the annual demographic file.

¹⁶ In these models, we allow for arbitrary correlation in the errors within a state.

In both data sets, we find that the EITC expansions had a large impact on the labor supply of single women. For example, in the regression-adjusted difference-in-difference models with the CPS data, we estimate that the expansions increased labor force participation by 5.95 percentage points and increased current employment probabilities by 5.4 percentage points. Both of these estimates are statistically significant. This final number is very close to the estimate that we generate for the same outcome but with the BRFSS sample. We cannot reject the null hypothesis that these two estimates are equal. These results are also similar to the existing literature. Hotz and Scholz (2006), the first study to utilize this particular empirical methodology to evaluate the 1993 expansions, found an increase in employment from this EITC expansion for women with two or more children of 3.4 percentage points. Differences between the magnitude of our estimate using the CPS and this earlier estimate are due, at least in part, to the fact that the sample for Hotz and Scholz (2006) is composed of women on welfare and that the authors specifically focus on single-parent cases. Similar to the estimate in Table 4, Adireksombat (2010) used CPS data and found a of 5.2 percentage point increase in labor supply for high school graduates with two children compared to similar women with only one child. For the currently employed outcome in the CPS and BRFSS samples, controlling for demographic covariates does not change the results much.

In contrast to the results for single women, there is a modest labor supply response among married women to the 1993 EITC expansions. In the CPS samples, we find a statistically insignificant one-percentage point increase in labor force participation and current employment in the regression-adjusted models. For this group in the BRFSS sample, we find a 1.8 percentage point increase in current employment rates that is statistically significant at the 10 percent level. In no case can we reject the null that the estimated impact of the EITC in the current employment models is the same for the two samples.

The disparity in the estimates for single and married women is in line with previous work. Eissa and Hoynes (2004) examined the effect of the EITC on labor supply by marital status. Their theoretical model and empirical results suggest any labor force participation increase from the EITC should be primarily experienced by single women. In contrast, they found that the labor supply of married woman decreased following the EITC expansion. This result is caused by the fact that the EITC combines income from both spouses into family income for the purposes of calculating benefit levels. Therefore, there may be some concern about the marginally statistically significant positive labor supply effect for married women that we find in

the BRFSS. It is important to consider that the theory predicts there can be a positive labor supply for married women if their family income is in the phase-in range. Data from the 1994 March CPS indicates that roughly 12 percent of single married mothers with a high school degree or less are in families that are in the phase in range of the EITC for that year. The 95 percent confidence interval for the marginally significant BRFSS results is wide enough to include the modest changes in labor supply that would be consistent with these income values.

A second point of interest concerning the results in Table 4 is that the fraction currently working among “treated” mothers in the pre-EITC expansion period is similar. This is in contrast to the entire sample described in Table 3, where the fraction currently working among all BRFSS mothers is much higher than in the March CPS. In addition, the estimated impact of the EITC expansion on current employment for mothers with two or more children is very similar in the CPS and BRFSS samples. In the CPS sample, the EITC expansions are estimated to have increased employment rates for single, married and all women (standard error) by 5.3 (1.3), 1.0 (1.1), and 1.3 (0.9) percentage points. The corresponding numbers from BRFSS are 4.6 (1.1), 1.8 (1.1), and 2.0 (0.7). Within all three subsamples in the March CPS data, the increase in labor force participation is captured fully by an increase in employment. The pre-expansion mean of current employment for the treatment group in the BRFSS sample is 58 percent—suggesting that the EITC increased labor supply by approximately 3.4 percent.

VII. Maternal Health Results from the Behavioral Risk Factor Surveillance Survey Samples

Table 5 contains both the simple and regression-adjusted DD coefficients for different measures of self reported health allowing for within-state correlation in errors. In this table, the numbers in parentheses are standard errors while the numbers in brackets are p-values on the null that the parameter is zero. The first row of estimates repeats the “currently employed” results from the BRFSS sample reported in Table 4 and the next three rows we report results for various self-reported health outcomes in the BRFSS. The second row of results are for a dependent variable that equals one if an individual reports being in either excellent or very good health. The regression-adjusted coefficient suggests that the EITC increased the probability of women with a high school degree or less and with two or more children reporting these high levels of health by 1.35 percentage points (p-value 0.10). In the third and fourth rows of the table, we report estimates for a model where the outcomes of interest are the counts of poor mental and physical health days in the past 30 days, respectively. Since these data are count in nature, we

estimate a negative binomial model which allows for over-dispersion in the dependent variable.¹⁷ The third row contains the estimates for a negative binomial model with the number of bad mental days as the dependent variable. The regression-adjusted coefficient shows that following the expansion of the EITC, women with two or more children and a high school degree or less experienced a statistically significant (p-value <0.05) 7.5 percent reduction in the number of bad mental health days compared to similarly educated women with only one child.¹⁸ The final row contains a similar set of estimates for the presence of bad physical days. These results, however, are generally small, positive, and imprecisely estimated.

In statistical tests for single parameters, we reject the null hypothesis if the p-value falls below a critical value of α . Because we are producing multiple tests from the same data set, we report the p-value so that readers can use the Bonferroni correction for statistical tests of multiple parameters. Specifically, Bonferroni suggests that to reduce Type I error, with j tests from the sample, the confidence level for a statistical test should be expanded to be $1-\alpha/j$ and nulls are rejected if the p-values are lower than α/j . In this case, with three health outcomes, critical p-values would be 0.0167 for a 95% confidence level and 0.033 for a 90% confidence level. With this correction, our estimates for the impact of EITC on bad mental health days are still statistically significant at the 90% confidence level. We should note that while this simple correction reduces the chance of a Type I error, it does greatly increase the probability of a Type II error.

Although the estimates in Table 5 are in most cases of marginal statistical significance, they are very large responses to the EITC expansion. From Table 1, we see that in the post-1993 expansion period, the average difference in EITC payments between mothers with two children versus one is roughly \$200. However, among recipients in the post-1995 period, the difference in benefits between eligible families with two or more compared to one child is about \$750. This suggests a rather sharp gradient in health for these low income families, which may not be that surprising. These results are similar in magnitude to the estimates obtained by Milligan and

¹⁷ In the negative binomial model, the variance to mean ratio is defined as $1+\delta$ where δ is the over dispersion parameter. If $\delta=0$, the model collapses to a Poisson where the variance equals the mean. In the models in Table 5, we can easily reject the null that $\delta=0$.

¹⁸ A potential concern with the negative binomial model in this case is that the PDF is defined over all counts from 0 to infinity but by construction, our counts vary only from 0 to 30. We can easily adjust for this fact in any econometric estimation. If $f(y_i | x_i, \beta)$ is the PDF of the negative binomial for person i and $F(y_i | x_i, \beta)$ is the CDF evaluated from 0 to 30, the actual value of the likelihood for individual i is then $f(y_i | x_i, \beta) / f(30 | x_i, \beta)$. Programming a maximum likelihood version of this censored negative binomial, the estimated coefficient on the EITC expansion variable and the standard error are unchanged out to three decimal places.

Stabile (forthcoming) for similar sized changes in income among low income households with children in Canada.

The key assumption in the difference-in-difference model is that the comparison sample (low educated mothers with one child) provides an estimate of the time path of outcomes that would have occurred for low-educated mothers two children had the EITC not been expanded for this group. We can never directly test this hypothesis but we can provide some evidence that the trends for these two groups were similar in the pre-treatment period. Specifically, we take model (2), restrict the sample to include data from the pre-treatment period only and allow the year effects to vary across mothers with one and two children. We can then test the null hypothesis that the year effects are the same across the two groups. Using BRFSS data, in the currently employed and excellent/very good self reported health equations, the p-values on the test of the null hypothesis that the trends are the same across the two groups are 0.65 and 0.32 respectively.

One limitation of this test using the BRFSS data is that the pre-treatment period is relatively short. While there is little that we can do to overcome this limitation in the BRFSS, we can supplement this evidence with a similar test using data from the National Health Interview Survey (NHIS). These data have the advantage of a longer pre-treatment time period, but are not appropriate for the full analysis because the NHIS was dramatically redesigned in 1996—the same year as the full implementation of the EITC expansion. The sample for the NHIS is constructed in the same manner described above for the BRFSS. Specifically, we pool data for women with a high school degree or less and with at least one child from the 1988-1993 NHIS and construct two outcomes that are directly comparable to the BRFSS: one for those reporting excellent or very good health and another indicating whether the mother is currently employed. The p-values on the same test implemented above for the BRFSS for these two equations are 0.21 and 0.12, respectively.

Table 6 contains the estimated coefficients for a number of robustness checks to the above discussed results. The first column reprints the regression-adjusted estimates from Table 5. The second column of results attempts to account for the potentially confounding effects of changes in other state based policies. For example, given our sample characteristics (low educated mothers), a large fraction in the sample are single mothers with low income and therefore, many will be eligible for welfare assistance. The 1990s witnessed tremendous changes in welfare policies such as the Personal Responsibility and Work Opportunity

Reconciliation Act (PWRORA)¹⁹ which placed time limits on welfare, instituted family caps on benefits, mandated work requirements, increased earnings limits and provided more generous asset limits for eligibility (Meyer and Rosenbaum, 2001; Blank, 2002; Bitler et al., 2005).

Welfare reform was accomplished piecemeal across states with many states adopting some of these policies prior to 1996 through waivers. Likewise, the PWRORA reforms were instituted in roughly half the states in 1997 and the other half in 1998. Using the same data set as we use below, Bitler et al. (2005) found that welfare reform reduced insurance coverage, reduced preventive care such as pap smears and breast exams but had no impact on self-reported health status or the number of poor physical or mental health days.

Changes in federal policies are a threat to our identification strategy if they *differentially* affect low-income families with two or more children compared to families with only one child. The variation in the implementation time of welfare reform across states could potentially contaminate our estimates. We guard against this by using low-educated moms with one child as a comparison sample. Welfare reform should in general impact low-income mothers with one and two children to similar degrees. Since events such as welfare reform vary across states and over time, we can capture their common impact by including state-specific year effects in the model. These results are contained in the second column of Table 6 and for most outcomes there is little change in the estimates. However, the inclusion of state-specific year effects increases the p-values on the treatment effect in the self-reported health status to 0.118. The negative binomial estimate for the total number of bad mental health days now has a p-value of 0.059.

The EITC expansions of 1993 were phased in over tax years 1994 and 1995. Because most recipients take the credit in the form of a tax rebate, the full effect of the expansions should be witnessed in calendar year 1996 which is how we have defined the treatment dummy above. However, there could be a concern that some of the women exposed to first portion of the EITC expansion in calendar year 1995 are incorrectly classified as being in the pre-treatment period. Even if this were the case, we should expect that this would bias the estimate against finding an effect on health outcomes. This can be seen in the data. If we change the treatment period to start in calendar year 1995 when rebates from tax year 1994 were distributed, our results are slightly stronger. The coefficients (standard errors) on the OLS regression-adjusted difference-in-difference estimates for the “at work” and “excellent/very good” linear probability estimates are 0.0189 (0.0075) and 0.0202 (0.009), which are similar to estimates in final column of Table

¹⁹ Public Law 104-193.

5. Likewise, the estimate on the treatment effect for the regression-adjusted negative binomial model for the number of bad mental health days is -0.0846 (0.040).

The third and fourth columns of results are for samples split by marital status. The negative binomial result for the total number of bad mental health days reported by married women is large and statistically significant at a p-value of 0.05 . The estimated effect for single women is statistically insignificant but is negative and relatively large in magnitude. Similarly, while the estimate on reported excellent or very good health is small in magnitude and statistically insignificant for single women, the result among married women is large (a 2.1 percentage point increase) and has a small p-value. Across both columns, we cannot reject the null hypothesis that the single and married results are different in magnitude. The more precise estimates for the married sample may be caused by the much larger sample sizes for these women.²⁰

The final column of Table 6 contains the estimates for the DDD identification strategy in equation (2). These results provide no statistically significant estimates—though this is not surprising. The basic results in the first column of Table 6 are of marginal statistical significance. Because the DDD models absorb additional dimension of the data, the model is using much smaller variation in the covariate of interest. Comparing the first (DD estimates) and last column (DDD estimates) of results in Table 6, the standard errors double in size. Holding the DD coefficient estimates constant, none of the parameters would be statistically significant at a p-value of 0.05 with the standard error estimates from the DDD models.

The results derived from equations (1) and (2) are reduced-form estimates that examine the impact of higher EITC payments on outcomes. A question remains about the mechanism linking higher payments to health. The improved outcomes can be due to the benefits of higher income but at the same time, previous research concerning the EITC has shown a number of effects from the program with the most prominent being an increase in labor supply. Therefore, it is unclear if the increase in health results from changes in labor supply induced by the EITC or from some other intermediate event produced by higher transfer payments. To test this specific question, we re-estimated specifications of equation (1) for the outcomes contained in Table 5 including an indicator variable for current employment as an additional covariate.²¹ Overall,

²⁰ The reported p-values are obviously subject to the criticism that they treat each test as an independent event and hence the critical p-value for hypothesis tests may be adjusted for multiple comparisons.

²¹ This model is by construction subject to an omitted variables bias since current employment is impacted by EITC treatment. If those in the best health are those most likely to increase work in the face of the EITC expansions, then the bias imparted by including this endogenous variable as a covariate will most likely reduce our ability to detect a

these results suggest that changes in employment are not driving the results. Adding a “currently employed” dummy to the linear probability models measuring excellent/very good self-reported health status reduces the magnitude (standard error) of the treatment effect to 0.0121 (0.0074), an estimate that is only marginally smaller than the estimate in Table 5. Likewise, the coefficient on the treatment effect in the negative binomial model for the number of bad mental health days falls in magnitude to only -0.0694 (0.0330). As these results suggest, increased work on its own cannot explain the results in Table 5.

Even if increase in labor supply were a component of the observed increase in health from the EITC, the results in Table 5 are important for policy considerations. In contrast to nearly all of the previous sources of variation used to identify the effect of income on health, the EITC represent a feasible (if not the most feasible) means of distributing money to low-income Americans. Therefore, the totality of the effect of the program on health is important, even if some portion is a result of changes in labor-supply induced by the program.

While the results controlling for labor force status suggest that the health estimates are caused by the increased income resulting from the EITC expansion, some concern may remain that changes in other government policies that occurred concurrent with the expansion are actually driving the estimates. For example, it is possible that the above results are driven by the start and rapid expansion of the State Child Health Insurance Program (SCHIP) in the late 1990s or the Medicaid expansion that began in the late 1980s. These programs would only contaminate our results if there was a *differential* change in insurance status for two-plus child families among low income women compared to single child families with similar incomes. Evans and Garthwaite (2010) used data from the March CPS and found that there was no differential effect on health insurance status from these government programs between families with one child and those with two or more children. This suggests that there is little credibility to the argument that the observed improvements in maternal health are a result of changes in health insurance status as resulting from these changes.

Another federal policy that may be of concern is the Child Tax Credit. This credit, which went into effect in tax year 1998 (calendar year 1999) provided \$400 per child under the age of 17. Over time the credit grew and it currently provides \$1,000 per qualifying child. In the first three years of its existence the credit was not refundable. As of tax year 2001, families with children were eligible for the Additional Tax Credit which provided additional money to families

relationship between the EITC expansions and health. This model is meant to be illustrative rather than representing a causal estimate.

with children that did not receive their full Child Tax Credit as a result of its nonrefundable nature. There are several reasons why the Child Tax Credit is not of great concern for this analysis. The first is that the Child Tax Credit is only in place for three years of the sample used for this analysis. Second, for the entire BRFSS sample this tax credit is not refundable, limiting its availability to many low-income families. If you limit the sample to exclude the years where the nonrefundable credit was in existence, the DD estimates reporting excellent or very good health is 0.019 (0.0087), and the negative binomial estimate for the number of bad mental health days in the past month is -0.0806 (0.034). If anything, these estimates are stronger than the main results, showing that the Child Tax Credit is not a source of bias in these results.

Finally, the results are also not driven by large families in our treatment sample. Reducing the sample to including only women with one and two children does not change the basic results.

VIII. Maternal Health Results from the National Health and Nutrition Examination Survey Samples

While the above results provide some evidence of the effect of higher transfer payments on health, all of the outcomes are self-reported and all are subjective measures of health. Self reported health is a relatively easy measure to collect. It is also an excellent predictor of objective measure of health such as mortality. In a review of twenty seven community studies, Idler and Benyami (1997) found that global self reported health was an independent predictor of mortality, even when indicators of morbidity were included in the analysis. In a meta-analysis of 163 studies, DeSalvo et al. (2006) found similar results even after controlling for a variety of demographic factors and co-morbidities. Similarly, Maddox and Douglas (1973) found that self reported health status was a better predictor of future physician ratings than the reverse. This led the authors to claim that self reported health data “clearly measure something more—and something less—than objective medical ratings.”

However, the use of self-reported health as an outcome does have some drawbacks. The subjective nature of self-reported health survey questions lead to a lack of comparability across individuals which introduces classical measurement error into the model (Bound, 1991). Because we use self-reported health as an outcome, this type of measurement error should primarily reduce precision which is costly in this case given the marginal statistical significance of our results from the BRFSS samples. In an attempt to overcome this measurement error,

researchers have proposed using self-reported data regarding objective medical conditions and diagnoses as opposed to health status. These data, however, are also subject to measurement error. Baker, Stabile, and Deri (2004) analyzed a unique dataset that contained self-reports of disease presence and indicators of disease presence from insurance claims data. They found that these self-reported measures produced both false positive and negative indications of disease presence. Self-reported measures of health can also be subject to systematic measurement error. Using a self-reported measure of hypertension, Johnston, Propper, and Shields (2009) found no evidence of an income health gradient. When the authors used data on blood pressure readings from medical professionals for the same individuals, they found a large income-health gradient with respect to blood pressure.

Despite the concerns associated with self-reported measures of health, these outcomes have predictive power concerning mortality and therefore changes in these are important indicators of health status. A perhaps more important shortcoming of self report outcomes is that they are limited in their ability to provide information regarding the mechanism driving the observed increase in health. In this way, the BRFSS results should be seen as an important first step towards understanding the effect of the EITC on health. The second component of the analysis is to consider more detailed indicators of health.

Increasingly, researchers have turned their attention to biomarkers of physical and mental stress as indicators of health. Karlamangla et al. (2010) notes this movement has occurred for several reasons. First, individuals can experience significant reductions in health even without the presence of identifiable chronic conditions. Often, these decreases in health can be identified through the use of biomarkers even when specific diseases are not detectable. In addition, biomarkers have been found to be useful in predicting a wide variety of health outcomes. Finally, due to the fact that biomarkers precede the onset of major diseases they are believed to be more susceptible to external factors such as psychological stressors and other interventions. Due to these factors, biomarkers appear to be the ideal setting for comprehensively estimating the health effects of the EITC.

To obtain a better understanding of the mechanism underlying the identified changes in health we found in the previous section, we conduct a similar analysis using biomarker data. This data, obtained from several panels of the National Health and Nutrition Examination Survey (NHANES), directly confronts the two concerns about self-reported indicators discussed above. The biomarker data in the NHANES are measured by medical professionals—addressing any

lingering concerns about relying on self-reported health outcomes. Biomarker data could also provide evidence about the causal pathways generating the previously documented relationship between socioeconomic status and health. This strategy does, however, come at a cost: the NHANES has much smaller sample sizes than other health datasets.

The NHANES is a national survey designed to measure the health and well being of the American population. Dating back to the 1960s, the survey component of the NHANES contains data on demographic, socio-economic, and health related issues. The unique aspect of NHANES is the examination component that is conducted in mobile examination centers staffed by medical professionals. The examination component provides detailed medical information including data from blood and urine tests and medical exams.

The first three NHANES surveys were approximately 8-10 years apart. After NHANES III, which interviewed people from 1988-1994, surveys were fielded on two-year intervals but with smaller samples. Since NHANES III occurred during the pre-1993 expansion period, we pair this data with the first three samples from the new timing framework—the NHANES 1999-2000, NHANES 2001-2002, and NHANES 2003-2004. These four samples provide roughly equal samples sizes in the pre- and post-EITC expansion periods.

The econometric model outlined in Section V requires that we identify the number of EITC-eligible children in families. This is accomplished in different ways depending on the particular NHANES sample. In NHANES III, the sample respondent for the household was asked to identify the number of people in the family. We estimate the number of children as family size minus two for married heads of households and family size minus one for families with single mothers. We will overstate the number of qualified children if some of the children in the family are being claimed as a qualifying child by a non-custodial parent in another household or if some of the children are above the EITC-qualifying age. There is little we can do about the former situation but by restricting the top end age range of mothers, we can eliminate counting “boomerang children” who do not qualify as EITC qualifying child because of their age. As with the BRFSS, we will restrict the sample to women aged 21-40.²²

The final three NHANES surveys do not ask about family size, but rather, household size. In this instance we first eliminate all households where the woman reports zero live births in her lifetime since few women who never gave birth live in families with children from their

²² In the 2000 Census One-Percent Public Use Micro Sample, the fraction of mothers aged 21-40 with a high school degree or lower in families with non-qualifying children (e.g., children aged 19-24 and not in school, or any child over the age of 24) was only 3 percent.

spouse.²³ In these surveys, we estimate the number of children as 2 minus household size for married women and 1 minus household size for single mothers.²⁴

Although there may be concerns that this procedure has the potential to miscount the number of children in the household, the procedure appears to be very accurate. Using data from the 2000 1% PUMS (Ruggles et al., 2010), we generate a sample of mothers aged 21-40 with a high school degree or less. We compare the number of own children in the household (which may include step children) which is generated from the detailed relationship codes in the Census, with the number of children we calculate using the method employed for those reporting household size in the NHANES sample. Our estimate of whether the mother has one or two or more children in the family matches the number of children in 90% of the cases. Because we restrict the sample to only women who have had a birth, when our proposed method doesn't match we tend to overstate the number of children in the house. Therefore, in these cases we have too many single children families in our two plus child treated group, which should bias our estimates towards zero.

The NHANES has a wealth of information from physical, blood and urine tests. Table 7 contains the definitions and sample means of the biomarkers we utilize from the NHANES data sets. Following Seeman et al. (2008), we classify individuals based on whether they have risky levels of these biomarkers (e.g., high blood pressure, low levels of albumin) and we group the risky biomarkers into four groups: those that measure inflammation, cardiovascular conditions, metabolic disorders and aggregate risks across all three groups.

The first two biomarkers are acute-phase proteins where concentration levels are altered in response to inflammation. For example, atherosclerosis (considered the main cause of coronary artery disease) is an inflammation process where fatty material collects on the walls of arteries. Acute-phase proteins are thought to be independent predictors of heart disease (Hansson, 2005). The two acute-phase proteins we consider are c-reactive (CRP) and albumin.

CRP is produced by the liver and is only present in the blood when there is inflammation. It is measured as milligrams per deciliter of blood (mg/Dl). Because CRP is only produced during inflammation, medical researchers have investigated whether it is an independent

²³ Using data from the Fertility Supplement to the June 2000 CPS, only 6 percent of women aged 21-40 who have never had a live birth report they have their "own children" under the age of 18, a variable that measures not only biological children but step and foster children as well.

²⁴ Among families with children, the fraction of households with 2 or more children is very similar across the four surveys. In our sample, we find 77 percent have two or more children in the NHANES III survey and about 72 percent in the final three NHANES surveys.

predictor of coronary heart disease (Ridker, 2003; Koenig et al., 1999). Owen et al. (2003) found elevated levels of CRP among lower employment classes in the Whitehall II survey while Alley et al. (2006) found higher levels of CRP in lower income groups. Respondents are defined to have risky CRP levels when concentrations are ≥ 0.3 mg/Dl.

Albumin is a blood protein made by the liver and is measured as grams per deciliter (g/Dl). Albumin levels decline during inflammation (Gillum et al., 1994). Lower levels of albumin may indicate liver disease, and is predictive of coronary heart disease, cardiac events (Danesh et al., 1998), and stroke (Gabay et al., 1999). Risky albumin levels are defined to be when concentrations fall below 3.8 g/Dl (grams per deciliter). Seeman et al. (2008) found little correlation with low albumin levels and education but found risky albumin levels decline with higher income.²⁵

Looking at the sample means in Table 7 for these inflammation biomarkers, roughly 44 percent of the mothers in our sample have elevated CRP levels while about a quarter have risky albumin levels. About 53 percent of women in the sample have at least one risky inflammation condition and the average number of risky inflammation conditions is about 0.7.

The second group of biomarkers measure cardiovascular conditions and we include three: diastolic blood pressure, systolic blood pressure and resting pulse. Blood pressure is measured in millimeters of mercury (mmHg) while resting pulse is measured in beats per minute. High blood pressure is predictive of heart disease, heart failure, stroke, and kidney failure.²⁶ A detailed review by Colhoun, Hemingway, and Poulter (1998) notes that 30 years of research has found a consistent connection between low socio-economic status and elevated blood pressure across several developed countries. Further research has also found a relationship between increased stress levels and blood pressure. These studies indicate elevated blood pressure among medical students taking their final licensing exams (Zeller et al., 2004), among workers in high-stress jobs (Stephoe, Cropley, and Jokes, 1999), and in families with increased financial strain (Stephoe, Brydon, and Kunz-Ebrecht, 2005). Additionally, studies have connected reduced stress levels on decreases in blood pressure (Schneider et al., 2005).

²⁵ High levels of albumin may also signal malnutrition so a lower fraction of risky albumin could be due to either reduced inflammation or improved nutrition. We believe malnutrition is not a problem in our sample given the high obesity rates in this population. In the pre-EITC expansion period, roughly 30 percent of the women in our sample are obese and 70 percent are overweight. In contrast, there are only 6 percent of women in our sample during this time period that report body-mass indexes of 20 or under.

²⁶ http://www.nhlbi.nih.gov/health/dci/Diseases/Hbp/HBP_WhatIs.html

Elevated resting pulse rates are predictive of future coronary heart disease and other cardiovascular events (Gillum et al., 1991). Seeman et al. (2008) found a strong negative relationship between education, income and elevated pulse rates. Respondents are defined to have risky blood pressure if the systolic levels are 90 and above or the diastolic levels are 140 and above. Likewise, a resting pulse rate of 90 beats or more per minute is considered risky. The sample means in Table 7 indicate that only about 4 percent of mothers have elevated blood pressure but roughly 11 percent have an elevated pulse rate. Approximately one in six mothers in our sample has at least one risky cardiovascular condition.

The third group of biomarkers indicates metabolic disorders and the conditions for this category include total cholesterol, the concentration of high density lipoproteins (HDL) and the concentration of glycated hemoglobins. Total cholesterol and HDL are measured in mg/Dl. Research has detected a connection between periods of mental and physical stress and cholesterol levels. Average cholesterol are higher among medical students taking academic examinations (Grundy and Griffin, 1959), among male accountants during times periods surrounding urgent tax deadlines (Friedman et al., 1958), during periods of mental stress (Muldoon et al., 1995), and during periods of increased unemployment risk (Mattiasson et al., 1990). This work demonstrates that changes in HDLs in response to stress is less consistent. Total cholesterol levels of 240 mg/Dl and above and HDL levels below 40 are thought to increase risk and one in ten mothers have elevated cholesterol while one in 7 have elevated HDLs.

The third biomarker in this group is the level of glycated hemoglobin (HbA1c) which is a substance in red blood cells that is created when glucose attaches to hemoglobin (the protein in red blood cells that carries oxygen). This biomarker is measured as percent of the red blood cells that are composed of HbA1c and it is thought to be a better long-term measure of blood glucose than the point-in-time glucose tests done on a daily basis by diabetic patients. Elevated levels of HbA1c are associated with eye damage, kidney disease, heart disease, nerve damage, and stroke.²⁷ HbA1c levels have been found to be inversely associated with SES status. Kelly et al. (2000) used data from the NHANES 1999-2000 and found that HbA1c levels among non-diabetics were correlated with a variety of measures of SES. Research has also shown that changes in on-the-job stress can alter HbA1c levels in the blood (Netterstrom et al., 1991; Kawakami et al., 2000). Concentrations of HbA1c of 6.4 percent or above are thought to be

²⁷ <http://www.nlm.nih.gov/medlineplus/ency/article/003640.htm>

risky but only 2.6 percent of women have elevated levels of this biomarker. Although there are low levels of risky biomarkers for each of the elements in this group, the fraction of women in the sample with any risky cardiovascular biomarker is about 25 percent.

In the final group of biomarkers, we generate aggregate measures of risk by summing the number of risky conditions across all 8 biomarkers. Aggregating the data in this manner is suggested by medical research which has shown that this count has more predictive capacity than the individual variables themselves. This measure is sometimes referred to as measure of “Allostatic load” (McEwen and Stellar, 1993; McEwen, 1998). Researchers have found that the strains and stressors that accompany lower socioeconomic status are related to higher Allostatic loads (Evans, 2003). Research has demonstrated that those currently with low SES tend to have higher Allostatic loads (Geronimus et al., 2006; Seeman et al., 2008) while Singer and Ryff (1999) found those with a history of low socioeconomic status had higher Allostatic load levels in midlife. Crimmins, Kim and Seeman (2009) found higher Allostatic loads predicted a greater risk of mortality over a 6 to 12 year follow-up period while Karlamanga et al. (2010) found that all cause mortality was monotonically increasing in an Allostatic load measure containing nine biomarkers.

Medical studies show that unweighted count scores across a variety of biomarkers do a better job of predicting future outcomes such as mortality than any individual measure (Seeman, Singer, and Rowe, 1997; Berenson et al., 1998). Therefore, we sum all 8 risky biomarker measures into a composite score. In our sample, the average respondent has 1.2 risky conditions with this number ranging from 0 to 7. Two-thirds of women have at least one risky condition, a third have two or more and an eighth have at least three conditions. Results for this composite measure of biomarkers provides the most complete picture of the health effects of the EITC expansion. The use of a composite measure of risk allows us to more effectively aggregate information into a single metric and hence increase the power of our test.

In Table 8, we report estimates for regression-adjusted difference-in-difference models of the effect of the EITC expansion on maternal health as measured by Allostatic load—the aggregate measures of risky biomarkers thought by the medical community to be most predictive of negative health outcomes. The sample includes women aged 21-40 with a high school degree or lower. The covariates in these models include dummies for the survey year plus the mother’s age, race, marital status and number of children. The treatment effect is captured by a simple interaction: respondents with two or more children in the final three NHANES surveys. In all

models, we estimate standard errors that allow for an arbitrary form of heteroskedasticity across observations.

In the first three rows of the table, we report estimates from linear probability models where we estimate the impact of EITC expansion on having one or more, two or more, or three or more negative conditions. For the first two models, we estimate that the EITC expansion increased the probability of having one or more or two or more conditions by 9 percentage points, and both of these results have p-values less than 0.10. Moving to three or more conditions, the marginal effect declines to 6.1 percentage points (p-value of 0.118) but the impact as a percent of the baseline sample mean is very large (60 percent). In the fourth row of the table, we utilize the total number of counts as the dependent variable and estimate a simple Poisson model that explicitly accounts for the count nature of the data. In this case, the coefficient on the EITC expansion suggests that counts of risky conditions are 23 percent lower for mothers who received the larger EITC payments. This estimate is statistically significant at a p-value of 0.015.²⁸

The results in Table 8 suggest a large increase in the quality of the biomarkers for mothers impacted by the EITC expansions. In Table 9, we attempt to disaggregate the data and detect the source of this advantage by estimating results for particular metabolic, cardiovascular, and inflammation disorders in that order for the same sample as in Table 8. It is important to note that any one of these individual biomarkers is less indicative of health than the aggregate measures above, and given the small NHANES sample size these biomarker-level results are less precisely estimated.

We group results by metabolic, cardiovascular and inflammation disorders. For each group, we estimate linear probability estimates for whether the respondent has a risky level of the individual biomarker, a linear probability for whether the person has any risky biomarker in the group, and a count-data model for total subgroup counts.

²⁸ The Poisson model is restrictive in that the expected value of outcomes to equal the variance. In many cases, data is subject to over-dispersion where the variance grows faster than the mean and when over-dispersion is present, imposing the Poisson distribution on the data will tend to bias standard error estimates down (Hausman et al., 1984). In our sample, over-dispersion is not an issue since the maximum count value is 7. Estimating the model with a negative binomial model allows for a variance to mean ratio of $1+\delta$ but if $\delta=0$ the model collapses to the Poisson. In our case, when the model is estimated as a negative binomial, we estimate δ to be 0.054 with a standard error of 0.028 indicating some but very little over-dispersion. It is therefore no surprise that we estimate a value of the EITC expansion treatment to have a coefficient (standard error) of -0.234 (0.096) in the negative binomial model. As in the results from the BRFSS data above, the PDF in our case is censored in that by construction, counts vary only from 0 to 8. Programming a maximum likelihood version of this censored Poisson model, we estimate a value of the EITC expansion coefficient (standard error) that equals -0.236 (0.098).

Among metabolic disorders, we find a persistent decline in risky biomarkers (cholesterol, HDL and glycated hemoglobin) but in all cases, the standard errors are larger than the parameter estimates. The estimated effect for having any metabolic disorder is large but is statistically insignificant. Similarly, the estimated EITC treatment effect from a Poisson model with the outcome the number of metabolic disorders is large but statistically insignificant.

The second block of results in Table 9 contains estimates for the presence of cardiovascular disorders. The results suggest a 3.2 percentage point decrease in the probability of reporting high diastolic blood pressure. This estimate is statistically significant at a p-value of 0.10. While no other results in this section are statistically significant, the coefficient for the Poisson model is large, negative, but with a large p-value.

The final block of results is for the presence of inflammation biomarkers. All of the results in this section are statistically significant at least with a p-value of 0.10. The estimates suggest that the expansion of the EITC decrease the probability of reporting risky levels of Albumin by 8.8 percentage points. The estimated effect on CRP is a decrease of 8.3 percentage points. Both of these results are statistically significant at a p-value of 0.10. The probability of reporting any risky inflammation biomarker falls by 9.6 percentage points and the pre-expansion mean for this variable is about 50 percent. The Poisson model estimate for the number of inflammatory biomarkers shows that the EITC expansion is associated with a 21.7 percent decrease in the number of these biomarkers (p-value < 0.05). These results for inflammatory biomarkers are the most precisely estimated of the three sub-groupings which is not surprising given the high incidence rate for these outcomes relative to the other biomarkers. As we note above, the medical literature has also found that inflammatory biomarkers are independently predictive of outcomes such as heart attacks, strokes, and mortality (Tracy et al., 1997; Tice et al., 2003; Ridker et al., 2002; Schmidt et al., 2002; Ridker, 2003) so there are a vast array of physical insults that can be captured by these outcomes.

While there are a large number of estimates in Table 9, the analysis primarily considers the effect of the EITC on eight biomarkers using different specifications. Given that none of the linear probability estimates of the impact of the EITC on an individual risky biomarker are statistically significant at the 95% confidence level, any correction for multiple comparisons such as the Bonferroni adjustment will reduce the confidence in these results. There is however a striking persistence in the biomarker results. In seven of eight cases, risky levels declined for mothers in the treatment group. If obtaining a negative estimate in any regression is a Bernoulli

draw with a probability of 0.5, the probability we would obtain seven or more negative coefficients is only 3.5 percent. In four of eight cases we obtained estimates that were statistically significant at the 10 percent level. The probability of obtaining four estimates of the same sign with this precision from eight trials is 0.04 percent. The persistence in these individual level results, even with a general lack of precision, helps explain why the results are so much more precise when we aggregate the biomarkers into Allostatic load.

IX. Conclusion

One of the more promising avenues that can potentially explain the pathway linking SES status and health involves stress. A large medical literature has demonstrated that those in poor economic conditions exhibit more stress and this manifests itself in physiological transformations in the body. Those with more stress tend to have higher pulse, higher blood pressure, higher cholesterol and more inflammation—physiological conditions that are predictive of future disease incidence and mortality. The literature to date has primarily generated a number of robust correlations but this work has failed provide convincing evidence that exogenously changing underlying economic conditions would alter markers of stress. In this paper, we exploit the OBRA93 expansions of the EITC that gave dramatically more money to families with two or more children compared to other families with one child to examine whether this change in income translates into better health. Utilizing self-reported data from the large sample of respondents to the BRFSS, we find that the expansion of the EITC decreased the number of reported bad mental health days for mothers with a high school degree or lower and two or more children compared to a similar woman with only one child. Suggestive evidence was also found that the increase in payments increased the probability of reporting excellent or very good health status. We also find strong evidence that the expansion of the EITC lowered the counts of the total number of risky biomarkers for women with two or more children and a high school degree or less compared to similar women with only one child. These effects were strongest for measures of inflammation and suggestive evidence was found for a decrease in women with risky levels of diastolic blood pressure.

This work also creates a new dimension to the understanding of the EITC and other income maintenance programs. While a vast literature has developed about this large program, its potential effect on health has gone relatively unnoticed. The results above demonstrate a new dimension of benefits that can accrue from income support programs. Given that the

determination of the size of these programs results from an implicit discussion of costs and benefits, demonstrating a clear (and previously not discussed) set of benefits from the nation's largest anti-poverty can lead to a more fruitful and concrete discussion about appropriate program. This could lead to more optimal allocation of government resources. The results also highlight that from a statistical standpoint, there is tremendous amount that can be gained by aggregating many different biomarkers into omnibus measures of health. The literature on Allostatic load has stressed the enhanced predictive power of aggregating multiple measures into one outcome rather than any one measure in isolation. In much the same way, although there was a consistent pattern in results across most of the eight biomarkers used in this analysis, few were statistically significant. We did, however, obtain much more precise estimates of a reduction in aggregate poor health from the combined measured of risk than from any individual marker in particular.

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Figure 1: EITC Payments for Families with 2 or more Children
1993 and 1996

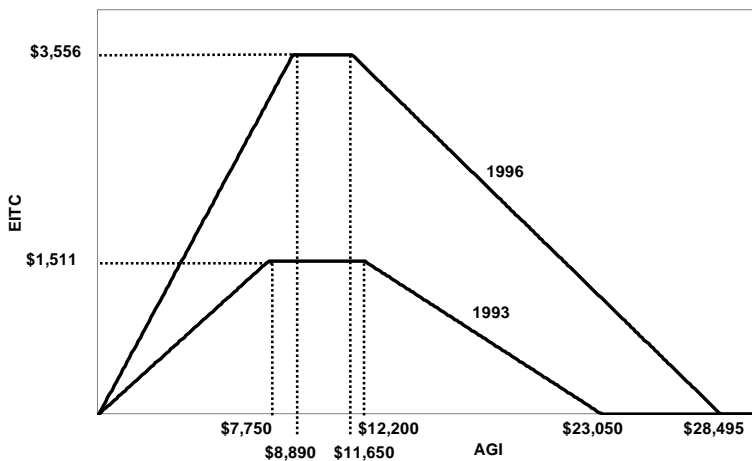


Figure 2: EITC Payments for Families with 1 Child
1993 and 1996

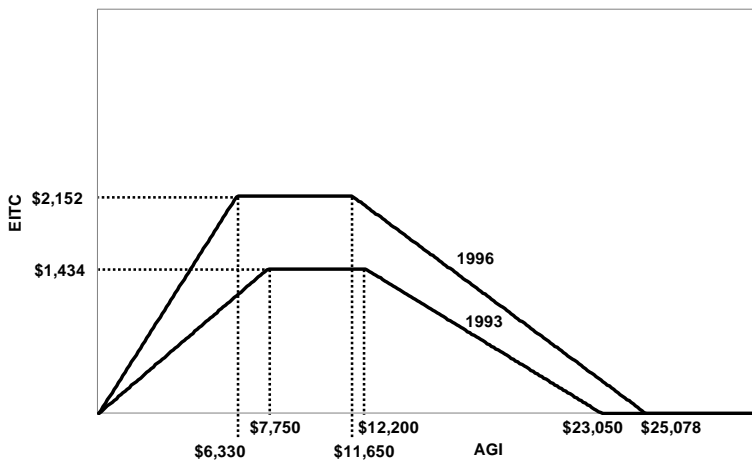


Figure 3: Difference in EITC Benefit in Families with Different Numbers of Children, 1996 and 1993

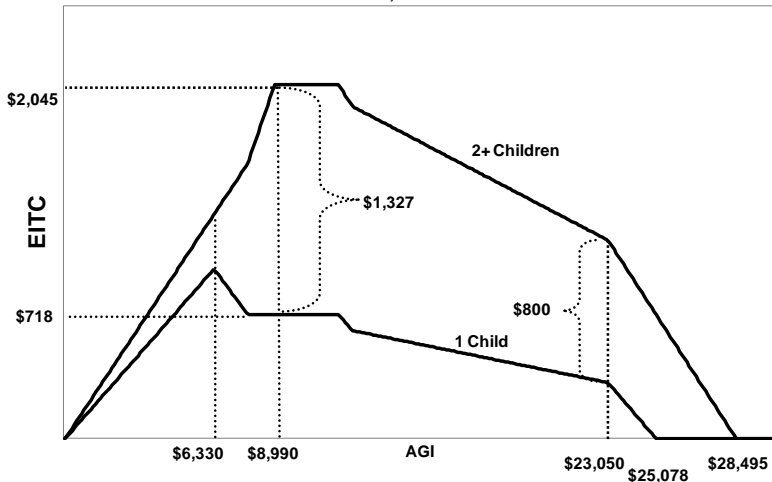


Table 1
Earned Income Tax Receipt by Education and Number of Children, Women Age 21-40
Tax Years 1993-1995 and 1998-2001

	High School Diploma		College Graduate	
	I Child	2 + kids	I Child	2 + kids
<i>Percent Receiving the EITC</i>				
Tax Years 1993-1995	27.23	22.1	7.36	4.6
Tax Years 1998-2001	28.76	26.54	6.4	4.51
<i>Size of EITC Payment</i>				
Tax Years 1993-1995	\$316.95	\$306.84	\$72.89	\$53.63
Tax Years 1998-2001	\$420.08	\$585.88	\$83.42	\$80.17
<i>Size of EITC Payment Among Recipients</i>				
Tax Years 1993-1995	\$1,164.12	\$1,388.33	\$989.75	\$1,174.61
Tax Years 1998-2001	\$1,460.54	\$2,207.28	\$1,306.54	\$1,776.65

Source: Current Population Survey 1994-1996 and 1999-2002

Table 2
Sample Characteristics, Women Aged 21-40 with Children, 1993-1996 BRFSS

Variable	≤HS Education			College Graduates		
	I Child	2 + kids	p-value	I Child	2 + kids	p-value
Average Age	31.0	32.0	0.000	32.2	34.6	0.000
% currently employed	0.679	0.580	0.000	0.796	0.714	0.000
Race						
% White, non Hispanic	0.761	0.713	0.000	0.789	0.846	0.000
% Black, non-Hispanic	0.129	0.141	0.139	0.104	0.069	0.000
% Hispanic	0.076	0.101	0.006	0.044	0.036	0.070
% Other race	0.033	0.044	0.036	0.063	0.049	0.002
Marital Status						
% married	0.548	0.655	0.000	0.709	0.846	0.000
% sep./div./widowed	0.238	0.210	0.000	0.162	0.120	0.000
% never married	0.183	0.111	0.000	0.111	0.025	0.000
Family income						
% <\$20K	0.405	0.386	0.057	0.110	0.082	0.000
% ≥\$20K, <\$50K	0.421	0.442	0.063	0.441	0.410	0.002
% ≥\$50K	0.078	0.086	0.060	0.374	0.432	0.000
% income missing	0.096	0.088	0.016	0.075	0.076	0.961
Health outcome						
% Excellent/very good health	0.582	0.577	0.446	0.805	0.809	0.580
% with any bad mental health days ^a	0.432	0.447	0.039	0.418	0.424	0.603
% with any bad physical days ^a	0.351	0.343	0.218	0.357	0.356	0.969
# of bad mental health days ^a	4.27	4.52	0.030	2.93	2.89	0.753
# of bad physical days ^a	2.81	2.65	0.072	1.89	1.95	0.656
Observations	7,315	15,737		3,881	6,740	

^aIn past 30 days.

The P-value is for the test of the null hypothesis that the means across the samples are the same. The test is performed allowing for an arbitrary correlation for observations within a state.

Table 3
Sample Characteristics, Women Aged 21-40 with Children,
1993-2001 March CPS and BRFSS

Variable	March CPS	BRFSS	P-value
Average Age	31.9	31.8	0.044
% < high school degree	29.9%	21.2%	0.000
% married	68.0%	59.4%	0.000
% with 2+ kids	69.8%	69.3%	0.276
% currently in labor force	64.8%		
% currently working	59.3%	63.8%	0.003
Race			
% White, non Hispanic	58.3%	69.0%	0.005
% Black, non-Hispanic	12.1%	13.6%	0.192
% Hispanic	25.5%	12.9%	0.003
% Other race	4.1%	4.5%	0.555
Observations	65,713	82,907	

The P-value is for the test of the null hypothesis that the means across the samples are the same. The test is performed allowing for an arbitrary correlation for observations within a state.

Table 4
 Difference-in-Difference Estimates for Labor Supply Outcomes,
 Mother Aged 21-40 with a High School Education or Less,
 1993-2001 March CPS and BRFSS

	March CPS		BRFSS
	Currently in labor force	Currently Employed	Currently Employed
Single mothers	20,998 observations		33,690 obs.
Simple DD estimate	0.0679 (0.0126) [<0.0001]	0.0640 (0.0140) [<0.0001]	0.0453 (0.0108) [<0.0001]
Regression-adjusted DD estimate	0.0595 (0.0123) [<0.0001]	0.0534 (0.0133) [<0.0001]	0.0457 (0.0110) [<0.0001]
Pre-expansion mean for treatment group	0.538	0.445	0.554
Married mothers	44,715 observations		49,217 obs.
Simple DD estimate	0.0053 (0.0099) [0.588]	0.0063 (0.0097) [0.514]	0.0099 (0.0106) [0.354]
Regression-adjusted DD estimate	0.0088 (0.0110) [0.420]	0.0102 (0.0110) [0.357]	0.0183 (0.0109) [0.099]
Pre-expansion mean for treatment group	0.612	0.569	0.593
All mothers	65,713 observations		82,907
Simple DD estimate	0.0128 (0.0079) [0.108]	0.0141 (0.0083) [0.089]	0.0170 (0.0073) [0.023]
Regression-adjusted DD estimate	0.0130 (0.0082) [0.112]	0.0129 (0.0087) [0.140]	0.0203 (0.0074) [0.008]
Pre-expansion mean for treatment group	0.591	0.534	0.580

Standard errors are reported in parentheses and p-values on the test of the null that the coefficient is zero are reported in square brackets. All standard errors allow for arbitrary correlations between observations within the same state.

Other covariates in the DD model include: Complete set of dummies for age, race, marital status, and number of children for the respondent, plus a complete set of dummies for the year of survey, and state of residence. In the BRFSS models, we also include a complete set of month of survey effects.

*P-value<0.10, **P-value<0.05, ***P-value<0.001.

Table 5
 Difference-in-Difference OLS and Negative Binomial Estimates
 Women age 21-40, 1993-2001 BRFSS

Outcome	Pre-expansion mean of outcome for treatment group	Estimation Method	Difference-in-Difference Estimates (82,907 obs.)	
			Simple	Regression Adjusted
Currently employed?	0.580	OLS	0.0170 (0.0073) [0.023]	0.0204 (0.0074) [0.008]
Excellent/very good health?	0.577	OLS	0.0095 (0.0079) [0.233]	0.0135 (0.0075) [0.078]
# bad mental health days in past month	4.52	Neg. Binomial	-0.0474 (0.0306) [0.121]	-0.0754 (0.0328) [0.021]
# bad physical health days in past month	2.65	Neg. Binomial	0.0140 (0.0390) [0.719]	0.0105 (0.0390) [0.788]

Standard errors are reported in parentheses and p-values on the test of the null that the coefficient is zero are reported in square brackets. All standard errors allow for arbitrary correlations between observations within the same state.

Other covariates in the Difference-in-Difference model include: Complete set of dummies for age, race, marital status, and number of children for the respondent, plus a complete set of dummies for the month of survey, year of survey, and state of residence.

Table 6
Robustness Tests, Women age 21-40, 1993-2001 BRFSS

	Method	DD results 82,907 obs.	DD Results StateYear 82,907 obs.	DD Results Married 49,217 obs.	DD Results Single 33,690 obs.	DDD Results 127,209 obs.
Currently employed?	OLS	0.0204 (0.0074) [0.008]	0.0206 (0.0076) [0.009]	0.0183 (0.0109) [0.099]	0.0457 (0.0110) [<0.0001]	0.0130 (0.0123) [0.298]
Excellent/very good health?	OLS	0.0135 (0.0075) [0.078]	0.0119 (0.0075) [0.118]	0.0211 (0.0099) [0.021]	0.0082 (0.0127) [0.521]	-0.0045 (0.0114) [0.695]
# bad mental health days in past 30 days	Neg. Bin.	-0.0754 (0.0328) [0.021]	-0.0615 (0.0326) [0.059]	-0.1027 (0.0519) [0.048]	-0.0514 (0.0519) [0.322]	-0.0622 (0.0729) [0.394]
# bad physical health days in past 30 days	Neg. Bin.	0.0105 (0.0390) [0.788]	0.0249 (0.0391) [0.525]	0.0432 (0.0508) [0.394]	-0.0377 (0.0675) [0.577]	0.1226 (0.0911) [0.178]

Standard errors are reported in parentheses and p-values on the test of the null that the coefficient is zero are reported in square brackets. All standard errors allow for arbitrary correlations between observations within the same state.

Other covariates in the Difference-in-Difference model include: Complete set of dummies for age, race, marital status, and number of children for the respondent, plus a complete set of dummies for the month of survey, year of survey, and state of residence.

Other covariates in the Difference-in-Difference-in-Difference model include: Complete set of dummies for age, race, marital status, education, and number of children for the respondent, a complete set of dummies for the month of survey, year of survey, state of residence, plus interactions between the education and the year effects, the number of children and the year effect, the education and number of children effects.

Table 7
Biomarkers for Mothers Aged 18-40 with a High School Education or Less,
NHANES III, 1999/2000, 2001/2002, 2003/2004

Biomarker	Measured in:	Obs.	Sample mean	Risky levels of biomarker	% with risky levels
Measures of inflammation					
C-reactive protein ^a (CRP)	mg/Dl	2,950	0.573	≥ 0.3 mg/Dl	0.437
Albumin	g/Dl ^b	2,935	4.07	< 3.8 g/Dl	0.262
# of risky inflammation conditions		2,934	0.699		
Any risky inflammation conditions		2,934	0.526		0.526
Measures of cardiovascular conditions					
Diastolic blood pressure	mmHg ^c	2,947	69.3	≥ 140 mmHg	0.046
Systolic blood pressure	mmHg	2,952	112.2	≥ 90 mmHg	0.035
Resting pulse	Beats/minute	3,090	74.97	≥ 90 BPM	0.108
# of risky cardiovascular conditions		2,947	0.184		
Any risky cardiovascular conditions		2,947	0.155		0.155
Measures of metabolic conditions					
Total cholesterol	mg/Dl ^d	2,949	189.95	≥ 240 mg/Dl	0.102
High density lipoproteins	mg/Dl	2,942	53.62	< 40 mg/Dl	0.156
Glycated hemoglobin	percent	2,992	5.2	≥ 6.4%	0.026
# of risky metabolic conditions		2,933	0.283		
Any risky metabolic conditions		2,933	0.259		0.259
Aggregate risks					
# of risky conditions		2,683	1.156		
1 or more risky conditions		2,683	0.657		0.657
2 or more risky conditions		2,683	0.333		0.333
3 or more risky conditions		2,683	0.127		0.127

^aTo make the data sets comparable over time, we censored the lower values of C-reactive protein at 0.21 in the final three NHANES samples.

^bg/Dl=Grams per deciliter

^cmmHg = Millimeters of mercury

^dmg/Dl=Milligrams per deciliter

Table 8
Regression-adjusted DD and DDD Estimates for Effect of EITC expansion on Allostatic Load
Women Aged 21-40, NHANES III, 1999/2000, 2001/2002, 2003/2004

Outcome	Pre-expansion mean for treatment group	DD	DDD
One or more risky conditions	0.640	-0.091 (0.050) [0.066]	-0.093 (0.072) [0.199]
Two or more risky conditions	0.305	-0.094 (0.053) [0.074]	-0.054 (0.071) [0.448]
Three or more risky conditions	0.108	-0.061 (0.039) [0.118]	-0.038 (0.053) [0.507]
Poisson Model: Total # risky conditions	1.092	-0.234 (0.096) [0.015]	-0.207 (0.150) [0.169]

Standard errors are reported in parentheses and p-values on the test of the null that the coefficient is zero are reported in square brackets. All standard errors allow for arbitrary form of heteroskedasticity. Other covariates in the DD model include: Complete set of dummies for age, race, marital status, and the year of survey. Other covariates in the DDD model include: Complete set of dummies for age, race, marital status, education, plus interactions between the education and the year effects, the number of children and the year effect, the education and number of children effects.

Table 9
Regression-adjusted DD and DDD Estimates for Individual Biomarkers,
Women Aged 21-40, NHANES III, 1999/2000, 2001/2002, 2003/2004

Outcome	Pre-expansion mean for treatment group	DD	DDD
<i>Metabolic Biomarkers</i>			
Risky Glycated Hemoglobin	0.026	-0.004 (0.013) [0.773]	-0.012 (0.019) [0.529]
Risky Total Cholesterol	0.102	-0.022 (0.034) [0.520]	0.043 (0.046) [0.351]
Risky HDL	0.156	-0.027 (0.036) [0.453]	-0.044 (0.047) [0.348]
Any risky metabolic condition	0.251	-0.042 (0.045) [0.348]	-0.007 (0.078) [0.776]
Poisson Model: # risky metabolic conditions	0.277	-0.185 (0.177) [0.294]	0.030 (0.276) [0.914]
<i>Cardiovascular Biomarkers</i>			
Risky Diastolic Blood Pressure	0.045	-0.032 (0.017) [0.059]	-0.030 (0.026) [0.251]
Risky Systolic Blood Pressure	0.035	0.004 (0.014) [0.800]	-0.0005 (0.023) [0.984]
Risky Pulse	0.108	-0.016 (0.037) [0.669]	-0.043 (0.049) [0.381]
Any risky cardiovascular condition	0.131	-0.034 (0.041) [0.407]	-0.048 (0.055) [0.392]
Poisson Model: # risky cardiovascular conditions	0.164	-0.317 (0.233) [0.174]	-0.423 (0.343) [0.217]
<i>Inflammation Biomarkers</i>			
Risky Albumin	0.262	-0.088 (0.045) [0.052]	-0.087 (0.063) [0.167]
Risky C-Reactive Protein	0.437	-0.083 (0.050) [0.098]	-0.012 (0.070) [0.778]
Any risky inflammatory condition	0.493	-0.096 (0.050) [0.057]	-0.060 (0.071) [0.402]
Poisson Model: # risky inflammatory conditions	0.493	-0.217 (0.099) [0.029]	-0.136 (0.159) [0.394]

Standard errors are reported in parentheses and p-values on the test of the null that the coefficient is zero are reported in square brackets. All standard errors allow for arbitrary form of heteroskedasticity. Other covariates in the DD model include: Complete set of dummies for age, race, marital status, and the year of survey. Other covariates in the DDD model include: Complete set of dummies for age, race, marital status, education, plus interactions between the education and the year effects, the number of children and the year effect, the education and number of children effects.