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What a difference a day makes: Quantifying the effects of birth timing manipulation on infant health

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ABSTRACT

Scheduling births for non-medical reasons has become an increasingly common practice in the United States and around the world. We exploit a natural experiment created by child tax benefits, which rewards births that occur just before the new year, to better understand the full costs of elective c-sections and inductions. Using data on all births in the U.S. from 1990 to 2000, we first confirm that expectant parents respond to the financial incentives by electing to give birth in December rather than January. We find that most of the manipulation comes from changes in the timing of c-sections. Small birth timing changes, even at full-term, lead to lower birthweight, a lower Apgar score, and an increase in the likelihood of being low birthweight.

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1. Introduction

Over the past 30 years, the United States has seen a dramatic change in the way that both doctors and parents approach labor and delivery. An increasingly larger fraction of births are being scheduled at times that are more convenient for doctors or parents (Gans and Leigh, 2008; Gans et al., 2007; Lo, 2003). Inductions, stimulations, and cesarean sections (both elective and non-elective) are becoming more common. In 2008, c-sections accounted for 32.3% of all births in the U.S, while 23.1% of all births in the U.S. were induced, up from 21% and 8.5%, respectively, in 1989.[1] While the higher c-section rate may be a reflection of an increase in the number of women who genuinely need a cesarean, the incidence of c-sections is much lower in other parts of the world, such as France, the Netherlands, and Sweden, whose c-section rates in 2008 were 18.8%, 13.5%, and 17.3%, respectively (Gibbons et al., 2012). The growing trend toward c-sections, inductions, and scheduled births has alarmed some health professionals, expectant mothers, and policy-makers. In Healthy People 2010, the U.S. government outlined a goal of reducing the rate of c-sections among low-risk women by 2010.[2] Instead, the cesarean section rate *increased* during the decade, even among low-risk women, and as a result, the government has restated the same goal in Healthy People 2020. At the local level, some hospitals and clinics have set smaller-scale goals to decrease the prevalence of c-sections, inductions, and elective birth timing manipulation (Perkes, 2010; Langrew and Morgan, 1996; Landro, 2011). However, there are those who support the current trend, arguing in favor of the convenience that c-sections and inductions offer mothers and doctors.

The consequences of small changes in birth timing on the health of the newborn, when used for non-medical reasons, have not been assessed. Despite interest among physicians to understand the ramifications of birth timing changes, traditional experiments are challenging to conduct due to financial, time, and possibly also ethical constraints. However, by exploiting a natural experiment created by the U.S. income tax system – the child tax benefit – we are able to determine the consequences of small, exogenous

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[1] Induction status is unknown for 1.2% of births in 2008 and for 5.5% of births in 1989.

[2] Healthy People is a 10-year plan, now in its third round, aimed at improving the health of Americans.

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changes in birth timing. A parent can claim a new child on their yearly income taxes if the child was born anytime within the calendar year. [3] This strict rule gives parents who are expecting a child at the beginning of January a financial incentive to time the birth at the end of December instead. Although this may seem “extreme” to some parents, there is both anecdotal and statistical evidence showing that the timing of births is altered in order to receive the maximum financial benefit. [4] Countless stories document that the tax benefit is on the minds of many expectant parents; on pregnancy forums, mothers-to-be talk openly about wanting December babies and how they were able to persuade their doctors to schedule a December birth. [5] Dickert-Conlin and Chandra (1999) find that an additional $500 in tax benefits increases the probability of having a child in the last week of December versus the first week of January by 26.9%. In addition, there is evidence that other personal decisions are responsive to tax incentives. Both Sjoquist and Walker (1995) and Alm and Whittington (1995) find that a higher marriage tax penalty increases the probability of a couple delaying marriage from the last quarter of one year to the first quarter of the next year. [6] There is also evidence that the timing of death is responsive to changes in tax consequences (Kopczuk and Slemrod, 2003; Gans and Leigh, 2006).

In this study, we exploit the natural experiment created by the strict timing requirement for tax benefits, as well as exogenous variation in the generosity of child tax benefits during the 1990s, to investigate the mechanisms through which women alter their birth timing and whether these elective timing changes affect the health of the newborn. Using data from the Vital Statistics on the universe of all births in the United States from 1990 to 2001, in addition to census income data to estimate the size of tax benefits, we first verify that expectant mothers are in fact manipulating the timing of their births in order to gain additional tax benefits. We then assess the mechanisms through which women are altering their births; through shifting the timing of inevitable cesarean sections and inductions or by forcing a c-section or induction in December when a vaginal birth would have occurred in January. Finally, we explore the effects of birth timing manipulation on the health of the newborn.

Our results complement those of Dickert-Conlin and Chandra (1999), whose analysis is based on a sample of 170 births from the National Longitudinal Survey of Youth, supporting their finding that larger child tax benefits are associated with a higher chance of being born in December relative to the adjacent January. Our paper is the first to confirm their assumption that this effect is driven by shifts in inevitable c-sections from January to December. We also find that manipulation in the timing of births results in lower birthweight and Apgar scores at birth. Since these measures are associated with many short- and long-term consequences, our findings suggest that doctors and parents should use extreme caution when scheduling a birth for non-medical reasons before the natural arrival of the baby.

2. Child tax benefits in the United States

Like many other countries, the United States offers subsidies to families based on the number of children they have, with the goal of helping to offset the cost of raising children. In the U.S. these subsidies work through the income tax system and have been evolving over time. There are different channels through which child tax benefits operate: the Earned Income Tax Credit, Dependent Exemption, Child Tax Credit, Dependent Care Credit, and education benefits. For this study, we focus on the first three, as they are the most widely claimed and offer the most substantial benefits.

The Dependent Exemption is the broadest of the child credits, including older children and higher income families as well. The exemption is available for any child under 19 and any full-time student under 24 for whom the family provides over half of the support. The Dependent Exemption reduces a family's tax liability by lowering their taxable income. The value of the exemption to the family depends both on the amount of the exemption and the marginal tax rate the family faces. Because of this, the dependent exemption is largest for the wealthiest families facing the highest marginal tax rates. For example, in 1999, the dependent exemption was $2750 per dependent. For families in the 15% tax bracket, the benefit was worth $413 in tax savings, but for families in the 31% tax bracket, it was worth $853. [7]

The Child Tax Credit, which began in 1998, allows families with children a credit against their federal income tax for each qualifying child. The credit was initially $400 for each child, but has increased gradually to $1000. To qualify for the credit, the child must be under the age of 17 and live with the claiming parent for more than half the year. The credit begins phasing out at moderately high income levels. [8] Unlike the dependent exemption, the Child Tax Credit is a direct reduction in a family's tax liability, not just a deduction from taxable income. The credit is also partially refundable.

The Earned Income Tax Credit (EITC) was first introduced in 1975 as an earnings subsidy for low-income families with children. The EITC has been expanding over the decades, with its largest overhaul occurring in the early 1990s. Unlike the other credits, the EITC is fully refundable and its benefits are more generous, in terms of percent of earnings refunded, for those with lower income. In its most generous phase, earnings are matched by an EITC equal to 34% of earnings for a family with one child, and 40% for a family with two children. For example, in 1999, the credit's maximum was $2312 for a family with one child and $3816 for a family with two or more children. The EITC begins to phase out at a family income of $12,460. The EITC offers a large subsidy for children in low income families, especially as a proportion of the family's income.

The biggest changes in the tax treatment of children came in the 1990s, because of both the introduction of the Child Tax Credit in

[3] If a child is born on December 31, 2010, he can be claimed as dependent beginning on his parents' 2010 taxes. However, a child born on January 1, 2011 can be claimed starting in 2011. Children are claimed on parents' taxes every year they are a dependent. Thus, having a child born at the end of the year gives parents an additional year of tax benefits.

[4] Because health insurance deductibles and health savings accounts often reset at the beginning of the year, some parents have an additional incentive to schedule their births in December.

[5] For example, from Baby Community Center, posted October 6, 2010: “My Dr. told me she wont let me go past 38 weeks (January 7) but I'm hoping to deliver in December for two reasons. . .

1 Taxes
2 I have a high deductible insurance plan. I've met my deductible for the year, so everything from now on until December 31 will be covered 100%. After January 1, my $5600 deductible refreshes!”

[6] A couple that is married anytime within a year must claim themselves as married when filing their taxes for that year. Those who delay their marriage until the new year are able to avoid the marriage tax penalty on one additional year of income taxes.

[7] $2750 \times 0.15 = 412.5$; $2750 \times 0.31 = 852.5$.

[8] Specifically, the phase-out begins at $110,000 for parents who are married filing jointly, $55,000 for married filing separately, and $75,000 for all others (for tax year 2008).

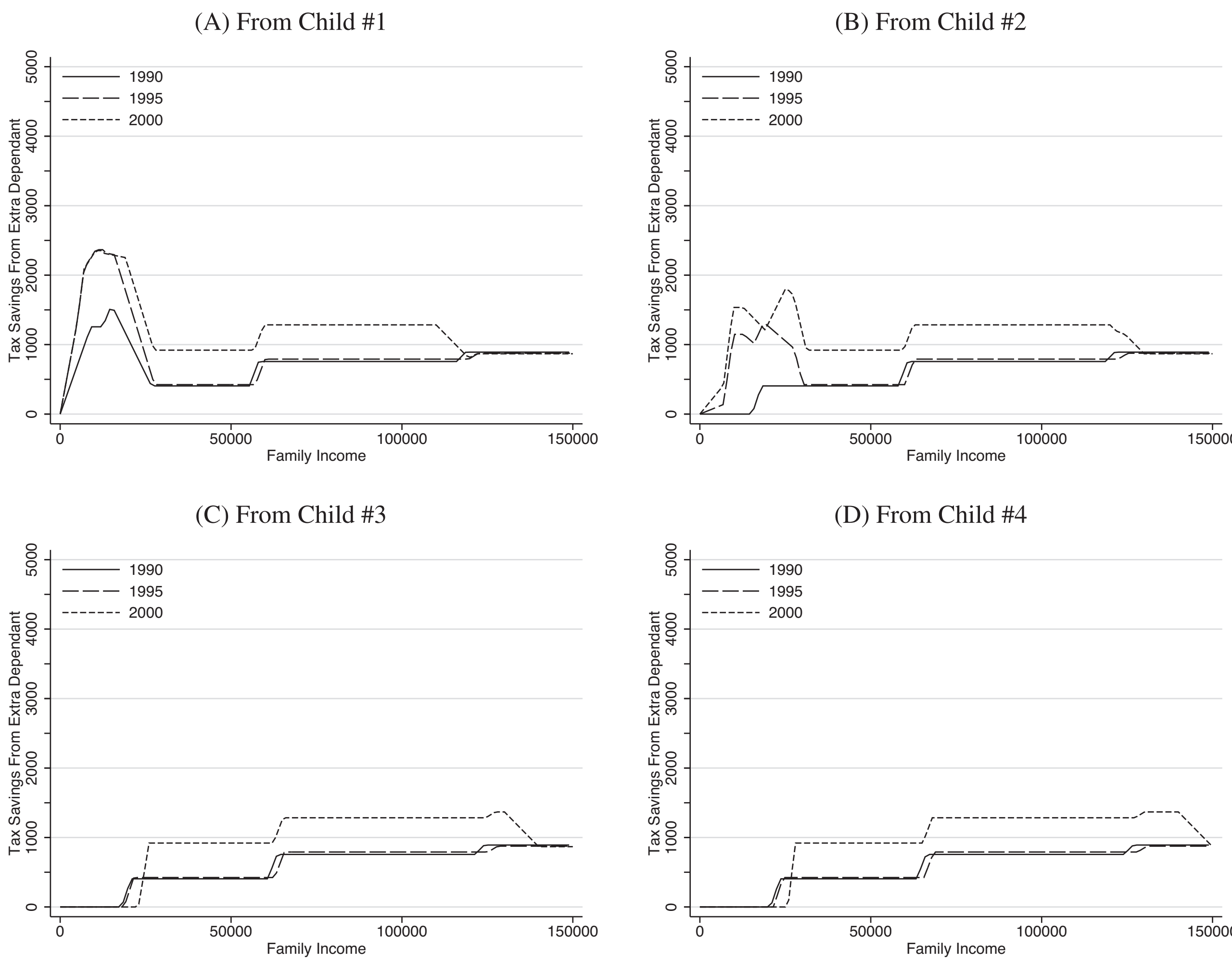

Fig. 1. Child tax benefits – married parents.

1998 and the expansions of the EITC in 1990 and 1993. We exploit this exogenous variation in tax liability to identify the impact of tax benefits on the timing of births. This allows us to examine the mechanism through which parents manipulate birth timing and to identify the causal effect of birth timing manipulation on infant health.

The size of a family's tax benefit for an additional child varies by income, state, year, marital status and how many children they already have. Figs. 1 and 2 show how the size of tax benefits varies by income for married couples and single mothers, respectively. The graph in panel (A) shows how the size of the benefit varies by income for a family having their first child. The other three graphs show the same relationship, but for families having their second, third or fourth child. The variation in benefits within income level in each graph comes from yearly variation in tax policy. There is some additional variation by state, but it is not shown in these graphs. [9] For families having their first or second child, the largest savings come at lower income levels, while savings increase with income for families having their third or fourth child. An analysis of variance shows that most of the variation in the magnitude of the tax benefit is generated by variation across birth order and year.

[9] Some states offer state income tax relief for families with children. These benefits are generally quite small.

3. Previous studies

3.1. Financial incentives and the timing of births

Dickert-Conlin and Chandra (1999) first examined the extent to which tax benefits shift births from the first week of January to the last week of December using the National Longitudinal Survey of Youth (NLSY). They find that a $500 increase in tax benefits increases the probability of having a child in the last week of December by 26.9%. Our study focuses on the health consequences of this manipulation. In order to do so, we use Vital Statistics data, which includes information on the type of delivery and health measures of the newborn. This allows us to determine the mechanisms through which mothers are altering the timing of their births and to assess the health consequences of these actions.

The first part of our study confirms the Dickert-Conlin and Chandra (1999) result, although the magnitudes of our point estimates are much smaller. There are several notable differences between the Dickert-Conlin and Chandra (1999) study and the first stage of ours. First, because they use the NLSY their sample is limited to 170 births. In contrast, our Vital Statistics analysis is based on all births that occurred in the U.S. between 1990 and 2000. In addition, Dickert-Conlin and Chandra (1999) exploit tax benefit variation during the 1980s and early 90s, when benefits were less generous. Because we use more recent data, we are able to leverage the variation in benefits brought forth by the expansions of the

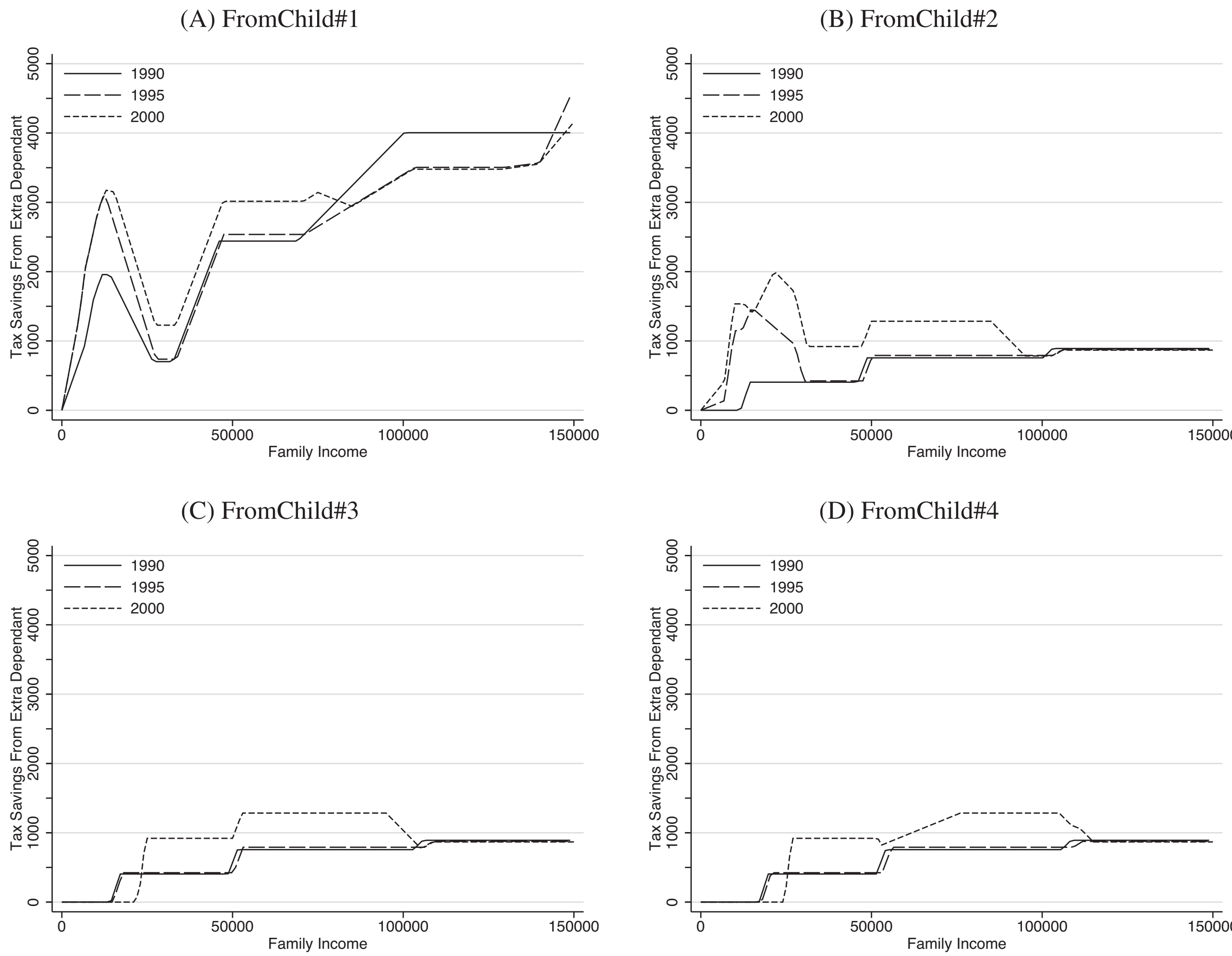

Fig. 2. Child tax benefits – single mother.

EITC and the Child Tax Credit in the 1990s. This is particularly useful variation because it is not always an increasing function of income, as it almost exclusively was in the 1980s. This is important if we are concerned that higher income individuals are more likely to alter the timing of their child's birth, independent of the financial benefit of doing so. If child tax benefits simply increase with income and those with higher income are more likely to alter birth timing, the estimate of the effect of tax benefits on birth timing would be biased upward. The non-linear relationship between income and tax benefits in the 1990s tax code allows us to eliminate this bias.

More recently, LaLumia et al. (2013) have revisited this question using data from the universe of tax returns filed between 2001 and 2010. They find a positive, but very small effect of tax incentives on birth timing, very much in line with the findings from this study. They also document that those with a late-December newborn are substantially more likely to report self-employment income that maximizes their benefits from the Earned Income Tax Credit compared to low-income filers with an early-January newborn. This is strong evidence of tax evasion.

In a similar vein, Gans and Leigh (2009) explore how the 2004 Australian Baby Bonus affected the timing of births. The one-time bonus, worth approximately $2250 USD, was announced just seven weeks before its introduction, when the treatment group was already in their third trimester of pregnancy. They estimate that over 1000 births were moved to a later date to ensure that the parents would receive the bonus. Most of the effect was due to changes in the timing of cesarean sections and inductions. Babies whose births were shifted to be eligible for the bonus were more likely to be of higher birth weight. Because the bonus was offered to babies born *after* a certain date, those altering their date of delivery were almost entirely those originally planning a c-section or induction. Of course, women who naturally went into labor before the bonus date could not opt to wait. Using the year-end cutoff of the U.S. tax benefit system allows us to see how all expectant mothers react to such incentives, and whether they "switch" their delivery method in order to ensure benefit receipt. We are also able to leverage variation in the size of benefits, rather than examining a flat benefit.

A similar result was found by Neugart and Ohlsson (2010) in Germany when the government changed its parental benefit system. Expectant mothers who were working would receive more generous maternity benefits if their child was born on or after January 1, 2007. The policy change increased economic incentives for women to postpone delivery, provided they were employed. They found that employed women near the end of their term were shifting their births to the new year so they could benefit from the new, more generous parental benefit system. There was no change in birth timing for mothers who were not employed and would not benefit from the policy change.

We make four important contributions to the current literature. First, by using the Vital Statistics, our study includes the universe of all births that occurred in the U.S. during our time period. Thus, we are also able to examine how the tax benefits affect the behavior of different subgroups whom we expect may either behave differently than the average parent or may be more at-risk for health complications. Second, by focusing our analysis on the 1990s, we are able to exploit the large variation in tax benefits caused by the Earned Income Tax Credit and the Child Tax Credit. Third, we determine the mechanisms through which birth timing manipulation is occurring. Lastly, and perhaps most importantly, we assess the health consequences to the newborn of elective birth timing changes. Although the current literature has been able to establish that parents respond to financial incentives in the timing of their child's birth, they have not determined what the costs of this behavior are in terms of the health of the child. To our understanding, we are the first study to do so.

3.2. Manipulating birth timing

The advancement of medicine has allowed patients and doctors increased discretion over the timing of births. Cesarean sections allow doctors to deliver babies days (sometimes weeks) before they would have been born naturally. Labor can also be induced using a combination of drugs that first dilate the cervix (Cervidil or Cytotec) and then contract the uterus (Pitocin) (Block, 2008).

Unfortunately, not all women are well-informed about the consequences of elective labor induction or c-sections. Induced labors tend to be more difficult and painful and they increase a woman's chance of having a c-section by two to three times (Block, 2008).[10] A c-section is major surgery and brings with it its own set of risks and complications: infection, hemorrhage, injury to organs, scar tissue formation inside the pelvic region, and extended recovery time. Babies born by cesarean are at higher risk for breathing problems, low Apgar scores, and fetal injury, as they may be nicked or cut during the incision (Shearer, 1993).

Although the altering of birth timing is often done for medical reasons, there is substantial evidence that birth timing is sometimes manipulated for reasons other than the health of the fetus or mother. A study of patients undergoing repeat cesarean sections found that of the 24,077 repeat cesarean deliveries at term observed, over half were performed electively; of these, 35.8% were performed before 39 completed weeks of gestation (Nita et al., 2009). Doctors often schedule c-sections at convenient times or at times when the hospital is better-staffed; far fewer babies are born on Saturdays and Sundays than on weekdays. In fact, throughout the 1990's, birthrates were approximately 30% higher on weekdays compared to weekends.[11] Gans and Leigh (2008) find that nearly one-third of U.S. and Australian babies who would have been born on a weekend were "moved" to a weekday. They conclude that increases in cesarean sections and inductions account for around four-fifths of the shift away from weekend births. Also using data from Australia and the U.S., Gans et al. (2007) find that there are fewer births on the days of the annual obstetricians and gynecologists' conferences. There is, alternatively, a rise in births prior to the conferences, suggesting that physicians are shifting the births earlier to accommodate their schedules.

Parents may also want to shift the timing of their child's birth. For instance, in Taiwan some people believe that date of birth determines much of a child's fate; thus, they ask for their child's birth to be scheduled on particular days (Lo, 2003). Using data from Australia, Gans and Leigh (2012) estimate the bargaining power that parents have over the timing of the birth of their children by exploiting the fact that parents are averse to having their children born on February 29 and April 1. Since physicians are not averse to working on those days, unless those days fall on a weekend, they examine what happens when parents' desire to avoid those birth dates conflicts with doctors' desire to avoid working on a weekend. Their results suggest that in approximately three-fourths of cases, the physician's views prevail over those of the patient. Although most of the power rests with the doctors, parents win in approximately one-quarter of the cases. This suggests that parents are often willing and able to alter their delivery dates.

4. Data

We use data on the universe of all births in the United States from 1990 to 2001 from the National Center for Health Statistics' Vital Statistics Natality Files. These data are constructed using birth certificate information from each state, and include demographic characteristics of the mothers and detailed information about the birth. Relevant to our study, the data include information on the month and year of birth, how the child was delivered, and a number of health measures for the newborn.

One limitation of the Natality data is that it does not include any information on family income, which is essential for determining the potential financial benefit of having a baby in December versus January. Because of this, we impute tax benefits based on the demographic information available to us in the Vital Statistics. These include state, year, race, mother's education, mother's age, marital status, and parity(number of children). A benefit of using education and other demographic characteristics to derive income estimates is that they are largely predetermined and not easily modifiable, whereas income can be manipulated in order to receive additional benefits LaLumia et al. (2013). We derive income estimates using the 1990 5% Public-Use Census to calculate the average total family income by demographic cell. In order to exploit variation in the generosity of tax benefits over time, rather than potentially endogenous variation in cross-sectional income growth, we hold income fixed in real terms by adjusting the 1990 income estimates for inflation and using those income estimates for 1991–2000.[12] This is a common strategy in the public finance literature, and is used in order to avoid endogenous changes in income due to tax structures (Medicaid expansions: Gruber, 2005; EITC: Meyer and Rosenbaum, 1999).

We use NBER's TAXSIM to calculate yearly tax liabilities based on income, filing status (single, married, or head of household), and number of dependents. To determine the financial benefit of having a birth in December instead of January, we find the difference between the tax liability without and with an additional dependent for each observation from the 1990 Census. We then average the benefit of the December birth by demographic cell and assign the average benefit to the observations in the Natality data. If a mother is listed as married, we assume she files her taxes jointly with her husband. If she is single, we assume that her filing status is "single" when she has no children, but changes to "head of household" once she has at least one child. Our sample includes all 50 states and the District of Columbia and all White and Black mothers.[13]

[10] Pitocin creates contractions that are stronger and more frequent than those produced naturally by the body; this can lead to hyperstimulation of the uterus – contractions that come too hard and too fast – which can cause oxygen deprivation for the fetus. If the baby is not delivered quickly, a c-section is needed to remove the baby before its oxygen levels drop too low.

[11] Authors' calculation using the Vital Statistics.

[12] We are most concerned that changes in the child tax benefits led to changes in family income. Particularly with the EITC, small adjustments in income could lead to larger benefits. The annual inflation measures are from the Bureau of Labor Statistics.

[13] Hispanics are included in these race categories. Black Hispanics as Black and White Hispanics as White.

Table 1
Summary statistics – comparison of December and January.

Variable	(1) December	(2) January	(3) Difference	(4) Standard Error
Number of births	2,432,731	2,383,621		
Mean family income ($000)	49.14	49.02	0.12	0.02***
Mean tax saving ($000)	0.787	0.783	−0.003	0.0004***
Mean percent of income saved	2.862	2.843	0.019	0.0034***
% Married	76.67	76.88	−0.21	0.04***
% Black	13.66	13.82	0.16	0.03***
Mean age	28.21	28.12	−0.09	0.004***
% C-sections	24.87	24.39	−0.48	0.04***
% Inductions	16.80	15.32	−01.48	0.03***

* $p < 0.10$.
** $p < 0.05$.
*** $p < 0.01$.

Mother's education is categorized as less than high school diploma, high school diploma, and college degree or more. We limit our sample to mothers between the ages of 20 and 45, and group mother's age into five year age intervals. Marital status is assigned according to whether the mother is married or single at the time of birth. Finally, we define parity as the number of children the mother has, including the new birth. We limit our sample to families with four or fewer children in order to avoid the idiosyncrasies of especially large families.

Table 1 presents the summary statistics for the births in our sample that occur in December or January. Mean family income for our sample is approximately $49,000 (in year 2000 dollars) and the average savings from having a December birth is approximately $790. While the benefit is, on average, 2.9% of family income, the largest savings are for families at the lowest income levels. The table also shows mean attributes of mothers and births that occur in December and January. Mother who give birth in December are, on average, more likely to be White, married, and older, than mothers who give birth in January. December births are also more likely to be induced or c-sections than those in January. These differences are statistically significant. We seek to assess how much of these differences are attributable to changes in birth timing due to tax incentives.

In later analyses, we expand our sample to include a set of comparison months, October and November, in order to assess the effect of the tax benefits on birth and health outcomes. Table 2 shows summary statistics for December and January and October and November. The second column shows the summary statistics for the months of December and January combined, while the third column shows the statistics for October and November combined. The fourth column of the table shows the difference in means between the attributes of December/January births and October/November births. While the differences in the mean characteristics of these births are not very large, many of the differences are statistically significant. These differences may be attributed to changes in birth timing and/or seasonality in the types of women who give birth across the year, with winter babies having, on average, poorer health (Stupp and Warren, 1994; Doblhammer and Vaupel, 2001; Bound and Jaeger, 1996). That is, the average mother who gives birth in December or January is slightly different than the average woman who gives birth in October and November. In order to assess how much of these differences in infant health is due to changes in birth timing, we are careful to control for demographic characteristics in our estimating equation as well as demographic-specific time trends so that seasonality does not contaminate our results.

5. Empirical analysis

5.1. Altering the timing of births

The empirical analysis utilizes individual-level Vital Statistics data, and we begin by restricting our sample to births that occurred in December or January. Consistent with the empirical strategies used in Dickert-Conlin and Chandra (1999) and LaLumia et al. (2013), we relate the probability that a child is born in December to the size of the benefit parents receive from having the child born before the new year. Thus, we estimate regressions in which the dependent variable, DecBirth, is an indicator variable equal to one if the birth occurred in December and zero if it occurred in January. The regressions take the form:

$$\text{DecBirth}_i = \alpha + \beta \text{TaxBenefit}_i + \gamma X_i + \phi_t + \varepsilon_i \quad (1)$$

where TaxBenefit_i is the tax savings associated with a December birth relative to a January birth for individual i (in thousands), X_i is a vector of demographic characteristics as described below and ϕ_t is a vector of year fixed effects. [14] Our coefficient of interest is β, which measures the effect of a one thousand dollar increase in child tax benefits on the likelihood of having the birth in December relative to January. Tax benefits vary across year, family income, parity, marital status, and state. We exploit all aspects of this variation in order to determine whether those with higher tax benefits are more likely to have a December birth. However, if we are concerned that those with higher child tax benefits are more likely to have a December birth, independent of the benefit, then we want to control for these characteristics as well as possible. Table 3 shows estimates from this regression for a number of different specifications.

All specifications in Table 3, with the exception of Columns (3) and (4), include year fixed effects to control for unobservable factors within each year that might impact the likelihood of having a December birth and are estimated using a linear probability model. [15] Standard errors are clustered by state-year, because much of the variation in tax policy occurs at this level. [16] Columns (1) and (2) replicate Dickert-Conlin and Chandra (1999)'s methodology by controlling for total family income, mother's income, mother's age, race, marital status and education. We include an indicator variable equal to one if the family lives in an urban area and an indicator variable equal to one if the child is the first or second child born to the mother. Column (2) also includes an interaction term between the tax benefit for having the child in December and total family income to account for the possibility that families differ in their responsiveness to tax incentives across income levels.

The estimate in Column (1) of 0.0037 indicates that a $1000 increase in tax benefits is associated with approximately a 0.37 percentage point increase in the probability of a December birth. This point estimate corresponds to approximately 1 out of every 134 January births being moved to December for a $1000 increase in tax benefits. When we additionally control for the interaction between tax incentives and income in Column (2), the estimated effect of the tax benefit increases to 0.54 percentage points. Both effects are precisely estimated. These translate to approximately a 0.7% change on the base probability of being born in December

[14] Dollar values are indexed to the year 2000 and imputed based on the individual's demographic characteristics.

[15] The marginal effects from a probit estimation, as in Dickert-Conlin and Chandra (1999), are very similar, which is not surprising because the predicted probabilities for all observations are close to 0.5. Linear probability is our preferred model because in many specifications we will be interpreting interaction terms, and this can be problematic in nonlinear models (Ai and Norton, 2003).

[16] Alternatively, we have clustered on year-parity and year-state-parity, and this does not substantively change our results.

Table 2
Summary statistics – December and January compared to comparison months.

Variable	(1) Dec. and Jan.	(2) Oct. and Nov.	(3) Difference	(4) Standard Error
Number of births	4,816,352	4,850,797		
Family income ($000)	49.08	49.55	0.47	0.02***
Tax saving ($000)	0.785	0.787	−0.001	0.00***
% of income saved	2.85	2.83	−0.02	0.00***
% Married	76.77	77.29	0.52	0.03***
% Black	13.74	13.25	−0.49	0.02***
Age	28.17	28.25	0.08	0.00***
% C-sections	24.63	24.56	−0.07	0.02***
% Inductions	16.09	16.43	−0.36	0.02***
% Non-induced C-sections	20.90	20.79	−0.11	0.03***
% Induced C-sections	3.11	3.20	0.08	0.01***
% Non-induced vaginal	63.36	63.06	0.29	0.03***
% Induced vaginal	13.02	13.29	−0.27	0.02***
Birthweight (g)	3352.83	3362.11	9.27	0.38***
% Low birthweight (<2500 g)	6.79	6.59	−0.20	0.02***
1 Min Apgar Score	8.071	8.070	−0.001	0.0014
5 Min Apgar Score	8.950	8.948	0.0013	0.0006**
% Normal Apgar Score (>6)	99.00	99.00	0.00	0.00
% Ventilation (<30 min)	1.86	1.90	0.04	0.01***
% Ventilation (≥30 min)	0.797	0.788	−0.01	0.006*
% Any ventilation	2.66	2.69	−0.03	0.01***

* $p < 0.10$.
** $p < 0.05$.
*** $p < 0.01$.

Table 3
Probability of a December birth.

	(1) Dec birth	(2) Dec birth	(3) Dec birth	(4) Dec birth	(5) Dec birth	(6) Dec birth	(7) Dec birth	(8) Dec birth
TaxBen (000s)	0.0037***	0.0054***	0.0002	0.0031**	0.0035***	0.0031**	0.0032**	
	(0.0008)	(0.0012)	(0.0011)	(0.0015)	(0.0009)	(0.0013)	(0.0014)	
% Saving								0.0003***
								(0.0001)
Family Inc (000s)	−0.0002***	−0.0002**		−0.0001***	−0.0001***			
	(0.0001)	(0.0001)		(0.0000)	(0.0000)			
Mother's Inc (000s)	0.0001	0.0001						
	(0.0001)	(0.0001)						
TaxBen × Family Inc		−0.0000						
		(0.0000)						
First or Second Child	−0.0016**	−0.0015**						
	(0.0007)	(0.0007)						
Marital Status	0.0122***	0.0118***		0.0099***	0.0091***			
	(0.0023)	(0.0023)		(0.0020)	(0.0022)			
Mother 's Ed.	−0.0001	−0.0001						
	(0.0002)	(0.0002)						
Urban Residence	−0.0039***	−0.0039***						
	(0.0010)	(0.0010)						
Mother's Age	0.0005***	0.0005***						
	(0.0001)	(0.0001)						
Black	−0.0007	−0.0006			−0.0010			
	(0.0011)	(0.0011)			(0.0014)			
Year FE	Yes	Yes	No	No	Yes	Yes	Yes	Yes
State FE	No	No	No	No	Yes	Yes	Yes	Yes
Age FE	No	No	No	No	Yes	No	No	No
Parity FE	No	No	No	Yes	Yes	Yes	Yes	Yes
Education FE	No	No	No	No	Yes	No	No	No
Income × Parity	No	No	No	No	No	Yes	Yes	No
Demographic Group FE	No	No	No	No	No	Yes	Yes	Yes
Dem. Group Time Trend	No	No	No	No	No	No	Yes	Yes

Standard errors in parentheses. *Notes*: Standard errors are clustered by state-year. The mean of Dec Birth is 0.505. There are 4,816,352 observations in columns (1)–(7), and 4,816,076 observations in column (8). The estimates displayed in each column include different control variables, as described in pages 16–18.
* < 0.10.
** $p < 0.05$.
*** $p < 0.01$.

of 50.5%. These estimates are significantly smaller in magnitude than those of Dickert-Conlin and Chandra (1999), who estimate the effect to be between 22.7 and 34.4 percentage points. Even when we scale up our estimates by four to account for the fact that we are looking at the entire month of December instead of just the last week as in Dickert-Conlin and Chandra (1999), our estimates are less than one sixteenth the magnitude of theirs. While this discrepancy in our findings may be attributable to number of causes, which we discussed in 3.1, we are confident that this difference is not a result of attenuation bias caused by our imputed income measures as our findings are quite close to those of LaLumia et al. (2013), who use exact income data from tax returns to measure this relationship.

In Columns (3)–(7), we test the sensitivity of these results to alternative specifications. Column (3) does not include any control variables, while Column (4) includes only a linear control for income, an indicator variable equal to one if the mother is married, and a full set of parity fixed-effects to account for the fact that parents may be more (or less) comfortable altering the timing of their subsequent births as they become more familiar with the birth process and child tax benefits. To control more flexibly for demographic characteristics, we first include age-group and education level fixed-effects (Column (5)), and subsequently use demographic group fixed-effects, where a demographic group is defined by race, age-group, education level, and marital status (Column (6)). [17] The demographic group fixed-effects control for differences in preferences and ensure our results are not driven by seasonality in births that vary across demographic groups. We include state fixed-effects to account for time-invariant geographic variation in medical practices might influence the probability of a December birth and are spuriously correlated with tax values. We also control for income interacted with parity. We include these interaction terms, rather than one linear control for income, because of income's differential effect on child tax benefits by parity. [18] In Column (7), we also include a linear time trend for each demographic group to control for changes in the prevalence of birth timing manipulation by demographic group as well as changes in seasonality over time. Finally, in Column (8) we show estimates when using the percent of income saved as the treatment variable. This specification is otherwise identical to that in Column (7), except we exclude the income controls. In additional analyses shown in Section 6, we isolate separate components of the variation in child tax benefits. Those estimates lack statistical power, which implies that in order to identify the relationship between the tax benefits and birth timing, we must utilize all the different sources of variation in the tax benefits.

The difference between Column (3) and Column (4) highlights the importance of including controls for marital status, income, and parity. Including additional control variables in Columns (5)–(7) has very little effect on the point estimates, and although the precision of our estimates drops slightly, they are still statistically significant at the 5% level. Our preferred specification is that in Column (7), which includes the full set of fixed-effects as well as the demographic group time trend. This is the most conservative specification, as the demographic group time trend absorbs some of the variation in tax benefits. However, in order to identify the causal effect of the tax benefit on birth timing (and subsequently on health outcomes), it is vital that we properly control for differences in demographic characteristics of the mothers.

The estimate on the effect of the percent of income saved from the tax benefit shown in Column (8) further supports our findings. A one percentage point increase in the percent of income saved, increases the probability of being born in December by 0.03 percentage points. This point estimate, along with the negative coefficient on the interaction between income and tax benefit from Column (2), implies that it may be the lower income families who are most responsive to the tax incentive. We investigate this further in Section 6 when we assess the effect of an EITC policy change on birth timing for low income mothers. We use the level of tax savings as our treatment variable in all subsequent analyses for consistency with the previous literature.

5.2. Mechanisms

Next, we seek to identify the mechanisms through which birth timing manipulation occurs. We estimate two additional equations for each delivery method in order to determine whether birth timing and/or delivery methods are manipulated in response to the child tax benefits. By interpreting these coefficients together with our preferred specification from Column (7) of Table 3, we are able to identify two different types of manipulation: "shifting" and "switching." We define shifting as having a birth occur in December rather than in January. If only shifting occurs, the type of delivery remains unchanged, but occurs earlier than it would have in the absence of the tax benefits. Switching is defined as a change in the *type* of delivery. An example of switching is having a c-section instead of a vaginal delivery in order to ensure a December birth. A birth can be shifted and/or switched in order to receive the tax benefits. One can schedule an inevitable c-section for December rather than January. In this instance, the birth is shifted, but not switched. A birth that is switched, but not shifted would occur if a vaginal birth would have happened in December, but is switched to a December c-section to ensure receipt of the benefit. A birth can also be both shifted and switched. This would occur if a child would have been born by vaginal delivery in January, but is instead born by c-section in December. Both types of manipulation may have health consequences for the newborn, which will be addressed later in the paper.

To determine whether c-sections are shifted in order to receive the tax benefit, we estimate Eq. (1), while further isolating our sample to births that occurred by c-sections. [19] In this regression, the coefficient on TaxBenefit_i, displayed in Column (2) of Panel A of Table 4, can be interpreted as the effect of an additional \$1000 in tax benefits on the likelihood of a December *c-section* birth. We estimate that an additional \$1000 in tax benefits shifts approximately 1 per 75 January c-sections to December. An analogous estimate for vaginal births is displayed in Column (3) and for induced births in Column (4). While the coefficient for vaginal births is not statistically significant, the coefficient for induced births suggests that 1 per 72 January inductions are shifted to December. Evaluating these point estimates at their respective 95% confidence intervals suggests that between 2 and 11 for every 489 January c-sections shift earlier and between 1.5 and 12 for every 489 January inductions shift earlier. These results allow us to confirm that changes in birth timing happen through the most likely channel: scheduling c-sections and inductions earlier. [20]

[17] There are 60 demographic groups: 5 age groups × 3 education groups × 2 races × 2 marital statuses.

[18] Results do not rely on the manner in which we control for income. Results are robust to inclusion of a smooth polynomial or spline in income.

[19] Births that result in a c-section can either be induced or non-induced.

[20] It is important to note that if we were to also find evidence of "switching", that effect would account for part of this estimated effect. For example, if five December births switch from vaginal to c-section deliveries, it will inflate the number of December c-sections relative to January c-sections, even though they were not shifted earlier. We do not, however, find evidence of switching.

Table 4
Shifting and switching births.

(Panel A) Shifting

	(1) All	(2) C-section	(3) Vaginal	(4) Induction	(5) C-section Non-Induced	(6) C-section Induced	(7) Vaginal Non-Induced	(8) Vaginal Induced
TaxBen (000s)	0.0032**	0.0065***	0.0024	0.0068**	0.0051**	0.0089	0.0020	0.0051*
	(0.0014)	(0.0023)	(0.0015)	(0.0027)	(0.0025)	(0.0057)	(0.0016)	(0.0030)
Mean	0.505	0.511	0.503	0.511	0.510	0.507	0.501	0.512
N	4,816,352	1,184,823	3,631,529	789,270	993,602	151,391	2,976,285	637,879

(Panel B) Switching

	(1) C-section	(2) Vaginal	(3) Induction	(4) C-section Non-Induced	(5) C-section Induced	(6) Vaginal Non-Induced	(7) Vaginal Induced
TaxBen × DJ Birth	−0.0005	0.0004	0.0003	−0.0009	0.0000	0.0002	0.0002
	(0.0013)	(0.0007)	(0.0007)	(0.0008)	(0.0003)	(0.0008)	(0.0006)
TaxBen (000s)	0.0018	−0.0012	−0.0032***	0.0049***	−0.0034***	−0.0015	0.0002
	(0.0015)	(0.0013)	(0.0012)	(0.0013)	(0.0006)	(0.0016)	(0.0010)
DJ Birth	0.0008	−0.0008	0.0013*	0.0016*	−0.0002	−0.0024***	0.0016***
	(0.0017)	(0.0007)	(0.0007)	(0.0010)	(0.0003)	(0.0008)	(0.0006)
Mean	0.246	0.763	0.165	0.208	0.032	0.630	0.134
N	9,667,149	9,553,218	9,551,033	9,551,033	9,551,033	9,505,286	9,505,286

Standard errors in parentheses. *Notes*: All regressions include state, birth order, year and demographic group fixed effects, as well as demographic group linear time trends and income × birthorder controls. Standard errors are clustered by state-year.
* $p < 0.10$.
** $p < 0.05$.
*** $p < 0.01$.

Columns (5)–(8) further divide the sample into four mutually exclusive delivery methods: non-induced c-section, induced c-section, non-induced vaginal, and induced vaginal. We are more cautious about these results since the data for inductions is less complete. Induction status is unknown for 3.4% of c-sections and 0.7% of vaginal births, and we only use observations for which we know the induction status. However, this analysis provides additional insight on which types of births are most likely to have their timing changed for non-medical reasons. Induced c-sections should be considered unplanned c-sections, as labor is only induced when a vaginal birth is intended. Both induced c-sections and vaginal births can be scheduled or not. A non-induced vaginal birth is the closest proxy we have for a spontaneous, non-scheduled birth. We consider the estimates for these births a falsification test to show that birth timing, and not conception, is being manipulated in order to gain the additional tax benefit of a December birth. We lose some precision when assessing each delivery type separately, particularly for induced c-sections which account for only 3.2% of all births. Nonetheless, we find that non-induced c-sections and induced vaginal births are driving the shifting results. The coefficient for vaginal non-induced births is small and is not statistically different from zero, which confirms that shifting is only occurring for births whose timing *can* be shifted.

In order to examine whether any of the changes in the timing of birth can be attributable to switching the type of delivery a woman has, we assess whether the tax benefit is correlated with the likelihood of having a certain type of delivery in December or January relative to a set of comparison months. For c-section births, the regression takes the form:

$$\mathrm{CSec}_i = \alpha + \beta_1 \mathrm{TaxBenefit}_i \times DJ_i + \beta_2 \mathrm{TaxBenefit}_i + \beta_3 DJ_i + \gamma X_i + \phi_t + \varepsilon_i \tag{2}$$

where we extend our sample to also include two comparison months – October and November.[21] CSec_i is an indicator variable equal to one for c-section births and zero otherwise and DJ_i is an indicator variable equal to one for births that occur in December or January and zero otherwise. The remaining variables are the same as in Eq. (1). We assign the observations from the comparison months the benefit they will receive from having their child. It is important to note that these parents are not at risk of not receiving the benefit based on small timing changes (unlike the December and January parents). However, they act as a comparison group as they allow us to observe how those who receive the same benefits as their December counterparts behave in the absence of a timing change incentive. β_1 is our coefficient of interest; it measures whether c-sections are more likely in December and January compared to October and November for families with higher potential tax benefits. Families with higher tax benefits may be more or less likely to have a c-section for reasons unrelated to the timing change incentive. Similarly, babies born in December or January may be more or less likely to be born by c-section (again, unrelated to the timing change incentive). By controlling for these factors, we can isolate the impact of the tax benefit on type of delivery. This regression does not capture shifts from January to December, as these two months are both “treated.” Rather, it captures whether the tax benefits affect the difference in the likelihood of having a c-section in December or January relative to the comparison months. In order to fully understand whether mothers are shifting and/or switching their births in order to gain additional tax benefits, we interpret

[21] Results are robust to using a variety of comparison months. In order to minimize potential confounding factors resulting from differences in mothers' characteristics throughout the year, we use the two months most similar to December and January based on a number of predetermined characteristics, including number of births, income, tax savings, percent of income saved, mother's age, education, marital status, and race. Compared to all other consecutive month-pairs in the year, October and November is the most similar month-pair to December and January.

these results together with the shifting results. We estimate an analogous equation for each type of delivery.

The estimates of β_1 for each delivery type are found in the top row of Panel B of Table 4. For all delivery methods, the coefficients are small and are not statistically different from zero. This suggests that parents are not manipulating birth timing by changing the method of delivery and that changes in birth timing are driven solely by scheduling inevitable c-sections and inductions earlier. Accordingly, any health effects we find in our following analyses are considered to be primarily a result of changing the timing of the births and not changing the type of delivery.

5.3. Infant health outcomes

One of the benefits of using the Vital Statistics for this analysis is the great deal of information it has about each birth, including information about labor and short-term health outcomes of the infant. By assessing the effect of the tax benefit on a number of health outcomes, we seek to answer the broader question of whether small, elective changes in birth timing has any effect on newborn health. Our main infant health outcomes of interest are birthweight and Apgar score. We consider two birthweight measures: log birthweight and whether a newborn is low birthweight. The log of birthweight is used rather than the level in order to more easily relate our findings to the existing literature on the longer-term consequences of lower birthweight. Low birthweight babies are those weighing less than 2500 g at birth. The Apgar scores come from a test given 1 and 5 min after birth that quickly assesses the infant's health through activity, pulse, grimace (reflex irritability), appearance, and respiration. Each category is scored from 0 to 2. A total score between 7 and 10 is generally considered normal. Birthweight and Apgar score are valuable outcomes to assess for four main reasons. First, they both are reported for nearly all births. This allows us the statistical power necessary to determine even small effects of birth timing changes on these outcomes. Second, since they are both continuous variables, we can observe marginal changes in the newborns' health. They are also both strong indicators of overall health, and can reflect more specific health problems not observed in our data. Important developmental gains are made in the final weeks of gestation that would be reflected in birthweight and Apgar score. For instance, the lungs and liver are not fully developed until 36 to 38 weeks, and even when fully developed, they continue to strengthen during the final week(s) (March of Dimes, 2012c). Basic weight gain is also an important part of the final weeks of fetal development. In the third trimester, babies gain an average of 225 g per week (The American College of Obstetricians and Gynecologists, 2010). Finally, a number of studies have sought to determine the causal effect of birthweight on longer-term outcomes allowing us to estimate the effect of birth timing changes on the health and well-being of children later on in life. We discuss these in more detail in Section 7.

We also assess the effect of birth timing manipulation on assisted ventilation. Newborns with respiratory failure are placed in mechanical ventilators, which help them breathe. The Vital Statistics reports whether the newborn used assisted ventilation for less than 30 min or for 30 min or more. Since lung function is one of the final stages of fetal development, ventilation is a valuable health outcome to assess. Newborns that are born early are particularly susceptible to respiratory difficulties. [22]

To assess the health consequences of birth timing manipulation, we compare infant health measures in December and January to the comparison months, October and November, as in Eq. (2). Again, β_1 (the coefficient on the tax benefit and December/January birth interaction) is the coefficient of interest, as it describes the effect of an additional \$1000 in tax benefits on the health of babies born in December or January relative to those born in the comparison months. β_3 captures the difference in the average health of the "treated" and comparison babies that is not attributable to the tax benefit. By controlling for the tax benefit and December/January births separately, we account for factors that may affect infant health independent of birth timing changes. The summary statistics from Table 2 show the average infant born in December or January is different (both in terms of their health and delivery method) than the average infant born in October or November, as are their parents.

We use this approach, rather than comparing December to January, in order to avoid the issue of selection in the decision to manipulate birth timing. For example, it is likely that only low-risk births are shifted earlier, in which case the estimated impact would be biased downward. [23] By assessing the impact on the health of December *and* January babies together, we are able to eliminate this source of bias.[24] In Appendix A Table A.10, we show that not using a comparison group causes one to conclude that babies born in December to look healthier as a result of shifting. This is what we would expect from positive selection into birth timing.

It is important to note that the estimates from our estimation strategy are also capturing the health effects of births that shift *within* December or January as a result of the tax benefit. That is, there may be parents who would have had a baby born in December anyway who decide to move up the birth to ensure a December delivery. There may also be parents who try to have a December birth, but because of delays from the induction process still end up having a January birth. [25] Doctors may also move up the scheduled births of December babies in order to accommodate the additional babies that will be born in the final week of December. In all these cases the birth is hastened as a result of the tax benefit, but there is no shifting from January into December. Our estimates in Sections 5.1 and 5.2 do not capture these changes in birth timing; however, the estimates from our health regressions do. As a result, we are cautious when interpreting the estimates from our earlier analyses with the health results together.

Table 5 displays the health results for all births. Columns (1) and (2) show that higher potential tax benefits are associated with a decrease in log birthweight and an increase in the likelihood of being a low birthweight baby. This is consistent with the shorter gestational periods that result from shifting births earlier (which we investigate in more detail in Section 5.4). Evaluating the point estimate from Column (1) at its 95% confidence intervals suggests that an additional \$1000 in benefits causes between a 0.07% and 0.19% decrease in average birthweight, or between 2.41 and 6.37 g. We also estimate a 0.08 percentage point increase in the likelihood of being born below 2500 g. On average, approximately 68 babies

[22] There are two main drawbacks of assessing ventilation as an outcome is that ventilation status is not reported for all births in a way that appears to be non-random.

[23] Imagine two babies who are due on January 5. Baby A would weigh 3500 g if born on January 5, while baby B would be 2500 g. The doctors know that baby B is very small, so they hold off as long as possible, and she is delivered on January 5, weighing 2500 g. The c-section to deliver Baby A is scheduled for December 31 and she born at 3300 g. If we compare the shifted baby to the non-shifted one, we would think that the shifting made the baby bigger. In reality, Baby A is 200 g smaller because of the earlier delivery.

[24] This would also eliminate any *upward* bias stemming from selection, though we have no reason to expect this.

[25] The time between induction and delivery differs greatly across women. It takes just a few hours for some and up to three days for others (March of Dimes, 2012a).

Table 5
Infant health outcomes for all births.

	(1) Log BW	(2) Low BW (<2500 g)	(3) Apgar1	(4) Apgar5	(5) Apgar5 ≥7	(6) Any Vent	(7) Vent. <30 m	(8) Vent. >30 m
TaxBen × DJ Birth	−0.0013***	0.0008**	−0.0029	−0.0030**	−0.0003	0.0002	0.0001	0.0002
	(0.0003)	(0.0004)	(0.0054)	(0.0015)	(0.0002)	(0.0003)	(0.0002)	(0.0002)
TaxBen (000s)	0.0021***	−0.0015**	−0.0035	0.0048**	0.0000	−0.0006	−0.0006	−0.0000
	(0.0006)	(0.0006)	(0.0062)	(0.0021)	(0.0003)	(0.0005)	(0.0005)	(0.0002)
DJ Birth	−0.0017***	0.0012***	0.0061*	0.0015	0.0003*	−0.0002	−0.0002	0.0000
	(0.0003)	(0.0004)	(0.0035)	(0.0013)	(0.0002)	(0.0002)	(0.0002)	(0.0001)
Mean	8.096	0.068	8.074	8.947	0.987	0.027	0.019	0.008
N	9,659,321	9,659,321	3,233,350	7,493,949	7,493,949	9,082,543	9,082,543	9,082,543

Standard errors in parentheses. *Notes*: All regressions include state, birth order, year and demographic group fixed effects, as well as demographic group linear time trends and flexible income controls interacted with demographic characteristics. Standard errors are clustered by state-year.
* $p < 0.10$.
** $p < 0.05$.
*** $p < 0.01$.

per 1000 are classified as low birthweight, and we find that this number increases by approximately 0.8 for an additional $1000 in benefits. The 5-min Apgar scores of the newborn is also negatively affected by the tax benefit, but the point estimate is quite small (a 0.003 point increase for a $1000 increase in benefits). The point estimate for the 1-min Apgar score is very similar, but it is only recorded for approximately one third of births and is less precisely estimated. We find no effect of the tax benefit on ventilation. While the point estimates from the ventilation regressions are positive (suggesting an increased need for ventilation), ventilation is a rare occurrence and the standard errors on the estimates are large.

The average effects that we find are quite small, but we know that these are driven by small number of births that are actually shifted. If we assume that the entire change in the average is driven by the few births that are successfully shifted from January to December (what we show in Table 4), the health cost to each of the shifted babies would be quite large. [26] We also test whether $\text{TaxBenefit}_i \times \text{DJ}_i$ has negative health effects across all outcomes we look at, [27] and we are able to reject the null hypothesis through bootstrapped replications. [28]

Table 6 contains the results for the delivery methods responsible for the majority of the manipulation: non-induced c-sections and induced vaginal deliveries. The magnitudes of the birthweight point estimates are larger for babies born via non-induced c-sections, suggesting that this group is driving the health estimates. It is important to keep in mind, however, that while we do not find evidence of switching from one delivery method to another, the observed responses in these tables *could* be a result of changes in the composition of babies born via the delivery method examined, and should be interpreted with caution.

5.4. Gestation

We next seek to determine the magnitude of the birth timing changes. While the Vital Statistics does not include any information on the due date of the infant or whether the birth was considered "earlier than normal," we can assess the effect of the tax benefit on gestational age at birth. Similar to the methodology used to determine whether delivery methods were being switched, we estimate the following equation:

$$\text{Gestation}_i = \alpha + \beta_1 \text{TaxBenefit}_i \times DJ_i + \beta_2 \text{TaxBenefit}_i + \beta_3 DJ_i + \gamma X_i + \phi_t + \varepsilon_i \quad (3)$$

where gestation is measured in weeks. Again, β_1 is our coefficient of interest, as it tells us whether the tax benefit is correlated with gestation for infants born in December or January. Column (1) of Table 7 shows results from this regression. We find that, on average, a $1000 increase in the tax benefit for a child leads to a 0.033 week *decrease* in gestation relative to the comparison months. Once again, this estimate captures the decrease in gestation caused by moving births from January into December as well as births that may have occurred earlier in December or January than they would have otherwise in order to secure receipt of the benefits. Thus, these estimates do not only tell us by how much the January births are shifted in order to receive the benefits as a December birth. The 0.033 decrease in gestation, on average, equates to approximately a 0.23 day (5.5 h) reduction in gestation per child born in December and January (not just those who are shifted). The implied birthweight decrease from this reduction in gestation is consistent with the birthweight estimates in Table 5. Babies gain approximately 225 g per week in the final trimester of pregnancy. 225 g/week × −0.033 weeks = −7.43 g. A 7.43 g decrease in birthweight is 0.22% of the mean birthweight 3351 g.

While gestation is certainly an important outcome for us to assess, it is not without its shortcomings. Gestation is measured in weeks, while the shifting we observe may only be on the magnitude of days. Thus, not all shifts in birth timing translate to changes in their reported gestation. Gestation is computed using the dates of the birth of the child and the mother's last normal menstrual period (LMP). The LMP is used as the initial date because it can be more accurately determined than the date of conception, which usually occurs 2 weeks after the LMP. In some instances (5.1% of births in 1999) a clinical estimate of gestation is used rather than the time between the LMP and birth. This is done for normal weight births of apparently short gestations and very low birthweight births reported to be full term. The most common use of the clinical estimate is for instances when the date of the LMP is not reported.

[26] For example, using our estimates from Section 5.1 and dividing our "second stage" estimate by our "first stage" estimate, we can calculate that each shifted baby's birthweight declines by an average of 40% (−0.0013/0.0032). We remind the reader, however, that this calculation assumes that the effect on birthweight *only* operates through the births shifted into December, an assumption that we believe is very unlikely to be true. In addition, the 90% confidence interval, calculated using a bootstrap with 200 replications is quite large. The 5th and 95th percentile of the sampling distribution are −111% and −20%, respectively.

[27] We include ventilation of <30 min and >30 min, but don't include "any" ventilation, because it is a linear combination of those two variables.

[28] Specifically, we test the null hypothesis that the β_1 for the outcomes: LogBW ≥ 0 & LowBW ≤ 0 & Apgar1 ≥ 0 & Apgar5 ≥ 0 & NormApgar ≥ 0 & Vent less than 30 m ≤ 0 & Vent more than 30 m ≤ 0. In 200 bootstrap replications, there weren't any where all 7 of these restrictions held. Table A.12 in Appendix A gives the information needed to test alternative joint tests. It lists the percent of replications where 0–7 of the restrictions held.

Table 6
Infant health outcomes by delivery type.

(Panel A) Non-induced C-section births

	(1) Log BW	(2) Low BW (<2500 g)	(3) Apgar1	(4) Apgar5	(5) Apgar5 ≥7	(6) Any Vent	(7) Vent. <30 m	(8) Vent. >30 m
TaxBen × DJ Birth	−0.0029***	0.0030***	−0.0179	−0.0023	−0.0002	−0.0007	−0.0003	−0.0004
	(0.0010)	(0.0011)	(0.0141)	(0.0039)	(0.0007)	(0.0007)	(0.0006)	(0.0005)
TaxBen (000s)	0.0020	−0.0048***	0.0050	−0.0116**	−0.0021***	0.0000	−0.0006	0.0007
	(0.0013)	(0.0014)	(0.0151)	(0.0045)	(0.0008)	(0.0010)	(0.0008)	(0.0006)
DJ Birth	−0.0013	0.0010	0.0122	−0.0006	0.0001	0.0012*	0.0005	0.0007
	(0.0009)	(0.0011)	(0.0094)	(0.0034)	(0.0006)	(0.0007)	(0.0005)	(0.0004)
Mean	8.065	0.120	7.870	8.872	0.979	0.045	0.027	0.018
N	1,988,256	1,988,256	676,412	1,507,491	1,507,491	1,868,544	1,868,544	1,868,544

(Panel B) Induced vaginal births

	(1) Log BW	(2) Low BW (<2500 g)	(3) Apgar1	(4) Apgar5	(5) Apgar5 ≥7	(6) Any Vent	(7) Vent. <30 m	(8) Vent. >30 m
TaxBen × DJ Birth	−0.0004	−0.0006	0.0471***	0.0004	0.0002	0.0002	−0.0002	0.0004
	(0.0007)	(0.0009)	(0.0156)	(0.0034)	(0.0005)	(0.0007)	(0.0007)	(0.0004)
TaxBen (000s)	0.0017	0.0011	−0.0071	0.0103**	0.0003	0.0001	−0.0002	0.0003
	(0.0011)	(0.0013)	(0.0167)	(0.0041)	(0.0006)	(0.0011)	(0.0010)	(0.0004)
DJ Birth	−0.0027***	0.0017**	−0.0289***	−0.0001	0.0000	−0.0007	−0.0003	−0.0003
	(0.0007)	(0.0008)	(0.0109)	(0.0031)	(0.0004)	(0.0007)	(0.0006)	(0.0003)
Mean	8.125	0.047	8.073	8.957	0.991	0.028	0.023	0.005
N	1,271,380	1,271,380	350,223	1,067,918	1,067,918	1,229,634	1,229,634	1,229,634

Standard errors in parentheses. *Notes*: All regressions include state, birth order, year and demographic group fixed effects, as well as demographic group linear time trends and flexible income controls interacted with demographic characteristics. Standard errors are clustered by state-year.
* $p < 0.10$.
** $p < 0.05$.
*** $p < 0.01$.

For about 1% of births in our sample, gestation is not known. Column (2) of Table 7 shows results from estimating the regression while excluding observations for which a clinical estimate of gestation is used. These results are very similar in magnitude from the results using all measures of gestation, which assures us that mis-measurement of gestation from the clinical estimate is not biasing our results.

We may also be concerned that gestation may be more likely to be misreported or missing when the timing of the birth has been changed for non-medical reasons. Thus, we estimate the regression, first using an indicator variable that is equal to one if gestation information is missing from the vital statistics for that birth as the dependent variable, and second using an indicator variable equal to one if the gestation measure is a clinical estimate. The estimates are shown in Columns (3) and (4) are not statistically different from zero.

To complete our gestation analysis, we seek to determine at what gestational age the shifting is occurring. That is, are the infants more or less likely to be born at a certain gestational age as a result of the tax benefit? To do this, we estimate the following equation for each gestational age between 30 and 45 weeks:

$$\text{GestAgeInd}_i = \alpha + \beta_1 \text{TaxBenefit}_i \times DJ_i + \beta_2 \text{TaxBenefit}_i + \beta_3 DJ_i + \gamma X_i + \phi_t + \varepsilon_i \qquad (4)$$

where GestAgeInd_i is equal to one if the infant is born at that specific gestational age. That is, if the infant is born at 37 weeks,

Table 7
The effect of tax benefits on gestation-related outcomes.

	(1) Gestation	(2) Gestation	(3) Gestation Missing	(4) Gestation Imputed
TaxBen × DJ Birth	−0.0330***	−0.0323***	0.000351	−0.00101
	(0.00524)	(0.00534)	(0.000285)	(0.000719)
TaxBen (000s)	0.0422***	0.0467***	−0.000307	0.00749***
	(0.00631)	(0.00618)	(0.000420)	(0.00182)
DJ Birth	0.0275***	0.0219***	0.000407*	0.00732***
	(0.00480)	(0.00501)	(0.000237)	(0.00104)
Mean	38.983	39.004	0.008	0.125
N	9,594,292	8,433,510	9,667,149	9,667,149

Standard errors in parentheses. *Notes*: All regressions include state, birth order, year and demographic group fixed effects, as well as demographic group linear time trends and income × birthorder controls. Column (1) shows the regression results from estimating Eq. (3) for all individuals and Column (2) displays the results from estimating the regression while excluding observations for which a clinical estimate of gestation is used. Column (3) shows the regression results when the dependent variable is an indicator variable that is equal to one if gestation information is missing from the vital statistics for that birth, and Column (4) shows the results for an indicator variable equal to one if the gestation measure is a clinical estimate. Standard errors are clustered by state-year.
* $p < 0.10$.
** $p < 0.05$.
*** $p < 0.01$.

the indicator variable for gestational age of 37 weeks will be equal to one, while all other gestation age indicator variables will equal zero for that observation. Once again, β_1 is the coefficient of interest as it tells us whether a larger tax benefits affects the likelihood of being born at a certain gestational age in the "treated" months – December and January, compared to October and November. By assessing the estimates for all regressions simultaneously, we can better understand which babies are being shifted to earlier births. To best show these findings, we graph the estimated coefficients from Eq. (4) along with their 90% confidence interval in Fig. 3. The first figure is for all births, while the two subsequent figures are for non-induced c-sections, and induced vaginal births, respectively. While many of the estimates are not statistically different from zero, a pattern emerges in the first two figures– babies may be more likely to be born before 38 weeks, but less likely to be born at 41 weeks or later. For induced vaginal births, the pattern isn't as clear. However, babies born by induced vaginal birth are more likely to be born at exactly 37 weeks the larger the tax benefit. Babies are considered full-term beginning at 37 weeks of gestation; thus, it appears that physicians wait to cross this potentially important threshold before inducing the birth. It is worth noting that since the mean of each of the gestation age variables is quite different, the magnitudes of the plotted coefficients should not be interpreted as the magnitude of the effect at the average.

Since most of the changes in birth timing occur after 35 weeks of gestation, we replicate our main analysis isolating the sample to births that occurred at 35 weeks of gestation or later to verify that our results are driven by the higher gestational aged newborns. Table A.11 in Appendix A shows these results, which are very consistent with our main findings.

6. Supplemental analyses and robustness checks

6.1. Heterogeneity in responsiveness and consequences

We next investigate whether any heterogeneity exists in both the timing response to the tax incentive and in the health consequences of birth timing changes across subgroups. To assess differences in the timing of births, we estimate the following equation:

$$\text{DecBirth}_i = \alpha + \beta_1 \text{TaxBenefit}_i \times \text{Group}_i + \beta_2 \text{TaxBenefit}_i + \beta_3 \text{Group}_i + \gamma X_i + \phi_t + \epsilon_i \quad (5)$$

As in our main estimates from Section 5.1, this analysis only includes December or January births, but seeks to determine whether certain groups are more responsive to the tax benefit. *Group$_i$* is an indicator variable equal to one if the birth belongs to one of the various groups we assess. β_1 is the coefficient of interest, as it shows the differential effect of the tax benefit for the subgroup.

The results from these analyses are shown in Column (1) of Table 8. The first subgroup we consider are higher-order births. We find that parents having their second, third, or fourth child are less likely to alter the timing of the birth in order to receive the tax benefits. This may be because higher-order births have shorter gestation, on average, and are less likely to go to post-term (42 weeks or more) than first births. This gives parents having their subsequent child less of an opportunity to change the timing of the birth while still being able to deliver at "full-term." The second subgroup we consider are those who had a repeat c-section. This group does not respond any differently than the average birth. While this may be surprising, considering our earlier finding that most of the shifting in the timing of births is driven by inevitable c-sections,

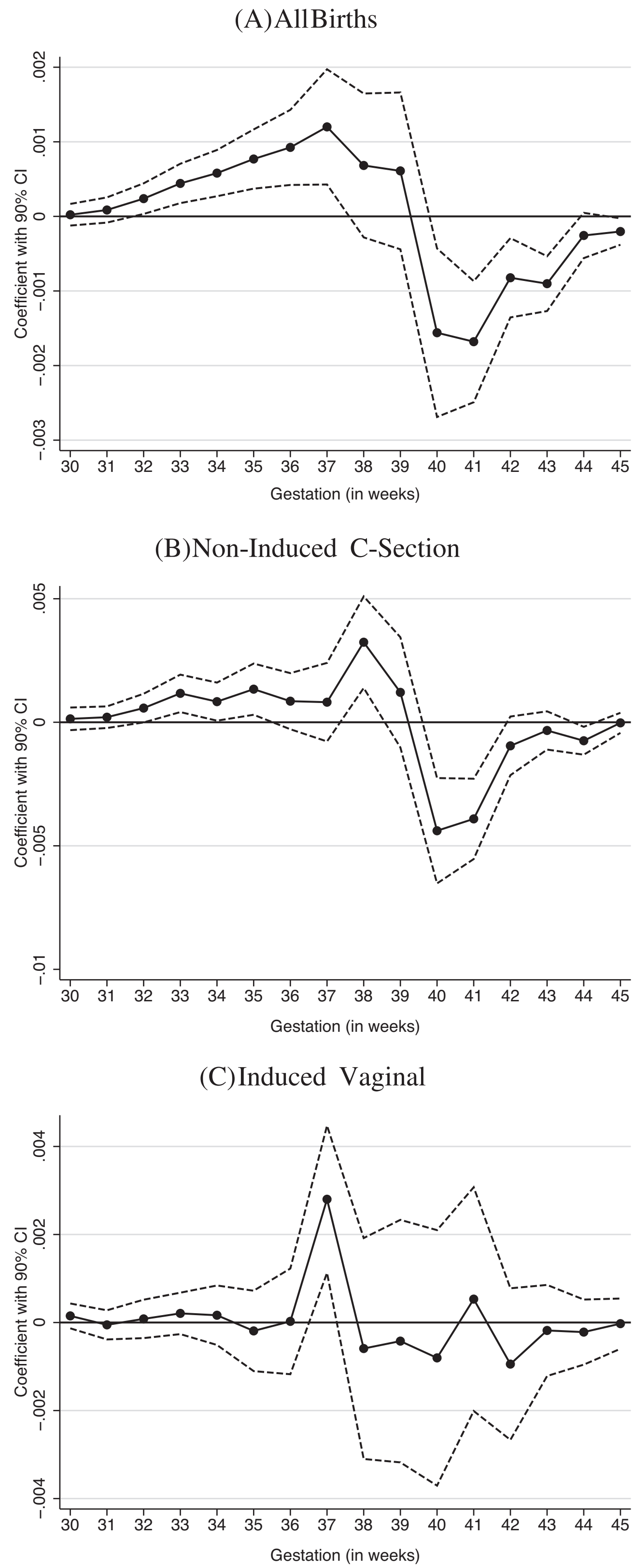

Fig. 3. Effect of tax benefit on share of December and January births at each week of gestation.

Table 8
Additional sub-groups.

	Dec Birth	Gestation	Log BW	Low BW	Apgar Score	Ventilation
Not 1st Birth	−0.0038**	−0.0970***	−0.0071***	0.0046***	−0.0124***	0.0018***
	(0.0019)	(0.0091)	(0.0008)	(0.0010)	(0.0034)	(0.0006)
Mean	0.505	38.926	8.101	0.067	8.96	0.026
Repeat C-Sec	−0.0024	−0.0472***	−0.0041***	0.0024	−0.0048	−0.0004
	(0.0029)	(0.0166)	(0.0015)	(0.0018)	(0.0060)	(0.0013)
Mean	0.517	38.695	8.114	0.063	8.968	0.034
Female	0.0006	0.0061	0.0004	0.0002	0.0013	−0.0003
	(0.0009)	(0.0079)	(0.0006)	(0.0008)	(0.0030)	(0.0005)
Mean	0.505	39.044	8.079	0.074	8.959	0.025
Weekday	0.0119**	0.0009	−0.0005	0.0004	−0.0008	−0.0003
	(0.0051)	(0.0089)	(0.0008)	(0.0010)	(0.0036)	(0.0006)
Mean	0.508	39.007	8.100	0.066	8.951	0.027
Longer Work Week	0.0025	−0.0105	0.0000	0.0001	0.0013	−0.0006
	(0.0017)	(0.0121)	(0.0008)	(0.0009)	(0.0033)	(0.0006)
Mean	0.507	38.955	8.096	0.068	8.947	0.027
Smoke or Drink	−0.0011	−0.0148	0.0000	0.0011	−0.0005	0.0003
	(0.0017)	(0.0119)	(0.0011)	(0.0013)	(0.0040)	(0.0009)
Mean	0.503	38.921	8.037	0.109	8.936	0.033
Black	0.0038***	−0.0161	−0.0016*	0.0012	−0.0067*	0.0003
	(0.0013)	(0.0110)	(0.0009)	(0.0011)	(0.0036)	(0.0006)
Mean	0.503	38.465	8.024	0.119	8.869	0.028
Less than HS	−0.0011	−0.0363***	−0.001	0.0000	−0.0023	0.0008
	(0.0015)	(0.0101)	(0.0008)	(0.0011)	(0.0037)	(0.0006)
Mean	0.505	38.99	8.079	0.076	8.945	0.022
Age 35+	0.0005	−0.0157	−0.0013	0.0031	0.0021	−0.0002
	(0.0021)	(0.0166)	(0.0015)	(0.0020)	(0.0059)	(0.0009)
Mean	0.507	38.696	8.092	0.083	8.929	0.03

Standard errors in parentheses. *Notes*: Each coefficient comes from a separate regression. All regressions include state, birth order, year and demographic group fixed effects, as well as demographic group linear time trends and income × birthorder controls. Standard errors are clustered by state-year. For rows 1–6 the 60 demographic groups are the same as in the rest of the paper, and they are defined by age, race, marital status and education. For rows 7–9, the characteristic that is being examined is excluded from the demographic group creation and is included as a separate fixed effect.
* $p<0.10$.
** $p<0.05$.
*** $p<0.01$.

since parents having their first child seem to be the most responsive to the benefits, we aren't alarmed that parents having *repeat* c-sections are not responding additionally to the incentive.

Because of the widespread use of ultrasound, parents often know the gender of the child prior to birth. Thus, we can assess whether parents behave differently depending on the gender of their child. Consistent with previous literature on gender preference in the United States, we do not find that parents having female babies are any more likely to alter the timing of the birth (Lhila and Simon, 2008). Next, we determine whether timing changes as a result of the tax benefits are more likely to happen on weekdays rather than weekends. In this case the $Group_i$ is equal to one if the birth occurs on a Monday–Friday and zero otherwise. Since doctors prefer to work on weekdays and a birth timing change requires a doctor's compliance, we expect that the timing of weekday births would be most responsive to the tax benefit. We find that this is certainly the case. All of the effect of tax benefits on birth timing operates through weekday births. The estimate of 0.0119 equates to approximately a one percentage point increase in the probability of being born in December, if the birth occurs on a weekday. Finally, we assess whether the timing response is stronger in years in which the last week of December has more work days. Depending on the year, Christmas and New Year's Day may fall on a weekend or weekday day. Thus, the last week of December may have either 4 or 5 work days. We find no differential effect on birth timing for those years.

We also look at the responsiveness of four subgroups who are more likely to have "high-risk" pregnancies (March of Dimes, 2012b): mothers who reported smoking or drinking while pregnant, Black mothers, mothers with less than high school education, and mothers aged 35 or older. We find that Black mothers are more responsive to the incentive to change the timing of their child's birth, but the other groups show no statistically significant differences.

We next want to determine whether the consequences of altering the timing of births differ across these subgroups. To do so, we again use our comparison months, October and November, and estimate the following equation:

$$\begin{aligned}\text{Health}_i = \quad & \alpha + \beta_1\text{TaxBenefit}_i \times \text{Group}_i \times \text{DJ}_i + \beta_2\text{TaxBenefit}_i \times \text{DJ}_i + \beta_3\text{Group}_i \times \text{DJ}_i + \\ & \beta_4\text{TaxBenefit}_i \times \text{Group}_i + \beta_5\text{TaxBenefit}_i + \beta_6\text{Group}_i + \beta_7\text{DJ}_i + \gamma X_i + \phi_t + \epsilon_i\end{aligned} \tag{6}$$

The coefficient on the triple interaction term, β_1, tells us whether the health of babies born to mothers in the subgroup during the treated months is especially responsive to the size of the tax benefit. We first look at gestation in Column (2) of Table 8. Estimates from this column tell us the magnitude of the timing shifts for these groups relative to the average. Both higher-order births and repeat c-sections show larger shifts in birth timing. The only other subgroup that shows a statistically significant differential response in the magnitude of the timing shift are mothers with less than high school education. The health outcomes we assess are log birthweight (shown in Column (3)), low birthweight (Column (4)), 5-min Apgar score (Column (5)), and ventilation (Column (6)). We consider the health estimates indicative of whether certain groups are more adversely affected by birth timing changes (as a result of the tax incentive).

We begin by considering higher-order births. The health of higher-order births are more adversely affected by birth timing changes on all our measures. This is likely caused by the larger timing shifts they experience, even though they are less likely to be shifted than first births. We see a similar pattern for repeat c-sections. While higher-order births are no more likely to switch from January to December as a result of the tax benefit, the magnitude of the switch is larger. A \$1000 increase in tax benefits decrease mean gestation by about 0.10 weeks. This is three times the magnitude as for the general population. While some of the loss in health is occurring because the births are moved earlier, the decrease in gestation does not explain all of it, suggesting that higher-order births are either a particularly vulnerable group or that the consequences of timing changes differ across the gestation distribution. [29] The birthweight and Apgar scores of Black newborns are also more adversely affected by birth timing changes. While mothers with less than high school education are hastening their birth timing by a larger magnitude than others, the health of their newborns are not more adversely affected by these timing changes. The infant health of the other subgroups we assess are not more adversely affected by birth timing changes than that of the average shifted newborn.

This analysis provides us with information that may help inform optimal maternity care policies. We have found that a few groups are especially vulnerable to changes in birth timing – Blacks, higher-order births, and repeat c-sections. These three subgroups have lower mean gestation, suggesting that the adverse affects of birth timing changes are exacerbated the earlier the birth occurs. All these birth timing changes are presumably occurring under the supervision of a physician. Thus, timing changes even at the end of pregnancy can have adverse effects on the health of the infant.

6.2. Isolating the variation in the tax code

Identification in our main analysis from Eq. (1) relies on all sources of variation in the tax benefits – year, income, parity, marital status, and state. As shown in Figs. 1 and 2, there are several sources of variation in tax benefits that we can leverage in order to identify how one specific source of variation affects birth timing.

First, we isolate variation in benefits over time by looking at the effect of the introduction of the Child Tax Credit in 1998. As previously discussed, the Child Tax Credit was a \$400 per child credit in tax liability. Everyone except those with the lowest and highest incomes were eligible to claim the benefit. Thus, we use a difference-in-differences approach to determine the effect of the increase in tax benefits from the Child Tax Credit. The treated group consists of those families that become eligible for the benefit in 1998, and the control group consists of families that do not benefit from its introduction. We compare outcomes in 1996, the year before the legislation was introduced, and 1998, the first year of eligibility. [30]

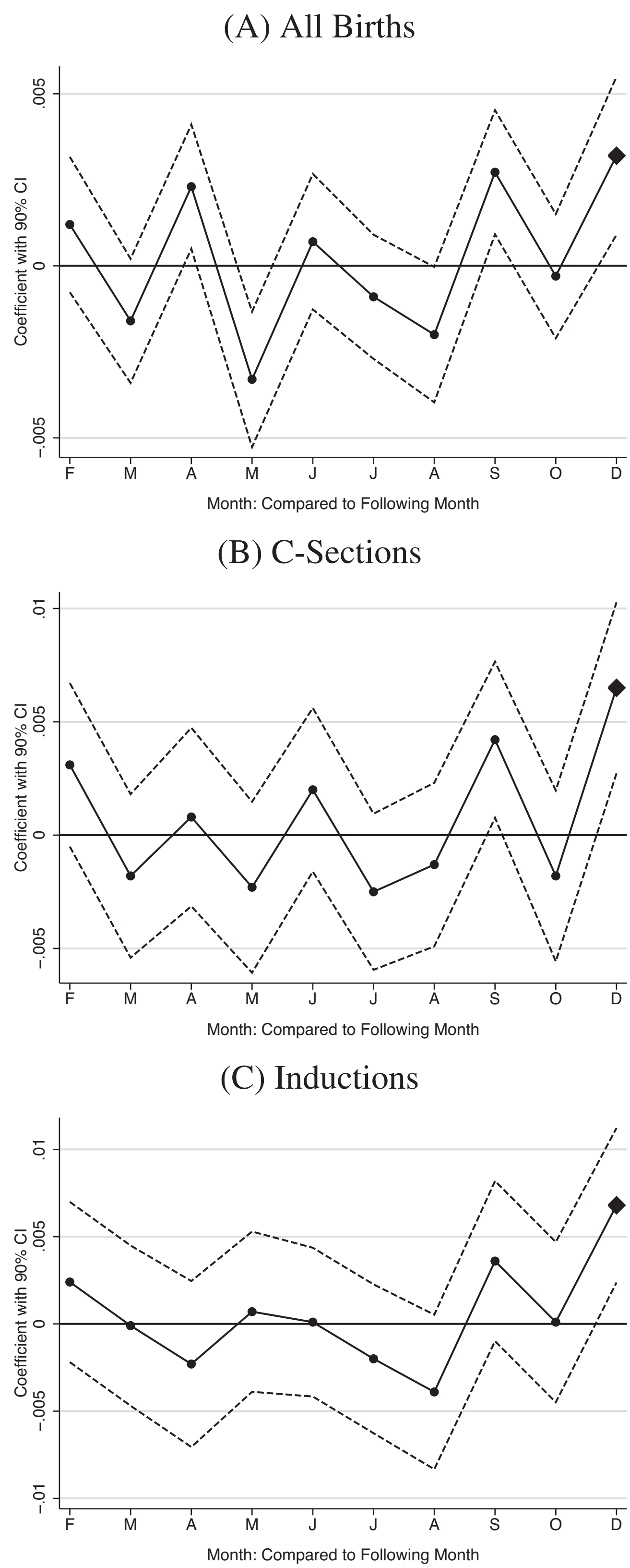

Fig. 4. Falsification test: difference in births for all month pairs.

[29] The health effects are more than 3 times as large as for the rest of the population.
[30] A full, more precise, description of the creation of the treatment and control groups is included in B.

Table 9
Isolating variation in the tax code.

(Panel A) Child tax credit

	(1) Total	(2) C-Sec	(3) Induct	(4) Log BW	(5) Low BW	(6) Apgar5	(7) Gestation
CTC × After	0.0050	0.0045	−0.0124	−0.0039	0.0024	−0.0314**	−0.0611
	(0.0036)	(0.0070)	(0.0090)	(0.0040)	(0.0036)	(0.0144)	(0.0421)
CTC × After × DJ				−0.0027	0.0039	0.0197	−0.0131
				(0.0039)	(0.0044)	(0.0143)	(0.0453)
Mean	0.253	0.256	0.257	8.101	0.064	8.952	39.009
N	873,899	198,169	171,699	873,264	873,264	700,045	868,120

(Panel B) Earned income tax credit

	(1) Total	(2) C-Sec	(3) Induct	(4) Log BW	(5) Low BW	(6) Apgar5	(7) Gestation
EITC × After	0.0101	0.0037	0.0001	−0.0077	0.0071	−0.0420	−0.1723*
	(0.0072)	(0.0146)	(0.0254)	(0.0077)	(0.0092)	(0.0265)	(0.0947)
EITC × After × DJ				0.0031	0.0014	0.0421	0.1410
				(0.0071)	(0.0092)	(0.0282)	(0.0964)
Mean	0.255	0.258	0.252	8.035	0.111	8.909	38.613
N	79,085	15,683	7305	78,976	78,976	70,772	78,717

Standard errors in parentheses. *Notes*: Regressions in Panel A include state, birth order and demographic group fixed effects, as well as income × birthorder controls. Regressions in Panel B include state and marital status fixed effects. Standard errors are clustered by state-year.
* $p < 0.10$.
** $p < 0.05$.
*** $p < 0.01$.

Panel A of Table 9 displays the results of this analysis. Row 1 displays the coefficients on the interaction of CTC, an indicator variable equal to one for families in the eligible income range, and after, an indicator variable equal to 1 in 1998. The estimation in Column (1) examines whether a December birth is more likely for those who are newly eligible for the Child Tax Credit. While the coefficient is positive, it is not statistically significant. The same is true in Column (2), when the sample is restricted to only c-sections. Column (3) shows a statistically insignificant decrease in December inductions. Row 2 in Columns (4)–(7) shows the coefficient of interest for the health outcomes, with an additional term in the interaction, DJ, which is equal to one for births in December and January and zero for births in October and November. With the exception of a positive coefficient for the 5-min Apgar score, the coefficients show the same pattern as the main results, but are not statistically different from zero.

Next, we leverage differences in benefits across birth order. As shown in Figs. 1 and 2, the 1993 EITC reform increased benefits for the second child for those with low incomes, but had no effect on the benefit (or lack thereof) for third and fourth children. Thus, we perform a differences-in-differences analysis isolating our sample to 1992 (the pre period) and 1994 (the post period) and to families with incomes that make them eligible for the EITC benefits for their second child. Mothers having their second child are the treated group and mothers having their third or fourth child are the control group. [31] In Columns (1)–(3) of Panel B of Table 9, we see a statistically insignificant increase in December births for all three categories, and in Columns (4)–(7) we see a statistically insignificant increase in all health measures. The standard errors are quite large for all of the estimates. Isolating only across birth order variation or the over-time variation in benefits (as in the Child Tax Credit regressions) limits our statistical power greatly and justifies our need to use all sources of variation in tax benefits to in our main identification strategy.

6.3. Falsification tests

To verify that the difference in births between December and January is not just spuriously correlated with tax benefits, we estimate Eq. (1) for every other month pair. For example, we look at whether the likelihood of being born in February rather than March is correlated with the child tax benefit they will receive. We assign each observation the benefit they will receive for having their child in that year. This is the same benefit that is assigned to the December/January observations from that year, except, unlike the December/January observations, these families are not on the margin of receiving the benefit. [32] Panel (A) of Fig. 4 plots the estimated effect of the tax benefit for each month pair as well as their 90% confidence interval. For reference, we include the December/January estimate – the rightmost point – in the graph as well. Two of the estimates are statistically different from zero, but the point estimates are smaller in magnitude than the December/January estimate. We next estimate whether the likelihood of having a c-section and induction in the earlier month of each of the month pairs is correlated with the tax benefit (this is our “shifting” analysis). The estimates from these regressions are plotted in Panels (B) and (C) of Fig. 2. For c-sections, only one of the month-pairs, September and October, has a statistically significant estimate. However, it is again smaller in magnitude than the December/January estimate. For inductions, none of estimates for the other month pairs is statistically different from zero.

We next estimate the health regressions for the other month pairs. As in our main analysis, we use the two months prior to each month pair as their comparison group. In order to exclude December and January from any of the comparison groups, April/May is the first month pair of the year we can assess. The plotted estimates for each of the health outcomes are shown in Fig. 5. The estimates for birthweight are statistically significant for a number of the month-pairs, but in most months are less

[31] A full, more precise, description of the creation of the treatment and control groups is included in B.

[32] We do not include January/February or November/December, as these comparisons would be contaminated by the shifting into (out of) December (January).

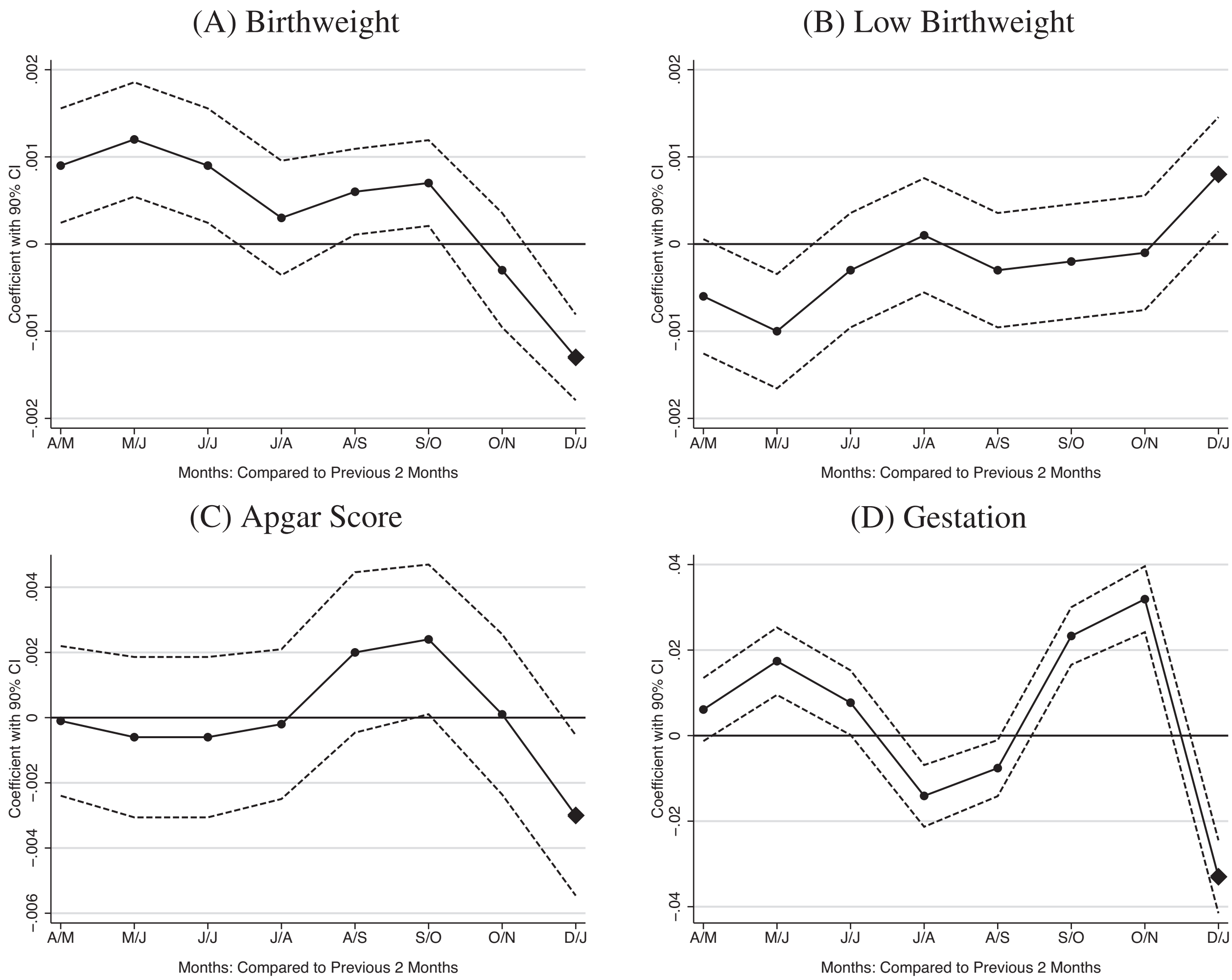

Fig. 5. Falsification test: difference in health for all month pairs.

precisely estimated then the December/January estimate. The estimates for low birthweight are not statistically significant, except for May/June, which is inconsistent with the finding for log birthweight showing that the tax benefit is correlated with *higher* birthweight for May/June babies. Additionally, Apgar score is not statistically significantly correlated with the benefits for any of the month pairs except our actual estimate for December/January. Panel (D) shows the estimates when considering gestation as the outcome. For these regressions, nearly all of the estimates are statistically significant, but not correctly signed to support the estimates in Fig. 4 Panel (A). However, these findings further contribute to our doubt about drawing any conclusions based on the gestation results. While the falsification tests are not all statistically insignificant, the inconsistency of the estimates makes us confident that our main findings are not driven by spurious correlation, but reflect the true relationship between tax benefits and birth timing changes and its health consequences.

7. Discussion

There have been a number of studies on the effects of birthweight on both short- and long-term outcomes. To better understand the full costs of birth timing changes, we assess our estimated effects of the tax benefits on birthweight in conjunction with the estimates from other studies. There are a couple caveats to these calculations. First, we are assessing the *average* effect of the tax incentive on these later-life outcomes. While we know that approximately 1 for every 155 January births is moved to December for a $1000 increase in tax benefits, we believe that there are a number of December and January births that are hastened as a result of the tax benefit but do not result in being born in a different month. Our estimates on health outcomes include the effects on all December and January births and not just those whose birth month is changed because of the incentive. Second, since the relationship between birthweight and other life outcomes is subject to a number of omitted variables, the most reliable estimates come from within-twin comparisons. While these studies eliminate much of the bias, they consider a very different sample than we do. Twins have a lower gestational age and are smaller, on average, than the births we believe are impacted by elective changes in birth time. While we do see an increase in low birthweight babies as a result of the tax incentive, on average, there is positive selection into which babies' births are shifted as a result of the tax benefits. Hence, we are estimating the effects on a population of newborns who may be larger and healthier than those from the twins-based studies.

One way of assessing this potential difference in the vulnerability of the births in our samples is by comparing the estimated effect of birth timing on the 5-min Apgar score, a common outcome measure among our studies. We estimate that a 1 g decrease in birthweight corresponds to a 0.0007 point decrease in the Apgar score. For the twins samples in both the Almond et al. (2005) and Black et al. (2007) papers, a 1 g decrease in birthweight corresponds to approximately a 0.015 decrease in Apgar score. This discrepancy in the magnitudes of our estimates suggests that Apgar score is not as strongly affected by birthweight changes for our sample of likely larger and healthier newborns. Our sub-analysis of higher-order births in Table 8 supports this assertion. higher-order births have a lower birthweight than first births and their health is more adversely affected by birth timing changes than the average shifted birth. [33] This brings to light the fact that the relationship between birthweight and Apgar score may differ greatly across the birthweight distribution.

Keeping these important caveat in mind, we continue by assessing the relationship between birthweight and hospital costs. Using twin fixed-effects, Almond et al. (2005) estimate that a 1 g increase in birthweight decreases hospital costs by $4.93. Thus, the 95% confidence interval of our estimates on log birthweight from Table 5 would suggest that a $1000 increase in the tax benefit would increase hospital costs between $11.59 and $31.40 per child, on average, for every child born in December or January. With approximately 480,000 babies born each December and January, a $1000 increase in tax benefits per family would roughly lead to a $10 million increase in overall hospital costs each year.

Also using a population of twins, Figlio et al. (2013) use administrative data from the state of Florida linking Vital Statistics records to standardized test scores to estimate the effect of birthweight on academic outcomes. They estimate that a 10% increase in birthweight corresponds to a 0.045 standard deviation increase in reading and math test scores in third through eighth grade. Using this estimate, we would conclude that a $1000 increase in tax benefits leads to a 0.006 standard deviation decrease in standardized test scores, on average, for children born in December or January. Black et al. (2007) estimate the relationship between birthweight and longer-term outcomes using a sample of twins and rich administrative data from Norway. Consistent with other studies on the consequences of birthweight, they find that relative to their counterparts, low birthweight infants tend have lower IQ, lower educational attainment, poorer self-reported health status, lower earnings as adults, and lower birthweight children (Behrman and Rosenzweig, 2004; Currie and Hyson, 1999; Black et al., 2007). Interpreting our estimates with that from Black et al. (2007), we find that a $1000 increase in tax benefits will decrease the average height of those born in December/January by 0.041 cm, will increase their Body Mass Index [34] by 0.0015, will decrease their IQ by 0.0008 stanines, and will decrease their earnings by 0.02%.

In interpreting the magnitude of these effects, there are two points we encourage the reader to keep in mind. First, work by Almond and Mazumder (2013) suggests that parental investments in their children reinforce initial endowment differences. While many of the twin fixed-effect analyses differ from their OLS counterparts in initial health outcomes, they are actually quite similar when assessing longer-term outcomes. If this is the case, many of the longer-term effects of birthweight that have been estimated are biased by differential parental investment and overestimate of the causal relationship between birthweight and later-life well-being. Second, we estimate the average effects for the entire population of December/January babies subject to the tax benefit. While we expect that the birth timing of more than just the few births we estimated to be shifted from January to December are impacted by the benefit increase, these effects are driven by a relatively small number of births. That is, while the average effects may be quite small, the effects for the treated are likely quite sizable.

8. Conclusion

Altering the timing of births for convenience has become an increasingly common practice in the United States and around the world. The consequences of these actions were previously unknown, however. The U.S. income tax system is structured in such a way that it gives women who are due to deliver in the beginning of January a financial incentive to give birth in December instead. This gives us a unique natural experiment with which to estimate how elective birth timing changes affect the wellbeing of newborns.

We verify that expectant mothers are in fact manipulating the timing of their births in order to gain additional tax benefits, and that this increase in December births is mostly driven by women who shift their January c-sections and inductions to December. Roughly 1 of every 75 January c-sections are moved to December for every $1000 in additional tax benefits while 1 of every 72 January inductions are moved to December for the same increase. These results support previous findings that parents respond to financial incentives when planning the birth of their child, although the magnitudes of our estimated effects are much smaller. There is no evidence that parents switch their method of delivery in order to guarantee a December birth and receipt of the benefits. These findings have allowed us to estimate the health effects of changing the timing of birth, holding fixed the method of delivery.

Our health results indicate that this type of medical intervention has consequences for infant health: an increase in tax benefits, and, therefore an increased probability of altering the timing of birth, is associated with lower birthweight for the infant and a higher probability that the infant will be low birthweight. Apgar scores are also negatively affected by changes in the timing of births. The health effects are mostly driven by non-induced c-section births. Our gestation analysis suggests that the infants being shifted are those who would have otherwise been born at 40 weeks of gestation or later, but are born at 37–39 weeks of gestation instead. While still considered full-term, these infants still experience negative consequences of small changes in the timing of their births. As we compare our findings with those from previous studies on the short-term health effects of birthweight, we find that the relationship between birthweight and the 5-min Apgar score is less pronounced for the presumably larger and heavier infants whose birth timing is changed in order to receive the tax benefit. Findings from our subgroup analysis suggests that the adverse affects of birth timing changes are exacerbated the earlier the birth occurs.

Over time, both patients and doctors have experienced increasing discretion with respect to when a baby is born. Because even small reductions in gestation length can have negative, long-term effects on the newborn, the cost of timing births for convenience or financial gain may be far greater than the benefit. Since the consequences of birth timing manipulation were previously unknown, doctors and parents may have felt comfortable making adjustments to the delivery date, especially when done at full-term and when there were personal gains to doing so. Our findings shed light on the fact that these changes are not without consequence, and there should be increased scrutiny within the medical community regarding elective birth timing prior the baby's natural arrival. As the prevalence of c-sections and inductions continues to rise, an important next step in this line of research will be to determine whether changing the method of delivery, especially for non-medical reasons, has any effect on infant health.

[33] The mean for first births is 3351 g and 2989 g for second–fourth births.

[34] Body mass index is calculated by dividing weight in kilograms by height (in meters) squared.

Table A.10
Health results: December vs. January.

	(1) Log BW	(2) Low BW (<2500 g)	(3) Apgar1	(4) Apgar5	(5) Apgar5 ≥7	(6) Any vent.	(7) Vent. <30 m	Vent. >30 m
TaxBen × Dec Birth	0.0005	0.0003	0.0031	0.0012	−0.0002	0.0004	−0.0000	0.0004
	(0.0005)	(0.0006)	(0.0065)	(0.0021)	(0.0003)	(0.0004)	(0.0003)	(0.0002)
Dec Birth	−0.0005	−0.0005	0.0084*	0.0020	0.0003	−0.0006*	−0.0001	−0.0005***
	(0.0005)	(0.0005)	(0.0043)	(0.0019)	(0.0003)	(0.0003)	(0.0003)	(0.0002)
TaxBen (000s)	0.0010	−0.0008	−0.0052	0.0003	−0.0003	−0.0004	−0.0003	−0.0001
	(0.0008)	(0.0008)	(0.0085)	(0.0027)	(0.0004)	(0.0005)	(0.0005)	(0.0003)
Mean	8.096	0.068	8.078	8.948	0.987	0.027	0.019	0.008
N	4,812,365	4,812,365	1,531,961	3,740,278	3,740,278	4,525,480	4,525,480	4,525,480

Standard errors in parentheses. *Notes*: All regressions include state, birth order, year and demographic group fixed effects, as well as demographic group linear time trends and income × birthorder controls. Standard errors are clustered by state-year.
* $p < 0.10$.
** $p < 0.05$.
*** $p < 0.01$.

Table A.11
Shifting and switching births (35+ weeks gestation).

(Panel A) Shifting

	(1) All	(2) C-section	(3) Vaginal	(4) Induction	(5) C-section Non-induced	(6) C-section Induced	(7) Vaginal Non-induced	(8) Vaginal Induced
TaxBen (000s)	0.0034**	0.0073***	0.0025	0.0071**	0.0061**	0.0085	0.0020	0.0055*
	(0.0014)	(0.0025)	(0.0015)	(0.0028)	(0.0027)	(0.0060)	(0.0016)	(0.0030)
Mean	0.505	0.511	0.503	0.512	0.511	0.508	0.501	0.513
N	4,570,839	1,091,779	3,479,060	767,355	909,745	145,723	2,841,429	621,632

(Panel B) Switching

	(1) C-section	(2) Vaginal	(3) Induction	(4) C-section Non-induced	(5) C-section Induced	(6) Vaginal Non-induced	(7) Vaginal Induced
TaxBen × DJ Birth	−0.0008	0.0007	0.0006	−0.0013	0.0001	0.0003	0.0004
	(0.0013)	(0.0007)	(0.0007)	(0.0008)	(0.0003)	(0.0008)	(0.0006)
TaxBen (000s)	0.0024	−0.0018	−0.0034***	0.0054***	−0.0034***	−0.0018	0.0000
	(0.0015)	(0.0013)	(0.0012)	(0.0013)	(0.0006)	(0.0017)	(0.0010)
DJ Birth	0.0008	−0.0007	0.0014**	0.0016*	−0.0003	−0.0024***	0.0017***
	(0.0017)	(0.0007)	(0.0007)	(0.0010)	(0.0003)	(0.0008)	(0.0006)
Mean	0.239	0.770	0.169	0.201	0.032	0.633	0.137
N	9,187,161	9,080,800	9,080,637	9,080,637	908,0637	9,036,988	9,036,988

Standard errors in parentheses. *Notes*: All regressions include state, birth order, year and demographic group fixed effects, as well as demographic group linear time trends and income × birthorder controls. Standard errors are clustered by state-year.
* $p < 0.10$.
** $p < 0.05$.
*** $p < 0.01$.

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Appendix A. Additional tables

See Tables A.10–A.12

Appendix B. Construction of groups for child tax credit and EITC analysis

The treatment and control groups for the CTC regressions are created according to the income ranges displayed in Table B.13. In each case, the treated group consists of those families that are eligible for the full CTC when having a child at the parity listed in the first column of that row. Those who are not eligible are included in the control group. Those who fall in either the phase in or phase

Table A.12
Health outcomes joint test.

# Restrictions	# Reps	% Reps
0	50	25.0
1	82	41.0
2	50	25.0
3	12	6.0
4	4	2.0
5	1	0.5
6	1	0.5
7	0	0.0

This table gives the number and percent of bootstrap replications where 0–7 restrictions held. When <7 restrictions hold (as described in footnote 28), we can reject the null hypothesis that TaxBen × DJ has a *positive* effect on *all* health outcomes. When 0 restrictions hold, we can reject the null hypothesis that TaxBen × DJ has a positive effect on *any* health outcome.

Table B.13
Child tax credit.

	Single		Married	
Parity	Treat	Control	Treat	Control
1st	–[a]	–[a]	28–116	>125
2nd	31–88	>109	32–122	>133
3rd	19–96	<20, >111	26–125	<18, >141
4th	21–105	<24, >114	25–128	<21, >150

All values are in thousands of dollars, indexed to the year 2000.

[a] Single mothers having their first child are not included because there is no clear range where the child tax credit matters but the EITC does not.

Table B.14
Earned income tax credit.

	Single		Married	
Parity	Treat	Control	Treat	Control
1st	–	–	–	–
2nd	<12	–	<16	–
3rd	–	<12	–	<16
4th	–	<12	–	<16

out of the CTC are not included in either group. Similarly, those who are eligible for the EITC are also excluded.

The treatment and control groups for the EITC regressions are created according to the income ranges displayed in Table B.14. The treatment group consists of those families that are newly eligible for the EITC for their second child after the 1997 reform. The control group consists of those families who give birth to their 3rd or 4th child, and are not eligible for additional EITC benefits even after the reform.

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