Income Effects in Labor Supply: Evidence from Child-Related Tax Benefits*

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Abstract

A parent whose child is born in December can claim child-related tax benefits when she files her tax return a few months later. Parents of children born in January must wait more than a year before they can receive child-related tax benefits. As a result, families with December births have higher after-tax income in the first year of a child's life than otherwise similar families with January births. This paper estimates the corresponding income effect on maternal labor supply, testing whether mothers who give birth in December work and earn less in the months following birth. We use data from the American Community Survey, the Survey of Income and Program Participation, and the 2000 Decennial Census. We find that December mothers have a lower probability of working, particularly in the third month after a child's birth. Earnings data from the SIPP indicate that an additional dollar of child-related tax benefits reduces annual maternal earnings in the year following a child's birth by approximately one dollar.

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The birth of a child, at any point during a year, reduces her parents' tax liability for that year. This creates a discontinuity in the tax treatment of children born around the turn of the year. A child born on December 31 can be claimed on that year's tax return, typically filed in the early months of the following calendar year. As a result of the December birth, this child's parents will have lower tax liability, and will typically receive a larger tax refund, in the first few months of the child's life. With a January 1 birth, parents must wait for more than a year before they receive the tax benefits associated with claiming the child. This paper uses the discontinuous tax treatment of children born around the turn of the year to identify income effects on maternal labor supply, testing whether December mothers work and earn less during a child's first year of life as a result of the tax benefits they receive.

Tax policy changes have often been used to estimate the elasticity of taxable income and of labor supply. However, many tax changes affect both a filer's marginal tax rate and after-tax income, making it difficult to separately identify the substitution effect and income effect. The tax treatment of newborns generates differences in after-tax income of December and January parents in the first year of a child's life. However, both sets of parents face the same schedule of marginal tax rates on income earned after the child's birth. This situation provides an opportunity for a well-identified estimate of the income effect on labor supply.

Implementing this empirical test requires a dataset with precise information on date of birth, and with information on labor supply and earnings measured at various points after a mother gives birth. No single dataset is ideal for our purposes, and in this paper we present related evidence from three different datasets, each with particular advantages and disadvantages. We use data from the restricted-access version of the 1999 through 2008 rounds of the American Community Survey (ACS); restricted-access data from the 2000 Decennial Census; and publicly available data from the 1996, 2001, and 2004 panels of the Survey of Income and Program Participation (SIPP). The key advantage of the ACS and Decennial Census is that the data include exact date of birth. This allows us to identify mothers giving birth in narrow windows around the turn of the year. We can also identify exactly how much time has elapsed between when a mother gives birth and when she reports employment status and earnings. This is important because the time surrounding birth of a child is, on average, characterized by a rapid changes in employment (Laughlin 2011). However, neither the ACS nor the Decennial Census has a longitudinal component. The SIPP data offer the important advantage of following mothers over time. Because we can compare a mother's pre-birth and post-birth earnings in the SIPP, these data allow us to better address concerns that mothers giving birth in December and January might have systematically different attachment to the labor force even before receiving different tax treatment.

We find that mothers receiving more generous tax treatment, by virtue of a December rather than January birth, reduce their labor supply and earnings in the year following a child's birth. Data describing contemporaneous labor supply measures in the ACS and SIPP show very similar patterns. In both datasets we find that December mothers are about 6 percentage points less likely to be currently working in the third month after birth. As this corresponds to the time at which many maternity leaves come to an end, it is a plausible moment at which additional cash might have a strong impact on labor supply decisions.

Earnings data from the SIPP suggest that every additional dollar of tax benefit reduces earnings in the year after birth by about a dollar. In other words, the income effect of tax benefits on annual earnings is approximately -1. Because earnings are reported at the monthly level in the SIPP, we can see that the fourth and fifth months after birth are particularly important in creating the aggregate annual income effect of -1, although we do observe sustained differences in monthly earnings throughout the first year following birth. Data from the ACS and Decennial Census do not show a statistically significant relationship between tax benefits and earnings after birth. We argue that the longer retrospective period over which earnings are reported in the ACS and Decennial Census as well as the specific time structure of reported earnings make it more difficult to statistically distinguish this effect. Although an income elasticity of -1 is much larger than the consensus estimates of this parameter (Blundell and MaCurdy 1999), the year after a child's birth could plausibly be a time at which mothers' labor supply decisions are particularly sensitive to non-wage income.

The assertion that any difference in the labor supply of December and January mothers measures an income effect relies on the assumption that the two groups of mothers are similar, except for their tax treatment. This assumption will be violated if mothers are strategically manipulating birth timing around the turn of the year in response to tax benefits. In fact, there is evidence that the probability of a birth in the last week of December, relative to the first week of January, is strongly increasing with the value of child-related tax benefits (Dickert-Conlin and Chandra 1999). This result is based on observations from years 1979 to 1993, before a significant expansion in the child tax credit. Extrapolating, it was predicted that December's share of all births in the two-week window spanning the turn of the year would increase substantially when the child tax credit was expanded. This pattern has not been observed in the data. Instead, December's share of births has remained steady. More recent estimates of the tax sensitivity of birth timing are much smaller (Wingender 2010, Schulkind and Shapiro 2014, LaLumia, Sallee and Turner Forthcoming). Our analysis of ACS data shows no relationship between the size of tax benefits and the probability of a December birth for the main analysis sample, with evidence of a small positive relationship when the sample is restricted to births occurring at most one week before or after the turn of the year. Furthermore, we show that many of the mean characteristics of December and January mothers are statistically indistinguishable. Therefore, we argue that our finding of reduced labor supply on the part of December mothers, in the year that they receive more generous tax treatment, represents a response to the tax benefits they receive rather than sample selection bias.

The paper proceeds in the following way. Section 1 summarizes previous estimates of income effects on female labor supply. Section 2 provides detail on the three datasets used in the analysis. The validity of the paper's identifying assumption, that the labor supply of

December and January mothers would evolve similarly absent any difference in tax treatment, is probed in section 3. Section 4 outlines the empirical methods used, section 5 presents results, and section 6 concludes.

1 Literature Review

This paper adds to the body of evidence estimating the elasticity of female labor supply. A comprehensive survey of this work is provided by Blundell and MaCurdy (1999). Many estimates are of the uncompensated labor supply elasticity, which reflects both income and substitution effects. Among the papers cited by Blundell and MaCurdy (1999), most estimates of wives' uncompensated wage elasticity are in the range of 0.5 to 1, with a median of 0.78, and most estimates of the income elasticity are in the range of -0.05 to -0.4.[1] All of these estimates predate the 1999-2008 period analyzed in this paper, and there is evidence that female labor supply elasticities, both uncompensated and compensated, have fallen over time. Blau and Kahn (2007) estimate that wives' labor supply elasticities with respect to non-wage income are very small over the entire 1980-2000 time period, but particularly close to zero by the last years analyzed. In each time period, they find slightly larger (in absolute value) elasticities with respect to non-wage income for wives with children under age 6. Similarly, Heim (2007) finds evidence that wives' labor supply elasticities have fallen over time.

The income elasticity of female labor supply may be quite different in the months shortly after a child's birth than at other times. One reason to expect a difference is that the marginal utility of time at home is likely particularly high when an infant is first born, before gradually falling as the child grows (Klerman and Leibowitz 1994). A second reason is that maternity leave policies explicitly provide job protection for new mothers. Outside of maternity leave, responding to a positive non-wage income shock by temporarily reducing

[1] Estimates of the income effect on labor supply that rely on a very different source of exogenous variation in income, lottery winnings, find an income elasticity of -0.11, with no significant difference between men and women (Imbens, Rubin and Sacerdote 2001).

labor supply could result in job loss. However, the federal Family and Medical Leave Act (FMLA) mandates that eligible parents can take up to 12 weeks off of work to care for a newborn, while retaining the right to return to the same or an equivalent job (Ruhm 1997). Some states offer mothers longer periods of entitlement to unpaid leave. Evidence shows that the legal protection provided by mandated unpaid leave is associated with increased probabilities of mothers being on leave shortly after birth (Han, Ruhm and Waldfogel 2009).

Evidence from the introduction of *paid* maternity leave policies shows that this source of additional income lengthens maternity leaves. Rossin-Slater, Ruhm and Waldfogel (2011) study California's 2004 introduction of a paid leave benefit equivalent to 55% of pre-leave wages, up to a cap of approximately $1000 per week. This program increased average maternity leaves by about three weeks, with the largest effects estimated for relatively disadvantaged mothers. If paid maternity leave lengthens mothers' time away from work, it seems reasonable that income from other sources, including tax benefits, could also reduce labor supply following a child's birth. The most direct evidence on how government-provided cash benefits for families with children affect labor supply of new mothers comes from policy changes in Spain (Gonzalez 2013) and Canada (Milligan and Stabile 2009). Gonzalez (2013) uses regression discontinuity techniques to compare mothers giving birth just before and after the unanticipated introduction of a substantial universal child benefit.[2] Eligible mothers were four to six percentage points less likely to be working one year later. Milligan and Stabile (2009) find that a more generous child benefit for Manitoba welfare recipients with children ages 0 to 5 is associated with reductions in work among low-income families.

The welfare consequences of delayed return to work following the birth of a child are not clear. Some evidence suggests that longer maternity leaves are associated with positive outcomes: improved infant health (Rossin 2011), increased rates of breastfeeding and infant immunization (Berger, Hill and Waldfogel 2005), and reductions in the number of depressive

[2]The one-time benefit payment is €2500, about 4.5 times the monthly earnings of a full-time minimum wage worker. For comparison, the average child-related tax savings in our ACS sample is about $1200, or about 1.1 times the monthly earnings of a full-time worker receiving the $6.55 minimum wage in place at the end of our sample period.

symptoms displayed by new mothers (Chatterji and Markowitz 2005). On the other hand, studies analyzing expanded maternity leave in Canada find no significant positive effect on outcomes of young children (Baker and Milligan 2010, Baker and Milligan 2015).

Outside of the specific case of maternity leave benefits, the welfare implications of any transfer that affects labor supply will depend on the extent to which the overall labor supply effect is composed of income and substitution effects. Chetty (2008) shows that optimal unemployment insurance (UI) benefits are larger when UI benefits primarily affect unemployment duration through a non-distorting income (liquidity) effect rather than through a distortionary substitution effect. Our finding of a substantial income effect for new mothers suggests that larger benefits for this group would generate little deadweight loss.

Other authors have used variation in child-related tax benefits to estimate parameters other than income elasticities. Looney and Singhal (2005) and Dokko (2008) consider the anticipated change in marginal tax rates experienced by some parents when a child turns 19 and ages out of eligibility for the dependent exemption. Only those parents whose income is very close to the edge of a tax bracket are pushed into a different bracket, with a different marginal tax rate, by losing a dependent exemption. Both papers find that parental labor supply responds to the resulting change in marginal tax rate, estimating large intertemporal substitution elasticities. Feldman, Kawano and Katuscak (2014) consider the predictable, lump-sum change in tax liability associated with aging out of eligibility for the Child Tax Credit at age 17 in order to estimate taxpayers' understanding of the tax code. A fully informed taxpayer should anticipate this loss and make no adjustment to labor supply at the time of the loss. Taxpayers who learn about the loss of credit ex post either understand that losing a lump sum credit has no effect on marginal tax rates, and do not reduce labor supply, or are confused about the nature of the tax change. The authors find a reduction in labor income among families losing the credit, interpreting this as evidence that taxpayers incorrectly attribute the increase in tax liability to an increase in marginal tax rates.

2 Data

This paper makes use of data from the ACS, the SIPP, and the Decennial Census. Here we describe the samples drawn from each of these datasets, as well as variables that are particularly important for our analysis.

2.1 ACS

We use data from the 1999 through 2008 rounds of the ACS. This is a continuously-fielded cross-sectional survey. It was introduced as the Decennial Census long form was being phased out, and covers many of the same topics. Respondents are asked about their employment status at the time of the interview, and about their income over the 12-month period prior to the interview.

In the restricted-access ACS files, exact date of birth is recorded for all household members. The exact date of the ACS interview is also available. We use these dates to calculate the time elapsed between a child's birth and the ACS interview. We restrict attention to cases in which a child was born no more than one year prior to the interview. Using information on relationships between household members, we match children to their mothers. If a mother is matched to more than one child born within the year prior to the interview date, we use information on the youngest child to compute the time elapsed since the mother's most recent birth. We restrict the sample to mothers whose most recent birth took place in the four weeks spanning the turn of the year, stretching from December 18 to January 14. We further restrict attention to mothers who are between the ages of 15 and 50 and who have had some recent attachment to the labor force, measured as having worked in the previous five years. This process results in a sample of 16,384 mothers, with approximately equal numbers giving birth in late December and in early January.

We calculate the real tax savings a mother would have realized if her child had been born in December using the NBER's TAXSIM program.[3] We estimate tax liabilities in two

[3]We are grateful to Dan Feenberg for providing a version of TAXSIM for use in the RDC.

different scenarios. First, we use household composition as reported at the time the mother is interviewed. Second, we remove the newborn child (or children, in the case of twins). For all mothers, the removal of a child from the return changes the number of dependent exemptions she can claim. Depending on income level, it may change the amounts of child tax credit and EITC for which she is eligible. If an unmarried mother has no other children living with her, this removal of children also causes her filing status to change from head of household to single. We compute the difference between the tax liability when the newborn child is excluded from the household (the higher of the two tax values) and the tax liability when the newborn is included, and refer to this difference as the child tax benefit. This value represents combined federal and state tax liabilities.[4] We assume that taxpayers take the standard deduction rather than itemizing deductions.

The TAXSIM calculations use information on several types of income. Ideally, we would like to have income corresponding to a tax year, either the year in which the birth occurred (in the case of December births) or the year just prior to the birth (in the case of January births). However, the reference period for ACS respondents does not correspond to a tax year. Instead, ACS respondents report income for the 12 months prior to the interview date. The difference between our preferred reference period and the actual reference period is unlikely to affect types of income that are independent of female labor supply. However, if December and January mothers have different patterns of labor supply after a child's birth, they may report systematically different labor income amounts and thus have different calculated child-related tax benefits. This problem will not be particularly severe for mothers whose interview date occurs very close to the date at which they gave birth, but will likely be larger for mothers interviewed several months after birth.

In order to address this problem, we compute one version of the tax benefit variable in which all components of income are taken directly from respondents' self-reports, and one

[4] In aggregate, the real federal tax savings associated with child-related provisions totaled approximately $105 billion in 2000 and $145 billion in 2010 (Isaacs, Hahn, Rennane, Steuerle and Vericker 2011). The major tax provisions contributing to these totals are the dependent exemption and child-related tax credits, with refundable portions of tax credits accounting for most of the growth over the decade.

version in which predicted labor income replaces self-reported labor income. The income components always taken directly from respondents' reports are property income (interest, dividend, and rental income) and, for married women, husbands' earnings.[5] We use demographic variables to predict a mother's wage earnings and, if she reports being self-employed, her business earnings. Our methodology is to use a sample of mothers whose youngest child is at least one year and no more than two years old at the time of the ACS interview, to regress their earnings on demographic characteristics, and then to use the coefficients from that regression to predict earnings for the mothers of younger children in our main sample.

2.2 SIPP

We use data from the 1996, 2001, and 2004 panels of the SIPP. Each panel is a nationally representative longitudinal survey that follows households for three to four years. Households are interviewed every four months, and respondents provide information on monthly employment status and income.

To construct the sample, we identify all children born during a SIPP panel or up to twelve months before the start of the panel, and with a birth month of December or January. Because exact date of birth is not reported in the public-use SIPP, we cannot restrict attention to those with birth dates in the last two weeks of December or the first two weeks of January. Children are linked to their mothers, and mothers are linked to their spouses. Only households in which a mother and child can be linked are included in the analysis. We keep mothers who are between the ages of 15 and 50 and who have had some recent attachment to the labor force, as indicated by working at least one week in the twelve months prior to birth. After applying all restrictions, we have a sample of 1051 mothers.

As we did for the ACS sample, we calculate the potential tax savings associated with a December birth for every mother in the SIPP sample. We use reported income amounts from

[5]If a mother is living with a husband or male partner, we control for his income in the regression analysis. However, following the provisions of the tax code, his income is used in calculating child-related tax benefits only in cases where the couple is married.

pre-birth months as inputs to the TAXSIM calculation. We sum monthly income amounts to an annual level, scaling up in cases where we observe fewer than twelve months of pre-birth income. We use information on wage income, property income, dividends, pensions, and unemployment benefits. We assume that married couples file jointly, that unmarried mothers use head of household filing status, and that filers take the standard deduction.

2.3 Decennial Census

We use data from the 2000 Decennial Census, conducted in April of 2000. Exact date of birth is reported for every household member. We identify infants born between December 18, 1998 and January 14, 1999 as well as infants born between December 18, 1999 and January 14, 2000. We link these infants to their mothers using information on household relationships. We restrict the sample to mothers linked to children born in the windows of interest, who are between the ages of 15 and 50, and who have worked at some point in the five years prior to the 2000 Decennial Census.

Adult respondents reported annual income earned during calendar year 1999. We consider two groups of mothers. The 33,397 mothers who gave birth between December 18, 1998 and January 14, 1999 are reporting earnings from a period that is almost entirely post-birth. The entire earnings reference period is post-birth for the December 1998 mothers. At most two weeks of the earnings reference period is pre-birth for the January 1999 mothers. We use this group to test the hypothesis that December mothers will earn less in the year after a child's birth than similar January mothers because of the more generous tax treatment they face. Additionally, we consider another 33,860 mothers who gave birth between December 18, 1999 and January 14, 2000. The 1999 wage income reported by these mothers is entirely pre-birth for the January mothers, and includes at most two weeks of post-birth earnings for the December mothers. We use this group of mothers to test the identifying assumption that December and January mothers would behave similarly absent any difference in tax treatment.

Decennial Census respondents also report employment status at the time of interview, but we cannot use this information to test for differences in contemporaneous labor supply. Decennial Census interviews happen at the same time for all respondents. For mothers who gave birth in December 1999, we observe employment four months after giving birth. For mothers who gave birth in January 2000, we observe employment three months after giving birth. The rapid change in the probability of being currently at work between the third and fourth month after giving birth makes it necessary to compare mothers observed at the same interval after giving birth.

3 Identifying Assumption

The identifying assumption made in this paper is that December and January mothers are similar except for the tax treatment they receive, and that if the two groups of mothers faced the same tax treatment then their post-birth labor supply would evolve similarly. If this assumption holds, the greater after-tax income enjoyed by December mothers in the year after giving birth is exogenous to their previous labor supply behavior and other choices. In this scenario, the exogenous variation in income is what permits a well-identified estimate of the income effect on labor supply.

This assumption could be violated if mothers giving birth at different points in time have systematically different characteristics. Differences in attachment to the labor force would be particularly problematic for our empirical strategy. While there is seasonal variation in maternal characteristics over the full course of a year, the characteristics that can be measured in Vital Statistics data drawn from birth certificates are similar for December and January mothers (Buckles and Hungerman 2013). In our ACS and SIPP samples, the observable characteristics of mothers giving birth in late December and in early January are similar, with differences in means generally not statistically different from zero. Summary statistics are shown in Table 1, with ACS data described in Panel A and SIPP data described in Panel B. In the ACS sample of December and January mothers, 74% of each group is white, approximately 71% of mothers are married, and average age is 29 years. About 20% of these mothers have completed some college and an additional 44% have obtained college degrees. The difference in means across December and January mothers is not statistically significant for any of the variables shown in Panel A. Panel B shows similar means for the SIPP sample. There is a great deal of similarity in demographic characteristics across the two samples. Mothers in the SIPP sample are about 28 years old on average, 74% are married, and the mean number of children is about two.

One particular threat to identification is that mothers with the largest tax benefit may shift births from early January to late December through use of a scheduled C-section or induction. If this behavior is occurring, our treatment of child-related tax benefits as an exogenous shock to income is problematic. We reduce the potential impact of this behavior on our results by drawing a sample of births from the last two weeks of December and first two weeks of January, rather than using a sample of births occurring within just one week of New Year's Day. With a longer window of time, there is less potential for tax-motivated shifting of births. Table 1 reports the average amount of tax savings associated with a December birth for mothers in each group. On average, mothers in the ACS sample receive about $1205 in child-related tax benefits, when benefits are computed as a function of reported income, and about $1300 when predicted labor income replaces reported labor income. Late-December mothers receive about $10 more in child-related tax benefits than early-January mothers would have realized, had their children been born earlier. We see this pattern if tax benefits are computed as a function of reported income or of predicted income. The direction of this difference is consistent with the possibility of tax-motivated shifting of birth timing, although the difference is not statistically different from zero. Because of the SIPP's longitudinal structure, income from months before a child's birth can be observed, and the potential tax savings associated with a December birth can be computed as a function of pre-birth income. The average amount of this benefit for all mothers in the SIPP sample is

$978, with no statistical difference in the benefit amount for December and January mothers. The average dollar amount of tax benefit is smaller for the SIPP sample than for the ACS sample in part because an earlier set of years is included in the SIPP sample, and legislative changes have made child-related tax benefits more generous in more recent years.

We find it reassuring that average tax benefits are statistically indistinguishable for the groups of December and January mothers. To further investigate the potential impact of tax-motivated shifting of birth dates on our main analysis, we estimate equations predicting December birth as a function of tax benefits. Using ACS data, we regress a December birth dummy on the real dollar value of child-related tax benefits (computed as a function of predicted wage income) as well as controls for maternal age, race, education, marital status, and a dummy for being the first child.[6] If larger tax benefits cause women to shift their births forward to occur in late December rather than early January, the coefficient on the tax benefit term will be positive. Results are shown in Table 2. The first column includes our main analysis sample, consisting of mothers with births in the last two weeks of December and first two weeks of January. In this sample, a larger tax benefit does not have a statistically significant effect on the probability that a child is born in December. The second column of the table narrows the sample to births taking place within a two-week window, including only the last week of December and the first week of January. In this narrower window, which matches the set of birthdays used in Dickert-Conlin and Chandra (1999) and LaLumia et al. (Forthcoming), there is evidence that larger tax values are associated with a higher probability of a December birth. An additional $1000 of child-related tax benefits is associated with a 1.7 percentage point increase in the probability of a late-December vs. early-January birth. We view this as a reason to rely on the sample of births occurring in the wider December 18 to January 14 window.

Finally, we provide additional tests of the validity of our identifying assumption using Decennial Census data. For the mothers giving birth in December 1999 and January 2000,

[6]The survey does not ask about all previous children born to a mother. We characterize a child as first-born if a mother has no other own children living in her household at the time of the ACS interview.

who are reporting pre-birth earnings in the 2000 Decennial Census, we expect to see no discontinuous difference in reported earnings when we compare mothers giving birth on either side of the new year. Figure 1 plots average annual earnings for mothers giving birth on each day between December 1, 1999 and January 31, 2000.[7] Earnings appear to evolve in a smooth fashion through the turn of the year, offering support for our identifying assumption that the earnings of December and January mothers are similar before these mothers are exposed to differing tax treatment.[8]

4 Empirical Strategy

To investigate whether the child-related tax benefits received by December mothers shortly after giving birth reduce their labor supply and earnings, we estimate both OLS and IV regressions. The OLS specification estimates the reduced-form effect of having a child in late December relative to early January. These regressions, which are estimated using both ACS and SIPP data, take the form:

$$Y_i = \sum_{k=1}^{T-1} \alpha_k MonthsElapsed{=}k_i + \sum_{k=1}^{T} \beta_k (MonthsElapsed{=}k_i \cdot DecBirth_i) + \gamma X_i + \epsilon_i \quad (1)$$

where the dependent variable is a measure of labor supply and the vector X controls for mothers' demographic characteristics such as age, education, marital status, and race.

[7]We cannot produce similar figures for our ACS sample, as the number of mothers giving birth on a particular calendar date, and observed t months after birth, is so small as to generate disclosure risk.

[8]As an additional test of the validity of our identifying assumption, we estimate OLS and IV regressions predicting annual wage income for mothers whose most recent birth occurred in December of 1999 or January of 2000. We use Equation 2, described below, and a related IV equation in which a December birth dummy instruments for the amount of child-related tax benefits. If mothers who give birth in late December and early January would have otherwise had similar labor supply patterns, absent any difference in tax treatment, we expect no difference in pre-birth earnings of these December and January mothers. In other words, the assumption would be supported by a zero coefficient on the *DecBirth* dummy in the OLS regression and on the tax term in the IV regression. Contrary to expectations, we estimate negative values for both of these coefficients, significant at the 10% level. The OLS (IV) results suggest that mothers giving birth in the last two weeks of December 1999 earn $847 less ($763 less) in 1999 than mothers giving birth in the first two weeks of January 2000. A likely explanation for this pattern is that some post-birth days are included in the wage reference period for December mothers.

Regardless of tax treatment, the birth of a child is associated with substantial short-run changes in labor supply. Mothers are very unlikely to be working just after giving birth. The probability of being at work remains low for a few months and then increases, rapidly at first and then more slowly. Because of this strong temporal pattern in labor supply following the birth of a child, Equation 1 controls for the number of months elapsed between the date of a mother's most recent birth and the interview date. This set of controls is represented by the vector of *MonthsElapsed* dummy variables, with the excluded category being twelve months following birth. Reductions in labor supply in the month immediately after birth will be represented by negative coefficients when $k = 1$. As labor supply gradually increases to pre-birth levels, the coefficients should become less negative for larger values of k.

The variables of greatest interest in Equation 1 are the interaction terms $MonthsElapsed \cdot DecBirth$. The coefficients on these terms measure any difference in labor supply of December and January mothers, allowing this difference to vary with the length of time elapsed between the child's birth and the data collection. If more generous tax treatment reduces the labor supply of December mothers in a particular month, the coefficient on the corresponding interaction term will be negative.

There are a few differences involved in estimating Equation 1 using cross-sectional ACS data and longitudinal SIPP data. Equation 1 corresponds to the ACS sample, with no t subscripts on the *MonthsElapsed* terms. Each mother is observed in only one of the twelve months after giving birth. In contrast, each mother is observed multiple times in SIPP data. If written to correspond to the SIPP sample, the dependent variable and the *MonthsElapsed* terms in Equation 1 would have t subscripts. In the SIPP sample, the dependent variable is defined as the change in monthly earnings relative to a mother's average monthly earnings calculated over the six months prior to giving birth. The vector X_i is expanded to include the mother's average monthly earnings in each of the six months prior to birth. If there are systematic differences in the earnings of December and January mothers even absent any differential tax treatment, the use of first-differenced earnings data and the inclusion of these additional variables should control for the difference. The definition of *MonthsElapsed* is slightly different in the two samples. In the SIPP, this variable is a function of the calendar month of birth and the calendar month of the reference period. Given that the ACS reports exact date of birth and exact date of interview, we instead define the number of months elapsed as the number of 30-day periods elapsed. In the SIPP analysis, we cluster standard errors at the mother level to account for any serial correlation in the error term.

We consider several dependent variables. The first, available in both the ACS and the SIPP, is a dummy equal to one if a mother is employed and working at the time of the interview. When this is the dependent variable, ACS regressions include all mothers interviewed up to one year after giving birth. A value of zero indicates either that a woman is on temporary leave from a job or that she is out of the labor force. The second dependent variable, available in the ACS, is a dummy equal to one if a mother is employed but is currently not working. Regressions with this as the dependent variable are estimated only for employed mothers, and a value of zero indicates that a woman is working at the time of the interview as opposed to being temporarily absent from an ongoing job.

The third dependent variable that we use is wage and salary income. This is measured over the preceding 12 months in the ACS, as the monthly change relative to pre-birth earnings in the SIPP, and in calendar year 1999 in the Decennial Census. For the ACS mothers in our sample, all of whom are interviewed within one year of having given birth, reported wage income will include a mixture of pre-birth and post-birth earnings. For mothers interviewed very shortly after a child's birth, the window of time covered by the wage question falls almost entirely before the birth, while mothers interviewed twelve months after a child's birth will be reporting entirely post-birth earnings.

Estimating the effect of a December birth on earnings in either the ACS or SIPP can be carried out using Equation 1, where the dependent variable is 12-month earnings in the ACS and change in monthly earnings in the SIPP. It is just as important to include the *MonthsElapsed* dummies in the wage regressions as in the contemporaneous employment status regressions, but the expected pattern of coefficients is somewhat different. We expect positive coefficients for particularly low values of k in the ACS sample, as the mothers interviewed very shortly after giving birth will be including many months of (likely higher) pre-birth earnings in their 12-month wage measures. We expect these coefficients to decline as k increases, as the time period over which wage income is summed includes more months of post-birth earnings. In the SIPP-based analysis of wage income, we set the dependent variable equal to the difference between currently monthly earnings and the mother's own average earnings in the six months prior to birth. For comparability, we include a similar set of *MonthsElapsed* dummies in SIPP wage income regressions. Because wage income is measured at the monthly level, though, we expect a different temporal pattern in the coefficients on these terms. We expect the most negative coefficients in months when maternal labor supply is lowest, very shortly after birth. We expect the coefficients on *MonthsElapsed* terms to become gradually less negative as k increases.

To estimate the effect of a December birth on annual earnings in the Decennial Census, we use the following OLS regression:

$$Annual\ Wage\ Income_i = \beta DecBirth_i + \gamma X_i + \epsilon_i \qquad (2)$$

For mothers who gave birth in December of 1998, the Decennial Census wage measure will include post-birth earnings exclusively. For mothers who gave birth in the first two weeks of January of 1999, up to two weeks of pre-birth earnings will be included in the Decennial Census wage measure. This can complicate the interpretation of Census results. Under the hypothesis that the higher after-tax income received by December mothers, due to their more generous tax treatment, reduces their labor supply, we would expect a negative coefficient on *DecBirth* when estimating equation 2. However, suppose that all mothers take exactly the same number of weeks of unpaid maternity leave before returning to work, and suppose that this maternity leave commences on the date of the child's birth. All of the time on unpaid maternity leave will be included in the Census wage measure for January mothers. For December mothers, however, some portion of this maternity leave will have occurred before calendar year 1999 and will not be reflected in 1999 calendar year wages. This will cause 1999 wage income to be systematically higher for December mothers than for January mothers, and will bias upwards the coefficient on *DecBirth* in Equation 2. This problem is mitigated somewhat by using a sample of women who give birth within a narrow window of time, the last two weeks of December and the first two weeks of January, but it still has the potential to affect our results.

In addition to the OLS regressions described above, we also estimate IV regressions in which the December birth dummy is used as an instrument for the dollar value of child-related tax benefits received by December mothers. For analysis of ACS and SIPP data, the set of equations we estimate in carrying out this IV strategy is:

$$\begin{aligned} Y_i &= \sum_{k=1}^{T-1} \alpha_k MonthsElapsed{=}k_i + \sum_{k=1}^{T} \beta_k (MonthsElapsed{=}k \cdot \widehat{TaxValue}_i) + \gamma X_i + \epsilon_i \\ \widehat{TaxValue}_i &= \delta DecBirth_i + \lambda X_i + \nu_i \end{aligned} \quad (3)$$

For IV analysis of Decennial Census data, we replace the *DecBirth* dummy in Equation 2 with the predicted tax savings associated with a December birth. We use the IV estimation strategy with the same set of dependent variables described above. For mothers with children born in December, the *TaxValue* term is defined as the difference between the tax liabilities computed with and without the newborn child. For mothers with children born in January, *TaxValue* is set equal to zero. The coefficient can be interpreted as the impact of receiving an additional \$1000 in tax savings on the probability of being at work.

The receipt of child-related tax benefits is not universal, as it requires filing a tax return. We would not expect any difference in the after-tax incomes of December and January mothers among parents who are not filing tax returns. While it would be sensible to estimate regressions only for the set of people who file returns, or to compare results for filers and nonfilers as a further test of whether any labor supply response is causal, we do not observe tax filing status in any of our datasets. However, we expect that the large majority of individuals in our analysis are tax filers. Nationally, about 87% of individuals are represented on a tax return (Orszag and Hall 2003).

5 Results

5.1 Current Employment Status

Table 3 shows the results of regressions predicting whether a mother is employed and currently working at the time of the ACS interview. Results from a reduced-form OLS regression are shown in the first two columns of the table and results from an IV regression are shown in columns 3 and 4. Columns 1 and 3 show the coefficients on the series of *MonthsElapsed* dummy variables measuring the number of months between when a mother gave birth and when she was interviewed. As expected, these coefficients indicate a strong temporal pattern in labor supply. Relative to the omitted group of mothers interviewed in the 12^{th} month after having given birth, women interviewed one or two months after birth are about 30 percentage points less likely to be currently at work. The probability of being currently at work gradually rises over time. It is 8.7 percentage points below the average for the omitted group in the third month after birth, and then mostly returns to a steady long-run level. These patterns are the same in the OLS and IV regressions. They are also consistent with patterns documented for other samples of new mothers (Laughlin 2011).

The coefficients of particular interest in Table 3 are those on the December interaction terms, shown in columns 2 and 4. These results indicate that, in most time periods, the probability that a December mother is currently working is not statistically different from the probability that a January mother is currently working. One exception to this pattern is in the third month after having a child. The OLS regression indicates that, three months after giving birth, December mothers are 6.3 percentage points less likely than January mothers to be currently working. In the IV regression, an additional $1000 of *TaxSavings* is associated with a 4.9 percentage point reduction in the probability of being at work in the third month after giving birth. The other exception occurs eight months after birth. In this month, the OLS regression shows that, relative to January mothers, December mothers are 5.4 percentage points less likely to be working. The IV regressions show that $1000 of *TaxSavings* reduces the probability of working by 4.2 percentage points.

Table 4 shows a parallel set of results estimated using SIPP data. The overall pattern of results is quite similar. December mothers are relatively less likely to be working in the third month after birth and in the eighth month after birth. Relative to January mothers, December mothers are about 5.6 percentage points less likely to be employed and working three months after birth and are 6.3 percentage points less likely to be working eight months after giving birth. In this case, the OLS reduced-form effect of a December birth is associated with an effect almost exactly equal in magnitude to the IV effect associated with an additional $1000 of child-related tax benefits. This is to be expected, as the average tax benefit received by December mothers in the SIPP sample is very close to $1000.

In the next set of regression results, shown in Table 5, the dependent variable is a dummy equal to one if a woman is employed and is temporarily not working at the time of her ACS interview. Mothers who are on maternity leave when interviewed would fall into this category.[9] The number of observations is smaller here because the sample is restricted to women who are employed at the time of the ACS interview. Being employed and temporarily not working is a relatively uncommon employment status, particularly as time elapsed since giving birth increases. To avoid creating cells containing only a small number of observations, the omitted comparison category is defined differently in this regression. Here, the omitted category includes mothers observed seven to 12 months after having given birth.

Again there is evidence of a strong temporal pattern in employment status around the

[9] These mothers may be on either paid or unpaid leave. The employment status variables in the SIPP do not allow identification of a corresponding group. In the SIPP, anyone who is receiving pay from a job is categorized as being employed. It is impossible to identify those who are currently being paid while temporarily absent from work.

time of birth. Relative to the omitted group of women interviewed seven to twelve months after giving birth, women observed within one month of giving birth are 54.3 percentage points more likely to be temporarily not working. Temporary absences remain high in the second month after birth and then drop off quickly. The likelihood of temporary absence is statistically indistinguishable from the long-run level by six months after birth. Comparing mothers who give birth in December and in January, in most months there is no statistically significant difference in the probability of being temporarily absent from work. One exception occurs three months after birth. In this month, OLS results indicate that December mothers are 7.0 percentage points more likely to be temporarily not working and IV results indicate that $1000 of tax benefits increases the probability of temporary absence by 5.2 percentage points. The positive coefficients here are consistent with an income effect of the expected direction. The higher levels of after-tax income enjoyed by December mothers are associated with higher probabilities of being absent from work. There is also evidence that December mothers are more likely to be absent from work five months after birth, with point estimates about half the size of the effect observed three months after birth.

There are at least two reasons why it is plausible that an income effect on maternal labor supply is consistently observed three months after birth. First, this is a time period in which the 12 weeks of unpaid leave mandated by FMLA run out. Second, tax benefits may have the greatest impact on labor supply at the moment that they are received by filers. Three months after birth corresponds to the calendar month of March for December mothers and to the month of April for January mothers. This is the time when many tax refund payments are distributed. Approximately half of all refund dollars are disbursed in March and April.

The differing tax treatment of mothers who give birth in December and January does not persist beyond the first year of a child's life.[10] Thus, if the results shown so far indeed

[10] Tax treatment does differ again when children age out of eligibility for child-related tax benefits. For most tax provisions, this occurs when the child turns 18 or when a child who is a full-time student turns 24. Several authors have made use of variation in the time at which a child becomes ineligible for benefits to estimate labor supply and other effects. See Looney and Singhal (2005), Dokko (2008), and Feldman et al. (2014).

represent an income effect on labor supply rather than strategic timing of birth or some other seasonal pattern that differs for December and January mothers, labor supply differences should not persist beyond the first year following a child's birth. To investigate this possibility, we consider mothers interviewed in the ACS anywhere between one and two years after their most recent birth. We define a set of *MonthsElapsed* dummies for these mothers that correspond to the time between a child's first birthday and the interview. As we did before, we interact these dummies with an indicator for giving birth in December. We repeat regressions predicting whether a mother is currently at work, with the number of months elapsed since the child's first birthday replacing the number of months elapsed since the child's birth. Because December mothers and January mothers in this sample face exactly the same tax treatment, we expect none of the interaction terms to be statistically different from zero. In results not shown, we find that almost all of the coefficients on the *MonthsElapsed* dummies are insignificant. In addition, almost all of the *DecemberBirth* interaction terms are small and insignificant.[11] In particular, there is no divergence in the labor supply of December and January mothers three months after a child's first birthday. This makes it less plausible that the earlier results, estimated for mothers observed up to one year after a child's birth, are merely a reflection of bias due to sample selection. In other words, it is not the case that December mothers are always less likely to work in March than January mothers are to work in April.

Not all mothers face the same constraints in deciding when to return to work after a child's birth. The possibility of relying on the income of their husbands may allow married women to take longer maternity leaves than single women, and may be associated with an increased elasticity of labor supply. Using ACS data, we estimate OLS regressions predicting the probability of being currently working separately for married and for unmarried women. The results are shown in Table 6. As in the baseline regression, labor supply falls sharply

[11]In periods one to eleven months after a child's first birthday, the difference between the probability of being at work for December mothers relative to January mothers is never greater than 1.9 percentage points, and is never significant at the 10% level. Twelve months after a child's first birthday, December mothers are 3.4 percentage points more likely to be at work, significant at the 5% level.

in the months immediately after birth. Trends in labor supply are similar for December and January mothers in most months, with an exception in the third month after giving birth. The child-related tax benefits associated with giving birth in December appear to reduce the probability of working in the third month after birth by 7.9 percentage points among married women, but to have no significant effect among unmarried women.

Employers of highly-skilled women may offer greater flexibility in the choice of when to return to a job than employers in unskilled labor markets. If so, income effects on labor supply may be larger for women with higher levels of education. To investigate this possibility, we divide the sample into groups of mothers with and without some college education, and estimate OLS regressions predicting whether a woman is currently working separately for each educational group. The results are shown in Table 7. Both educational groups display declines in the probability of working in the months just after giving birth. In most months, there is no significant difference in the probability of working across December and January mothers. Again, an exception occurs three months after birth when December mothers with at least some college education are 7.3 percentage points less likely to be at work. Among mothers with no college education, there is no significant difference in the probability that December and January mothers are at work. The results in Tables 6 and 7 are consistent with Han et al. (2009), who find that legislated increases in available parental leave led to increased use of leave only among married mothers and highly-educated mothers.

5.2 Earned Income

The next set of regressions makes use of a different dependent variable, earned income. In the ACS, income from wages and salary is reported for the twelve months prior to the interview. This is a summary measure of labor supply decisions made throughout the year. If December mothers are slower to return to work because of the tax treatment they receive, then they should have lower amounts of wage income in the year following birth. However, we do not find evidence of this behavior in the ACS sample. Results using ACS data are shown in Table 8, with results from an OLS regression in the first two columns and results from an IV regression in columns 3 and 4. The format of this table matches the format of earlier employment status tables. Columns 1 and 3 report coefficients on a series of dummies measuring the number of months elapsed between a woman's most recent birth and the time at which she is reporting wage income. As expected, women who are reporting wage income just a few months after giving birth report higher values than the comparison group, women interviewed 12 months after giving birth. This is consistent with most of the reference period for their wage reports coming from months before giving birth. The observed temporal pattern in reported wage income is very similar in OLS and IV regressions. Columns 2 and 4 show the differential effects for December mothers. None of the interaction terms are statistically different from zero, indicating that in no month is there a statistically significant difference in reported earnings for the preceding 12-month period. Moreover, the coefficients vary substantially from month to month in columns 2 and 4 despite the large overlap in recall periods from one month to the next.

Results of monthly earnings regressions estimated with SIPP data are shown in Table 9. In this case, the dependent variable is the difference between a mother's earned income in a particular month and her average monthly earnings from the six months prior to giving birth. Columns 1 and 3 show the average temporal pattern in earnings. As expected, monthly earnings decline the most shortly after birth. In the second month after birth, monthly earnings are about $200 lower than pre-birth levels. Monthly earnings gradually return to pre-birth averages by the end of a year. The temporal pattern in monthly earnings is essentially the same in OLS and IV regressions. Columns 2 and 4 present evidence on whether the earnings of December mothers evolve differently. There are several months in which December mothers earn significantly less. December mothers earn less four, five, eight, and ten months after giving birth. Each interaction term shows the differential earnings of December mothers in a particular month. Summing all coefficients in column 2 to yield an annual effect shows that December mothers earn about $988 less than January mothers over the 12 months following birth. In the IV regression, the coefficients in column 4 show the difference in monthly earnings associated with receiving an additional $1 of tax benefit. Again, tax benefits are associated with significant reductions in earnings in several months. Summing all monthly interaction terms indicates that, across a year, each additional dollar of tax benefit reduces maternal earnings by $1.02. This result indicates an income elasticity of -1. In Table 10, monthly earning amounts are aggregated up to quarterly earnings. In this specification, the income effects of additional after-tax income on earnings are concentrated in the second quarter after birth. Once again, summing the interaction coefficients to arrive at an annual income effect produces an income elasticity very close to -1.

Table 11 shows the results of estimating both OLS and IV regressions predicting annual wage income using Decennial Census data. The dependent variable is equal to a woman's total wage and salary income earned during calendar year 1999. We restrict the sample to mothers whose most recent child was born in the last two weeks of December 1998 or in the first two weeks of January 1999. If in fact more generous tax benefits have a negative effect on labor supply, it is for this group of mothers that we would expect to see a negative coefficient on *DecBirth* in the OLS regression, and a negative coefficient on $\widehat{TaxValue}$ in the IV regression. However, these coefficients are statistically not different from zero. The point estimates are positive, which could reflect the mechanical bias due to calendar year aggregation discussed earlier. It is also possible that measurement error is attenuating the results. Discrepancies between reported and actual earnings may be more common when respondents are asked to recall changes over the longer window of time involved in the Decennial Census than in the SIPP. We have also carried out a regression discontinuity analysis of the annual earnings reported by mothers giving birth in December 1998 and in January 1999. An income effect of child-related tax benefits on labor supply would produce discontinuously lower earnings for the December mothers than for the January mothers. Figure 2 shows the results of this analysis. Earnings appear to evolve in a smooth fashion through the turn of the year.

6 Conclusion

This paper uses a discontinuity in the tax treatment of children to identify income effects on maternal labor supply. Mothers who give birth at the end of December can claim tax benefits for their children almost a year earlier than mothers who give birth in early January. As a result, in the first year of a child's life, December mothers have higher after-tax income than similar January mothers. However, both December and January mothers face the same set of marginal tax rates and other tax parameters on income earned in the year following the birth. This situation provides a useful opportunity to isolate income effects on labor supply.

The results indicate that mothers who give birth in late December are slower to return to work and earn less in the year following a child's birth. Results from both the ACS and the SIPP indicate that, relative to early January mothers, late December mothers are about six percentage points less likely to be working in the third month after a child's birth. This pattern is evident for married mothers but not for unmarried mothers, and only for mothers with some college education. Analysis of earned income using SIPP data indicates that, aggregated over the year following a child's birth, each additional dollar of child-related tax benefits lowers earnings by approximately one dollar. In other words, the income elasticity of maternal earnings in the year just after birth is approximately -1. Estimates from the ACS and Decennial Census show earnings results not statistically different from zero, but differences in the time period over which earnings are reported likely introduce more noise into the ACS and Decennial Census earnings measures than into the SIPP measure.

These estimates are in contrast to the consensus view that income effects on labor supply are quite close to zero. We hypothesize that the income elasticity of labor supply is not a constant, and that labor supply decisions made shortly after the birth of a child are indeed much more income elastic than labor supply decisions made at other points in the lifecycle.

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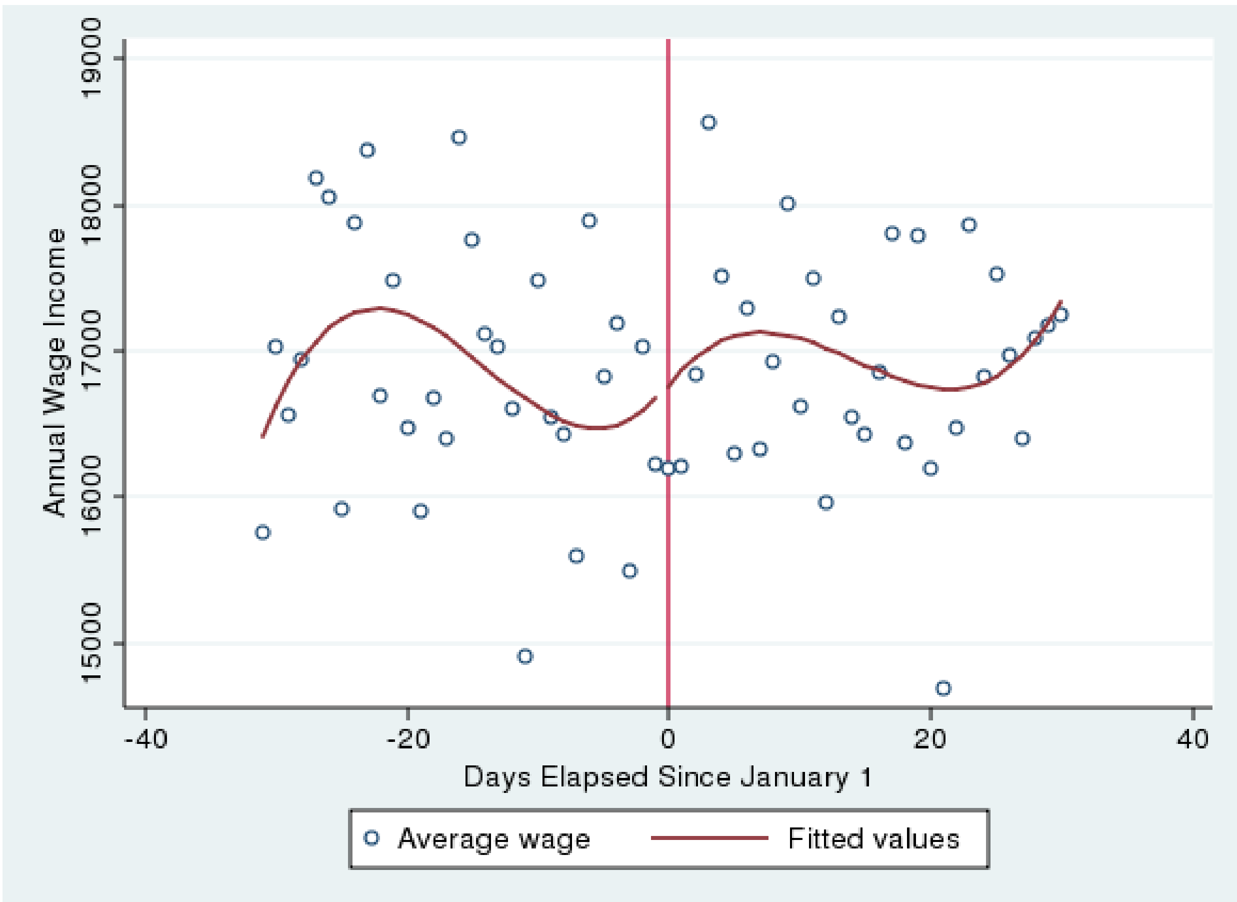
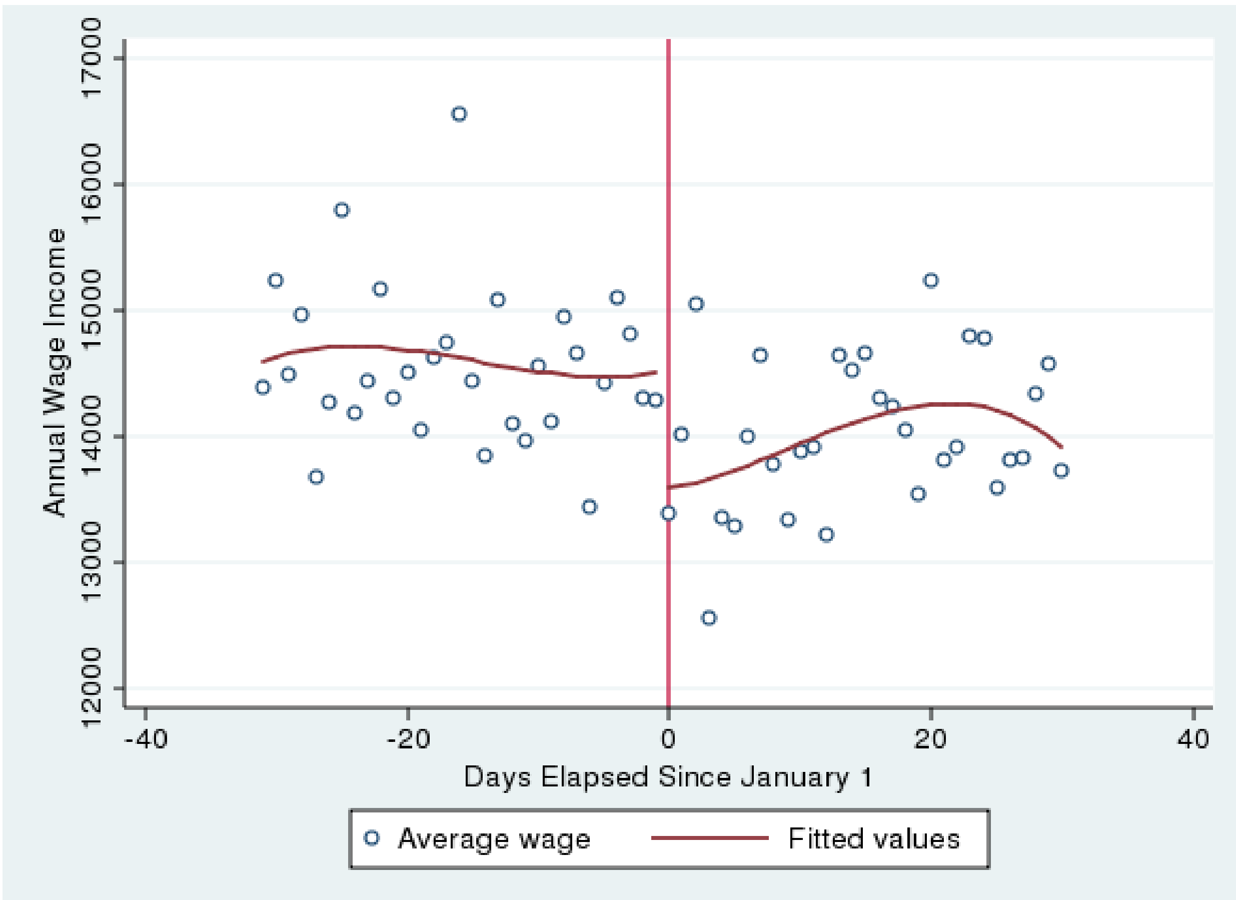
Figure 1: Test of Identifying Assumption: Average Annual 1999 Wage Income of Mothers Giving Birth In December 1999 and January 2000



The figure plots average wage income for calendar year 1999, as reported in the 2000 Decennial Census. Each point represents average earnings for all mothers giving birth on a particular day. Mothers giving birth in December 1999 and January 2000 are included.

Figure 2: Average Annual 1999 Wage Income of Mothers Giving Birth in December 1998 and January 1999



The figure plots average wage income for calendar year 1999, as reported in the 2000 Decennial Census. Each point represents average earnings for all mothers giving birth on a particular day. Mothers giving birth in December 1998 and January 1999 are included.

Table 1: Summary Statistics, December and January Mothers

	Full Sample	January Births	December Births
A. ACS Sample			
Age	28.85	28.81	28.88
	(5.94)	(5.95)	(5.94)
White	0.73	0.74	0.73
	(0.41)	(0.41)	(0.42)
Married	0.71	0.71	0.71
	(0.43)	(0.44)	(0.43)
Has Male Spouse/Partner	0.80	0.80	0.80
	(0.38)	(0.38)	(0.38)
Number of Kids<19	1.96	1.95	1.96
	(1.03)	(1.03)	(1.03)
Some College	0.21	0.21	0.20
	(0.39)	(0.40)	(0.39)
College Grad	0.44	0.43	0.45
	(0.50)	(0.50)	(0.50)
Child Tax Benefit, Actual Income	1205.69	1200.50	1210.93
	(776.94)	(777.03)	(776.83)
Child Tax Benefit, Predicted Income	1300.65	1296.22	1305.12
	(761.65)	(763.44)	(759.92)
N of observations	16384	8158	8226
B. SIPP Sample			
Age	28.52	28.48	28.56
	(5.92)	(5.93)	(5.92)
White	0.81	0.83	0.79
	(0.39)	(0.37)	(0.41)
Married	0.74	0.76	0.72
	(0.44)	(0.43)	(0.45)
N of Dependents	1.98	1.94	2.02
	(0.99)	(0.98)	(1.01)
Some College	0.33	0.34	0.32
	(0.47)	(0.47)	(0.47)
College Grad	0.30	0.29	0.31
	(0.46)	(0.46)	(0.46)
Labor Earnings	1303.99	1310.55	1297.36
	(2215.76)	(2216.90)	(2216.71)
Child Tax Benefit, Pre-Birth Income	978.09	979.67	976.50
	(636.82)	(626.33)	(647.83)
N of Mothers	1051	523	528

The table reports means, with standard deviations in parentheses.

Table 2: Predicting Whether Child Is Born in December, ACS Sample

	Full Sample	Two-Week Sample
Child Tax Benefits ($1000s)	0.005	0.017**
	(0.006)	(0.007)
Age	-0.001	0.008
	(0.005)	(0.008)
Age Squared	0.00001	0.0001
	(0.00008)	(0.0001)
White	-0.012	-0.008
	(0.010)	(0.013)
Some College	-0.006	-0.021
	(0.009)	(0.016)
College Grad	0.0003	0.002
	(0.009)	(0.014)
Married	0.013	0.016
	(0.010)	(0.012)
First Child	-0.015	-0.020
	(0.010)	(0.012)
Spouse/Partner Income ($1000s)	0.00002	0.00007
	(0.00006)	(0.0001)
Constant	0.443**	0.314***
	(0.101)	(0.159)
N	16384	7932

The table reports coefficients and standard errors from unweighted OLS regressions predicting whether a child is born in December. Each regression also includes year fixed effects, state fixed effects, and day-of-week fixed effects. Standard errors are clustered at the state level.

Table 3: Predicting Whether Mom is Currently Employed and Working, ACS Results

	OLS Results		IV Results	
	Months Elapsed Since Birth	Months Interacted with DecBirth	Months Elapsed Since Birth	Months Interacted with $\widehat{TaxValue}$
	(1)	(2)	(3)	(4)
Month 1	-0.331***	0.019	-0.331***	0.016
	(0.031)	(0.033)	(0.031)	(0.026)
Month 2	-0.305***	0.015	-0.305***	0.013
	(0.027)	(0.023)	(0.027)	(0.019)
Month 3	-0.087***	-0.063**	-0.087***	-0.049**
	(0.031)	(0.030)	(0.031)	(0.023)
Month 4	-0.049	0.011	-0.049	0.009
	(0.031)	(0.034)	(0.031)	(0.026)
Month 5	-0.002	-0.034	-0.002	-0.026
	(0.029)	(0.021)	(0.029)	(0.016)
Month 6	0.002	0.008	0.002	0.007
	(0.017)	(0.024)	(0.017)	(0.020)
Month 7	-0.047*	0.019	-0.047*	0.015
	(0.024)	(0.031)	(0.024)	(0.025)
Month 8	-0.002	-0.054**	-0.002	-0.042**
	(0.027)	(0.024)	(0.027)	(0.019)
Month 9	0.007	-0.023	0.007	-0.018
	(0.023)	(0.024)	(0.023)	(0.020)
Month 10	0.016	0.017	0.016	0.013
	(0.026)	(0.028)	(0.026)	(0.022)
Month 11	0.002	0.026	0.002	0.021
	(0.023)	(0.026)	(0.023)	(0.021)
Month 12		-0.001		-0.001
		(0.027)		(0.021)
N	16384		16384	

Columns 1 and 2 report unweighted OLS coefficients and standard errors. Columns 3 and 4 report second-stage results from an IV regression. Each regression also controls for maternal age and age squared, income earned by a male spouse or partner (set equal to zero if there is no male partner), the number of own under-19 children in the household, state fixed effects, year fixed effects (where a year is defined as an adjacent December/January pair), the number of days elapsed between December 1 and the birth, day-of-week dummies for the date of birth, and dummies for being white, having some college education, having completed a college degree, and being married.

Table 4: Predicting Whether Mom is Employed and Working in Current Month, SIPP Results

	OLS Results		IV Results	
	Months Elapsed Since Birth	Months Interacted with DecBirth	Months Elapsed Since Birth	Months Interacted with $\widehat{TaxValue}$
	(1)	(2)	(3)	(4)
Month 1	-0.045*	-0.025	-0.045*	-0.025
	(0.027)	(0.025)	(0.027)	(0.025)
Month 2	-0.068***	-0.041	-0.069***	-0.042
	(0.026)	(0.026)	(0.026)	(0.026)
Month 3	-0.059**	-0.056**	-0.060**	-0.057**
	(0.025)	(0.028)	(0.025)	(0.029)
Month 4	-0.048**	-0.025	-0.048**	-0.026
	(0.024)	(0.027)	(0.024)	(0.028)
Month 5	-0.024	-0.026	-0.025	-0.027
	(0.022)	(0.027)	(0.022)	(0.028)
Month 6	-0.015	-0.040	-0.015	-0.041
	(0.021)	(0.027)	(0.021)	(0.028)
Month 7	-0.017	-0.035	-0.018	-0.037
	(0.020)	(0.027)	(0.020)	(0.029)
Month 8	-0.013	-0.063**	-0.013	-0.065**
	(0.019)	(0.028)	(0.019)	(0.029)
Month 9	-0.019	-0.032	-0.020	-0.032
	(0.018)	(0.029)	(0.018)	(0.029)
Month 10	-0.016	-0.014	-0.016	-0.015
	(0.015)	(0.029)	(0.015)	(0.030)
Month 11	-0.000	-0.014	-0.001	-0.014
	(0.013)	(0.030)	(0.013)	(0.031)
Month 12		-0.024		-0.025
		(0.031)		(0.032)
N	11,075		11,075	
R-squared	0.26			

Columns 1 and 2 report unweighted OLS coefficients. Columns 3 and 4 report second-stage results from an IV regression. Reported standard errors are clustered at the mother level. Each regression also controls for pre-birth monthly earnings, maternal age and age squared, income earned by a husband (set equal to zero if there is no spouse), the number of own children in the household, state fixed effects, year fixed effects (where a year is defined as an adjacent December/January pair), and dummies for being white, having some college education, having completed a college degree, and being married.

Table 5: Predicting Whether Mom is Employed and Temporarily Not Working, ACS Results

	OLS Results		IV Results	
	Months Elapsed Since Birth	Months Interacted with DecBirth	Months Elapsed Since Birth	Months Interacted with $\widehat{TaxValue}$
	(1)	(2)	(3)	(4)
Month 1	0.543***	-0.014	0.543***	-0.010
	(0.030)	(0.047)	(0.030)	(0.034)
Month 2	0.477***	-0.002	0.477***	-0.002
	(0.024)	(0.028)	(0.024)	(0.021)
Month 3	0.151***	0.070**	0.151***	0.052**
	(0.021)	(0.027)	(0.021)	(0.020)
Month 4	0.067***	-0.0001	0.067***	0.0002
	(0.021)	(0.026)	(0.021)	(0.019)
Month 5	0.029*	0.034*	0.029***	0.025*
	(0.011)	(0.019)	(0.011)	(0.014)
Month 6	0.017	0.018	0.017	0.014
	(0.011)	(0.017)	(0.011)	(0.014)
Months 7-12		0.021		0.015
		(0.012)		(0.009)
N	9863		9863	

Columns 1 and 2 report unweighted OLS coefficients and standard errors. Columns 3 and 4 report second-stage results from an IV regression. Each regression also controls for maternal age and age squared, income earned by a male spouse or partner (set equal to zero if there is no male partner), the number of own under-19 children in the household, state fixed effects, year fixed effects (where a year is defined as an adjacent December/January pair), the number of days elapsed between December 1 and the birth, day-of-week dummies for the date of birth, and dummies for being white, having some college education, having completed a college degree, and being married.

Table 6: Heterogeneity in Probability of Being Currently Employed and Working by Marital Status, ACS Results

	Mother is Married		Mother is Not Married	
	Months Elapsed Since Birth	Months Interacted with DecBirth	Months Elapsed Since Birth	Months Interacted with DecBirth
	(1)	(2)	(3)	(4)
Month 1	-0.308***	-0.007	-0.397***	0.107**
	(0.036)	(0.041)	(0.041)	(0.046)
Month 2	-0.295***	0.008	-0.340***	0.051
	(0.032)	(0.031)	(0.041)	(0.042)
Month 3	-0.089**	-0.079**	-0.074**	-0.008
	(0.036)	(0.037)	(0.043)	(0.055)
Month 4	-0.041	0.007	-0.071	0.027
	(0.037)	(0.038)	(0.051)	(0.056)
Month 5	-0.010	-0.026	0.018	-0.045
	(0.029)	(0.028)	(0.049)	(0.059)
Month 6	0.014	0.005	-0.030	0.013
	(0.019)	(0.033)	(0.046)	(0.055)
Month 7	-0.061**	0.015	0.001	0.040
	(0.030)	(0.041)	(0.044)	(0.056)
Month 8	-0.021	-0.044	0.044	-0.070
	(0.028)	(0.029)	(0.050)	(0.059)
Month 9	0.009	-0.021	-0.002	-0.016
	(0.024)	(0.032)	(0.043)	(0.045)
Month 10	0.011	0.018	0.033	0.018
	(0.033)	(0.030)	(0.047)	(0.052)
Month 11	-0.015	0.047	0.036	-0.017
	(0.030)	(0.031)	(0.040)	(0.051)
Month 12		-0.004		0.025
		(0.029)		(0.055)
N	12270		4114	

The table reports coefficients and standard errors from unweighted OLS regressions. The additional controls not shown in this table are maternal age and age squared, income earned by a male spouse or partner (set equal to zero if there is no male partner), the number of own under-19 children in the household, state fixed effects, year fixed effects (where a year is defined as an adjacent December/January pair), the number of days elapsed between December 1 and the birth, day-of-week dummies for the date of birth, a dummy for being white, and dummies for having some college education or being a college graduate.

Table 7: Heterogeneity in Probability of Being Currently Employed and Working by Educational Attainment, ACS Results

	Some College Education		No College Education	
	Months Elapsed Since Birth	Months Interacted with DecBirth	Months Elapsed Since Birth	Months Interacted with DecBirth
	(1)	(2)	(3)	(4)
Month 1	-0.329***	0.044	-0.345***	-0.035
	(0.039)	(0.042)	(0.046)	(0.042)
Month 2	-0.304***	0.008	-0.310***	0.034
	(0.036)	(0.027)	(0.031)	(0.048)
Month 3	-0.081**	-0.073**	-0.107**	-0.052
	(0.037)	(0.036)	(0.044)	(0.067)
Month 4	-0.053	0.020	-0.050	-0.008
	(0.037)	(0.037)	(0.047)	(0.055)
Month 5	0.011	-0.035	-0.041	-0.022
	(0.031)	(0.025)	(0.046)	(0.057)
Month 6	0.013	0.015	-0.033	-0.003
	(0.023)	(0.030)	(0.056)	(0.064)
Month 7	-0.033	0.011	-0.093*	0.041
	(0.036)	(0.040)	(0.048)	(0.051)
Month 8	0.002	-0.044*	-0.016	-0.069
	(0.028)	(0.025)	(0.046)	(0.060)
Month 9	0.033	-0.050*	-0.062	0.043
	(0.029)	(0.028)	(0.053)	(0.052)
Month 10	0.039	0.003	-0.050	0.061
	(0.033)	(0.032)	(0.050)	(0.069)
Month 11	0.009	0.031	-0.022	0.010
	(0.033)	(0.036)	(0.037)	(0.052)
Month 12		0.003		-0.012
		(0.032)		(0.059)
N	11812		4572	

The table reports coefficients and standard errors from unweighted OLS regressions. The additional controls not shown in this table are maternal age and age squared, income earned by a male spouse or partner (set equal to zero if there is no male partner), the number of own under-19 children in the household, state fixed effects, year fixed effects (where a year is defined as an adjacent December/January pair), the number of days elapsed between December 1 and the birth, day-of-week dummies for the date of birth, a dummy for being white, and a dummy for being married.

Table 8: Predicting Dollar Amount of Annual Wage Income, ACS Results

	OLS Results		IV Results	
	Months Elapsed Since Birth	Interacted with Dec Birth	Months Elapsed Since Birth	Interacted with $\widehat{TaxValue}$
	(1)	(2)	(3)	(4)
Month 1	2784**	122	2784**	0.091
	(1268)	(1076)	(1264)	(0.853)
Month 2	2400**	656	2400**	0.538
	(1005)	(1564)	(1005)	(1.303)
Month 3	3825***	302	3826***	0.233
	(1082)	(1797)	(1082)	(1.418)
Month 4	3878***	-915	3877***	-0.722
	(1218)	(991)	(1215)	(0.776)
Month 5	1362	75	1360	0.053
	(1212)	(988)	(1211)	(0.780)
Month 6	2023*	303	2023*	0.247
	(1174)	(1149)	(1175)	(0.967)
Month 7	1092	698	1091	0.549
	(990)	(1272)	(991)	(1.004)
Month 8	1611	-1262	1609	-0.989
	(1092)	(1175)	(1093)	(0.911)
Month 9	729	1079	727	0.896
	(1190)	(1228)	(1192)	(1.021)
Month 10	1643	-172	1644	-0.138
	(1048)	(1400)	(1047)	(1.092)
Month 11	1156	-2347	1155	-1.874
	(1141)	(1555)	1140	(1.229)
Month 12		-488		-0.382
		(1450)		(1.128)
N	16384		16384	

Columns 1 and 2 report unweighted OLS coefficients and standard errors. Columns 3 and 4 report second-stage results from an IV regression. Each regression also controls for maternal age and age squared, income earned by a male spouse or partner (set equal to zero if there is no male partner), the number of own under-19 children in the household, state fixed effects, year fixed effects (where a year is defined as an adjacent December/January pair), the number of days elapsed between December 1 and the birth, day-of-week dummies for the date of birth, and dummies for being white, having some college education, having completed a college degree, and being married.

Table 9: Predicting Dollar Amount of Monthly Wage Income, SIPP Results

	OLS Results		IV Results	
	Months Elapsed Since Birth	Months Interacted with DecBirth	Months Elapsed Since Birth	Months Interacted with $\widehat{TaxValue}$
	(1)	(2)	(3)	(4)
Month 1	-88.816	-66.390	-90.078	-0.068
	(63.538)	(57.789)	(63.710)	(0.059)
Month 2	-206.765***	-4.374	-207.981***	-0.005
	(59.547)	(59.336)	(59.684)	(0.060)
Month 3	-191.422***	-69.555	-192.575***	-0.072
	(61.389)	(65.128)	(61.398)	(0.067)
Month 4	-123.063**	-108.233*	-124.222**	-0.112*
	(53.534)	(64.126)	(53.554)	(0.066)
Month 5	-68.344	-130.220**	-69.384	-0.135**
	(54.318)	(62.557)	(54.293)	(0.065)
Month 6	-105.436**	-53.970	-106.447**	-0.056
	(51.779)	(65.041)	(51.750)	(0.067)
Month 7	-90.392*	-105.818	-91.125*	-0.111
	(50.553)	(66.208)	(50.460)	(0.069)
Month 8	-97.569**	-114.566*	-98.402**	-0.119*
	(46.934)	(68.201)	(46.839)	(0.070)
Month 9	-104.980**	-84.917	-105.794**	-0.087
	(43.024)	(68.895)	(42.907)	(0.070)
Month 10	-43.380	-119.876*	-43.681	-0.124*
	(39.029)	(72.296)	(38.888)	(0.075)
Month 11	-23.481	-36.329	-23.723	-0.038
	(32.616)	(76.216)	(32.500)	(0.078)
Month 12		-94.123		-0.099
		(79.991)		(0.084)
Total Effect		-988.370		-1.026
		(238.114)		(0.246)
N	11,075		11,075	
R-squared	0.10			

Columns 1 and 2 report unweighted OLS coefficients. Columns 3 and 4 report second-stage results from an IV regression. Reported standard errors are clustered at the mother level. Each regression also controls for pre-birth monthly earnings, maternal age and age squared, income earned by a spouse (set equal to zero if there is no husband), the number of own children in the household, state fixed effects, year fixed effects (where a year is defined as an adjacent December/January pair), and dummies for being white, having some college education, having completed a college degree, and being married.

Table 10: Predicting Dollar Amount of Quarterly Wage Income, SIPP Results

	OLS Results		IV Results	
	Months Elapsed Since Birth	Months Interacted with DecBirth	Months Elapsed Since Birth	Months Interacted with $\widehat{TaxValue}$
	(1)	(2)	(3)	(4)
Quarter 1	-418.315***	-139.585	-421.256***	-0.144
	(153.862)	(160.302)	(154.392)	(0.164)
Quarter 2	-227.873*	-294.034*	-230.445*	-0.305*
	(129.187)	(177.671)	(129.329)	(0.184)
Quarter 3	-223.168**	-305.954	-224.952**	-0.317
	(94.075)	(186.955)	(94.003)	(0.193)
Quarter 4		-250.699		-0.262
		(215.072)		(0.223)
Total Effect		-990.271		-1.028
		(237.786)		(0.246)
N	11,075		11,075	
R-squared	0.10			

Columns 1 and 2 report unweighted OLS coefficients. Columns 3 and 4 report second-stage results from an IV regression. Reported standard errors are clustered at the mother level. Each regression also controls for pre-birth monthly earnings, maternal age and age squared, income earned by a spouse (set equal to zero if there is no husband), the number of own children in the household, state fixed effects, year fixed effects (where a year is defined as an adjacent December/January pair), and dummies for being white, having some college education, having completed a college degree, and being married.

Table 11: Predicting Dollar Amount of Post-Birth Annual Wage Income, Decennial Census Results

	OLS Results	IV Results
Dec Birth	922	
	(650)	
$\widehat{TaxValue}$		894
		(625)
N	33,397	33,397
F-stat from first stage		680.8

Column 1 reports unweighted OLS coefficients and standard errors. Column 2 reports second-stage results from an IV regression. The dependent variable is the amount of wage income earned in calendar year 1999. Each regression also controls for maternal age and age squared, income earned by a male spouse or partner (set equal to zero if there is no male partner), the number of own under-19 children in the household, state fixed effects, the number of days elapsed between December 1 and the birth, day-of-week dummies for the date of birth, and dummies for being white, having some college education, having completed a college degree, and being married. The sample is restricted to mothers giving birth in the last two weeks of December 1998 or the first two weeks of January 1999.