Education, Maternal Smoking, and the Earned Income Tax Credit

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August 8, 2012

We estimate and explore mechanisms of the impact of the Earned Income Tax Credit (EITC) expansions on the smoking behavior of women. Differential increases in federal EITC benefits by family size in the mid-1990's allow for a comparison of smoking status changes between mothers with one and more than one child. We exploit these changes in a difference-in-differences framework using data from the 1993-2001 waves of the Behavioral Risk Factor Surveillance System (BRFSS) and show that the increase in EITC benefits yielded a significant decline in the likelihood of being a current smoker among unmarried mothers with less than a college degree. Although women with a high school degree or less and women with some college education received similar benefit increases on average and exhibited similar labor supply responses, the reduction in the likelihood of smoking was concentrated among those with some college.

JEL classification codes: I38; H53; J22; I18

Keywords: EITC; smoking; income; employment

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Acknowledgments: The authors thank anonymous referees and seminar participants at Texas A&M University, Washington State University, The University of Washington, and the American Society of Health Economists 2012 conference (Minneapolis, MN) for helpful suggestions. All errors are attributable to the authors.

1. Introduction

Although smoking prevalence fell in the U.S. over the past few decades, negative health consequences of smoking continue to be a policy concern, particularly for individuals and families in low socioeconomic status groups. Using data from the National Health and Nutrition Examination Survey, Kanjilal et al (2006) estimate that the gap in smoking prevalence between persons with a) less and more than a high school diploma and b) the lowest and highest poverty-income-ratio quartiles approximately doubled between the early 1970s and the early 2000s. In 2008, high school dropouts were 18.6 percentage points more likely to smoke than college graduates, and persons with family incomes below $35,000 were 13.2 percentage points more likely to smoke than those with family incomes of at least $75,000 (Cawley and Ruhm, 2011).

Much empirical work in economics has examined the effects of tobacco control policies on smoking behavior in the United States. However, relatively little prior research has considered the effect of safety-net programs on smoking decisions.[1] Though such programs are rarely, if ever, explicitly intended to change smoking behavior, given that smokers are (increasingly) disproportionately poor, understanding how such policies affect smoking is of interest to policymakers considering how to reduce smoking among individuals of low socioeconomic status. This paper considers the effect on smoking of one such policy: the Earned Income Tax Credit (EITC).

To examine the effect of the EITC on smoking, we follow Hotz, Mullin, and Scholz (2006) and Evans and Garthwaite (2010) by primarily focusing attention on the legislative expansion of the federal program that, among other changes, created a substantial difference in

[1] Some recent exceptions include the impact on smoking of Head Start (Anderson, Foster, and Frisvold, 2010), WIC (Joyce, Racine, and Yunzal-Butler, 2008) and welfare reform (Kaestner and Tarlov, 2006).

the credit schedules and dollar values for families with one and more than one child. We use this differential expansion in a difference-in-differences (DD) framework to identify the effect of EITC expansions on the smoking status of mothers. The key assumption underlying this analysis is that, controlling for observable characteristics, trends in smoking rates would have been similar for the two family types in the absence of OBRA. We provide evidence on the plausibility of this assumption in our results section (Section 5), where we also compare the differential trends of individuals who were likely to receive the EITC (those without a college degree) to those who were unlikely to receive a credit (those with a college degree) in a triple differences (DDD) framework.

Overall, we find that OBRA reduced the likelihood of smoking among single mothers with less than a college degree by 2.5 percentage points (roughly 6 percent at the mean), which is roughly equivalent to the reduction that would be generated by a 10% increase in the price of cigarettes as estimated by Colman and Remler (2008). Because the EITC affects the labor supply and after-tax income of mothers simultaneously, it is impossible to separate the independent effect of each of these variables on smoking behavior without the use of additional instruments (we discuss this further in Section 2). However, because labor force participation and after-tax income are likely to be the two dominant channels through with the EITC affects smoking, we conclude that smoking participation among the population studied in this paper decreases with rising income, transition to employment, or both. In Section 2, we discuss why an increase in income or the probability of employment could reduce smoking in theory.

To our knowledge, we are also the first to examine differences in treatment effects associated with the EITC by education level. To this end, we present evidence that mothers with some college education are similar to those with a high school diploma or less in terms of EITC

eligibility, EITC benefits, and labor-supply responses to credit expansions (as opposed to mothers with a college degree, who appear different along all 3 dimensions). This stands in contrast to previous work, which has typically defined the EITC-eligible population as only those with a high school diploma or less (e.g. Evans and Garthwaite, 2010; Averett and Wang, 2011). The relevance of this point for our paper is that the bulk of the effect of the EITC expansion on smoking was for women with some college experience but no college degree: their likelihood of smoking fell by 4.4 percentage points (just over 13 percent at the mean). Therefore, our paper also contributes to the small but growing literature showing that individuals with higher levels of education exhibit stronger behavioral responses to external shocks that increase the perceived health risks or costs of smoking (e.g. Aizer and Stroud, 2010; Chaloupka and Wechsler, 1995; de Walque, 2010; Wray et al, 1998).

The remainder of the paper is organized as follows. Section 2 discusses two mechanisms—labor supply and income—by which the EITC is expected to affect smoking behavior and presents the existing evidence on the relationship between those variables. Section 3 begins with a detailed explanation of the EITC legislative changes exploited in the analysis and finishes with an explication of the empirical models used in the analysis. Section 4 identifies and summarizes the data. Section 5 offers a presentation and discussion of the main results and additional estimates, and Section 6 concludes and discusses current policy implications.

2. The relationships between income, labor supply, and smoking

2.1. Background

As discussed in Section 1, the EITC, to the extent that it affects smoking behavior, is most likely to do so by changing after-tax income or labor supply. In this section, we discuss

why the direction of the response of smoking to both income and labor supply is theoretically ambiguous, and we outline the existing empirical evidence on these responses. We begin by discussing income and then turn to labor supply.

When income rises such as through an expanded EITC benefit, smokers may wish to consume more cigarettes to enjoy more of the "smoking utility" they provide. However, the demand for health is also likely to increase with income, and since tobacco consumption is negatively associated with health, an increased desire to smoke may be partially, fully, or even more than offset as income rises (Cawley and Ruhm, 2011). This offsetting effect may be particularly strong for mothers who seek good health for their children since the benefit of smoking accrues only to the mother while the health cost may also be borne by her children through secondhand smoke or through its effects on infant birth weight (DHHS, 2010).[2] In addition, other researchers have found that economic insecurity raises the likelihood of smoking (Barnes and Smith, 2009). Thus, if the EITC reduces insecurity associated with poverty, it may reduce smoking by alleviating the need of mothers to "self-medicate." Thus, even if tobacco is a normal good for some populations, it may be an inferior good for the population studied in this paper.

Since the EITC is but one way that policymakers can affect household income, our paper more generally relates to the body of papers that examine the relationship between income and smoking. In a meta-analysis of 375 income elasticity estimates in published research, Gallet and List (2003) find a mean income elasticity of 0.49 and estimate that demand for tobacco is less income-elastic in the short-run than in the long-run. However, the range of estimates studied is

[2] Along these lines, it is conceivable that an estimated effect of EITC on smoking may arise from a direct effect of EITC on fertility. Baughman and Dickert-Conlin (2009) find that the EITC expansion reduced fertility, albeit by a very small amount, which would run counter to the noted discussion.

large and inconclusive regarding whether tobacco is inferior, with a minimum of -0.8 and a maximum of 3.03. In regression analysis, Gallet and List also estimate the effect of study characteristics on the reported elasticity estimate. Relevant for this paper, they find that elasticity estimates were lower in studies of individuals (rather than aggregated by geographic region), men, and young adults (the meta-analysis notably does not include explanatory variables for education). In addition, the authors note that estimation methods other than ordinary least squares (OLS), predominantly two-stage least squares (2SLS), yield lower income elasticity estimates. That 70% of the elasticities identified in their meta-analysis were estimated using OLS methods also suggests that a substantial portion of prior literature may not have addressed issues of endogeneity with respect to income and cigarette consumption. This collection of varying findings therefore warrants more work on the impact of income changes on smoking.

Because we examine responses for different education subgroups, it is important to consider previous work on how smoking behavior may respond to changes in income for varying levels of education (we are unaware of analogous research on labor supply responses). Other studies have found that a higher level of education has a strong negative effect on smoking behavior for all levels of income (e.g. Hersch, 2000; Bratti and Miranda, 2010), but only for high levels of education is an increase in income associated with lower levels of smoking (Crabtree, 2010). Studying over 40 years of data, Huang, Yang, and Hwang (2004) conclude that the overall income elasticity has declined over time due mainly to a sharply declining income elasticity for dividend income with smaller declines in transfer income and earnings elasticities. They infer that cigarette consumption is a normal good for individuals with low income and an inferior good for those with high income. Apouey and Clark (2010) examine the aftermath of winning a lottery and find that smoking increases (suggesting that tobacco is normal in these

circumstances). Kelly et al (2011) study the impact of initial occupational choice on later health behaviors and find that blue collar work is associated with an increased probability of smoking.

Increases in income due to changes in the EITC are often accompanied by a change in labor supply (Meyer and Rosenbaum, 2001). We discuss the full range of possible labor supply responses to OBRA in Section 3.1, but here we note that the expansion increased labor force participation (Hotz, Mullin, and Scholz, 2006) and thus increased hours worked among those who got a job as a result of the credit. If a mother increases her work hours in response to a higher after-tax wage arising from a lower marginal tax rate after the EITC expansion (indicating a dominant substitution effect), she may smoke less as a result. More specifically, time-intensive investments in her (or her children's) health will be less attractive since she will have less leisure time, leaving pecuniary investments or other investments with low time costs. Since smoking is itself time-intensive at work due to restrictions on where smoking may occur (Markowitz et al, 2011), investment in health through a reduction in smoking would both save time and improve health. Another reason why smoking may decline if individuals are induced to work as a result of EITC expansion is the economic penalty smokers endure in the labor market. Previous work (Levine, Gustafson, and Velenchik, 1997; Auld, 2005; Cowan and Schwab, 2011) provides evidence that smoking has a negative causal effect on wages. Since smokers do not receive a wage penalty if they do not work, the cost of smoking increases when one transitions from being unemployed to employed. On the other hand, individuals may smoke more when they work more as a result of job-related stress.

Most of the existing literature on labor supply and smoking finds that work increases lead to smoking increases. Ruhm (2005) finds that cigarette consumption decreases among heavy smokers as the rate of unemployment rises; he attributes the effect to an increase in the demand

for health and the concomitant increase in health inputs when the opportunity cost of time falls (Grossman, 1972), rather than the loss of income. Charles and DeCicca (2008) also find that smoking decreases with a rising unemployment rate. Xu and Kaestner (2010) estimate that an increase in work hours and wages are associated with an increase in cigarette smoking. Furthermore, they find that the work-hours effect operates on both the intensive and extensive margins of employment. On the other side, Barnes and Smith (2009) find that an increase in an individual's likelihood of becoming unemployed increases his or her likelihood of continuing to smoke. Our results are not necessarily inconsistent with prior literature that finds a positive association between hours worked and smoking prevalence. For example, the EITC increases income for many recipients even if labor supply does not change, so a negative income effect could dominate a positive labor supply effect.

2.2. Related research on the EITC

Because both income and labor supply have ambiguous theoretical effects on the demand for tobacco, the question of how the EITC affects smoking is empirical in nature. Two recent papers have also examined the EITC and smoking behavior.[3] Averett and Wang (2011) employ a DD strategy that is similar to ours to examine the effect of OBRA on maternal smoking. Using data from the National Longitudinal Survey of Youth, 1979 (NLSY), they estimate that in response to federal EITC benefit increases, the probability of maternal smoking declined among white women (but not black women). Our study, which uses BRFSS rather than NLSY, has the advantage of much larger sample sizes than NLSY and smoking data in every year before, during, and after the EITC expansion (whereas smoking information in the NLSY is only

[3] In a related study, Strully, Rehkopf, and Xuan (2010) use birth-certificate data to examine whether states with EITCs have differing average infant birth weights and maternal smoking rates than do states without EITCs, finding that the presence of a state EITC program is associated with a lower likelihood of smoking during pregnancy.

available in 1992 and 1998, raising the possibility that changes in smoking between these years are not only solely a result of the EITC). Nevertheless, we reach similar qualitative conclusions regarding the EITC and smoking as Averett and Wang (2011)—indeed, both papers find that the EITC expansion reduced the prevalence of smoking among affected women. However, our results differ from Averett and Wang's in some important respects. Most notably, our estimates of the effect of the EITC on the probability of smoking are smaller in magnitude than those found by Averett and Wang (2011).[4] In addition, we find similar (and significant) effects of the EITC on smoking for both white and black women (these results are available upon request). Lastly, we stratify our analysis by education level because of previous work identifying a robust education-smoking gradient (de Walque, 2010; Kenkel, 2007) and find important differences by education level that Averett and Wang (2011) do not explore.

Kenkel, Schmeiser, and Urban (2011) also analyze the EITC and smoking behavior using Tobacco Use Supplements of the Current Population Survey, but they do not estimate total EITC effects. They take a different approach by seeking to use the EITC as an instrument for identifying the income elasticity of current smoking and smoking cessation in a 2SLS framework. More particularly, they use the maximum state plus federal EITC benefits available to an individual by state, year, and number of dependent children as an instrument for income in their models. Contrary to results found in Averett and Wang (2011) and this paper, Kenkel, Schmeiser, and Urban (2011) find that increases in EITC benefits led to large, positive increases in smoking prevalence; their estimated income elasticity with respect to smoking participation is greater than 2.

[4] We conducted an analysis restricting our sample to include only 1993 and 1998, the closest years available in our data to those used by Averett and Wang. When doing so, our estimates approximately doubled and more closely matched theirs, suggesting that excluding years close to the OBRA expansion may bias the estimate of the smoking effect.

There are several important differences between our paper and Kenkel, Schmeiser, and Urban (2011) that may contribute to the discrepancy in our results. First, Kenkel, Schmeiser, and Urban (2011) include both men and women in their main sample (an uncommon practice in the EITC literature).[5] Second, the sources of identification are different in each paper. Whereas we focus narrowly on OBRA which substantially increased benefits for mothers with 2 or more children relative to mothers with 1 child, Kenkel, Schmeiser, and Urban also use variation in EITC benefits throughout the 1990's and early 2000's that arises from differences in state-level EITC benefits as well as federal and state benefit changes that differentially affected families with children as opposed to childless families. Because trends in smoking (in the absence of EITC changes) would have arguably been more similar for mothers of 1 child and mothers of 2 or more children than they would have for childless women and mothers, identifying EITC effects using childless women as a control group introduces an additional potential source of bias in the results (Evans and Garthwaite, 2010; Hotz, Mullin, and Scholz, 2006). In addition, state-level changes in EITC programs may be correlated with other policy changes (e.g. welfare reform) or other state trends (e.g. business cycles) that affect smoking on their own, which could lead to omitted variable bias (if such factors are not correctly controlled for). Our study does not rely on state-by-state changes in EITC benefits for identification (and indeed we are able to control for state-level trends in smoking in a very general way, as discussed in Section 5).

Lastly, we note that using EITC changes as the sole instrument for income in a smoking regression (as in Kenkel, Schmeiser, and Urban, 2011) suffers either from omitted variable bias (when omitting labor supply from the model) or potential endogeneity bias (because there is no

[5] Most EITC studies (e.g. Meyer and Rosenbaum, 2001; Evans and Garthwaite, 2010), including this one, focus on women rather than men. This is for at least 2 reasons: male labor force participation is much less elastic with respect to the wage, and single-parent households, which are much more likely to qualify for EITC benefits, are typically headed by women.

second instrument for labor supply when it is included as an explanatory variable). This is due to the fact the EITC affects income and labor supply simultaneously. Though this point is noted by Kenkel, Schmeiser, and Urban (2011), they downplay potential labor supply effects and interpret their results as an income effect. We prefer to focus on the total (reduced-form) effect of the EITC on smoking, noting that both income and labor supply responses could play a role in smoking reductions for the reasons described in Section 2.1. In Appendix A.1, however, we conduct an exploratory analysis of the mechanisms through which the EITC affects smoking that is similar to their analysis, and this has the benefit of offering a direct comparison with their results. In that section we also demonstrate that our results are robust to using state-level variation in EITC benefits to identify the reduced form effect of the EITC on smoking.

3. Empirical Strategy

3.1. Identification Using the EITC

The features of EITC legislation that are of most interest in the analysis presented below are the credit expansions contained in the 1993 Omnibus Reconciliation Act, or OBRA (OBRA, 1993). This legislation created a meaningfully different benefit structure for families with zero, one, and more than one child. Because there were only small benefit differences among families with children prior to 1994 (and no benefit for families without children), the legislation change allows for the opportunity to study the effects of EITC expansion in a DD framework with standard assumptions. We compare the differential expansion between families with one child and more than one child in order to avoid potential bias due to unobserved factors correlated with selection into parenthood. We follow Evans and Garthwaite (2010) in defining the post-OBRA expansion period as beginning in 1996 since the change in benefit structure came into effect for

the 1995 tax year (so that individuals received a larger tax refund from the government beginning in 1996).

The EITC, by providing a refundable tax credit to individuals with positive earnings, was formulated in part to provide incentives for individuals in low-income households to work more. The EITC therefore includes a "phase-in" subsidy range beginning at the lowest level of adjusted gross income (AGI) eligible for the credit. As AGI increases within this phase-in range the credit's dollar value increases at a fixed rate, thus increasing a worker's effective wage or, equivalently, decreasing her marginal tax rate. When AGI reaches a high enough level, the phase-in region ends and the worker enters the "plateau" region where the credit no longer increases with AGI. In this range, the worker receives the "maximum benefit" and the EITC has no effect on the worker's effective wage or marginal tax rate. The final AGI range that is eligible for the EITC is the "phase-out" range in which the credit decreases with AGI. Opposite the phase-in, the phase-out decreases the credit at a fixed rate, thus lowering a worker's effective wage and increasing her marginal tax rate. Even if the distribution of AGIs within the EITC eligibility range (and the corresponding distribution of effective wages and marginal tax rates) is known at the time of an EITC expansion, the average labor supply response is not known *a priori* because of offsetting substitution and income effects, which we discuss below.

The EITC expansion that was a part of OBRA changed the phase-in and phase-out ranges as well as the maximum benefits for families with one and more than one child.[6] These changes took effect for most individuals in 1996, after which the dollar values were indexed for inflation (Hotz, Mullin, and Scholz, 2006). Table 1 includes the 1993 tax year (pre-OBRA) and 1996 tax

[6] See the legislation text at http://thomas.loc.gov/cgi-bin/query/F?c103:1:./temp/~c103cbNbAU:e385579: for full details (last accessed July 29, 2011).

year (post-OBRA) benefit structures (and the change between these years) for several family types. Prior to expansion, all families with children faced a phase-in range from $0 to $7,750 and a phase-out range from $12,200 to $23,050 with a maximum benefit (in the plateau range) of $1,434 for families with one child and $1,511 for families with more than one child (only a $77 difference). For families with one child, OBRA shifted the top of the phase-in range down to $6,330, the bottom of the phase-out range down to $11,650, the top of the phase-out range up to $25,078, and the maximum benefit up to $2,152. For families with more than one child, the legislation shifted the top of the phase-in range up to $8,890, the bottom of the phase-out range down to $11,650, the top of the phase-out range up to $28,495, and the maximum benefit up to $3,556. The legislation also added a small benefit structure for families without children for the first time, which consisted of a phase-in range from $0 to $4,420 and a phase-out range from $5,280 to $9,500 with a maximum benefit of $323.[7]

From a worker's perspective, these changes had a few notable effects. First, over the entire range of incomes eligible for the EITC (which was widened by OBRA), the credit's dollar value increased for all families with children, and it increased by a greater amount, in some cases substantially, for families with more than one child. In this sense, a DD estimation may be viewed as identifying an income response. But complicating this interpretation is the collection of labor supply effects. OBRA has been shown to have increased labor force participation (Eissa and Leibman, 1996; Meyer and Rosenbaum, 2001; Eissa, Kleven, and Kreiner, 2008). By increasing the maximum benefit and the phase-in and phase-out rates (by 15.5 and 2.8 percentage points, respectively, for families with one child, and by 20.5 and 7.2 percentage points, respectively, for families with more than one child) of the EITC, the expansion has a

[7] There are several graphical representations of the OBRA EITC expansions in the literature, e.g. Figures 1-3 in Evans and Garthwaite (2010).

theoretically ambiguous effect on hours worked for those who were in the phase-in range and a theoretically negative effect on hours worked for those in the flat and phase-out ranges of the credit. However, previous work has found that at the intensive margin, the EITC has had either very little effect on labor supply (Eissa and Leibman, 1996; Eissa, Kleven, and Kreiner, 2008) or a modest effect on labor supply (Dickert, Houser, and Scholz, 1995; Keane and Moffitt, 1998; Meyer and Rosenbaum, 2001). Thus, it seems likely that the dominant effect of the expansion on labor supply came by way of inducing more individuals into the labor market. Overall, then, the EITC expansion may be viewed as an income expansion with labor supply incentive modifications.

Tables 2 and 3 report a summary of changes in EITC eligibility and receipts for all and unmarried mothers, respectively, between the ages of 21 and 40 (the group studied in our empirical analysis, as discussed below). The summary statistics are calculated using the Annual Demographic Survey of the March Current Population Survey (CPS) for the survey years 1994-1996 and 1999-2002 (the listed tax years are lagged because the survey asks about income earned in the previous year).[8] An overall comparison of the two tables reveals that a substantially greater fraction of unmarried women received the EITC, and therefore the average EITC payment was much greater for unmarried women. This is consistent with previous work on the labor-supply effects of the EITC, in which authors have often focused on single women as their population of interest (e.g. Eissa and Leibman, 1996; Hotz, Mullin, and Scholz, 2006). In our empirical analysis (Section 5), we separately consider single mothers alongside all mothers and find that smoking effects are particularly large for single women, consistent with differences in eligibility and receipts shown by comparing Tables 2 and 3.

[8] The EITC receipt values included in the CPS are calculated based on income and tax rules and therefore reflect the expected receipt if the credit is claimed, not the actual receipt.

Tables 2 and 3 also consider education subgroups (those with a high school diploma or less, those with some college but no college degree, and those with a college degree). Through a combination of labor supply changes and increases in actual benefits, EITC eligibility and average receipts increased for most education levels and family sizes between the pre- and post-OBRA periods. However, EITC participation and benefit payments tend to be very similar between women with a high school diploma or less and women with some college, whereas differences between either one of those two groups and college graduates are much larger. For example (and most relevant for the DD analysis below), the differential pre- vs. post-OBRA increase in average payment to single mothers with more than one child relative to those with one child was $554.61 for women with a high school diploma or less, $530.36 for women with some college, and $133.70 for women with a college degree.

Not only do these larger average benefit changes for women with less than a college education suggest that there may be a larger smoking behavior response for these women, but this finding also informs construction of a valid DDD model separated by education levels, as discussed below. This notably contrasts with previous work implementing a DDD framework by separating women with high school diplomas or less from other women but without presenting, to our knowledge, explicit empirical evidence to inform constructing the cutoff at that level (see for example Evans and Garthwaite, 2010; Averett and Wang, 2011). Given the evidence in Tables 2 and 3, in Section 5, we define women with less than a college degree as our sample in the DD analysis and the "treatment" group in the DDD analysis, while women with college degrees serve as the "control" group in the DDD analysis.

3.2. Empirical Methods

We implement standard DD regression methods[9] for the analysis of the differential impact of EITC expansion on smoking measures among families with varying numbers of children. The general form of the estimated regression models is:

$$(1) \qquad y_{ist} = \beta_0 + x_{ist}\beta_x + MultiKids_{ist}\beta_{Kids} + (MultiKids_{ist} * OBRA_t)\beta_{DD} + \lambda_t + \delta_s + \varepsilon_{ist}$$

where y_{ist} is the outcome of interest (a measure of current smoking behavior, which we define below) observed for each individual's interview at time *t* for state of residence *s*, x_{ist} is a vector of indicator variables for individual and household characteristics, $MultiKids_{ist}$ is an indicator variable identifying whether a family includes more than one child (rather than only one child), and $OBRA_t$ is an indicator variable for whether or not the interview occurred during the pre- or post-OBRA benefit expansion period.[10] As discussed earlier, we define the post-OBRA expansion period as beginning in 1996 since the rapid expansion in benefit structure went into effect for the 1995 tax year. Also, λ_t is a vector of indicator variables for each year and month, δ_s is a vector of indicator variables by state[11], and ε_{ist} is the error term.

The parameter of primary interest in equation (1) is β_{DD} (the DD coefficient), which is the coefficient on the interaction between the indicators for subgroups according to number of children and pre- or post-OBRA benefit expansion. β_{DD} represents the change in the outcome of interest after OBRA for a family with more than one child relative to a family with one child. The identifying assumption underlying the DD approach is that, conditional on covariates, nothing besides OBRA

[9] For a discussion of early work developing this technique, see Besley and Case (1994).
[10] The results reported in this paper were obtained by estimating linear probability models using ordinary least squares (OLS), but we also ran probit models to check the robustness of our main results. The resulting marginal effects, calculated according to the procedure explained by Norton, Wang, and Ai (2004) to properly account for interaction terms, were practically identical to those obtained via OLS estimation.
[11] We also estimated specifications that included unique state by year interactions (instead of separate state and year indicators) and the results were nearly identical.

differentially affected the treatment and control groups over the course of our sample frame. If trends in smoking would have differed by family size for unobservable reasons even in the absence of OBRA, then our strategy would not identify the effect of interest (we address this possibility in the next paragraph). However, time-invariant differences in smoking behavior by family size are not a threat to our strategy.[12]

After the DD estimation, we additionally compare women without a college degree to women with a college degree. The associated indicator variable is $Treatment_{ist}$ which is used to compare mothers without a college degree (the "treatment" group) to mothers with a college degree (the "control" group). These models are estimated by introducing a third difference as shown in the following equation:

$$
\begin{aligned}
y_{ist} = {} & \beta_0 + x_{ist}\beta_x + MultiKids_{ist}\beta_{Kids} + Treatment_{ist}\beta_{Treat} \\
& + MultiKids_{ist} * Treatment_{ist}\beta_{KidsXTreat} \\
& + (MultiKids_{ist} * Treatment_{ist} * OBRA_t)\beta_{DDD} \\
& + \lambda_t + MultiKids_{ist} * \lambda_{KidsXt} + Treatment_{ist} * \lambda_{TreatXt} + \delta_s + \varepsilon_{ist}
\end{aligned}
\tag{2}
$$

This DDD is equivalent to equation (1) except the interaction term is replaced by a full set of interactions between the indicator variables for subgroups according to number of children, pre- or post-OBRA benefit expansion, and the treatment group. It also differs in that we allow the effect of family size and treatment status to vary across years by including a full set of interactions between indicator variables for year and $MultiKids_{ist}$ and year and $Treatment_{ist}$ (the interacted year indicator variables are denoted λ_{KidsXt} and $\lambda_{TreatXt}$). The coefficient on the term multiplying all three, denoted by β_{DDD}, is then reported as the parameter of primary interest. In

[12] One indication of whether this assumption is violated is if narrowing the pre- and post- sample time frames produces different results. When restricting the sample years studied to 1993 through 1998 (instead of 1993 through 2001), our results broadly hold.

this case, it represents the change in the outcome of interest after OBRA for a family with more than one child relative to a family with one child, for a mother who did not complete college relative to a mother who did. Thus, the DDD technique will yield the effect of the EITC expansion on smoking behavior even if there are differential smoking trends by family size (as long as those trends are similar across women without and with a college degree).

The DD and DDD analyses yield the overall effect of EITC expansion on smoking behavior without the possibility of decomposing income and employment status effects. However, they offer a clean identification strategy based on differences according to family size as described above. In addition, identification does not rely on variation across states which may yield biased estimates if not all of the relevant time-varying state level factors are included as covariates.

4. Data

To investigate the relationship between the EITC and smoking behavior, we use data drawn from the Behavioral Risk Factor Surveillance System (BRFSS).[13] Summary statistics for the sample used in the analysis are reported in Table 4. BRFSS is used by the Centers for Disease Control and Prevention (CDC) to monitor indicators of the health of the U.S. population. The survey is collected by all U.S. states and territories throughout each year and allows for national and state level estimates of a number of health behaviors and characteristics. For this analysis, the ability to identify a respondent's state of residence is valuable in controlling for unobserved state-level determinants of smoking. The advantages of BRFSS over other data sets that might be used for the goals in this paper include a large sample size (greater statistical

[13] These data and their documentation are available for download at http://www.cdc.gov/brfss/ (last accessed July 28, 2011).

power), a nationally representative sample, and the inclusion of smoking-related survey questions in every year during, prior to, and after the OBRA expansion period.

Since the EITC likely has no effect on the behavior of much of the population (including those with earnings that are well above its income range), we need to restrict our sample to those individuals who are likely to be eligible for benefits and thus susceptible to behavioral changes when benefits change. A difficulty associated with this is that a restriction based on income could introduce sample-selection bias given that earnings may themselves depend on benefit levels. To deal with this issue, we follow the literature by restricting the sample based on educational level. In particular, we focus on women who have not obtained a college degree. In contrast to much of the previous literature, however, we break the results down by women with a high school education or less and with some college experience. The inclusion of women with some college experience (but no degree) in our sample is based on the analysis described in Section 3.1 and Tables 2 and 3, in which we show that in terms of eligibility and benefits, these women are very similar to women with no college experience. In addition, we follow Evans and Garthwaite (2010), who also use BRFSS in part of their analysis, by restricting the sample to those women between the ages of 21 and 40. Qualifying children for the EITC must be under age 19 or under age 24 and full-time students (Evans and Garthwaite, 2010). Our measure of qualifying children in BRFSS is those who are 18 and younger and living in the household. Thus, we do not include women who are older than 40, who are more likely to have qualifying children living outside the household.

BRFSS includes multiple questions on cigarette consumption. Here we follow CDC's construction of an indicator variable for whether a respondent's smoking status is "current

smoker."[14] More specifically, we assign the label "current smoker" to individuals who respond affirmatively to the question "Have you smoked at least 100 cigarettes in your entire life?" and who respond "Everyday" or "Some days" to the question "Do you now smoke cigarettes every day, some days, or not at all?". Accordingly, we assign the label "not a current smoker" to individuals who either respond negatively to the first question or respond "Not at all" to the second question. As seen in Table 4, approximately 34 percent of women with a high school education or less, 24 percent of women with some college education, and 12 percent of women with a college degree were considered current smokers in the mid- to late-1990's.

BRFSS also includes questions regarding employment status. In addition to exploring the EITC's effect on smoking behavior, in Appendix A.2 we estimate the EITC's effect on employment outcomes to compare with those identified in earlier studies.[15] Here, a respondent is defined as "employed" if he or she responds "Employed for wages" or "Self-employed" to the question "Are you currently: [employed for wages, self-employed, out of work, a homemaker, student, retired, or unable to work?]" and is identified as "not employed" if he or she responds otherwise. Almost two-thirds of the women in our sample are employed under this definition.

Our models also include indicators for demographic characteristics provided by BRFSS respondents. These characteristics are the age, sex, marital status (for those models that include both married and single mothers), number of children, race, and ethnicity of the respondent in

[14] We explored the possibility of analyzing the intensive margin of smoking, but the BRFSS data are incomplete in that they only ask daily smokers the quantity of cigarettes smoked.

[15] We find, consistent with previous studies, that OBRA has a positive employment effect on women, particularly those who are unmarried, and that this holds for both women with a high school diploma or less as well as women with some college experience but no college degree.

addition to whether the respondent received a high school diploma or attended some college but did not graduate (for those models that include both education groups).[16]

5. Causal Effects of the EITC on Smoking

We examine whether the 1996 EITC expansion affected the smoking behavior of women who were likely to experience a change in employment and/or earnings as a result of it. Table 5 contains DD effects of the EITC on smoking estimated by comparing women with 2 or more children to women with 1 child before and after 1996, separated by education level and marital status. Included in the regressions are dummy variables for each age between 21 and 40 years, dummy variables for number of children under age 18 in the household, year, state, and month dummies, a dummy variable indicating whether the individual graduated from high school and one indicating whether she attended college (excluded when we look at these groups separately), a dummy variable indicating whether the individual is married (excluded when we look at married and single women separately), and black and Hispanic dummies.

When considering all mothers with less than a college degree, we observe that the EITC expansion decreased the probability of reported daily smoking by 1.3 percentage points (significant at the 10 percent level) for all mothers and 2.5 percentage points (significant at the 5 percent level) among single mothers only. The latter effect represents a 6 percent decline at the pre-expansion mean (39 percent) of the smoking indicator among single mothers. Given that in Section 3.1 we found the differential average EITC payment to be larger for single women than for all women, it is not surprising that the effect of the EITC on smoking is much larger for

[16] We also estimated models removing controls for marital status and education level, adding a control for state cigarette taxes, and without any controls in an unadjusted DD framework. Results were very similar to those presented in the paper, so we do not include them for the sake of parsimony.

single mothers than it is for the sample as a whole. This is in line with the idea that the EITC had a larger effect on labor force participation and earnings for the former group.

Turning to estimates of EITC expansion effects by education level (mothers with a high school education or less separate from mothers with some college experience), it is evident that the decreases for the entire sample occurred mostly among women with some college education. Notably, for single mothers with some college, the probability of reported daily smoking declined by 4.4 percentage points (significant at the 1 percent level), which represents a 13 percent decline at the pre-expansion mean (33 percent). This point estimate is significantly different from the point estimate (1.3 percentage points) for single mothers with a high school diploma or less at the 10 percent level. The results in Table 5 therefore suggest that, even for two groups who receive similar EITC payments and who have similar labor supply responses (see the Appendix), their smoking behavior responses may be substantially different.

Although this is perhaps surprising, there may be differences in how single mothers change smoking behavior across education groups, particularly in response to exogenous shocks such as changes in income. Our results are consistent with earlier research demonstrating that individuals with higher levels of education exhibited greater or more rapid responses in reducing smoking. Kenkel (2007) explores the evolution of the well known smoking-education gradient over time, finding that the gradient persisted even after knowledge of smoking's adverse health effects became well known across all education levels. More specifically, Kenkel reports that the smoking cessation rate was approximately 0.5 percentage points higher for females with some college education compared to women with a high school diploma or less by the early 1990's, and this gap was if anything widening since the 1950's (the most recent reported year is 1991). Aizer and Stroud (2010) and de Walque (2010) similarly find that women with higher

levels of education exhibited faster responses to the spread of information regarding the health risks of smoking. Studying cigarette taxation and restrictions on smoking in public places, Chaloupka and Wechsler (1995) find large negative price elasticities of smoking participation and overall cigarette smoking among college students. Wray et al (1998) estimate that the probability of smoking cessation varies by education level in response to health shocks, specifically heart attacks. They find that the probability of cessation increases with each higher level of education, from less than high school to a post-baccalaureate degree.

We have discussed how the effect of the EITC on smoking status is likely due to influences that are related to both labor supply and income. The labor supply effect cannot be parsed out from the income effect using our data and methodology, but others (Dahl and Lochner, 2010; Evans and Garthwaite, 2010) argue that the predominant effect of the EITC on child achievement (in the former paper) and maternal health (in the latter paper) is through an increase in income.[17] As discussed earlier, Table 3 shows that the differential pre- vs. post-OBRA increase in average payment to single mothers with more than one child relative to those with one child was $554.61 for women with a high school diploma or less and $530.36 for women with some college. If the smoking response is interpreted as an income effect (again, likely a heroic interpretation), our results suggest that raising the incomes of single mothers with a high school diploma or less by $1,000 lowers their probability of smoking by around 2.3 percentage points, and raising the incomes of single mothers with some college education by $1,000 lowers their probability of smoking by around 8.3 percentage points.

The DD strategy used to obtain the results in Table 5 accounts for unobserved factors that influence smoking and are correlated with family size, as long as those factors do not change

[17] For example, we cannot control for labor-force participation effects simply by including it as a right-hand side variable in our regressions, since the composition of those in the labor force is likely to change as a result of the legislation.

over time. In other words, as long as the trends in smoking status (in the absence of the EITC expansion) are similar across mothers with 2 or more children and mothers with 1 child, the DD coefficient will represent the impact of the EITC expansion on smoking.[18]

A potential concern with our identification strategy is that for our sample, smoking trends over the 1993-2001 period would have been different by family size even in the absence of the EITC expansion, particularly because several other changes to U.S. safety-net programs also occurred during this time. As described in Evans and Garthwaite (2010), these include major changes to state welfare programs (these were gradually phased in by states receiving welfare waivers before the 1996 passage of the Personal Responsibility and Work Opportunity Reconciliation Act, which implemented welfare reform nationwide) as well as expansions in Medicaid coverage over the 1990's.[19] In both cases, it is likely that these changes had similar effects on women with two or more children and women with one child, such that any resulting changes to smoking would be absorbed by the year effects in our regressions (and thus pose no threat to the identification of our DD coefficient).[20] Furthermore, when we include a full set of state by year dummies in our regressions (to account for differential changes in welfare policies across states), our results are unchanged (see footnote 18). However, we are sensitive to the possibility that factors other than the EITC may have led to a relative decline in smoking for

[18] Though this assumption can never be tested directly, we can further scrutinize it by examining whether pre-treatment trends in smoking varied by number of children. Since BRFSS data on number of children in the household only became available in 1993, we can only examine whether 1994 and 1995 year effects differed (from 1993) by number of children. We do not find evidence of systematic differences along these lines. When comparing single mothers with 2 or more children to those with 1 child, the p-value associated with the F-test that 1994 and 1995 year effects jointly differed for those with 2 or more kids is 0.31.

[19] Welfare reform was generally designed to encourage work by mandating work requirements, implementing lifetime limits on benefits, increasing earnings and asset limits for eligibility, and other measures (Evans and Garthwaite, 2010).

[20] With regard to Medicaid, Evans and Garthwaite (2010) find that health insurance coverage did not increase for mothers with two or more children relative to mothers with one child around the time of the EITC expansion.

mothers of larger families over our sample frame, so we implement two additional robustness checks to guard against this possibility.

The first thing we do to account for the possibility that smoking trends over 1993-2001 were different depending on family size is to employ a triple-difference (DDD) technique in which we compare smoking changes before and after OBRA, by family size, but for women without a college degree against women with a college degree. If OBRA was responsible for the changes in smoking status by number of children for women who did not graduate from college, we would not expect to see the same trend by number of children for women who did graduate from college (since the latter group are unlikely to have been affected, given that their incomes are generally too high to qualify for EITC benefits). Thus, the advantage of this technique is that it will account for differential smoking trends that are correlated with family size, as long as those trends are similar by education level.

Table 6 displays the DDD results. This table shows that including a third difference in the estimation yields effects on smoking that are smaller than their DD counterparts in Table 5. With the caveat that the standard errors increased in magnitude, the results indicate that any negative smoking trend related to the EITC for college-educated women was smaller than the trend for women without a college degree, particularly when the comparison is with women with some college. According to the estimates in Table 6, as a result of OBRA, some-college mothers with two or more children saw a decline in the likelihood of smoking of 1.9 percentage points relative to some-college mothers of one child, all relative to the same difference among mothers with college degrees. Although not conclusive, these results are consistent with the argument that the decline in maternal smoking is attributable to the EITC expansion.

We also follow Hotz, Mullin, and Scholz (2006) by performing a "falsification" exercise in which we consider the effect of the EITC on mothers with 3 or more children relative to mothers with exactly 2 children in a DD framework. Because OBRA did not increase benefits more for 3-plus child families than it did for 2-child families, differential trends in smoking between these groups from before to after the legislation would likely not be attributable to the EITC. Rather, a negative relative trend for 3-plus child families would suggest that smoking rates were simply on a relative decline in general for women with larger families over the 1990's (which is a competing explanation of our main results). In short, we do not find evidence in favor of this alternative story. Table 7 shows the results of this placebo test. When comparing 3-plus child mothers to 2-child families, the DD coefficient is never significantly different from zero for each of the subgroups shown (DD coefficient are actually positive, though not statistically significant, in the case of mothers with some college). For example, the DD coefficient for our full sample is insignificantly positive (point estimate of 0.001 with a standard error of 0.007). These results are reassuring in the sense that the differential trends in smoking behavior by family size occur for two-plus child versus one-child families specifically (rather than occurring for larger families in general), adding further support to our hypothesis that OBRA is responsible for these results.

As a final robustness check of our results, we conduct an event study in which we estimate a modified version of equation (1) where instead of interacting an indicator for the treatment group with an indicator for pre- and post-OBRA expansion we include a full set of year dummy interactions with the treatment group indicator. From these estimates we constructed Figure 1 which depicts the coefficients on these interactions as well as 95% confidence intervals. Consistent with the stronger estimated effect for mothers with some

college experience in earlier models, there is a sustained decrease in the coefficient beginning in 1996 (particularly relative to the other education groups), although in no year is the coefficient statistically distinguishable from zero. In general, these results neither confirm nor refute our earlier results, but they are also difficult to interpret in part because the pre-OBRA period duration is relatively short.

Our DD and DDD results, in combination with the results regarding differential EITC benefit expansions by education level presented in Tables 2 and 3, underscore the importance of analyzing EITC effects with careful attention paid to education. Our findings are consistent with those in other literatures that suggest that educational attainment is correlated with responsiveness to health interventions (Chaloupka and Wechsler, 1995; Wray et al, 1998). Because other papers in the EITC literature have generally only considered effects for individuals with a high school diploma or less, it is possible that they are missing an important part of the full distribution of effects.

6. Discussion and Conclusions

This paper contributes to the growing body of evidence describing the effect of EITC expansions on low-income families and individuals. Using both difference-in-differences (DD) and triple-difference (DDD) estimation strategies, we find that a differential EITC expansion in the early- and mid-1990s reduced the smoking prevalence of mothers with 2 or more children relative to mothers with 1 child (by around 1.3 percentage points). These effects are larger for single women; in particular, we estimate that the EITC expansion reduced the probability of smoking among single mothers by around 2.5 percentage points. This effect is comparable to the estimated effects of other successful tobacco control policies. For example, Colman and Remler (2008) find that the price elasticity of smoking participation for individuals in the lowest family

income tercile is 0.243, so the EITC expansion had an effect similar to a 10% increase in the price of cigarettes. Furthermore, this effect was largely a result of changes in the smoking prevalence of women with some college education but without a college degree. The reduction in maternal smoking was likely through a combination of income and labor-supply related effects.

These results have direct implications regarding tobacco policy in the United States. We find that the EITC, a policy that incents workforce participation and raises income levels among low-wage workers, is an additional (and perhaps alternative) way to reduce smoking among a population for which smoking rates remain high. Many states have their own version of the EITC (these are usually patterned after the federal program, but on a smaller scale), and our results suggest that expansions of these programs are likely to improve health of low-income families by curtailing smoking. College experience plays a key role in mediating the effects of the EITC on smoking behavior: even among women who are likely to qualify for the EITC, those who have some college experience are most likely to respond to the EITC's work incentives and income supplementation by reducing their use of tobacco. Thus, existing policies that are targeted to improving health (e.g. reducing smoking) among low-income populations may be limited in their ability to reach those with the lowest levels of education (though more evidence is needed on this topic).

Many low-income families and individuals benefited from federal and state EITC expansions during the 1990s. More recently, however, some proposals to change the EITC have taken the form of benefit reductions. In 2011, the New Jersey EITC was reduced by 5 percentage points of the federal benefit while the average EITC benefit in Michigan was reduced from an estimated $432 per year to $138 per year. A recent newspaper editorial argued

that if the response of many Republican presidential candidates and lawmakers to the rise in the share of Americans that do not pay federal income tax since the 2007 recession – that "everyone needs to have some skin in the game" – were taken to its logical conclusion, then the federal EITC must be scaled back (The New York Times, 2011).

Regardless of whether a proposal to change the federal or a state's EITC benefit schedule represents an expansion or reduction, it is important to understand all of the results of such a change. If a decrease in EITC benefits were to increase smoking prevalence among low-income mothers, as our results suggest, then such a decrease would worsen the health of newly smoking mothers and their children and in turn increase associated medical costs. In addition, the distributional consequences of such a decrease would likely be a net transfer from low-income mothers to other taxpayers, both in the form of reduced EITC benefit payments received and increased cigarette taxes paid.

Finally, our results have broader implications for the success of policies aimed at reducing smoking through increasing income or labor supply. The EITC expansion in the mid-1990's was notably effective in reducing smoking among single mothers with some college education. However, the finding that mothers with a high school degree or less were less affected by the policy change highlights the continuing challenge of reducing smoking among populations with low educational attainment.

A. Appendix

A.1. Analysis of State and Federal EITC Benefits

In this section, we conduct an analysis leveraging variation in EITC benefits across states and over time. We present reduced form estimates of the effects on smoking in addition to an

exploratory analysis of what may contribute to the overall impact of EITC expansion on smoking by focusing specifically on labor force participation and household income. Other studies have considered labor supply effects on and income elasticities of smoking, so by analyzing these mechanisms we in part offer a comparison with those studies. To do this, we estimate OLS and 2SLS models where maximum total EITC benefits (the total of federal and state benefits) available in one's state in a given year for a given number of dependent children serve as an instrument for income or employment status.

For the sake of parsimony, we first describe the 2SLS model and then explain how the reduced form model is a simple modification of the 2SLS model. The 2SLS models are estimated in two stages, the first of which is:

(A.1.1) $$z_{ist} = \beta_0 + x_{ist}\beta_x + w_{st}\beta_w + TotalEITC_{ist}\beta_{TotalEITC} + \lambda_t + \delta_s + u_{ist}$$

Here, we regress household income or, alternatively, an indicator variable for employment status z_{ist} on a vector x_{ist} of indicator variables for individual and household characteristics, each state's unemployment rate and cigarette tax included in the vector w_{st}, and maximum total EITC benefits (described in further detail below). Also, λ_t is a vector of indicator variables for each year and month, δ_s is a vector of indicator variables by state, and u_{ist} is the error term. In the second stage of each 2SLS model the maximum EITC benefit serves as an instrument for income or employment status (z_{ist}) in a regression of smoking status y_{ist} on the same additional regressors as in equation (A.1.1) as well as z_{ist}:

(A.1.2) $$y_{ist} = \delta_0 + x_{ist}\delta_x + w_{st}\delta_w + z_{ist}\delta_z + \gamma_t + \tau_s + \varepsilon_{ist}$$

Thus, the standard IV assumption in this context is that EITC benefits are uncorrelated with unobserved factors that influence smoking (captured in ε_{ist}). The reduced form model is

estimated using OLS by substituting the variable measuring maximum total EITC benefits in for z_{ist} into equation (A.1.2).

In order for this approach to yield unbiased causal estimates of either income or employment status on smoking status it is necessary that the maximum total EITC benefit influences smoking behavior only through its effect on income or, alternatively, employment status. This is precisely why this analysis is exploratory, however; the models are motivated by the possibility that EITC expansion may affect smoking behavior through both income and employment status and yet assume that the other channel is not operative when focusing on each of the two effects. Controlling for income (when employment status is instrumented with EITC benefits) and controlling for employment status (when income is instrumented with EITC benefits) does not resolve the problem, since both variables are likely endogenous in the sense that they are correlated with unobserved factors that affect smoking. Thus, without a suitable second instrument, which is not available for this analysis, it is not possible to separately identify the true causal effect for each. Rather, the question we seek to answer here is, do we find suggestive evidence that both income and employment status each potentially affect smoking behavior of low-educated women (when they change as a result of changes in EITC benefits)?

The policy variable of interest is the maximum total EITC benefit available in one's state of residence in a given year for the number of dependent children in that person's household. In addition to the federal EITC program, many states provide their own EITC benefits (which are almost always simply a fraction of the federal EITC), which have varied significantly over time. In practice, since we control for time and state fixed-effects in our models, effective variation in benefits comes by way of within-state variation in benefit rules (including EITC program adoption itself) as well as variation in federal rules that affected families differently depending

on their number of children (for example, the OBRA expansion utilized in our main empirical analysis in Section 5).[21] Data on state-level EITC benefits were obtained from the University of Kentucky's Center for Poverty Research (UKCPR).[22] The UKCPR provides information on U.S. state-level economic and program transfer data for the years 1980-2010. In addition, we use unemployment rate information by state and year (also from UKCPR) as well as cigarette tax information by state and year (from Impact Teen and the Tax Foundation).[23]

Summary statistics for the sample used in this analysis are reported in Table A.1.1. The sample includes all women age 21-40 with less than a college degree. Since BRFSS only collects information on annual household income categories (specified in ranges of $5,000 or $10,000 for the income levels relevant to this analysis) we assign each respondent an income equal to the (deflated) midpoint of his or her income category. Tables A.1.2 and A.1.3 present results from the first and second stages of 2SLS models as described above. All estimates include dummies for individual-level demographic characteristics as described in Section 5.

The first stage coefficients for the effect of maximum total EITC benefits on log household income and employment status are reported in Table A.1.2. Not surprisingly, an increase in the maximum total benefit increases household income and the probability of employment, particularly for single women. These positive and significant effects are broadly similar to those found in previous research (see, for example, Kenkel, Schmeiser, and Urban, 2011). Because EITC benefits pass weak instrument tests for both income and employment among single mothers, we focus on that subgroup when reporting the second stage results.[24]

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[21] To be consistent with others (e.g. Kenkel, Schmeiser, and Urban, 2011), we include childless women in our analysis here, even though they are not included in our main empirical models in Section 5. However, if we drop these women from the analysis presented in this section, our results are qualitatively similar.
[22] These data are available for download at http://www.ukcpr.org/ (last accessed July 28, 2011).
[23] See http://www.impacteen.org/tobaccodata.htm and http://www.taxfoundation.org/publications/show/245.html.
[24] This is expected from our earlier discussion of average EITC payment changes reported in Tables 2 and 3.

Turning to the second stage of the 2SLS analysis, Table A.1.3 reports coefficients from the separate income and employment equations. Using these coefficients we calculate the income and employment elasticities with respect to the probability of being a current smoker, and these are reported as -0.80 and -0.81, respectively, in Table A.1.3. The income elasticity, for example, would imply that a $1000 increase in household income would result in a 1.2 percentage point decrease in the probability of being a current smoker (from a mean smoking prevalence of 37% and income of $24,583 for this sample). Kenkel, Schmeiser, and Urban (2011) conduct a similar 2SLS analysis of income (but not employment), and they find a notably larger (in absolute value) and *positive* income elasticity with respect to whether a respondent smoked in the last year (using data from the Current Population Survey's Tobacco Use Supplement between 1993 and 2007).

The calculated income and employment elasticities cannot be interpreted as causal for reasons already discussed, particularly because each equation omits the other endogenous variable.[25] Since we are not aware of a suitable second instrument we cannot instrument for both variables simultaneously.[26] We therefore consider the results in Table A.1.3 to be merely suggestive that both the income and employment effects of changes in EITC benefits could be important in influencing smoking behavior. In particular, two prominent goals of the EITC, namely to increase income and encourage labor force participation among low income households, may have beneficial effects with respect to the smoking behavior of women who have less than a college degree.

[25] For this reason, Kenkel, Schmeiser, and Urban (2011) explain that their estimated income elasticities really must be interpreted as a "combination of the income effect and the labor force participation effect" rather than a pure income effect.

[26] We also estimated each specification with the "other" endogenous variable included but treated as exogenous (by not instrumenting for it), and the estimated elasticities remained qualitatively similar. However, as we have argued, this specification does not resolve the basic problem of parsing the effect of income from the effect of labor supply changes.

In order to provide a more direct comparison of this analysis with the paper's primary analysis we report reduced form estimates of the effect of maximum EITC benefits on smoking in Table A.1.4. Here, an increase in the maximum total benefit yields a significant decrease in the probability of smoking for all and single women, with a larger effect for the latter. The signs of the coefficients and their relative magnitudes are consistent with the findings in the primary analysis. Also, the finding that a $1,000 increase in the maximum total benefit yields a decrease in the probability of smoking by 1.7 percentage points is roughly comparable to the back-of-the-envelope calculations presented earlier using the estimated average OBRA payment expansions. In this case the effect is somewhat smaller in absolute value, but the qualitative conclusions are robust to whether the source of variation studied is the federal OBRA expansion or state-level variation in maximum total federal and state EITC benefits.

A.2. The Effects of the EITC on Employment

In this appendix, we estimate the effect of the EITC on the probability of individual employment using BRFSS. This serves two purposes. The first is to examine whether our strategy for identifying an effect of the EITC on smoking behavior (using BRFSS) yields a significant positive effect on labor supply, as has been demonstrated in the literature (in which BRFSS has seldom been used). The second is to examine whether the disaggregation by education level in our primary analysis (of smoking behavior) is supported by a labor supply analysis. In other words, we seek further evidence that supports considering women with a high school diploma or less and women with some college to be the relevant comparison group to women with a college degree when conducting DDD analyses, in contrast to previous work which does not place the two former groups on the same "side" of the third difference.

Table A.2.1 has the same structure as Table 5 except the dependent variable is exchanged: it contains DD effects of the EITC on employment status rather than smoking status. All estimates are regression-adjusted as they were for the smoking analysis, and the same sample is used (21-40 year-old mothers with less than a college degree). When considering all mothers with less than a college degree, the EITC expansion taking effect in 1996 increased the probability of employment by 2 percentage points (which is roughly a 3 percent difference at the pre-expansion mean of the employment indicator, which is 62 percent). Not surprisingly, this effect is concentrated among single mothers (for whom the increase is 3.9 percentage points, or roughly 6 percent at their pre-expansion mean). The employment response for married women is expected to be lower than for single women because in the former case labor supply decisions are made jointly with a spouse.[27]

Turning to the results estimated when restricting the sample to mothers with a high school diploma or less separately from those with some college experience, the evidence is that the labor supply responses are similar for the two groups. All of the estimates are positive and significant at the 5% or 1% level. The estimates for all mothers are nearly identical and those for single mothers are not statistically significantly different across the two education subgroups.

Our estimates of the effect of the OBRA expansion on female employment are therefore similar overall to those obtained in Hotz, Mullin, and Scholz (2006) and Evans and Garthwaite (2010). Broadly speaking, these results confirm the positive labor supply effect of the OBRA expansion previously identified in the literature. It is also clear that women with some college experience have broadly similar labor supply responses to those of women with a high school diploma or less.

[27] Eissa and Hoynes (2004) find that labor force participation in fact decreases in response an EITC expansion among married women (while participation increases among married men).

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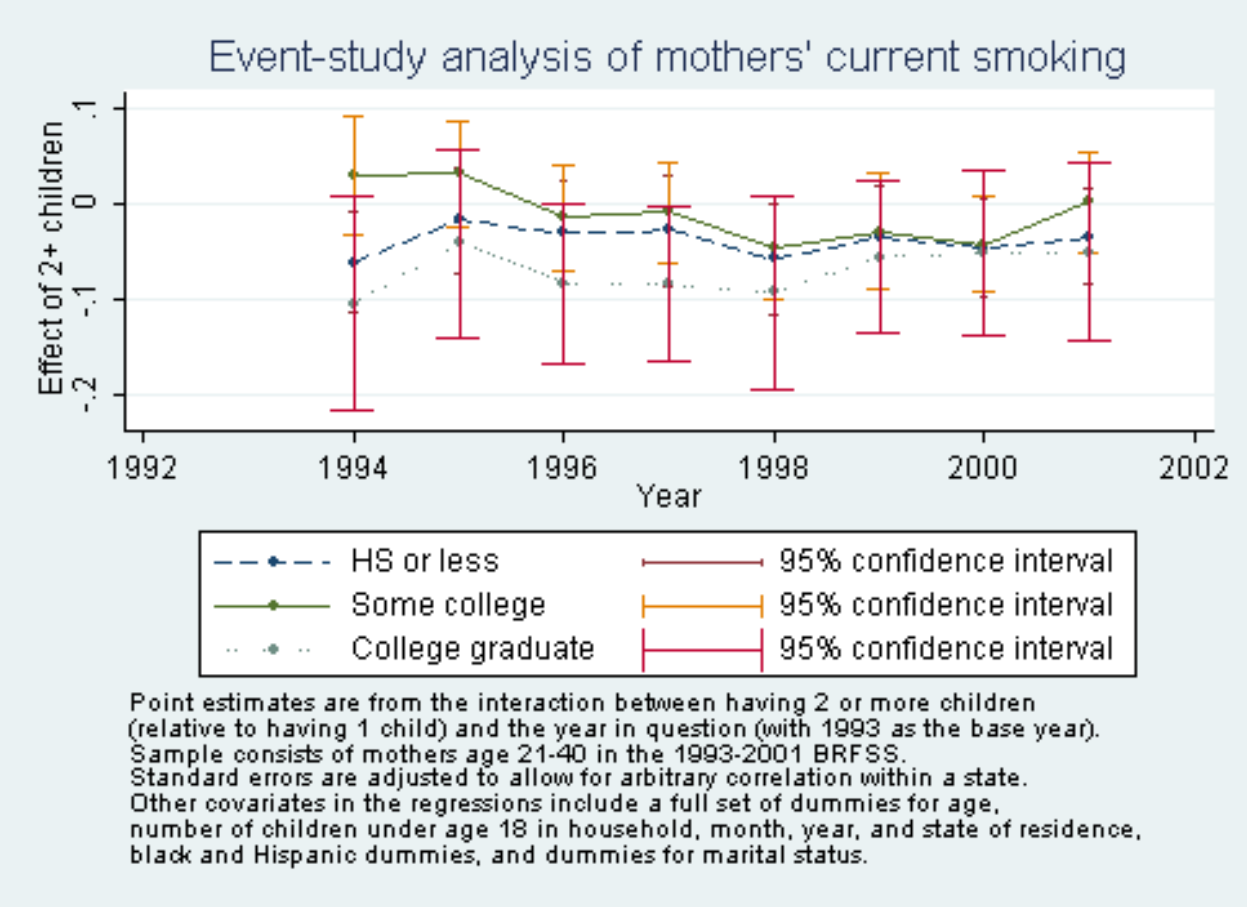
Figure 1. Event-study analysis of mothers' current smoking



Notes: Point estimates are from the interaction between having 2 or more children (relative to having 1 child) and the year in question (with 1993 as the base year). Sample consists of mothers age 21-40 in the 1993-2001 BRFSS. Standard errors are adjusted to allow for arbitrary correlation within a state. Other covariates in the regressions include a full set of dummies for age, number of children under age 18 in the household, month, year, and state of residence, black and Hispanic dummies, and dummies for marital status.

Table 1. EITC benefit structures, 1993 and 1996 Tax Years

	No children			One child			More than one child		
	1993	1996	Δ	1993	1996	Δ	1993	1996	Δ
Phase-in lower	N/A	$0	N/A	$0	$0	$0	$0	$0	$0
Phase-in upper	N/A	$4,220	N/A	$7,750	$6,330	-$1,420	$7,750	$8,890	$1,140
Phase-out lower	N/A	$5,280	N/A	$12,200	$11,650	-$550	$12,200	$11,650	-$550
Phase-out upper	N/A	$9,500	N/A	$23,050	$25,078	$2,028	$23,050	$28,495	$5,445
Max benefit	$0	$323	$323	$1,434	$2,152	$718	$1,511	$3,556	$2,045
Phase-in rate	0.0%	7.7%	7.7%	18.5%	34.0%	15.5%	19.5%	40.0%	20.5%
Phase-out rate	0.0%	7.7%	7.7%	13.2%	16.0%	2.8%	13.9%	21.1%	7.2%

Table 2. Earned Income Tax Receipt by Education and Number of Children, Women Age 21-40, CPS

	HS Grad or less		Some college		College graduate	
	One child	More than one child	One child	More than one child	One child	More than one child
	Tax Years 1993-1995					
% receiving EITC	25.15%	17.28%	23.83%	14.19%	7.34%	3.63%
Avg EITC payment	$308.88	$254.55	$269.95	$188.91	$80.58	$44.48
Avg EITC payment to recipients	$1,228.08	$1,472.96	$1,132.97	$1,331.63	$1,097.40	$1,226.52
N	7,443	14,987	5,221	8,311	2,819	4,605
	Tax Years 1998-2001					
% receiving EITC	31.59%	26.78%	25.79%	19.30%	7.89%	4.87%
Avg EITC payment	$504.57	$625.49	$380.32	$419.40	$107.09	$88.78
Avg EITC payment to recipients	$1,597.36	$2,335.60	$1,474.88	$2,173.03	$1,357.07	$1,824.09
N	9,222	18,364	6,899	11,378	4,524	7,335

Notes: Percents and averages are calculated using the Annual Demographic Survey of the (March) Current Population Survey, 1994-1996 and 1999-2002. The sample is restricted to all women age 21-40.

Table 3. Earned Income Tax Receipt by Education and Number of Children, Unmarried Women Age 21-40, CPS

	HS Grad or less		Some college		College graduate	
	One child	More than one child	One child	More than one child	One child	More than one child
	Tax Years 1993-1995					
% receiving EITC	52.59%	43.27%	47.98%	49.53%	29.70%	37.29%
Avg EITC payment	$646.85	$641.44	$548.56	$683.08	$326.22	$458.19
Avg EITC payment to recipients	$1,229.88	$1,482.31	$1,143.36	$1,379.24	$1,098.37	$1,228.63
N	2,987	4,296	2,299	1,898	569	362
	Tax Years 1998-2001					
% receiving EITC	59.33%	62.10%	48.90%	61.25%	29.85%	36.17%
Avg EITC payment	$974.19	$1,523.39	$733.97	$1,398.85	$413.36	$679.03
Avg EITC payment to recipients	$1,642.12	$2,453.04	$1,500.94	$2,283.74	$1,384.62	$1,877.31
N	4,032	5,214	3,184	2,635	958	611

Notes: Percents and averages are calculated using the Annual Demographic Survey of the (March) Current Population Survey, 1994-1996 and 1999-2002. The sample is restricted to all unmarried women age 21-40.

Table 4. Summary statistics for 21-40 year-old mothers by education level, 1993-2001 BRFSS

	HS diploma or less (N=84,045)		Some college (N=60,432)		College graduate (N=29,334)	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Current smoker	0.34		0.24		0.12	
Employed	0.60		0.69		0.72	
Age	31.39	5.59	31.71	5.50	32.82	4.75
Number of children under 18 in household	2.15	1.11	1.99	1.01	1.87	0.95
Two or more children under 18 in household	0.69		0.64		0.60	
Married	0.65		0.68		0.77	
Black	0.14		0.15		0.11	
Hispanic	0.21		0.11		0.08	

Notes: Estimates are weighted according to BRFSS sample weights.

Table 5. Difference-in-differences estimates of the effect of the EITC on mothers' current smoking status, 1993-2001 BRFSS

	HS or less & some college		HS or less		Some college		College graduate	
	all mothers	single mothers	all mothers	single mothers	all mothers	single mothers	all mothers	single mothers
(Two or more children) x (post EITC expansion)	-0.013*	-0.025**	-0.007	-0.013	-0.022**	-0.044***	-0.003	-0.016
	(0.007)	(0.011)	(0.009)	(0.013)	(0.008)	(0.016)	(0.009)	(0.020)
Observations	144,477	55,913	84,045	34,257	60,432	21,656	29,334	8,120
R-squared	0.084	0.083	0.081	0.094	0.059	0.055	0.05	0.04

Notes: *** p<0.01, ** p<0.05, * p<0.1. Sample consists of mothers age 21-40. Standard errors are adjusted to allow for arbitrary correlation within a state. Other covariates in the regressions include a full set of dummies for age, number of children under age 18 in household, month, year, and state of residence, black and Hispanic dummies, and dummies for marital status, high-school completion, and college attendance (where appropriate).

Table 6. Difference-in-differences-in-differences estimates of the effect of the EITC on mothers' current smoking status, 1993-2001 BRFSS

	HS or less & some college		HS or less		Some college	
	all mothers	single mothers	all mothers	single mothers	all mothers	single mothers
(Two or more children) x (post EITC expansion) x (no college degree)	-0.012	-0.011	-0.006	0.003	-0.019	-0.028
	(0.012)	(0.024)	(0.013)	(0.027)	(0.013)	(0.025)
Observations	173,811	64,033	113,379	42,377	89,766	29,776
R-squared	0.102	0.090	0.121	0.109	0.075	0.062

Notes: *** p<0.01, ** p<0.05, * p<0.1. Sample size consists of mothers age 21-40 with less than a college degree. In all cases, the third difference is relative to mothers with a college degree. Standard errors are adjusted to allow for arbitrary correlation within a state. Other covariates in the regressions include a full set of dummies for age, number of children under age 18 in household, month, year, and state of residence, black and Hispanic dummies, and dummies for marital status, high-school completion, college attendance, and college completion (where appropriate), plus a full set of interactions between year and college completion and between year and having two or more children.

Table 7. Difference-in-differences estimates of the effect of the EITC on mothers' current smoking status, 1993-2001 BRFSS

	HS or less & some college		HS or less		Some college	
	all mothers	single mothers	all mothers	single mothers	all mothers	single mothers
(Three or more children) x (post EITC expansion)	0.001	-0.005	-0.007	-0.021	0.013	0.025
	(0.007)	(0.014)	(0.009)	(0.018)	(0.010)	(0.021)
Observations	98,006	33,822	58,235	21,922	39,771	11,900
R-squared	0.085	0.084	0.084	0.099	0.058	0.052

Notes: *** p<0.01, ** p<0.05, * p<0.1. Sample size consists of mothers age 21-40 with less than a college degree and at least two children. Standard errors are adjusted to allow for arbitrary correlation within a state. Other covariates in the regressions include a full set of dummies for age, number of children under age 18 in household, month, year, and state of residence, black and Hispanic dummies, and dummies for marital status, high-school completion, and college attendance (where appropriate).

Table A.1.1. Summary statistics for 21-40 year-old women with less than a college degree, 1993-2001 BRFSS (N=161,664)

	Mean	Std. Dev.
Current smoker	0.31	
Employed	0.67	
Household income ($2000)	32,246	18,071
Maximum federal plus state EITC benefits (1000's of $2000)	2.49	1.48
State unemployment rate	5.19	1.35
State cigarette tax ($2000)	0.41	0.24
Age	30.74	5.82
Number of children under 18 in household	1.57	1.30
High school diploma	0.43	
Some college	0.43	
Married	0.58	
Black	0.14	
Hispanic	0.16	

Notes: Estimates are weighted according to BRFSS sample weights.

Table A.1.2. First-stage effects of maximum EITC benefits on women's income and employment, 1993-2001 BRFSS

	Dependent variable			
	log(income)		employed	
	all women	single women	all women	single women
Maximum federal plus state EITC benefits, $1000's	0.009	0.057***	0.023***	0.040***
	(0.007)	(0.011)	(0.004)	(0.007)
Observations	161,664	75,829	161,664	75,829
R-squared	0.295	0.134	0.071	0.077
F-statistic on excluded instrument	2.11	28.64	33.84	31.10

Notes: *** p<0.01, ** p<0.05, * p<0.1. Sample size consists of women age 21-40 with less than a college degree. Standard errors are adjusted to allow for arbitrary correlation within a state. Other covariates include the unemployment rate and cigarette tax (by state and year), a full set of dummies for age, marital status, number of children under age 18 in household, month, year, and state of residence, as well as dummies for high-school completion and college attendance and black and Hispanic dummies.

Table A.1.3. Instrumental-variables effects of income and employment on women's current smoking status, 1993-2001 BRFSS

	Dependent variable: current smoker	
log (income)	-0.300*	
	(0.159)	
employed		-0.431**
		(0.205)
Observations	75,829	75,829
Income elasticity	-0.80	
Employment elasticity		-0.81

Notes: *** p<0.01, ** p<0.05, * p<0.1. Sample size consists of single women age 21-40 with less than a college degree. Standard errors are adjusted to allow for arbitrary correlation within a state. Excluded instrument is the maximum federal plus state EITC benefits according to one's state, year, and number of children. Control variables include the unemployment rate and cigarette tax (by state and year), a full set of dummies for age, marital status, number of children under age 18 in household, month, year, and state of residence, as well as dummies for high-school completion and college attendance and black and Hispanic dummies.

Table A.1.4. The reduced-form effect of maximum EITC benefits on women's current smoking status, 1993-2001 BRFSS

	all women	single women
	-0.008*	-0.017**
Maximum federal plus state EITC benefits, $1000's	(0.004)	(0.007)
Observations	161,664	75,829
R-squared	0.075	0.075

Notes: *** p<0.01, ** p<0.05, * p<0.1. Sample consists of women age 21-40 with less than a college degree. Standard errors are adjusted to allow for arbitrary correlation within a state. Other covariates include the unemployment rate and cigarette tax (by state and year), a full set of dummies for age, marital status, number of children under age 18 in household, month, year, and state of residence, as well as dummies for high-school completion and college attendance and black and Hispanic dummies.

Table A.2.1. Difference-in-differences estimates of the effect of the EITC on mothers' employment status, 1993-2001 BRFSS

	HS or less & some college		HS or less		Some college	
	all mothers	single mothers	all mothers	single mothers	all mothers	single mothers
(Two or more children) x (post EITC expansion)	0.020***	0.039***	0.019**	0.044***	0.021***	0.027**
	(0.006)	(0.010)	(0.008)	(0.011)	(0.007)	(0.013)
Observations	144,477	55,913	84,045	34,257	60,432	21,656
R-squared	0.070	0.083	0.075	0.092	0.053	0.052

Notes: *** p<0.01, ** p<0.05, * p<0.1. Sample size consists of mothers age 21-40 with less than a college degree. Standard errors are adjusted to allow for arbitrary correlation within a state. Other covariates in the regressions include a full set of dummies for age, number of children under age 18 in household, month, year, and state of residence, black and Hispanic dummies, and dummies for marital status, high-school completion, and college attendance (where appropriate).