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GIVING MOM A BREAK: THE IMPACT OF HIGHER EITC PAYMENTS ON MATERNAL HEALTH

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ABSTRACT

The 1993 expansions of the Earned Income Tax Credit created the first meaningful separation in the benefit level for families based on the number of children, with families containing two or more children now receiving substantially more in benefits. 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. Using data from the Behavioral Risk Factors Surveillance Survey, we find in difference-in-difference models that for low-educated mothers of two or more children, the number of days with poor mental health and the fraction reporting excellent or very good health improved relative to the mothers with only one child. Using data from the National Health Examination and Nutrition Survey, we find evidence that the probability of having risky levels of biomarkers fell for these same low-educated women impacted more by the 1993 expansions, especially biomarkers that indicate inflammation.

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I. Introduction

From its relatively humble beginnings in the 1970s as a $400 reimbursement for payroll taxes, the federal Earned Income Tax Credit (EITC) has grown to be the nation's largest anti-poverty program. The 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 accounted for $49 billion in payments to 24 million people.[1] This is approximately 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.

This study exploits a unique feature of the 1993 EITC expansion to examine the impact of income on health of mothers in low income families. Unlike previous expansions of the EITC, the 1993 law created the first meaningful separation in benefit levels for families based on the number of children, with families of two or more children receiving substantially more in payments. 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) in their analysis of the 1993 expansions on female 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 the self-reported health of eligible women with two children compared to similar women with only one child. For example, we find decreases in the probability of having a bad mental health day in the previous month and increases in the probability of reporting excellent or very good health, results that are statistically significant at a p-value of 0.1. Using count data

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[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).

models, we also find statistically significant reductions in the number of poor mental health days (p-value<0.05).

Despite the fact that self-reported measures of health are predictive of objective medical outcomes such as mortality, a number of authors have noted the limitations of this class of variables. As a result, researchers in the health disparities literature have recently begun focusing more closely on biomarkers and other measured health indicators as the outcome of interest. In this spirit, we 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 (diastolic and systolic blood pressure and resting heart rate), metabolic disorders (total cholesterol, HDL and glycated hemoglobin) and inflammation (albumin and c-reactive protein levels). The expansion of the EITC is associated with a large and statistically significant decrease in the counts of risky biomarkers, especially for the measure of inflammation. These inflammation biomarkers have been shown to be independently predictive of a wide range of conditions including heart attacks and strokes (Colhoun, Hemingway, and Poulter, 1998; Crimmins, Kim and Seeman, 2009; Danesh et al., 1998; Gabay et al., 1999; Ridker, 2003; Onat, 2008).

The results and methods in this paper contribute to two distinct literatures. The first is a literature that examines the economic consequences of the EITC. Given its scope and rapid increase in size, it is not surprising that the EITC has attracted the attention of academics and policymakers. Analysts have examined the impact of the EITC on outcomes as diverse as labor supply (Scholz, 1996; Eissa and Liebman, 1996; Meyer and Rosenbaum, 2001; Eissa and Hoynes, 2004; Keane and Moffitt, 2004), fertility (Baughman and Dickert-Conlin, 2009), marriage (Dickert-Conlin, 2002), living arrangements (Ellwood, 2000), poverty (Scholz, 1994; Neumark and Wascher, 2001), educational attainment (Dahl and Lochner, 2008), and spending patterns (Barrow and McGranahan, 2000; Smeeding et al., 2000). Even with this breadth of outcomes, little attention has been paid to the effect of these large increases in income on the health of recipients. This is despite the fact that improving the living conditions of low-income individuals and their children is an explicit objective of the EITC. 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" (Clinton, 1993). To the extent that the size of federal income support programs are determined based on net benefits, ignoring this important dimension will lead to suboptimal program sizes.

Currently, considerations of the health effects generated by government programs are largely concentrated on those programs that directly affect the provision of medical services such as Medicaid (Currie and Gruber, 1996a,b) and Medicare (Card et al., 2009), 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). When the non-economic impacts of income support programs are considered, they tend be focused on non-health outcomes. 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." This current work advances the understanding of the causal relationship between income and health. Currently, there is a voluminous literature concerning socioeconomic status and health. This literature, with contributions from a variety of disciplines, has established that health outcomes are much better among individuals with higher socioeconomic status. A relationship between health and socio economic status (SES) has been documented for virtually all measures of health and health habits including mortality (Backland, Sorlie and Johnson, 1996), self-reported health status (House, Kessler, and Herzog, 1990), measures of child health (Case, Lubotsky and Paxson, 2002), smoking (Chaloupka and Werner, 2000), obesity (Chang and Lauderdale, 2005) exercise (Brownson et al., 2001), incidence of disease (Banks et al., 2006), a variety of cardiovascular risk factors (Karlamangla et al. 2005) and a variety of biomarkers (Steptoe et al., 2002a and 2002b; Muenning, Sohler and Mahato, 2007; Seeman et al., 2008).

Despite the robust correlations, the literature has failed to definitively answer whether the income/health gradient represents a causal mechanism or whether low income reflects some other underlying root cause that cannot be rectified with more income. Those with more income or higher education 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 (Fuchs and Farrell, 1992). Likewise, health shocks reduce both health status and income so poor health may cause lower income rather than the other way around (Bound, 1989; Haveman et al., 1995; and 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 SES is a casual factor in health by exploiting quasi-experimental variation in income and education. For example, authors have examined whether the increase in education generated by policies such as compulsory schooling (Adams, 2002; Lleras-Muney, 2005), an increase in access to colleges (Currie and Moretti, 2003) and the Vietnam Draft (de Walque, 2007; Grimand and Parent, 2007) have altered health outcomes. These papers all find improved health outcomes from greater education. In contrast, Royer and Clark (2009) showed that a large change in education produced by an increase in compulsory education in the U.K. had no impact on adult mortality.

Similar work exploits variation in income produced by such factors 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 caused by a phylloxera outbreak in the wine growing regions of France in the 1800s (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).

Unfortunately, the results from these papers are much less consistent concerning the role that income plays in health with some finding large benefits (e.g., from lottery winnings and South African pension expansions) some finding no impact (from inheritances) and others finding an increase in mortality from higher income (in the case of higher Social Security payments in the US). The disparity in the results for this literature is best illustrated in Adda et al. (2009) who found that an increase in the permanent income for cohorts has no impact on predictors of mortality such as self-reported health status, self-reported chronic conditions, but it increased smoking and reduced mortality.

In general, the conflicting evidence regarding the causal impacts of income on health is possibly due to at least two factors: the primary focus on mortality as the outcome of interest and/or the use of uncommon sources of variation in income. The problem of using mortality as an outcome is that, while it is an easily verifiable and obviously not self-reported outcome, it is rare among many demographic groups. Therefore, failing to detect a causal effect of income on mortality could be a Type-II error. This is of particular concern for the relatively young groups under consideration in this analysis. The second concern about previous work on the effect of income on health is the use of unique sources of quasi-experimental variation. Authors have used shocks to income such as winning the lottery (Lindahl, 2005), being awarded the Nobel Prize (Rablan and Oswald, 2008), receiving inheritances (Meer, Miller, and Rosen, 2003), and living through the reunification of Germany following the Cold War (Frijters, Haisken-DeNew and Shields, 2007). While these events may have the benefit of being uncorrelated with underlying characteristics of beneficiaries, some are not particularly replicable and the results might therefore have limited external validity. In contrast, the source of variation in this paper is a change in income resulting from a large income maintenance program—an event affecting tens of millions of low income Americans every year.

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 in this area. 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." Any existence of a causal relationship between health and income will be useful for understanding the full effect of a broad range of income support programs.

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

The federal EITC is the nation's largest anti-poverty program. Unlike traditional cash assistance programs, the EITC is a refundable tax credit available only to individuals with positive earnings. The program began in 1975 as a small credit—up to $400—to reimburse low-income families for their payroll tax contributions. By 2008, this program had grown and the federal government transferred $49 billion through the EITC in income to 24 million American families.

The general structure of the EITC is as follows. For low levels of income, EITC recipients are in a "phase-in" region where the amount of their benefit increases along with their income. After the maximum benefit level is reached, recipients are in a plateau region where the size of the EITC benefits neither increases nor decreases with income. Finally, after a certain level in earned income, recipients enter the "phase-out" region where the size of their benefit is decreasing in income. Policymakers can change the generosity of the EITC by altering the phase-in/phase-out rate, the maximum allowable credit, and/or the income points at which these regions begin and end.

Since its creation there have been several large expansions to the EITC. As part of the Tax Reform Act of 1986, the generosity of the EITC was greatly increased. Subsequently, as part of the Omnibus Reconciliation Act of 1990, the credit was expanded and a slightly larger benefit was created for families with two or more children. This marked the first time that benefit levels depended on family size—though this difference was small in both absolute and relative terms. In 1993, President Clinton signed the Omnibus Reconciliation Act (OBRA93)[3] that roughly doubled the size of the credit and dramatically increased the differences between the maximum available benefit available to families with two or more children as compared to families with only one child.

As a result of the OBRA93 expansion, the subsidy during the phase-in range for families with two or more children increased from 19.5 percent to 40 percent, and the maximum benefit increased from

[3] Public Law 103-66, http://thomas.loc.gov/cgi-bin/query/z?c103:H.R.2264.ENR:

$1,511 to $3,556. The impact of the 1993 expansions 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. The effect of the expansion of families with only one child is detailed in Figure 2. 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 based on the number of children that are 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. With respect to income, the difference in the maximum benefit for individuals earning $8,900 amounted to nearly 15 percent of family income.

III. Existing Literature on the Earned Income Tax Credit

There is a large literature that examines the effects of the EITC and its expansions on a wide variety of economic outcomes and an excellent review of this literature is contained 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 but a group that is expected to have similar trends in outcomes in the absence of the reform. For example, Eissa and Liebman (1996) estimated the effect of the 1986 EITC expansion on the labor supply of single women 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 couples. Meyer and Rosenbaum (2001) examine the labor supply consequences of the EITC and other federal transfer programs by comparing single women with and without children.

The results from this literature suggest that the EITC raises the labor supply of single women (Eissa and Liebman, 1996; Meyer and Rosenbaum, 2001) while depressing the labor supply of married women (Eissa and Hoynes, 2004). 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.

Despite the consensus results on labor force participation for single women, the evidence for change in hours worked is less clear with Liebman (1997) and Eissa and Leibman (1996) finding little evidence that EITC expansions altered this measure of labor supply which 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 on families on welfare. As we do in this paper, 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.

Due to the fact that the size of credit is based upon family income and size, it is possible that EITC expansions also impacted other family outcomes, but in general, there is little empirical evidence that the EITC has altered marriage or family formation rates (Dickert-Conlin and Hauser, 2003; Eissa and Hoynes, 1998; Ellwood, 2000) or fertility (Baughman and Dickert-Conlin, 2003 and 2009).

The large amount of cash transferred to working families as a result of the EITC has been demonstrated to have reduced poverty for this group (Scholz, 1994; Neumark and Wascher, 2001). Because EITC benefits are usually paid as a lump sum when recipients receive their tax refund checks, the infusion of income from the EITC has been demonstrated to affect consumption patterns among beneficiaries. Using data from the Consumer Expenditure Survey, Barrow and McGranahan (2000) estimated that one-fifth of the EITC benefits are spent during the month of receipt. 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 Effects of Income on Health

In many of the papers that utilize 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. 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). Given the small one-year mortality rate for this age group, the elasticity of mortality with respect to family income is -0.36 (-0.00064/0.0018) which is fairly large. However, the impact on mortality from even large changes in income is small in aggregate.

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 (α=0.05) reduced-form difference in mortality between the two groups.

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; McEwen, 1998) and this stress may accelerate cell aging (Epel et al., 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., 2002a and 2002b; Muennig et al., 2007; 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.

V. Constructing a Sample from the Behavioral Risk Factors Surveillance Survey

Initially, we utilize data from the Behavioral Risk Factors Surveillance Survey (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 them aggregated into a single annual file by the Centers for Disease Control (CDC). The program began in 1984, and in that year only 15 states participated in the survey. By 1994, BRFSS was collecting data from all states, DC, and three US territories. 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. BRFSS contains measures of self-reported health status as well as indicators of bad physical and mental health days in the past month.

The initial 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. The EITC expansion was passed in 1993 and became effective with tax year 1995, but because so few people collect their EITC benefits as the advanced EITC[4] and nearly all take the EITC as a refund on their federal taxes (which is received in the following calendar year), we consider 1996 as the first year when eligible families with two or more children were receiving dramatically greater EITC payments.

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 criteria 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 it to select likely eligible 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. Within BRFSS, respondents are asked to identify the number of children in the household less

[4] 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) estimates that only 3 percent of eligible taxpayers in the 2002-2004 period collected the AEITC.

than 18 years of age. 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. 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.

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.[5] 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. 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.[6] 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.

[5] The March CPS data was downloaded from www.ipums.org, King et al. (2010).
[6] 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

Table 1 also contains information about the estimated amount of the benefit received, again assuming all women eligible 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 last two rows of Table 1, women with two or more children and no high school diploma had experienced an increase in their estimated EITC payment of roughly $820 (59%). On the other 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 this group will represent the population eligible for the program in our econometric models.

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. Since 1993, BRFSS has asked respondents how many children under the age of 18 are living in the household. 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. Because the first checks under the new EITC schedule for families with two or more children are distributed in 1996, we look at data from the 1993 through 2001, giving us three years pre and six years post-EITC expansion.

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. 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.

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 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 we consider is a dummy that equals 1 if a person self-reports they are in excellent or very good health.[7] Next are two dummy variables that indicate whether the respondent reported any bad mental health or physical health days in the past 30 days.[8] The fourth and fifth 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, more likely to report any bad mental health days and have more bad mental health days. These same women are however less likely to have any bad physical health days and have lower counts of bad physical health 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 only statistically significant differences in the number of bad mental

[7] 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.

[8] The original question in the survey asks respondents how many bad mental health or bad physical heath days they had in the past month.

health days in the past month and the propensity to report any bad mental health days—and these differences are small in magnitude.

V. Econometric Models

Our initial 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 specification. Later, we also outline a difference-in-difference-in-difference specification where women likely ineligible for the EITC form a comparison sample. We construct notation that will incorporate both of these specifications.

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). Initially, within the eligible group, consider a data structure where there are two time periods (before and after expansion) and two family sizes (people with only one child versus those with two or more). These two variables are measured by dummy variables $Expand_{ji}$ (which equals 1 after the EITC has expanded and zero otherwise) and Two_{ji} (which equals 1 if a family has two or more children and zero otherwise). The dependent variable is defined as Y_{ei}, which includes outcomes only for the eligible population. We can obtain a difference-in-difference estimate with the simple equation:

$$(1) \quad y_{ei} = \alpha + Two_{ei}\phi + Expand_{ei}\theta + (Two_{ei}Expand_{ei})\delta_{dd} + \varepsilon_{ei}$$

where ε_{ei} is an idiosyncratic error and the reduced-form impact of additional income generated by the EITC is captured by δ_{dd}. We can enhance the explanatory power of the model by adding a set of covariates that describe the individual (represented by the vector X_{ei}), replace $Expand_{ei}$ with 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. Many of these differences should be captured by the state and year effects. These additions to the model lead to the following equation:

$$(2) \quad y_{ei} = \alpha + Two_{ei}\phi + \sum_{t=1993}^{2000} T(t)\pi_t + X_{ei}\gamma + \sum_{m=1}^{50} S(m)\lambda_m + (Two_{ei}Expand_{ei})\delta_{dd} + \varepsilon_{ei}$$

where again δ_{dd} is the difference-in-difference estimate of the impact of EITC expansions on health. In our results, we call the estimates from equation (1) as the simple difference-in-difference estimates and the results from equation (2) as the regression-adjusted difference-in-difference estimates.

As in any difference-in-difference 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 sample had there been no intervention. If there are unmeasured forces in the mid 1990s that differentially impacted low educated mothers with two kids more than mothers with one child (and vice versa) 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. 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 eligibly group is defined by the dummy variable $Elig_{ji}$ which equals 1 if mothers are in the lower education group. In this case, the difference-in-difference-in-difference model requires controls for group eligibility ($Elig_{ji}$), time period ($Expand_{ji}$) and treatment group (Two_{ij}), 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:

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

where X_{ji} is a vector of covariates, S(m) are state effects, and T(t) are year effects. We also allow for the unique year effects for those eligible for the EITC and those with two or more children.

The triple difference 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 in small samples such as those below this identification strategy may lead to relatively imprecise results.

VI. Labor Supply Results From the Current Population Survey and the Behavioral Risk Factor Surveillance System

The primary purpose of the paper is to examine the impact of higher EITC payments on maternal health. 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, there is only one question on the BRFSS about employment and from that question, we can construct a single measure of labor supply: whether the mother is currently employed. In this section, we briefly outline a companion sample constructed from the March Current Population Survey (CPS), a standard labor market data set that can be used to examine the role of the EITC on labor market decisions.[9] These results are presented for two primary reasons. First, obtaining similar labor supply responses to the EITC expansions in the CPS to the

[9] The data were downloaded from www.ipums.org and information about the data set can be found in King et al. (2004).

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. Household are surveyed for the same four months in a two year cycle and one-quarter of the sample exits the survey temporarily or permanently each month. For this section, we construct a sample from the March CPS that parallels the sample from the BRFSS used in the previous section. Therefore, we use data from the 1993 through 2001 March CPS and restrict the sample to women aged 21-40 with 12 or fewer years of education and children in the home. 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 CPS survey to construct the labor supply variables.[10] This question asks about the labor market status at the time of the survey which matches the employment question in the BRFSS. From the employment status variable, we construct two 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 we are able to generate in 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.[11] The samples look similar on many dimensions and very different on others. The average age and fraction of respondents with two or more children in their

[10] 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.

[11] Again, for these tests we allow for arbitrary correlation in the errors within a state.

household are very similar across the two samples. The BRFSS sample contains a smaller fraction lower-educated, lower fraction married and much lower fraction Hispanic mothers than the CPS. This last number is expected given the fact the 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. This may also be expected given the different survey design.

In Table 4, we report difference-in-difference estimates for labor supply outcomes using models based on equations (1) and (2). 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 these regression-adjusted models, we endeavor to make the covariates as similar as possible between the two data sets. In each of these regressions, 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.

In both data sets, we estimate 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 5.8 percentage points and increased current employment probabilities by 5.3 percentage points. Both of these estimates are statistically significant. This final number is very close to the estimate that we generate supply for the same outcome but with the BRFSS sample. We cannot reject the null hypothesis that these two estimates are equal. 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. 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. Within the CPS samples, we find approximately a one-percentage point increase in labor force participation and current employment in the regression-adjusted models, but neither of these estimates is statistically significant at conventional levels. In contrast, in the BRFSS sample, we find a 1.8 percentage point increase in current employment rates for married women that is statistically significant at the 10 percent level. As with single women, 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 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 that increases in labor force participation from the EITC should be primarily experienced by single women. Furthermore, they found that the labor supply of married woman decreased following the EITC expansion. This counterintuitive result is caused by the fact that the EITC combines income from both spouses into family income for the purposes of calculating benefit levels. While we do not find a negative result for married women, our labor force results are much larger and more precisely estimated for single mothers. Overall, the results of equations (1) and (2) using CPS data provide qualitatively similar results to the existing literature – specifically that the EITC is associated with an economically and statistically significant increase in labor supply and that this labor supply response is concentrated among single women.

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 1.4 (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.

Taken together, the fact that the estimates from our models applied to the CPS finds a qualitatively similar pattern of labor supply results to the existing literature and finds exceptionally similar results between the CPS and the BRFSS samples provides evidence that the health results presented below are not due to any particular characteristics of the BRFSS sample or our proposed identification strategy.

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

Table 5 contains both the simple and regression-adjusted difference-in-difference coefficients from equations (1) and (2) for a variety of outcomes. As in Table 4, we calculate standard errors allowing for within-state correlation in errors. The first row of estimates repeats the “currently employed” results from the BRFSS sample reported in Table 4 and the next five rows we report results for various self-reported health outcomes in the BRFSS. The second row of results reports estimates for the outcomes that indicates whether the respondent reports any bad mental health days in the previous 30 days. The regression-adjusted coefficient shows that following the expansion of the EITC, women with two or more children had a 1.4 percentage point decrease in the probability of reporting a bad mental health day. This result is statistically significant at a p-value of 0.10. In addition to data on any mental health days, the BRFSS contains data the total number of bad mental health days over the last 30 days. The second to last row of results 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 7.5 percent reduction in the number of bad mental health days compared to similarly educated women with only one child.[12] This estimate is statistically significant at a p-value of 0.05. The fourth and sixth columns of results contain a similar set of estimates for the presence of bad physical days. These results, however, are generally small, positive, and imprecisely estimated. The third 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. This estimate is statistically significant at a p-value of 0.10.

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. In a sample of low educated women, what would we expect an extra \$200 in income to raise self-reported health? To calculate a baseline, we used data from the 1996 through 2002 March CPS. This data set over the period in question is excellent for our purposes because it has much higher quality income data than BRFSS and starting in 1996, this survey began asking a question on self-reported health status. Using a sample of women aged 21-40 with a high school degree or less and at least one child, we regress a dummy for excel or very good health on a complete set of year, age, race, and marital status

[12] 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 $f(\beta|x_i, y_i)$ is the PDF of the negative binomial for person i and $F(\beta|x_i, 30)$ is the CDF evaluated from 0 to 30, the actual value of the likelihood for individual i is then $f(\beta|x_i, y_i)/F(\beta|x_i, 30)$. 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.

effects plus a linear term in family income. The results from this model suggest that each additional $1,000 in income raises the probability of reporting excellent/very good health by 0.2 percentage points (standard error of 0.009 percentage points). The results we've obtained in the second column of Table 5 suggest that post-expansion, the EITC raised the probability of being in excellent/very good health for women with two or more children by 1.35 percentage points, so the results in our model are an order of magnitude larger than what we would expect if the basic OLS model were an estimate of the true causal impact of income on health in this example.

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. Since there are only two year effects in this regression, the degrees of freedom in the numerator of this F-test are two. In the currently employed, any bad mental health 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, 0.85 and 0.32 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 as the Personal Responsibility and Work

Opportunity Reconciliation Act (PWRORA)[13] replaced the existing welfare program (Aid for Families with Dependent Children) with Temporary Assistance for Needy Families (TANF). The changes in the welfare program ushered in by PWRORA include placing limits on welfare, instituting family caps on benefits, mandating work requirements, increasing earnings limits and providing 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 characteristics of TANF 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.

The variation in the implementation time of welfare reform across states could potentially contaminate our estimates. We guard against this to some degree 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. To examine this issue to a greater degree, we estimate models that include state-specific year effects—which should capture any effect that is common to all low-educated mothers within a unique state/year cell. These results are contained in the second column of Table 6. The inclusion of these state-specific year effects does materially change some of our results. The estimates for currently employed, being at work, reporting excellent or very good health, and the negative binomial estimates for the total number of bad mental health days are all of similar magnitude to the original specification. However, the inclusion of state-specific year effects increases the p-values on the treatment effect in the self-reported health status and bad mental health days linear probability models to values above 0.10. The negative binomial estimate for the total number of bad mental health days now has a p-value of 0.059.

The third and fourth columns of results are for samples split by marital status and the results for the currently employed outcome by marital status are repeated from Table 4 and were discussed in the

[13] Public Law 104-193.

previous section. The estimated effect on the probability of reporting a bad mental health day is statistically insignificant for both samples. The estimate, however, is similar in magnitude across the two groups and is nearly identical in size to the statistically significant estimate for the whole sample. 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 in significant for single women, the result among married women is large (a 2.1 percentage point increase) and statistically significant at a p-value of 0.05. Across both columns, we cannot reject the null coefficient that the single and married results are different in magnitude. The relatively more precise estimates for the married sample may be caused by the fact that the sample of married mothers is nearly fifty percent larger than the sample of single mothers.

The final column of Table 6 contains the estimates for the D-D-D identification strategy in equation (3). 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 at best. 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 coefficient estimates constant from the first column of Table 6, 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) – (3) 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 these 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 (2) for the outcomes contained in Table 5 including an indicator variable for current employment as an additional covariate. Overall, these results suggest that changes in employment are not driving the results. Adding a "currently employed" dummy to the linear probability models measuring any bad mental health days and excellent/very good self-reported health status reduces the magnitude (standard error) of the treatment effect to -0.0137 (0.0084) and 0.0121 (0.0074), respectively, numbers that are only marginally smaller in magnitude that the estimates in Table 5 and remain statistically significant at the same levels. Likewise, the coefficient on the treatment effect in the negative binomial model for the number of bad mental health days when we add the "currently employed" falls in magnitude to only -0.0694 (0.0330). As these results suggest, increased work cannot explain the results in Table 5.

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. The SCHIP program was started in 1997 and it is a state/federal partnership that provides health insurance coverage to low income children and pregnant women whose incomes are too high to qualify for Medicaid.[14] SCHIP enrollment grew from 897,000 in 1998 to 3.9 million in 2003.[15] The implicit argument is that having children without health insurance induces stress for mothers and if the expansions increased coverage, then we should see a reduction in stress in a population of mothers with little education. The growth of the SCHIP program would potentially 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. This does not, however, appear to be the case. Using data from the March CPS outlined above, we matched low-educated women with children to their children aged 18 and under. This produces a sample of 171,614

[14] For an overview of the SCHIP program, please see http://www.cms.gov/LowCostHealthInsFamChild/
[15] http://www.kff.org/medicaid/upload/7348.pdf

children. For this group, we regress a dummy for whether a child has health insurance[16] on a complete set of dummies for states and years, the child's age and gender, plus a set of dummies for the mother's age, marital status, race/ethnicity and education. The key covariate is the difference-in-difference treatment effect which is defined as it was in equation (1). In models that allow for arbitrary correlations in errors within states, the coefficient (standard error) on the EITC expansion treatment effect in the full sample, children of single women and children of married women are -0.0025 (0.0064), -0.0071 (0.0091), and -0.0091 (0.0106). These estimates show that changes in health insurance coverage resulting from the expansion of the SCHIP program do not seem to be a credible alternate explanation to the EITC for the reduction in stress identified in Tables 5 and 6.

A similar argument could be made about the possible roll that Medicaid expansions have played in enhancing maternal health. Starting in the 1980s, Medicaid programs have been expanded to include a much larger portion of the population including low income mothers and pregnant women who would otherwise not have qualified for the program (Currie and Gruber 1996a and 1996b). As a result, Medicaid enrollment by adults that were not aged or disabled increased from 6 million in 1990 to 10.6 million in 2000.[17] Again, the growth in this insurance program should only prove problematic for our models if coverage for low educated mothers with two or more child grew differentially compared to mothers with only one child. Similar to the case of SCHIP, this does not appear to be the case. Using the sample of low-educated mothers from the March CPS, we estimate a linear probability model with an indicator for whether the mother had any health insurance as the outcome of interest. The coefficient (standard error) on the EITC expansion treatment effect in the full sample was -0.0000 (0.0069). Similar to the results for SCHIP, these estimates for Medicaid expansion show that changes in health insurance

[16] We would have preferred to include whether the child was enrolled in SCHIP or Medicaid but most federal surveys such as the CPS tend to under count Medicaid enrollment (Davern et al., 2009). Research has suggested that the undercount is primarily due to miscoding the source of the insurance rather than respondents confusing Medicaid with uninsurance (Call et al., 2008). LoSasso and Buchmueller (2004) present evidence that the problem is particularly pronounced for children, suggesting that because of the rise of Medicaid managed care, many with Medicaid report private insurance instead. As a result, we follow LoSasso and Buchmueller and use any insurance as the outcome of interest in this case.

[17] http://www.cms.gov/DataCompendium/15_2009_Data_Compendium.asp#TopOfPage, Table IV.8.

status caused by the newly created and expanded government programs are not the actual source of the reduced stress following the expansion of the EITC.

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, they are two potential concerns about the set of outcomes examined in the BRFSS data: all of the outcomes are self-reported and all are subjective measures of health.

Self-reported health has become a controversial method of measuring health in large datasets. On the one hand, the indicator is more easily obtained in large samples than other more objective measures of health. In addition, these data appear to be particular useful predictors of future health and 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. This was true even when indicators of morbidity were included in the analysis. 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." In a meta-analysis of 163 studies, DeSalvo et al. (2005) found that individuals with a self report health status of "poor" had a two-fold higher mortality rating than those with a status of "excellent." This relationship between self reported health and mortality was still found even after controlling for a variety of demographic factors and co-morbidities. This literature suggests that using self reported health as an outcome can provide useful information about a wide range of health outcomes.

On the other hand, some caution about should exist in using this type of data as outcomes measure. The subjective nature of self-reported health survey questions lead to a lack of comparability across individuals which is functionally the same as the introduction of classical measurement error (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. Research has found that even these data are subject to measurement error. Baker, Stabile, and Deri (2004) analyzed a unique dataset that contained self-reports of disease presence (e.g., cancer, heart disease, high cholesterol) that were matched to measured indicators of disease presence from claims data. They found that these self-reported measures produced both false positive and negative indications of disease presence.

In a study which is more pertinent to this analysis, Johnston, Propper, and Shields (2009) examined whether measurement error in self-reported measures of hypertension can explain the mixed findings regarding the income-health gradient. Using a self-reported measure of hypertension, the authors 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. Though these estimates still primarily identify correlations between income and health, they provide evidence that relying on self-reported data—even of objective health outcomes—can generate a biased estimate of the income-health gradient.

Self-reported measures of health can also be subject to systematic measurement error as well. Currie and Madrian (1999) examined the problems that labor economists have faced using variables such as self-reported health status in models explaining labor market outcomes. For example, researchers examining the role of disability and health status on labor force participation have found a "justification bias" where individuals who are not in the labor force are more likely to over-report their number of health conditions and disabilities. Baker, Stabile, and Deri (2004) found evidence that individuals who are not in the labor market report the presence of poor health in order to explain their lack of employment. Bound (1991) noted that this source of bias will lead to an overestimate of the role of health compared to other economic factors in the labor supply decision.

In addition to this justification bias, researchers have also examined whether the misreporting of both objective and subjective health outcomes are related to socioeconomic factors such as age and income. In this literature, authors have found underreporting of chronic conditions by both education and social class. (Mackenbach,Looman, and Van Der Meer, 1996; Elstad, 1996). Since our identification strategy is based on differences in both education and the number of children, it is possible that differences in self-reporting between these groups could bias results using these data.

Both arguments concerning the costs and benefits of using self reported health measures have merit. These measures clearly have some predictive power concerning mortality and therefore changes in outcomes such as health status or mental health days are important indicators of health status. However, there is potentially a tremendous amount of measurement error in these variables which could provide a biased estimate of the effects of income on health. Perhaps most troubling is that due to the differential effects between the two potential types of measurement error, the bias in these estimates is in an unknown direction.

A second shortcoming of omnibus measures of health such as self reported health or the number of bad mental health days is that these outcomes are limited in their ability to inform us regarding the mechanism driving this observed increase in health. One method of addressing this question is to examine more detailed indicators of health. Increasingly, researchers examining disparities in health outcomes by socio-economic factors have turned their attention to biomarkers of physical and mental stress. As discussed in Karlamangla et al. (2010), this movement has occurred for several reasons. First, individuals (particularly elderly individuals) have experienced 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 among even the non-elderly. 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. Because poor biomarker measurements are precursors to future diseases, it is thought that they require smaller sample sizes in order to estimate the effects of various interventions. Due to these factors, biomarkers appear to be the ideal setting for comprehensively estimating the health effects of the EITC.

As a result of concerns about potential bias in the BRFSS results and a desire for an understanding of the mechanism underlying the identified changes in health, in this section, we conduct a similar analysis using medically measured biomarker data. This data, obtained from several panels of the National Health and Nutrition Examination Survey (NHANES), directly confronts the two questions discussed above. The NHANES contains outcomes such as cholesterol levels and blood pressure that are measured by medical professionals—addressing concerns about relying on self-reported health outcomes. These biomarker data could provide evidence about the causal pathways generating the previously documented relationship between disease, mortality and socioeconomic status. This strategy does, however, involve tradeoffs. While the NHANES provides detailed health outcomes measured by health professionals, it also has a dramatically smaller sample size than the BRFSS or 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. An important difference between this dataset and others such as the National Health Interview Survey or the BRFSS is the examination component. Conducted in specially designed mobile examination centers staffed by a physician, medical and health technician and a health interviewer, the examination component provides detailed and documented medical information including that which can be gathered 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), the survey frequency was changed and surveys are now 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. Due to the structure of the survey, this is accomplished in different ways depending on the particular NHANES sample. In NHANES III, the sample respondent for the household is 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 quality as EITC qualifying child because of their age. As with the BRFSS, we will restrict the sample to women aged 21-40. 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.

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 spouse.[18] Next, we estimate the number of children as 2 minus household size for married women and 1 minus household size for single mothers. 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.

The NHANES has a wealth of information from physical, blood and urine tests that signal the current health of the mother. Table 7 contains the definitions and sample means of the biomarkers we utilize from the NHANES data sets. The selection of outcomes was guided by the template in Seeman et al. (2008). In that study, the authors classify individuals based on whether they are above or below certain medically-defined cutoffs for dangerous levels of these biomarkers (e.g., high blood pressure, high cholesterol levels) and we define outcomes in a similar way. Also following this template, we group the

[18] 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.

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 which are 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. Therefore, 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 predictor of coronary heart disease (Ridker, 2003; Onat, 2008; 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 among those at or near the poverty level. Respondents are defined to have risky CRP levels when concentrations are ≥ 0.3 md/Dl (milligrams per deciliter).

Albumin is a blood protein made by the liver and is measured as grams per deciliter (g/Dl). In this case, albumin levels decline during inflammation (Gillium et al., 1994). Lower levels of albumin may indicate liver disease, and is predictive of coronary heart disease and negative cardiac events (Danesh et al., 1998; Shalk et al. 2006) and stroke (Gabay et al., 1999). Seeman et al. (2008) found little correlation with low albumin levels and education but find risky albumin levels decline with higher income. Risky albumin levels are defined to be when concentrations fall below 3.8 g/Dl (grams per deciliter). Albumin has been criticized as a biomarker of inflammation because low levels of albumin may also signal malnutrition.[19] Therefore, a lower fraction of risky albumin could be due to either reduced inflammation or improved nutrition. We believe malnutrition is not a problem for the vast majority of women in our sample. In the pre-EITC expansion period, roughly 30 percent of the women in our sample

[19] http://www.nlm.nih.gov/medlineplus/ency/article/003480.htm

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.

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 in this case, we include three: diastolic blood pressure, systolic blood pressure and pulse. Blood pressure is measured in measured in millimeters of mercury (mmHg) while pulse is resting pulse measured in beats per minute.

High blood pressure is predictive of heart disease, heart failure, stroke, and kidney failure.[20] There is a long literature connecting blood pressure and socioeconomic status and this work is reviewed in Colhoun, Hemingway, and Poulter (1998). Their review 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. Zeller et al. (2004) found an increase in diastolic blood pressure among medical students taking their final licensing exams. Steptoe, Cropley, and Jokes (1999) found that individuals observed as having high job-related stress had a smaller decrease in blood pressure following the workday than did similar individuals with lower levels of job strain. Similarly, Light, Turner and Hinderliter (1992) found increases in blood pressure for healthy non-hypertensive men who experienced job strain compared to similar men without job strain. The pattern was not as large for woman. Schnall et al. (1998) found the estimated effects of stress on blood pressure were long lasting. In a study of particular interest to this analysis, Steptoe, Brydon, and Kunz-Ebrecht (2005) found that changes in financial strain were associated with changes in both systolic and diastolic blood pressure—with the effect of systolic pressure more precisely estimated. Additionally, studies have connected reduced stress levels on decreases in blood

[20] http://www.nhlbi.nih.gov/health/dci/Diseases/Hbp/HBP_WhatIs.html

pressure. For example, Schneider et al. (2005a) found that individuals over age 55 with high blood pressure who underwent a program of transcendental meditation and other stress relieving activities had reduced mortality. Schneider et al. (2005b) found similar results for a sample of African-American men. Rainforth et al. (2007) conducted a systematic review and meta-analysis of the literature on stress reduction techniques on blood pressure. They found a large and statistically significant effect of transcendental meditation but other stress-management techniques had little impact on blood pressure.

Elevated pulse rates are predictive of future coronary heart disease and other cardiovascular events (Gillum et al., 1991; Palatini and Julius, 1997) and 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 rates of 90 beats or more per minute is considered risky. Given the age of the population, 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 have 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. Observed connections between cholesterol levels and periods of mental and physical stress have been seen for many decades. Grundy and Griffin (1959) found increases in average serum cholesterol levels among medical students taking academic examinations compared to time periods of relative relaxation. Similarly, Friedman et al. (1958) found increased cholesterol among male accountants during times periods surrounding urgent tax deadlines. More recently, Muldoon et al. (1992) and Muldoon et al. (1995) found increases in cholesterol levels from periods of mental stress—with the latter study finding increases in HDL levels. Mattiasson et al. (1990) found that shipyard workers facing the threat of unemployment experienced increased serum cholesterol compared to workers of similar ages who were not facing an unemployment threat. More recently, Steptoe and Brydon (2005) found that acute mental stress increased cholesterol levels. These authors also found that these levels were still elevated during a follow-up measurement three years later. The strongest evidence in this literature is for changes in total cholesterol and LDL cholesterol as a result of stress. Increases in HDLs in response to stress is less consistent, and it must be noted that increases in HDL levels are associated with increases (and not decreases) in cardiac health. Total cholesterol levels of 240 mg/Dl and above and HDL levels below 40 are through 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). HbA1c 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.[21] 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 chronic stress can alter HbA1c levels in the blood. For example, Netterstrom et al. (1991) found that HbA1c levels were positively correlated with measures of objective job strain among a sample of Danish men. Similarly, Kawakami et al. (2000) found that high levels of job strain and low levels of social support were associated with increased HbA1c among a sample of Japanese males. Concentrations of HbA1c of 6.4 percent or above are thought to be risky but only 2.6 percent of women have elevated levels of this biomarker. Interestingly, 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.

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[21] http://www.nlm.nih.gov/medlineplus/ency/article/003640.htm

In the final group of biomarkers, we generate aggregate measures of risk by first summing the number of risky conditions across all 8 biomarkers. Aggregating the data in this manner is more than a means of increasing statistical power. Medical research has shown that changes in multiple biomarkers are seen to have effects that are greater than the sum of the predicted individual effects. The sum of risky biomarkers is often referred to as a measure of "Allostatic load" (McEwen, 1998). Researchers have found that the strains and stressors that accompany lower socioeconomic status are related to higher Allostatic loads (Evans, 2003; Evans and Kim, 2007, Evans and Schamberg, 2009). Singer and Ryff (1999) found that higher Allostatic load levels in midlife among participants in the Wisconsin Longitudinal Study were associated with a history of low socioeconomic status. Using the same dataset as this analysis, Geronimus et al. (2006) constructed a measure of 10 cardiovascular, metabolic, and inflammation biomarkers.[22] The authors found that individuals in poverty had higher levels of this measure of Allostatic load. Also using the NHANES, Seeman (2008) found that Allostatic load was higher among those with less education and a higher poverty to income ratio. Perhaps most importantly, Crimmins, Kim and Seeman (2009) found that Allostatic load level predicted a greater risk of mortality over a 6 to 12 year follow-up period. Similarly, Karlamanga et al. (2010) used data from NHANES III and found that all cause mortality was monotonically increasing in a measure of Allostatic load containing 9 biomarkers (8 of which are used in the measure of aggregate risk for this analysis).

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, this strategy is used in this portion of the analysis where all 8 risky biomarker measures are aggregated into a composite score. In the sample for this analysis, 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.

[22] We only use 8 of these biomarkers in this analysis because those are all that are consistent across all three panels of the NHANES.

Results for this composite measure of biomarkers provides the most complete picture of the health effects of the EITC expansion.

In Table 8, we report estimates for regression-adjusted difference-in-difference models of the effect of the EITC expansion on maternal health. 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 mothers 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.

Initially we report results for the aggregate measures of risky conditions—those thought by the medical community to be most predictive of negative health outcomes. 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 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 (t-statistic of -1.4) 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 0.05.

The Poisson model is restrictive in that it forces 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.[23]

In general, the results in the first block of Table 8 suggest a large increase in the quality of the biomarkers for mothers impacted by the EITC expansions. Since medical research has shown that all-cause mortality is monotonically increasing in this measure of Allostatic load, the results suggest there have been true improvements in health resulting from the EITC expansion. In the remaining three blocks of results in the table, we examine the source of this advantage by estimating results for particular metabolic, cardiovascular, and inflammation disorders in that order.

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 (4.1 percentage points) 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 third block of results in Table 8 contains the estimates for the presence of cardiovascular disorders (diastolic blood pressure, systolic blood pressure, and pulse). The estimated effect for diastolic blood pressure shows 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 a p-value of 0.10. The estimates for Albumin suggest that the

[23] 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).

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. These results for inflammatory biomarkers are the most precisely estimated of the three sub-groupings. This could be due to a number of factors. First, the incidence rate for these outcomes hovers near 50 percent reducing Type II error rates. Second, the medical literature has found that inflammatory biomarkers are independently predictive of a host of outcome such as heart attacks, strokes, cognitive decline, and mortality (Tracy et al., 1999; Tice et al., 2003; Ridker et al., 2002; Torres and Ridker, 2003; Schmidt et al., 2002; Ridker et al., 2003) so there are a vast array of physical insults that can be captured by these outcomes.

VIII. 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. Because self-reported mental and physical health are indirect measures of stress, we also examined medical data from the NHANES that was collected by medical professionals. We 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 the appropriate size of these programs. This could lead to more optimal allocation of resources by government programs and tax structures.

Although the results indicate a positive impact of higher transfer payments on outcomes, it is not clear these benefits will translate into lower disease incidence and lower mortality rates. Stress is but one cofactor of heart disease, stroke, etc., and given the ages of the mothers in our sample, many are years away from these diseases presenting. That said, the results are encouraging that higher income transfers can help reduce medical indicators and self reports of poor health.

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

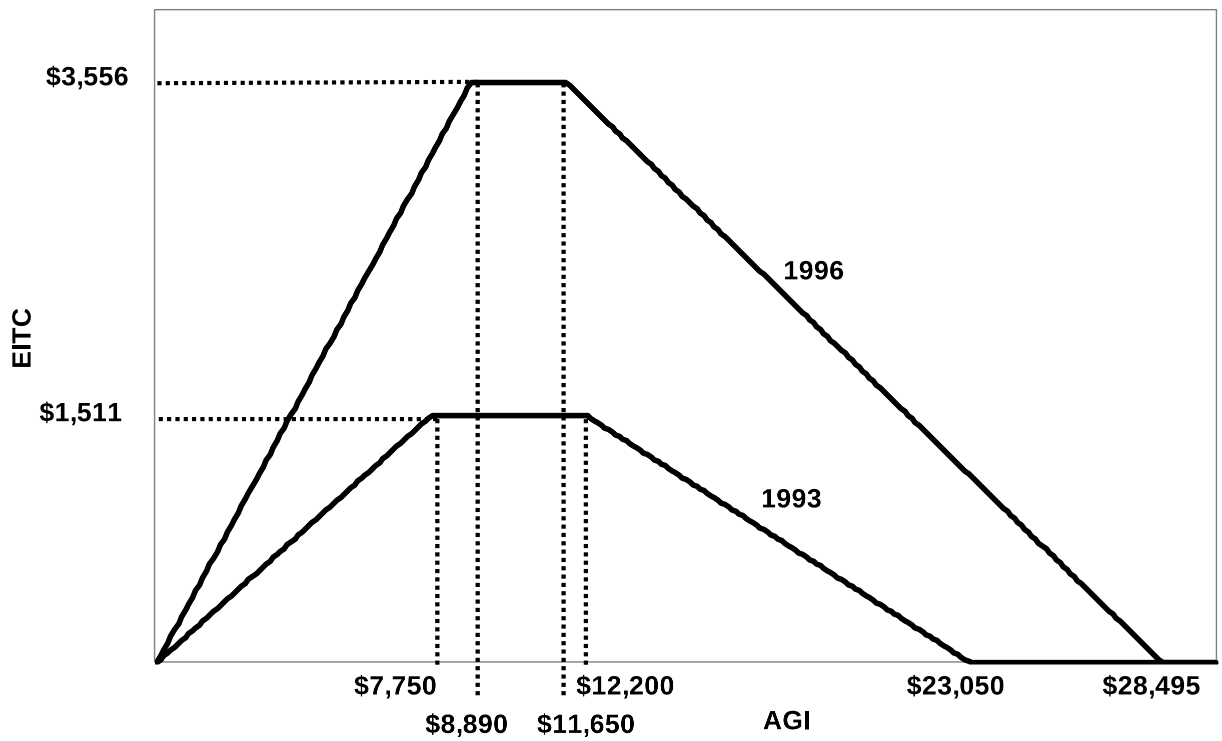

Figure 2: EITC Payments for Families with 1 Child
1993 and 1996

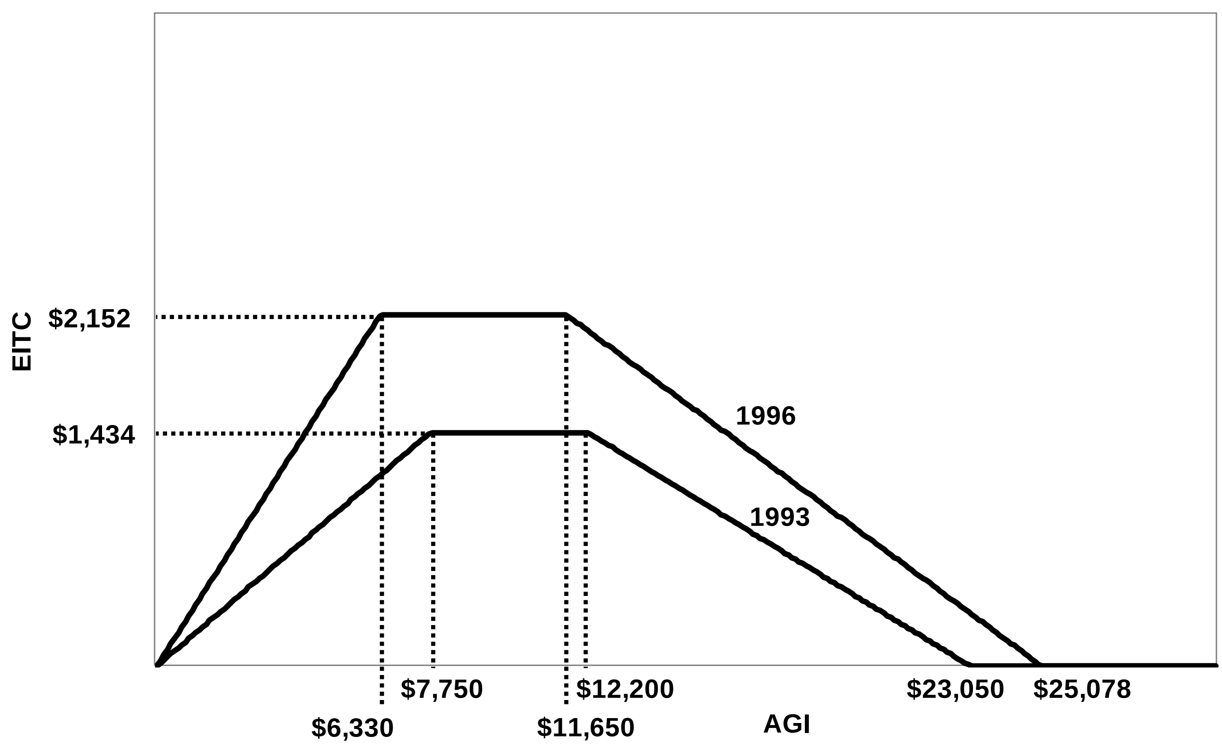

Figure 3: Difference in EITC Benefit in Families with Different Numbers of Children, 1996 and 1993

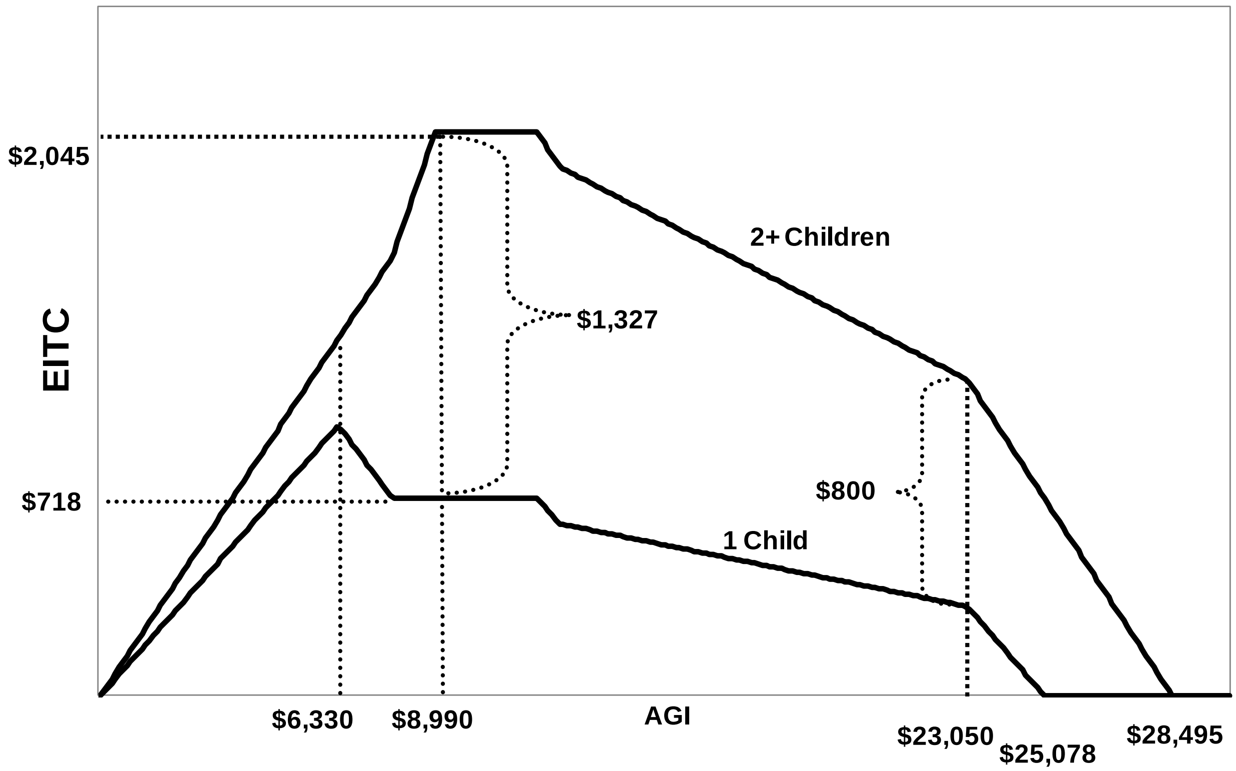

Table 1
Earned Income Tax Receipt by Education and Number of Children, Women Age 21-40
Tax Years 1993-1995 and 1998-2001

	High School Diploma		College Graduate	
	One Child	Two Plus Children	One Child	Two Plus Children
	Percent Receiving the EITC			
Tax Years 1993-1995	27.23	22.1	7.36	4.6
Tax Years 1998-2001	28.76	26.54	6.4	4.51
	Size of EITC Payment			
Tax Years 1993-1995	\$316.95	\$306.84	\$72.89	\$53.63
Tax Years 1998-2001	\$420.08	\$585.88	\$83.42	\$80.17
	Size of EITC Payment Among Recipients			
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

	≤HS Education			College Graduates		
Variable	One Child	2 or more Children	p-value	One Child	2 or more Children	p-value
Average Age	31.0	32.0	0.000	32.2	34.6	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
Labor supply						
% currently employed	0.679	0.580	0.000	0.796	0.714	0.000
Family income						
% <$20K	0.405	0.386	0.057	0.110	0.082	0.000
% $20K-$25K	0.132	0.124	0.050	0.059	0.047	0.036
% $25K-$35K	0.158	0.166	0.204	0.155	0.128	0.000
% $35K-$50K	0.131	0.151	0.003	0.227	0.235	0.260
% $50K and above	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 in past month	0.432	0.447	0.039	0.418	0.424	0.603
% with any bad physical days in past month	0.351	0.343	0.218	0.357	0.356	0.969
# of bad mental days in past month	4.27	4.52	0.030	2.93	2.89	0.753
# of bad physical days in past Month	2.81	2.65	0.072	1.89	1.95	0.656
Observations	7,315	15,737		3,881	6,740	

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 difference-in-difference estimate	0.0679***	0.0524***	0.0453
	(0.0126)	(0.0130)	(0.0108)
Regression-adjusted difference-in-difference estimate	0.0587***	0.0534***	0.0457
	(0.0121)	(0.0133)	(0.0110)
Pre-expansion mean of outcome for treatment group	0.538	0.445	0.554
Married mothers	44,715 observations		49,217 obs.
Simple difference-in-difference estimate	0.0053	0.0063	0.0099
	(0.0099)	(0.0097)	(0.0106)
Regression-adjusted difference-in-difference estimate	0.0091	0.0104	0.0183*
	(0.0107)	(0.0108)	(0.0109)
Pre-expansion mean of outcome for treatment group	0.612	0.569	0.593
All mothers	65,713 observations		82,907
Simple difference-in-difference estimate	0.0128	0.0141*	0.0170**
	(0.0079)	(0.0083)	(0.0073)
Difference-in-difference estimate	0.0137*	0.0136	0.0203**
	(0.0083)	(0.0087)	(0.0074)
Pre-expansion mean of outcome for treatment group	0.591	0.534	0.580

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 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**	0.0203**
			(0.0073)	(0.0074)
Any bad mental health days in past 30 days?	0.447	OLS	-0.0132	-0.0141*
			(0.0083)	(0.0083)
Excellent/very good health?	0.577	OLS	0.0095	0.0135*
			(0.0079)	(0.0075)
Any bad physical health days in past 30 days?	0.342	OLS	0.0038	0.0041
			(0.0071)	(0.0069)
# bad mental health days in past month	4.52	Neg. Binomial	-0.0474	-0.0754**
			(0.0306)	(0.0328)
# bad physical health days in past month	2.65	Neg. Binomial	0.0140	0.0105
			(0.0390)	(0.0390)

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.

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

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

	Method	DD results	DD Results State*Year	DD Results Married	DD Results Single	DDD Results
Outcome		82,907 obs.	82,907 obs.	49,217 obs.	33,690 obs.	127,209 obs.
Currently employed?	OLS	0.0203**	0.0206**	0.0183*	0.0457***	0.0130
		(0.0074)	(0.0076)	(0.0109)	(0.0110)	(0.0123)
Any bad mental health days in 30 days?	OLS	-0.0141*	-0.0092	-0.0146	-0.0120	-0.0089
		(0.0083)	(0.0083)	(0.0112)	(0.0145)	(0.0144)
Excellent/very good health?	OLS	0.0135*	0.0119	0.0211**	0.0082	-0.0045
		(0.0075)	(0.0075)	(0.0099)	(0.0127)	(0.0114)
Any bad physical health days in past 30 days?	OLS	0.0041	0.0059	0.0069	-0.0004	0.0169
		(0.0069)	(0.0069)	(0.0105)	(0.0110)	(0.0151)
# bad mental health days in past 30 days	Neg. Bin.	-0.0754**	-0.0615*	-0.1027**	-0.0514	-0.0622
		(0.0328)	(0.0326)	(0.0519)	(0.0519)	(0.0729)
# bad physical health days in past 30 days	Neg. Bin.	0.0105	0.0249	0.0432	-0.0377	0.1226
		(0.0390)	(0.0391)	(0.0508)	(0.0675)	(0.0911)

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.

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

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

[a]To 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.
[b]g/Dl=Grams per deciliter
[c]mmHg = Millimeters of mercury
[d]mg/Dl=Milligrams per deciliter

Table 8
Regression-adjusted DD and DDD Estimates,
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*	-0.125
		(0.040)	(0.099)
Two or more risky conditions	0.305	-0.094*	-0.018
		(0.043)	(0.092)
Three or more risky conditions	0.108	-0.061	-0.002
		(0.044)	(0.062)
Poisson Model: Total # risky conditions	1.092	-0.235**	-0.211
		(0.095)	(0.215)
Metabolic Biomarkers			
Risky Glycated Hemoglobin	0.026	-0.004	-0.012
		(0.013)	(0.019)
Risky Total Cholesterol	0.102	-0.022	0.043
		(0.034)	(0.046)
Risky HDL	0.156	-0.027	-0.044
		(0.036)	(0.047)
Any risky metabolic condition	0.251	-0.042	-0.007
		(0.045)	(0.078)
Poisson Model: # risky metabolic conditions	0.277	-0.185	0.03
		(0.177)	(0.276)
Cardiovascular Biomarkers			
Risky Diastolic Blood Pressure	0.045	-0.032*	-0.03
		(0.017)	(0.026)
Risky Systolic Blood Pressure	0.035	0.004	-0.0005
		(0.014)	(0.023)
Risky Pulse	0.108	-0.016	-0.043
		(0.037)	(0.049)
Any risky cardiovascular condition	0.131	-0.034	-0.065
		(0.041)	(0.065)
Poisson Model: # risky cardiovascular conditions	0.164	-0.317	-0.423
		(0.233)	(0.343)
Inflammation Biomarkers			
Risky Albumin	0.262	-0.088*	-0.087
		(0.045)	(0.063)
Risky C-Reactive Protein	0.437	-0.083*	-0.012
		(0.05)	(0.07)
Any risky inflammatory condition	0.493	-0.096*	-0.107
		(0.050)	(0.096)
Poisson Model: # risky inflammatory conditions	0.493	-0.217**	-0.136
		(0.099)	(0.159)

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. All standard errors allow for arbitrary for of heteroskedasticity.

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