# Born on the First of July: An (Un)natural Experiment in Birth Timing[*]

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## Abstract

It is well understood that government policies can distort behavior. But what is less often recognized is the anticipated introduction of a policy can introduce its own distortions. We study one such "introduction effect", using evidence from a unique policy change in Australia. In 2004, the Australian government announced that children born on or after July 1, 2004 would receive a $3000 "Baby Bonus." Although the policy was only announced seven weeks before its introduction, parents appear to have behaved strategically in order to receive the benefit, with the number of births dipping sharply before the policy commenced. On July 1, 2004, more Australian children were born than on any other single date in the past thirty years. We estimate that over 1000 births were "moved" so as to ensure that their parents were eligible for the Baby Bonus, with about one quarter being moved by more than one week. Most of the effect was due to changes in the timing of inducement and cesarean section procedures. We find evidence to suggest that babies who were shifted into the eligibility period were more likely to be of high birth weight. Two years later, on July 1, 2006, the Baby Bonus was increased, and we find that this again caused births to be moved from June to July. These birth timing events represent an opportunity for health researchers to study the impact of planned birthdays and hospital management issues.

**JEL Codes:** H31, J13
**Keywords:** introduction effect, timing of births, policy distortion

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> *TIM LESTER: Minister, with the benefit of hindsight, would it have been better to have announced and introduced this policy on the same day?*
>
> *KAY PATTERSON: This policy is a bonus to families.*
>
> *TIM LESTER: That doesn't answer my question, though, with respect, Minister.*
>
> *Would it have been better to have announced and introduced this policy at the same time?*
>
> *KAY PATTERSON: I believe this is a fantastic policy for mothers, they're going to get $3,000 --*
>
> *TIM LESTER: Minister, that still doesn't answer my question with respect.*
>
> *Would it have been better to have announced and introduced this policy at the same time?*
>
> *KAY PATTERSON: Well if I thought that mothers would put their babies at risk, but I don't believe mothers would put them at risk.*[1]

## 1. Introduction

Economists have long been concerned that seemingly subtle issues of policy announcements and the timing of policy shifts can have dramatic short-term effects on individual behavior. Consider a situation where a government announces a new tax benefit or a subsidy that will be introduced at some later date. It can be expected that – to the extent that they have some discretion as to the timing of their decisions – individuals will shift their actions around the policy introduction date to take advantage of a benefit or avoid a detriment. Just as the existence of government policies can distort behavior, so too policy *changes* can further distort individuals' decisions. Such an effect might be termed an *introduction effect*.

Frequently, the announcement that a new tax or subsidy will take effect upon a certain date will lead to news reports of individuals or firms rearranging their affairs so as

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[1] From the 7:30 Report (ABC Television, July 1, 2004); Kay Patterson was the Minister for Health in the Australian Government.

to take advantage of the policy change. However, it is rare to find an opportunity in which the effect of such policy changes can be quantitatively estimated.[2]

In this paper, we identify a policy change that created the potential for an introduction effect. On May 11, 2004, the Australian government announced that it would give a $3,000 Maternity Payment, commonly referred to as a 'Baby Bonus' to each family of a new born child.[3] For our purposes, the important feature of the Baby Bonus policy was that it was to only apply for babies born on or after July 1, 2004. Thus, a household with a child born at 11:59pm on the June 30, 2004, would receive no payment while one with a child born at 12:01am on the July 1, 2004, and thereafter would receive $3,000.[4] This creates the potential for an introduction effect especially given the lag between the policy announcement and the introduction date.

The seven week lag between the policy announcement and its introduction would not have an impact on the number of pregnancies as those affected by the introduction were already in the process; the third trimester, in fact.[5] However, it could have an impact on either (i) discretionary and planned birth timing decisions (such as inducements and elective caesarians) and/or (ii) the reporting of birth days. In each case, the impact on reported birth timing would be jointly determined by the parents and hospital involved.[6]

We obtained daily data on all recorded births in Australia from 1975 to 2004. Even without any statistical analysis, the effect appears dramatic. On June 30, 2004 (a Wednesday), 500 babies were registered as having been born, a birthrate at the 11th percentile of the births distribution over the entire period 1975-2004 (and the 1st percentile of the births distribution on weekdays).[7] On July 1, 2004, 1005 babies were born. This was the *highest number of births* recorded throughout the 10,958-day period 1975-2004. The fifth-highest number of births on a single day was July 2, 2004 with 928.

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[2] Exceptions to this are Kopczuk and Slemrod (2003) and Gans and Leigh (2006), who find that changes in the estate tax rate in the US and Australia had an impact on the number of reported deaths.

[3] All figures are in Australian dollars. In 2004, A$1=US$0.75 (approx).

[4] Precisely 12:00am was apparently a 'grey' area (*Sunday Telegraph*, June 27, 2004, p.5). Similar issues would apply for July 1, 2006 and July 1, 2008 when the bonus increased to $4,000 and $5,000 respectively. However, the potential distortions created by these changes are smaller, and births data for these years are not yet available.

[5] Including the third child of the first author; eventually born on July 25, 2004.

[6] In the case of home births, it would be the mid-wife. However, we do not separately identify these in our data.

[7] There were some births reported close to midnight: ABC Radio (July 1, 2004) reported a birth at 11:53pm.

With a flexible functional form, allowing for separate day-of-year, day-of-week and year effects, we find that the introduction of the Baby Bonus was associated with a substantial increase in the birth rate. Our results are highly statistically significant, as well as being economically significant. Over the window covering 28 days before and 28 days after the policy was introduced, we estimate that over 1000 births were moved into the eligibility range. Data from inducement and cesarean section procedures indicate that these accounted for most of the shift. Analyzing a subsample of birth records, we find that babies born in early-July were significantly heavier than those born in late-June, which would be consistent with parents delaying births to obtain the payment. All of this provides an indication that shifting was, in fact, real and not a result of reporting issues or fraud.

As well as providing an experiment that identifies an introduction effect, our result here is also a contribution to our understanding of the short-run drivers of the timing of births. Chandra et al (2004) demonstrated that birth numbers fall on weekends as compared with weekdays, and that this effect has grown over time. They also found that less risky births were more likely to be moved. A very similar trend away from weekend births has also occurred in Australia (Gans and Leigh, 2007). Indeed, as we discuss below, it is important that we take account of day-of-week effects when estimating the true magnitude of the introduction effect.

In relation to the impact of government policies, Dickert-Conlin and Chandra (1999) demonstrated that tax incentives in the US caused births to be shifted from the first week of January each year to the last week of December. They estimated that increasing the tax benefit of having a child by $500 raised the probability of an end of December birth by 26.9 percent.[8] Our paper offers a distinct insight into birth timing and incentives. First, the government policy created an incentive to *delay* the birth as opposed to bringing it forward. As babies are often born prematurely, there is intrinsically less control in this decision than a decision for an earlier birth. Second, the US tax incentive exists in every year, allowing hospitals to allocate additional resources to take account of

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[8] Milligan (2005) studied the introduction and de-introduction of a ‘baby bonus’ (or Allowance for Newborn Children) in Quebec, Canada. However, in that case, the policy commenced on the day of its announcement, so there was no potential for an introduction effect. In 2006, the German and Singaporean governments both announced that they planned to offer payments to new parents, potentially creating the opportunity for further investigation of introduction effects.

it. The introduction of the Baby Bonus in Australia had a *one-off effect*, and so likely created unplanned resource utilization problems. In this situation, one would expect hospitals and doctors to have been more constrained in their ability to respond to parental requests to adjust the timing of the birth so that they were eligible for the Baby Bonus. Our observed impacts are, therefore, informative as to the degree of power that patients have over the timing of births.[9]

While the Australian Baby Bonus represents a clean experiment in analyzing the introduction effect, there is some question whether what mattered was an increase in real incentives provided by the Baby Bonus or a reaction to a clearer and more transparent set of incentives. As we describe below, the Baby Bonus replaced an earlier child birth incentive. In contrast to the new Baby Bonus, however, this earlier benefit was income-related and so varied considerably depending on individual circumstance. Also, it was a benefit realized with some considerable delay whereas the Baby Bonus cash payment was immediate.[10] Given its relative complexity, it is unclear whether the earlier policy was recognized as an incentive at all.[11]

The paper proceeds as follows. In the next section, we describe in more detail the Australian Baby Bonus and its predecessor. Section 3 estimates the total number of births that were moved. Section 4 decomposes the effect across birth procedures. Section 5 analyses the characteristics of those babies that were born, and their parents. Section 6 presents the results of a subsequent increase in the Baby Bonus, which occurred in 2006. The final section concludes.

## 2. The Australian Baby Bonus

From July 1, 2004, the Baby Bonus operated very simply. If a family gave birth to a child after this date, they automatically received a cash payment of $3,000. The payment would be untaxed, and was to be given to all parents regardless of income. For the median household, this amount was worth 5.4 percent of annual disposable income

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[9] As Chandra et.al. (2004) noted, it is unclear whether the weekend/public holiday decline is driven by doctor/hospital or patient preferences. This is not the case for changes in timing based on the Baby Bonus.

[10] The first author received his $3000 payment via direct debit in 5 weeks.

[11] Again the first author's experience is instructive here. Prior to the introduction of the new Baby Bonus, his household was blissfully unaware of any financial benefit to having a child.

(equivalent to 2.8 weeks' of post-tax income).[12] The payment would be per baby and so for the multiple births the payment would be $6,000 for twins or $9,000 for triplets.

The policy was announced as part of the 2004-2005 Budget. It was not anticipated and can, therefore, be taken as essentially unknown prior to May 11, 2004.[13] The stated goal of the policy was "to provide further help at the crucial period around the birth of a child" (Australian Treasury 2004, 33) (though with Australian voters due to go to the polls in late-2004, purely political motives cannot be ruled out). There was considerable publicity about the introduction of the policy when it was announced, and further coverage in June 2004.[14] This included a media discussion as to whether the policy might encourage teenage pregnancy and suggestions that teenagers might in future be excluded from receiving the bonus.[15]

The Baby Bonus we described here replaced a previous policy, which operated as a refundable tax offset.[16] That policy worked as follows: for each baby born between July 1, 2001 and June 30, 2004, the primary carer of the child (that is, the parent staying at home) was eligible to claim for a bonus.[17] The bonus depended on the income the primary carer earned in the tax year (in Australia from July to June) that the child was born. If the primary carer's income was $25,000 or less, the taxpayer would receive $500 per year.[18] If the income exceeded this amount, then the taxpayer would only receive a payment if their income fell. In this case, the benefit could potentially be quite large. The size of the payment depended on the taxpayer's income level prior to having a baby

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[12] Authors' calculations, based on the Household, Income and Labour Dynamics in Australia survey (HILDA).

[13] A media search in the weeks prior to the budget reveals that the government made it known to some journalists in early May that the Baby Bonus would probably be revamped in the budget. However, details were not provided at the time, so the changes remained speculative.

[14] For example, much coverage was given to the Treasurer's suggestion to the media on May 11, 2004 that Australian parents should have "one for mom, one for dad, and one for the country.". A search of the *Factiva* database for the terms "Baby Bonus" and "Maternity Payment" returns 411 articles published in Australian newspapers during May and June 2004.

[15] On the furor over teens, see for example Arndt (2004); Grattan and Nguyen (2004). On 1 January 2007, the Australian government changed the rules so that parents aged 17 years or under would receive the Baby Bonus in 13 fortnightly installments, unless special circumstances existed to warrant the payment of a lump sum.

[16] The new Baby Bonus also replaced the Maternity Allowance, a means-tested payment to those families receiving Family Tax Benefit A. At the time of the change, the Maternity Allowance was worth $842.64 per child for eligible families.

[17] The rebate was also available to parents who gained legal responsibility for a child under 5 (eg. adoptive parents).

[18] The first year amount would be reduced to take into account the birth date of the child.

($Inc_0$), their average tax rate in that year ($\tau_0$), and an income level for in year $t$ (where $t \leq 5$). The yearly bonus in year $t$ was then calculated as follows:[19]

$$\text{Yearly Bonus}_t = \left(1 - \frac{Inc_t}{Inc_0}\right) \frac{\text{Min}\left(\tau_0 Inc_0, \$12500\right)}{5}$$

Thus, carers earning high salaries (in particular, those paying a lot of tax) just prior to having a baby and then earning very little thereafter would receive the maximum payment of $2,500 per year.[20] The new Baby Bonus announced in 2004 was anticipated to be considerably more expensive to the government than the previous scheme. In its last year of operation (tax year 2003-04), expenditures on the old child payment were $150 million (ATO 2004, 368).[21] In its first year of operation, the new Baby Bonus was expected to cost the federal government $741 million (Australian Treasury 2004, 37).[22]

Nonetheless, there may exist households for which there were incentives to time births prior to July 1, 2004 rather than on or after that date. For other households, the difference between the old payment may have been less than $3000. To the extent that this is the case, any introduction effect from the new Baby Bonus will understate the impact of financial incentives on birth timing.

## 3. Baby Bump?

To test the impact of the Baby Bonus on recorded births, we use daily data on the number of Australian births. There are two main sources of births data – figures collected by state and territory births registries, compiled by the Australian Bureau of Statistics (ABS); and data collected from hospitals, compiled by the Australian Institute of Health

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[19] There were adjustments to this based on part-years for the beginning and end of the first 5 years. The amount was payable following the lodgment of an income tax return with a minimum time to refund of 3 to 15 months depending upon when in the year the child was born.

[20] As Leigh and Wolfers (2002) note, the old Baby Bonus had potentially undesirable incentive properties including: (i) incentives for women to work long hours during pregnancy; (ii) incentives to concentrate all births in a 5 or so year window; and (iii) strong disincentives to return to work within 5 years of having a child. These properties were not a feature of the new Baby Bonus.

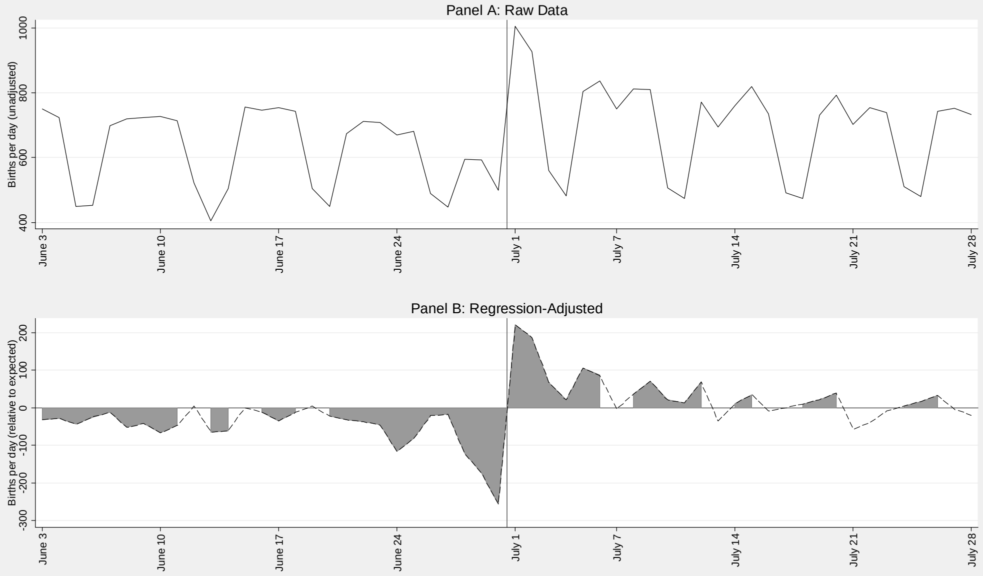
[21] Since payments under the old birth payment can continue for up to 5 years after the birth of the child, the old program will continue until tax year 2008-09. This makes it problematic to use tax office data to calculate total payments under the new Baby Bonus, since the tax office does not separately tabulate the cost of the two programs.

[22] Of course, any introduction effect would have increased the cost of the new Baby Bonus, and reduced the cost of the old Baby Bonus. We know of no evidence to suggest that the introduction effect was taken into account in the Australian Treasury's projections of the first year cost of the Baby Bonus.

and Welfare (AIHW). Each source has its limitations: unregistered births do not appear in the ABS data, while home births do not appear in the AIHW data. By using both data sources, we hope to circumvent the problems that would arise from only analyzing one or the other.

Since the ABS births data covers a longer time span, our central analysis uses these data. They include all recorded births from 1975-2004. While the birth rate in Australia has declined over this period, the number of births has remained relatively constant (there were 232,682 births in 1975, and 245,143 births in 2004). We, therefore, opt to focus on the number of births, rather than on the birth rate. This has the added advantage that we do not introduce noise into our series through mis-measurement of the total population, which is only available on a monthly basis.

Figure 1: The Introduction Effect in 2004



Panel A shows daily birth counts. Panel B shows births relative to expected, accounting for year, day of week, day of year, and public holidays.
In Panel B, shading shows days of unusually low births in June and unusually high births in July

Figure 1 focuses on the period from June 3 to July 28, being the last 28 days prior to the policy change, and the first 28 days from the policy change onwards. In Panel A, we simply show the raw data. We observe a trough in late-June, and a peak in early-July. An intuitive way of looking at the data is simply to calculate the number of births in the last seven days of June and the first seven days of July (this effectively averages out the day of week effect). In 2004, 3978 babies were born in the last week of June, and 5367 in the first week of July.

As the data from Panel A of Figure 1 indicate, there is a strong weekly cycle in the number of babies born. From 1975-2004, an average of 733 children were born on weekdays and 519 on weekends. This makes it harder to discern the impact of the introduction of the Baby Bonus on July 1, 2004 (which was a Thursday).

To purge the day of the week effect, we therefore adjust the series for day of week, day of year, holiday, and year effects. We do this by estimating the following regression, using all data *except* June and July 2004:

$$Births_i = I_i^{Year} \times I_i^{Day\ of\ Week} + I_i^{Day\ of\ Year} + I_i^{Public\ Holiday} + \varepsilon_i \tag{1}$$

In (1), the dependent variable is the number of babies born on day $i$. This is expressed as a function of indicators for the year interacted with the day of the week (eg. allowing for a separate effect for Thursdays in 2004), for the day of the year (eg. allowing for a separate effect on July 1), and an indicator for public holidays (which do not always fall on the same day of the week or day of the year).[23] For simplicity, parameters are omitted.

We then use this regression to make an out-of-sample prediction of the daily birth count ($\widehat{Births}$) for June and July 2004. Panel B shows the difference between this predicted birth count and the observed birth count ($Births - \widehat{Births}$). In the month before the policy change, births were well below the level that would have been expected, while in the month afterwards, births were well above the expected level.

To formally test the effect of the Baby Bonus on the number of births, we estimate the regressions:

$$Births_i = I_i^{Baby\ Bonus} + I_i^{Year} \times I_i^{Day\ of\ Week} + I_i^{Day\ of\ Year} + I_i^{Public\ Holiday} + \varepsilon_i \tag{2}$$

$$\ln(Births_i) = I_i^{Baby\ Bonus} + I_i^{Year} \times I_i^{Day\ of\ Week} + I_i^{Day\ of\ Year} + I_i^{Public\ Holiday} + \varepsilon_i \tag{3}$$

In (2) and (3), the dependent variables are the daily birth count and the log of the daily birth count, respectively. The indicator variable $I^{BabyBonus}$ denotes dates after which the Baby Bonus took effect. The other variables are as defined above.

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[23] We include all Australia-wide public holidays, plus the Queen's Birthday holiday, which is celebrated on the second Monday in June in all states and territories except Western Australia.

To see the effect of the Baby Bonus on the timing of births, we progressively widen the window of analysis. The first column of Table 1 restricts the sample to the last 7 days of June and the first 7 days of July, the second column to the last 14 days of June and the first 14 days of July, and so on.[24]

**Table 1: Birth Rate Effects**

| | **(1)** | **(2)** | **(3)** | **(4)** |
|---|---|---|---|---|
| **Window** | **±7 days** | **±14 days** | **±21 days** | **±28 days** |
| **Panel A: Dependent variable is number of births** | | | | |
| Baby Bonus | 210.507*** | 131.382*** | 101.624*** | 83.602*** |
| | [15.911] | [11.626] | [9.579] | [8.386] |
| Observations | 420 | 840 | 1260 | 1680 |
| R-squared | 0.97 | 0.94 | 0.94 | 0.93 |
| *Number of births moved* | *737* | *920* | *1067* | *1170* |
| **Panel B: Dependent variable is ln(number of births)** | | | | |
| Baby Bonus | 0.300*** | 0.187*** | 0.147*** | 0.123*** |
| | [0.023] | [0.017] | [0.014] | [0.013] |
| Observations | 420 | 840 | 1260 | 1680 |
| R-squared | 0.97 | 0.95 | 0.94 | 0.94 |
| *Share of births moved* | *16%* | *10%* | *8%* | *6%* |

Notes: Standard errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. Sample is daily births within the relevant window from 1975-2004. All specifications include day of year, public holiday, and year×day of week fixed effects. *Window* denotes the number of days before and after the start of July. For example, the ±7 day window covers the last seven days of June and the first seven days of July. *Number of births moved* is $W\beta/2$, where $W$ is the number of days in the window. *Share of births moved* is $\exp(\beta/2)-1$.

In Panel A of Table 1, we use the number of births as the dependent variable. Comparing the first week of July and the last week of June (column 1), the Baby Bonus coefficient is 211. Comparing the first fortnight of July and the last fortnight of June (column 2), the Baby Bonus coefficient is 131 births per day. With a three week window, the coefficient falls to 102, and to 84 with a four week window. In the last row of the panel, we estimate the number of births moved from June to July in each of these windows. As we note above, the policy can only have moved births, and cannot have

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[24] Widening the window has two purposes. First, it allows for births to have been moved by more than one week. Second, it accounts for the possibility that some parents may have attempted to delay their child's birth until July, but instead only moved the birth date from mid-June to late-June. Such 'unsuccessful moves' would attenuate the estimates derived from focusing on a narrow window.

affected conceptions in June and July (since it was only announced in May). Since a birth that is moved from June to July will reduce the number of June births by 1, and increase the number of July births by 1, we must calculate the total number of births moved by dividing the Baby Bonus coefficient by 2, and then multiplying it by the number of days in the window. Comparing the 28 days before and after the policy was introduced, we estimate that 1170 births were moved (28×83.602/2=1170).

In Panel B of Table 1, we use the log of the number of births as the dependent variable, with similar results. Again, because a birth that is moved from June to July decreases pre-period births and increases post-period births, we divide the coefficient by two before converting from log points to percentage points. With a seven-day window, 16 percent of births were shifted into the eligibility period. With a 28-day window, we find that 6 percent of the babies who would have been born in June were shifted to July. All the estimates in Table 1 are statistically significant at the 1 percent level.

The 'Baby Bonus effect' was clearly concentrated in the week before and after the policy came into effect; as can be seen from the fact that the coefficients in Table 1 decline as we widen the window of analysis. From a health standpoint, it is reassuring to see that most births were moved by a relatively short period. But were some births moved a greater distance? To check this, we re-estimated (2) and (3), replacing the indicator variable for the Baby Bonus with eight indicator variables, denoting the last four weeks of June and the first four weeks of July. (So as to allow us to also include the year×day of week fixed effects, we widen the window to ±56 days.[25])

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[25] Our results are not greatly affected by the width of this window. For example, if we use a window of ±112 days, the eight coefficients in Panel A would be (in order): -30, -31, -22, -109, 101, 30, 9, 1.

**Table 2: Birth Rate Effects – Medium Run**

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| **Panel A: Dependent variable is number of births** | | | | |
| | | **Total births moved** | | |
| | | **±14 days** | **±21 days** | **±28 days** |
| **Before** | | | | |
| June 3-9, 2004 | -22.036* | | | -1172 |
| | [11.908] | | | |
| June 10-16, 2004 | -26.830** | | -1017 | |
| | [11.908] | | | |
| June 17-23, 2004 | -15.884 | -830 | | |
| | [11.905] | | | |
| June 24-30, 2004 | -102.632*** | | | |
| | [11.905] | | | |
| **After** | | | | |
| July 1-7, 2004 | 107.875*** | | | 1169 |
| | [11.905] | | | |
| July 8-14, 2004 | 36.373*** | | 1117 | |
| | [11.905] | | | |
| July 15-21, 2004 | 15.353 | 829 | | |
| | [11.905] | | | |
| July 22-28, 2004 | 7.427 | | | |
| | [11.905] | | | |
| Observations | 3360 | | | |
| R-squared | 0.94 | | | |
| **Panel B: Dependent variable is ln(number of births)** | | | | |
| **Before** | | | | |
| June 3-9, 2004 | -0.035* | | | |
| | [0.018] | | | |
| June 10-16, 2004 | -0.046** | | | |
| | [0.018] | | | |
| June 17-23, 2004 | -0.022 | | | |
| | [0.018] | | | |
| June 24-30, 2004 | -0.158*** | | | |
| | [0.018] | | | |
| **After** | | | | |
| July 1-7, 2004 | 0.142*** | | | |
| | [0.018] | | | |
| July 8-14, 2004 | 0.052*** | | | |
| | [0.018] | | | |
| July 15-21, 2004 | 0.022 | | | |
| | [0.018] | | | |
| July 22-28, 2004 | 0.016 | | | |
| | [0.018] | | | |
| Observations | 3360 | | | |
| R-squared | 0.94 | | | |

Notes: Standard errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. All specifications include day of year, public holiday, and year×day of week fixed effects. Sample covers May 6 to August 25 (56 days before and 56 days after July 1) from 1975-2004. Total births moved is the sum of the coefficients (in weeks 1–2, 1–3 or 1–4), multiplied by 7.

The results from this exercise are shown in Table 2. Although the coefficients on the weeks immediately before and after the policy change are larger in magnitude than those further out, the shifting of births does not appear to be confined to this period. In either the levels specification (Panel A) or the logged specification (Panel B), all the June coefficients are negative and all the July coefficients are positive, and more than half are statistically significant.

Although the impact of the Baby Bonus is approximately symmetrical, there is some evidence that births began to fall from early-June onwards, and were higher than expected until mid-July. This is consistent with the incentives created by the sudden introduction effect: any mother who was due to give birth in June could have benefited from delaying birth until July. But after July 1, there was no benefit to further delay.

In columns (2) to (4), we estimate the amount by which births fell in June, and rose in July. Since the policy was only announced in May, we expect the introduction effect to have an impact only on timing, and not on the aggregate number of births. Accordingly, the reduction in births in June should approximately match the increase in July. This does indeed appear to have been the case. We estimate that births fell by 1172 over the last 28 days of June, and rose by 1169 over the first 28 days of July. About three-quarters of the reduction in births in June occurred in the last week, but one-quarter occurred in prior weeks. Similarly, about three-quarters of the rise in births in July occurred in the first week of July, but about one-quarter took place in the second, third or fourth week of July.

From a policy perspective, this result is perhaps the most troubling. To the extent that the Baby Bonus effect involved changing the timing of induced births by one or two days, it may not have had a significant impact on maternal or child health. But the results in Table 2 suggest that the introduction of the Baby Bonus led over 300 mothers to move their child's birth by more than 7 days, and over 150 mothers to move their delivery date by more than 14 days, potentially posing a significant risk to themselves and their children.

What is unclear from this exercise, however, is how far each individual birth was shifted. For example, if those births shifted off mid-June were moved to early-July, the health consequences are likely to have been less severe than if the same births were moved to mid-July. Nonetheless, the fact that the disruption caused by the Baby Bonus was not confined to a few days around July 1 is cause for some concern.

In addition, our methodology cannot tell us precisely the cause of the shift. One possibility is that the fact that births were moved from the last week of June to the first week of July may also have created congestion for that week. In that case, some births would have been pushed later into July. This may account for the significant effects in the second week of July, and the positive (though statistically insignificant) effects in the third and fourth weeks of July.

## 4. Caesarean Section and Inducement Procedures

In understanding the determinants of the shifting of births that occurred in June-July, 2004, it is instructive to examine those births by procedure. We already noted that the time period over which births appear to have shifted is supportive of the hypothesis that the majority of such shifting was real and not a result reporting issues or fraud. By examining procedure shifts, we can explore this in more detail. In particular, if there was mis-reporting of birth dates (with a baby born in June 30 reported as July 1), it is likely that shifts for non-induced vaginal births would be at least as high as other procedures.[26]

To examine this, we now turn to our second source of births data – hospital records compiled by the Australian Institute of Health and Welfare. We begin by comparing the total number of births from this data source with the total number of births from the ABS data. As McDonald (2005) has shown, the recent divergence between the two series raises concerns about their use for tracking aggregate trends in the birth rate. However, our study focuses on changes in the number of births within a short window, and for this purpose we find little difference between the two data sources. Figure 2 plots the two series against one another, demonstrating that they track one another closely. Regressing the raw ABS series on the raw AIHW series (using data for 2004) returns a beta coefficient of 0.9 (T=48.1) and a constant of 27.5 (T=2.0). This suggests that while

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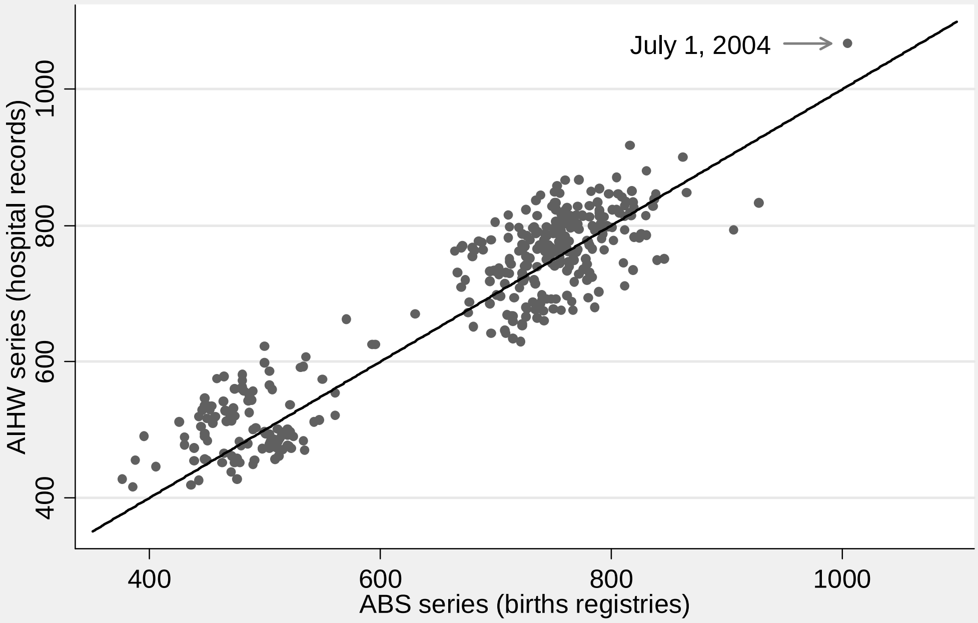
[26] To the extent that hospital birth records are interlinked with operating room and staffing records, falsifying dates for cesarean section procedures would be particularly difficult, since a larger number of medical personnel are in attendance for cesarean sections than for most vaginal births. In addition, planned caesareans are rarely conducted at night; the time on June 30 that there likely would have been most pressure for falsification of hospital records.

the two series are very highly correlated, the hospital birth counts are on average 27 births per day higher than birth registrations data. In both the series, births on July 1, 2004 (the first day on which the Baby Bonus took effect) are a clear outlier.

Figure 2: Comparing Two Sources of Births Data



Note: Dots show daily birth counts for 2004. The solid line is at x=y

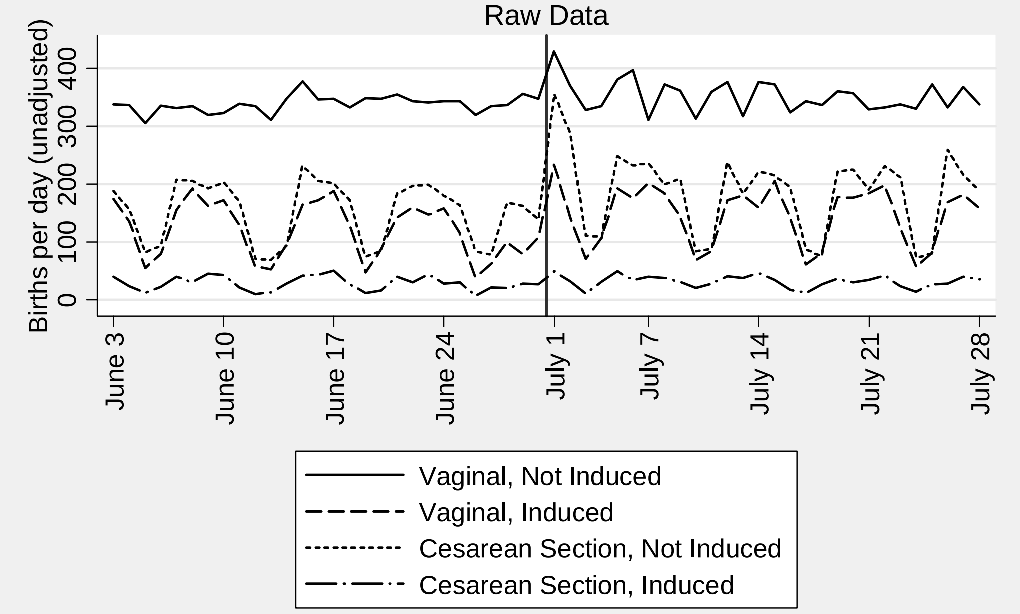
While the AIHW series has the disadvantage that it is not available over a long time span, it has the advantage that it allows us to separate births into four delivery procedures. The procedures, and their corresponding share of 2004 births are:

- vaginal, not induced (50 percent);
- vaginal, induced (20 percent);
- cesarean section, induced (5 percent); and
- cesarean section, not induced (25 percent).

(The category ‘cesarean section, induced’ refers to births in which labor was induced, and a cesarean section was subsequently performed – generally due to complications in labor.)

Figure 3 decomposes births by birth procedure across June and July 2004, showing just the raw data (unadjusted for day-of-week effects). In Figure 4, we adjust for day-of-week and public holiday effects (since our hospitals data do not span multiple years, we cannot also account for year and day-of-year effects). These two charts show that on July 1, births delivered by all four procedures increase, with the largest rise (in numerical terms) being for induced vaginal and non-induced cesarean section. Reported incidents confirm that the introduction of the Baby Bonus was the likely driver of this shift. In the final week of June, 2004, there were numerous reports of elective cesarean procedures being fully booked for the first week of July.[27]

Figure 3: Comparing Birth Procedures



Data cover June-July 2004, and show daily birth counts.

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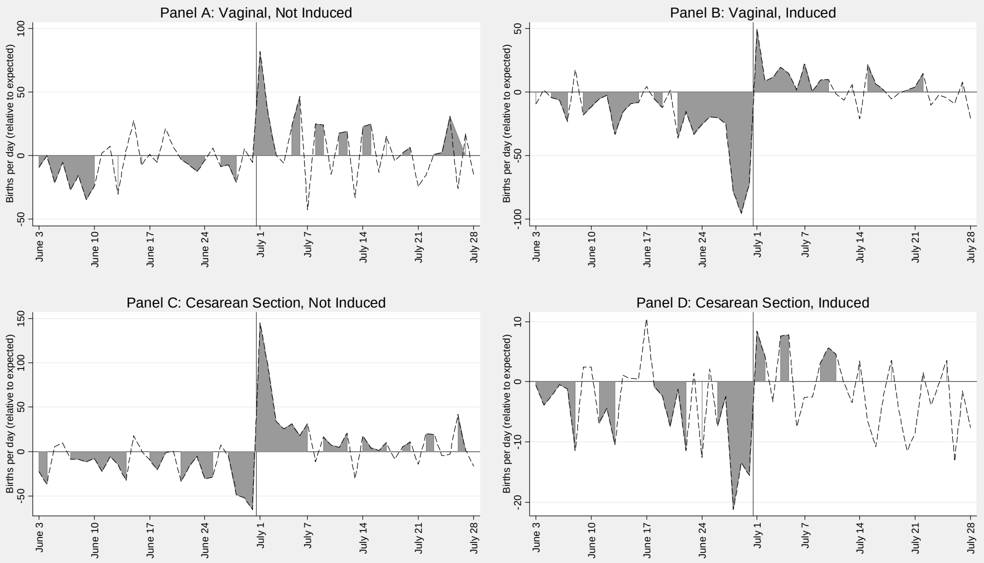
[27] For example, Anderson (2004) quoting that for one hospital “all of the planned spots were taken earlier than they normally would have been.” ABC Television (July 1, 2004) reported one hospital where women in labor were resisting coming in and one obstetrician who had only seen one scheduled induction and elective caesarian for the three last days of June and 14 on the first two days of July. See also Massoud (2004), Scott (2004) and Wells (2004). As early as June 18, 2004, *The Age* reported on pressure for Health Minister to change the introduction date of the Baby Bonus and bring it forward following reports that 10% of mothers had asked to postpone planned cesareans.

Figure 4: Comparing Birth Procedures

Regression-adjusted



Data cover June-July 2004, and show births relative to expected, accounting for day of week and public holidays.
Shading shows days of unusually low births in June and unusually high births in July.

To empirically estimate the effect of the introduction of the Baby Bonus on different types of birth procedures, we use a similar estimation strategy to that employed in Section 3. However, since we do not have data on births procedures over multiple years, we use only data from 2004. Consequently, we can no longer include day-of-year and year fixed effects, and our estimating equations are simply:

$$Births_i = I_i^{Baby\ Bonus} + I_i^{Day\ of\ Week} + I_i^{Public\ Holiday} + \varepsilon_i \tag{4}$$

$$\ln(Births_i) = I_i^{Baby\ Bonus} + I_i^{Day\ of\ Week} + I_i^{Public\ Holiday} + \varepsilon_i \tag{5}$$

Table 3 shows the results of our unlogged specifications. For each specification, we show the coefficient on the Baby Bonus, and estimate the number of births shifted using the same methodology as in Panel A of Table 1. For all but one of the specifications (non-induced vaginal births over a 7-day window), we observe a statistically significant increase in births after the introduction of the Baby Bonus. In terms of numbers of births, the coefficients are largest for induced vaginal births and non-induced cesarean section births.

**Table 3: Baby Bonus Effects for Different Birth Procedures**
***Dependent Variable is the Number of Births by Various Procedures***

| | **(1)** | **(2)** | **(3)** | **(4)** |
|---|---|---|---|---|
| **Window** | **±7 days** | **±14 days** | **±21 days** | **±28 days** |
| **Panel A: Vaginal, not induced** | | | | |
| Baby Bonus | 24.429 | 16.429* | 12.647* | 13.661** |
| | [14.651] | [9.088] | [6.963] | [5.679] |
| Observations | 14 | 28 | 42 | 56 |
| R-squared | 0.67 | 0.38 | 0.32 | 0.28 |
| *Number of births moved* | *86* | *115* | *133* | *191* |
| **Panel B: Vaginal, induced** | | | | |
| Baby Bonus | 66.143*** | 39.714*** | 33.186*** | 25.250*** |
| | [11.587] | [8.553] | [6.363] | [5.311] |
| Observations | 14 | 28 | 42 | 56 |
| R-squared | 0.94 | 0.86 | 0.87 | 0.87 |
| *Number of births moved* | *232* | *278* | *348* | *354* |
| **Panel C: Cesarean section, not induced** | | | | |
| Baby Bonus | 86.429*** | 51.214*** | 37.412*** | 32.448*** |
| | [19.798] | [13.489] | [10.089] | [7.856] |
| Observations | 14 | 28 | 42 | 56 |
| R-squared | 0.9 | 0.8 | 0.82 | 0.83 |
| *Number of births moved* | *303* | *358* | *393* | *454* |
| **Panel D: Cesarean section, induced** | | | | |
| Baby Bonus | 12.143** | 7.643*** | 4.304* | 3.089* |
| | [3.700] | [2.547] | [2.184] | [1.710] |
| Observations | 14 | 28 | 42 | 56 |
| R-squared | 0.85 | 0.76 | 0.72 | 0.73 |
| *Number of births moved* | *43* | *54* | *45* | *43* |

Notes: Standard errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. All specifications are based on data from 2004 only, and include day of week fixed effects and an indicator for public holidays. *Window* denotes the number of days before and after the start of July. For example, the ±7 day window covers the last seven days of June and the first seven days of July. *Number of births moved* is $W\beta/2$, where $W$ is the number of days in the window.

In Table 4, we estimate the effect using logged births as the dependent variable. These results suggest that induced vaginal, induced cesarean section, and non-induced cesarean section procedures rose by a similar amount: 23-31 percent within the ±7 day window, and 8-11 percent within the ±28 day window. Non-induced vaginal births rose by a smaller amount, and the effect is only statistically significant in the ±21 and ±28 day windows.

**Table 4: Baby Bonus Effects for Different Birth Procedures**
***Dependent Variable is the Log of the Number of Births by Various Procedures***

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| **Window** | **±7 days** | **±14 days** | **±21 days** | **±28 days** |
| **Panel A: Vaginal, not induced** | | | | |
| Baby Bonus | 0.064 | 0.043 | 0.034* | 0.038** |
| | [0.040] | [0.025] | [0.020] | [0.016] |
| Observations | 14 | 28 | 42 | 56 |
| R-squared | 0.67 | 0.37 | 0.31 | 0.28 |
| *Share of births moved* | *3%* | *2%* | *2%* | *2%* |
| **Panel B: Vaginal, induced** | | | | |
| Baby Bonus | 0.547*** | 0.333*** | 0.279*** | 0.211*** |
| | [0.072] | [0.068] | [0.053] | [0.044] |
| Observations | 14 | 28 | 42 | 56 |
| R-squared | 0.97 | 0.89 | 0.89 | 0.89 |
| *Share of births moved* | *31%* | *18%* | *15%* | *11%* |
| **Panel C: Cesarean section, not induced** | | | | |
| Baby Bonus | 0.448*** | 0.270*** | 0.206*** | 0.170*** |
| | [0.055] | [0.058] | [0.046] | [0.038] |
| Observations | 14 | 28 | 42 | 56 |
| R-squared | 0.98 | 0.91 | 0.91 | 0.91 |
| *Share of births moved* | *25%* | *14%* | *11%* | *9%* |
| **Panel D: Cesarean section, induced** | | | | |
| Baby Bonus | 0.421*** | 0.298*** | 0.200** | 0.151** |
| | [0.101] | [0.091] | [0.080] | [0.062] |
| Observations | 14 | 28 | 42 | 56 |
| R-squared | 0.94 | 0.82 | 0.78 | 0.79 |
| *Share of births moved* | *23%* | *16%* | *10%* | *8%* |

Notes: Standard errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. All specifications are based on data from 2004 only, and include day of week fixed effects and an indicator for public holidays. *Window* denotes the number of days before and after the start of July. For example, the ±7 day window covers the last seven days of June and the first seven days of July. *Share of births moved* is *exp(β/2)-1*.

In Table 5, we estimate regressions that are analogous to those in Table 2, but separated by birth procedure. This allows us to observe more precisely the decrease and increase in births across June and July. This suggests that the drop in births in late-June was predominantly driven by a reduction in cesarean section and inducement procedures, and the rise in early-July was driven most by cesarean section procedures, and to a lesser extent non-induced vaginal births and induced vaginal births. In the bottom rows of Panel A, we calculate the total number of births moved from June and to July by each procedure (this is done by summing the before coefficients or the after coefficients, and multiplying by seven in either case). These results suggest that the effect of the Baby Bonus was to reduce the number of vaginal induced births in June by 425, and the number of cesarean section, not induced births by 349; and to increase the numbers of non-induced vaginal births in July by 349, the number of induced vaginal births in July by 265, and the number of non-induced cesarean section births in July by 548. These results imply that the Baby Bonus caused not only a shift in timing, but also a shift between birth procedures. Approximately 200 babies who would have been induced in June were instead born vaginally without inducement or by cesarean section in July.

Although these figures are estimated using only one year of AIHW data instead of 30 years of ABS data, and do not include day-of-week and year fixed effects, it is reassuring to see that the sum of the estimated effects is approximately similar to Table 2 (the sum is -899 for June, and 1172 for July). The results using a log specification (Panel B of Table 5) show the proportionate effects. In percentage terms, the largest drop in births in June occurred in vaginal induced births, while the largest rise in July occurred in cesarean sections.

**Table 5: Baby Bonus Effects for Different Birth Procedures**

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| **Panel A: Dependent variable is number of births** | | | | |
| | **Vaginal, not induced** | **Vaginal, induced** | **Cesarean section, not induced** | **Cesarean section, induced** |
| **Before** | | | | |
| June 3-9, 2004 | -10.821 | -1.179 | -7.893 | -1.554 |
| | [7.880] | [5.421] | [8.169] | [2.548] |
| June 10-16, 2004 | 0.736 | -7.706 | -2.721 | -2.19 |
| | [8.480] | [5.833] | [8.791] | [2.742] |
| June 17-23, 2004 | 5.464 | -8.893 | -9.893 | -0.696 |
| | [7.880] | [5.421] | [8.169] | [2.548] |
| June 24-30, 2004 | 0.321 | -43.036*** | -29.321*** | -9.125*** |
| | [7.880] | [5.421] | [8.169] | [2.548] |
| **After** | | | | |
| July 1-7, 2004 | 24.750*** | 23.107*** | 57.107*** | 3.018 |
| | [7.880] | [5.421] | [8.169] | [2.548] |
| July 8-14, 2004 | 13.893* | 4.393 | 6.107 | 2.446 |
| | [7.880] | [5.421] | [8.169] | [2.548] |
| July 15-21, 2004 | 6.321 | 9.107* | 3.964 | -4.982* |
| | [7.880] | [5.421] | [8.169] | [2.548] |
| July 22-28, 2004 | 4.607 | 1.25 | 11.107 | -2.125 |
| | [7.880] | [5.421] | [8.169] | [2.548] |
| Observations | 112 | 112 | 112 | 112 |
| R-squared | 0.29 | 0.93 | 0.91 | 0.75 |
| *Total births moved from June* | *-30* | *-425* | *-349* | *-95* |
| *Total births moved to July* | *347* | *265* | *548* | *12* |
| **Panel B: Dependent variable is log births** | | | | |
| **Before** | | | | |
| June 3-9, 2004 | -0.031 | -0.009 | -0.02 | -0.029 |
| | [0.023] | [0.041] | [0.041] | [0.089] |
| June 10-16, 2004 | 0.002 | -0.086* | -0.037 | -0.128 |
| | [0.024] | [0.044] | [0.045] | [0.096] |
| June 17-23, 2004 | 0.017 | -0.067 | -0.044 | -0.033 |
| | [0.023] | [0.041] | [0.041] | [0.089] |
| June 24-30, 2004 | 0.002 | -0.379*** | -0.152*** | -0.320*** |
| | [0.023] | [0.041] | [0.041] | [0.089] |
| **After** | | | | |
| July 1-7, 2004 | 0.066*** | 0.167*** | 0.296*** | 0.101 |
| | [0.023] | [0.041] | [0.041] | [0.089] |
| July 8-14, 2004 | 0.039* | 0.051 | 0.048 | 0.141 |
| | [0.023] | [0.041] | [0.041] | [0.089] |
| July 15-21, 2004 | 0.019 | 0.062 | 0.028 | -0.134 |
| | [0.023] | [0.041] | [0.041] | [0.089] |
| July 22-28, 2004 | 0.014 | 0.007 | 0.048 | -0.023 |
| | [0.023] | [0.041] | [0.041] | [0.089] |
| Observations | 112 | 112 | 112 | 112 |
| R-squared | 0.29 | 0.95 | 0.95 | 0.81 |

Notes: Standard errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. All specifications include day of year, public holiday, and year×day of week fixed effects. Sample covers May 6 to August 25 (56 days before and 56 days after July 1) for 2004 only. Total births moved is the sum of the coefficients (in the before or after period) multiplied by 7.

Overall, the above results indicate that virtually all the drop in births in June was due to a fall in cesarean section and induced births. Of the rise in births in July, about three-tenths were vaginal non-induced births, about two-tenths were vaginal induced births, and about half were cesarean sections.

One corollary of the drop in induced births in late-June and the increase in cesarean sections in early-July is that the share of all births delivered by these measures rose with the introduction of the Baby Bonus. Figure 5 plots the share of births in which labor was induced and/or a cesarean section procedure was performed (as noted above, these are not mutually exclusive categories in the data: 5 percent of births are induced but the baby is delivered by cesarean section). Across 2004, the share of induced/cesarean births was 48 percent. However, there are substantial differences before and after the Baby Bonus was introduced. In the last 28 days of June, the share of induced/cesarean births was 45 percent, while in the first 28 days of July it was 49 percent. Starker still, the induced/cesarean share in the final week of June was 42 percent, while in the first week of July it was 52 percent.

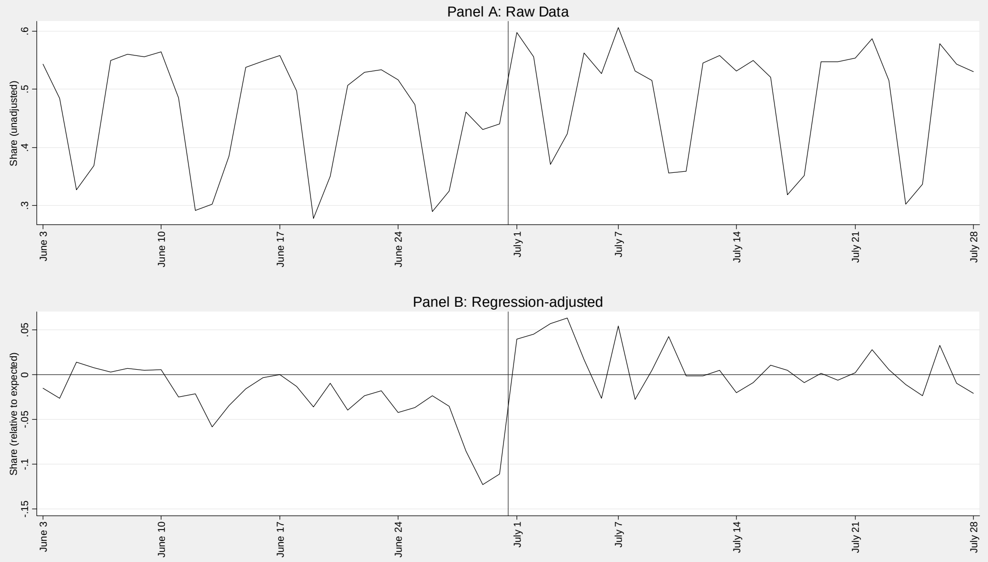


Figure 5: Share of Births Delivered by Inducement and/or Cesarean Section

Data cover June-July 2004. Panel A shows the unadjusted share, while Panel B shows the share relative to expected, accounting for day of week and public holidays.

## 5. Parental and Child Characteristics

In this section, we consider the characteristics of children and their parents; comparing those who were born just before and after the introduction of the Baby Bonus. In particular, we focus on three factors: whether the births that were moved were to parents of higher or lower socioeconomic status (SES) than average, whether those babies who were moved weighed more or less than average, and whether the Baby Bonus had any impact on infant mortality.[28]

The relationship between parents' SES and whether they moved their child's birth date to qualify for the Baby Bonus depends upon two considerations. If moving a child's birth date involved some trade-off between risk and SES, low-SES parents might be more likely to move their child's birth than high-SES parents. However, if parents with private health insurance were better able to affect the timing of their child's birth, then high-SES parents (who are more likely to have private health insurance) might be more likely to move their child's birth date than low-SES parents.[29] Since we are unable to observe in the data whether a parent has private health insurance, the relationship between moving birthdates and parental SES is theoretically ambiguous.

The second outcome we consider is the birth weight of children born before and after the introduction of the Baby Bonus. We focus on three variables – birth weight (in grams), the share of children who are low birth weight (less than 2500g), and the share of children who are high birth weight (more than 4000g). If the introduction effect caused parents to delay birth, then it should be the case that babies born after the Baby Bonus was introduced will weigh more than those born just before the introduction.

The final child-related outcome we consider is infant mortality. Plausibly, the movement of births for non-medical reasons could lead to additional complications. Alternatively, it might be the case that the record number of deliveries during the weeks

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[28] We also tested additional hypotheses, including whether the Baby Bonus effect differed for teen mothers, unmarried mothers, Indigenous parents or multiple births (eg. twins). We found no statistical association between these factors and whether a birth was moved into the eligibility period.

[29] In 2004-05, 51% of Australians aged 15 and over had a private health insurance policy. From ages 25 to 54, holding private health insurance is positively correlated with age. Private health insurance is also positively correlated with income. Holding age constant, 23% of those in the lowest income quintile had private health insurance, as compared with 76% of those in the highest income quintile (ABS 2006).

following the introduction of the Baby Bonus led to overcrowding, which increased the risk of death for newborns.

To test these hypotheses, we (via the Australian Bureau of Statistics) wrote to all eight state and territory births and deaths registrars to request unit-record data on births and infant deaths. Three states and territories (Victoria, Western Australia, and the Australian Capital Territory) agreed to provide us with births data, while five states and territories (New South Wales, Queensland, Western Australia, Tasmania and the Northern Territory) agreed to provide us with deaths data. Our births analysis is therefore based upon the three birth-reporting states, which accounted for 36 percent of Australian births in 2004. Analysis of the introduction effect for the birth-reporting states confirms that the impact of the Baby Bonus on births was very similar to the impact observed when using data for all of Australia.

We conduct our infant mortality analysis in two ways. One approach is to calculate the daily infant mortality rate using data only from Western Australia (the sole state to provide us with both births and deaths data), while the other approach is to calculate infant mortality rates by combining deaths data from the five death-reporting states with births data for all of Australia.[30] While using just Western Australia has the advantage of precision, it provides us with only a small sample (10 percent of 2004 births were in Western Australia). Using the five death-reporting states increases the sample size (67 percent of 2004 births were in these states), but requires that we assume that the pattern of births is the same in these states as it is in the rest of Australia.

We regress parental SES and children's birth weight on dummies for the day of the week, public holidays and, of course, the Baby Bonus introduction. Standard errors are clustered by birth date. Where the dependent variable is linear, we use OLS, and where it is binary, we use a probit model. We run a similar regression on daily infant mortality (that is, deaths per 1000 births on a given day).

Since our focus is on the effects of birth complications on infant mortality, infant deaths in our sample are restricted to those that occurred within one month of birth, and

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[30] In 2004, these five states accounted for 67% of the births in Australia, so we scale down the denominator by a factor of 0.67.

in which the death is classified as being caused by ‘conditions originating in the perinatal period’ (specifically, ICD-10 category P).

Table 6 presents the results from the parental regressions. Parents whose children were born shortly after the introduction of the Baby Bonus appear to be slightly older than parents of those born just prior to the introduction, though the coefficients are small and mostly statistically significant. We observe no systematic income differences. Overall, the results of Table 6 provide suggestive evidence that having private health insurance (more common among older and richer parents) mattered at least as much as simple income effects (which one would expect to be larger among younger and poorer parents).

**Table 6: Parental Characteristics**

| | **(1)** | **(2)** | **(3)** | **(4)** |
|---|---|---|---|---|
| **Window** | **±7 days** | **±14 days** | **±21 days** | **±28 days** |
| **Panel A: Dependent Variable is Mother's Age** | | | | |
| Baby Bonus | 0.180 | 0.074 | 0.106 | 0.118 |
| | [0.125] | [0.123] | [0.103] | [0.095] |
| Observations | 3287 | 6718 | 10120 | 13459 |
| R-squared | 0.01 | 0.01 | 0.01 | 0.01 |
| **Panel B: Dependent Variable is Father's Age** | | | | |
| Baby Bonus | 0.201 | 0.022 | 0.113 | 0.086 |
| | [0.114] | [0.125] | [0.111] | [0.091] |
| Observations | 3233 | 6604 | 9938 | 13225 |
| R-squared | 0.01 | 0.01 | 0.01 | 0.01 |
| **Panel C: Dependent Variable is Average of Mother's Age & Father's Age** | | | | |
| Baby Bonus | 0.181* | 0.046 | 0.100 | 0.102 |
| | [0.097] | [0.116] | [0.099] | [0.087] |
| Observations | 3287 | 6718 | 10120 | 13459 |
| R-squared | 0.01 | 0.01 | 0.01 | 0.01 |
| **Panel D: Dependent Variable is Log Mean Income in Mother's Zipcode** | | | | |
| Baby Bonus | 0.001 | -0.002 | 0.001 | 0.004 |
| | [0.007] | [0.004] | [0.004] | [0.004] |
| Observations | 2206 | 4527 | 6800 | 9045 |
| R-squared | 0.01 | 0.01 | 0.01 | 0.01 |

Notes: Standard, clustered at the birth date level, errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. All specifications are estimated using OLS, based on data from 2004 only, and include day of week and public holiday fixed effects. *Window* denotes the number of days before and after the start of July. For example, the ±7 day window covers the last seven days of June and the first seven days of July. Panel A to C regressions use data from the Australian Capital Territory, Victoria, and Western Australia. Average parental age is the mother's age if the father is not present at the birth. Log mean income in mother's zipcode is matched on from the 2001 Census (mother's zipcode is unavailable in the Victorian births file, so Panel D uses data from the Australian Capital Territory and Western Australia).

Table 7 presents results for child outcomes. On average, children born in the week after the introduction of the Baby Bonus weighed 75 grams more than children born in the week prior to its introduction (Panel A), and were 3 percent more likely to be of high birth weight (Panel C). The impact of the Baby Bonus on birth weight appears to be confined to the first 1-2 weeks, with results for the ±21 day and ±28 day windows being insignificant. Confirming this pattern, Figure 6 plots the share of high birth weight babies born in June and July 2004, indicating a spike immediately after the introduction of the Baby Bonus. (A regression of the high birth weight share on day of week and public holiday dummies reveals no significant relationship, so we show only the raw data.) Across 2004, 11.5 percent of babies in our sample were high birth weight. During the final week of June, the high birth weight share was 10.6 percent. In the first week of July, it was 13.8 percent.

**Table 7: Child Outcomes**

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| **Window** | **±7 days** | **±14 days** | **±21 days** | **±28 days** |
| **Panel A: Dependent Variable is Birth Weight (in grams)** | | | | |
| Baby Bonus | 75.170** | 17.602 | 1.621 | -6.933 |
| | [25.204] | [26.226] | [21.124] | [18.999] |
| Observations | 1040 | 2102 | 3174 | 4218 |
| R-squared | 0.01 | 0.01 | 0.01 | 0.01 |
| **Panel B: Dependent Variable is an Indicator for Low Birth Weight (<2500g)** | | | | |
| Baby Bonus | -0.005 | 0.005 | -0.001 | 0.001 |
| | [0.012] | [0.012] | [0.009] | [0.008] |
| Observations | 1040 | 2102 | 3174 | 4218 |
| Pseudo R-squared | 0.01 | 0.01 | 0.01 | 0.01 |
| **Panel C: Dependent Variable is an Indicator for High Birth Weight (>4000g)** | | | | |
| Baby Bonus | 0.029*** | 0.021** | 0.009 | 0.01 |
| | [0.011] | [0.010] | [0.010] | [0.008] |
| Observations | 1040 | 2102 | 3174 | 4218 |
| Pseudo R-squared | 0.01 | 0.01 | 0.01 | 0.01 |
| **Panel D: Dependent Variable is the Infant Mortality Rate (per 1000 live births) for Western Australia** | | | | |
| Baby Bonus | -2.976 | 0.367 | -0.943 | -0.674 |
| | [2.976] | [2.436] | [2.110] | [1.579] |
| Observations | 14 | 28 | 42 | 56 |
| R-squared | 0.54 | 0.19 | 0.14 | 0.1 |
| **Panel E: Dependent Variable is the Infant Mortality Rate (per 1000 live births) for 5 Death-Reporting States** | | | | |
| Baby Bonus | -0.692 | 0.825 | 0.421 | 0.465 |
| | [0.935] | [0.792] | [0.643] | [0.527] |
| Observations | 14 | 28 | 42 | 56 |
| R-squared | 0.76 | 0.24 | 0.17 | 0.21 |

Notes: Standard errors, clustered at the birth date level, in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. All specifications are based on data from 2004 only, and include day of week and public holiday fixed effects. Panels B and C show marginal effects from a probit model; the other results are OLS coefficients. *Window* denotes the number of days before and after the start of July. For example, the ±7 day window covers the last seven days of June and the first seven days of July. Regressions in Panels A to C use data from the Australian Capital Territory, Victoria, and Western Australia. Regressions in Panel D use data from Western Australia only. Regressions in Panel E use data from New South Wales, Queensland, Western Australia, Tasmania and the Northern Territory (with the denominator being 67% of the total number of births in Australia on each particular day).

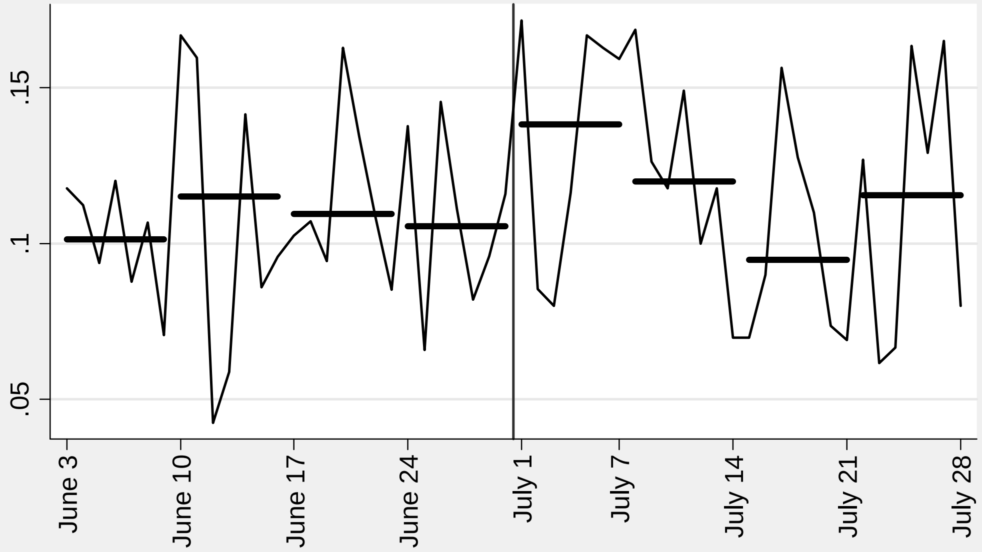

Figure 6: Proportion of Births That Are High Birth Weight

Solid horizontal lines show weekly means

Note: Based on 2004 births data from the Australian Capital Territory, Victoria, and Western Australia. High birth weight share does not vary systematically across the day of the week.

The increased share of high birth weight babies that accompanied the introduction of the Baby Bonus might potentially have led to adverse health consequences. While babies born pre-term and/or underweight are less likely to be healthy, the same is also true of babies born too late and/or overweight.[31] Evidence on this point comes from Thorngren-Jerneck and Herbst (2001), who analysed data from over 1 million births in Sweden in the 1980s and 1990s. As their outcome measure, they used the child's Apgar score (a five-item measure of skin color, heart rate, reflex irritability, muscle tone, and respiration, which ranges from 0 to 10). Apgar scores below 7 are regarded as being low, and Thorngren-Jerneck and Herbst plotted the relationship between low Apgar scores and birthweight, and between low Apgar scores and gestation length. Figure 7 (taken from Thorngren-Jerneck and Herbst 2001) demonstrates a clear U-shaped relationship for both

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[31] Guidelines defined by the American Academy of Pediatrics and the American College of Obstetricians and Gynecologists define pre-term births as those that occur up to the end of the 37th week of pregnancy, term births as those that occur from the 38th to the 42nd week, and post-term births as those that occur on or after the first day of the 43rd week of pregnancy (AAP, ACOG 2002; see also WHO 1992, Engle 2006).

variables. This suggests that exogenous increases in gestational age or birth weight may potentially have adverse health consequences.

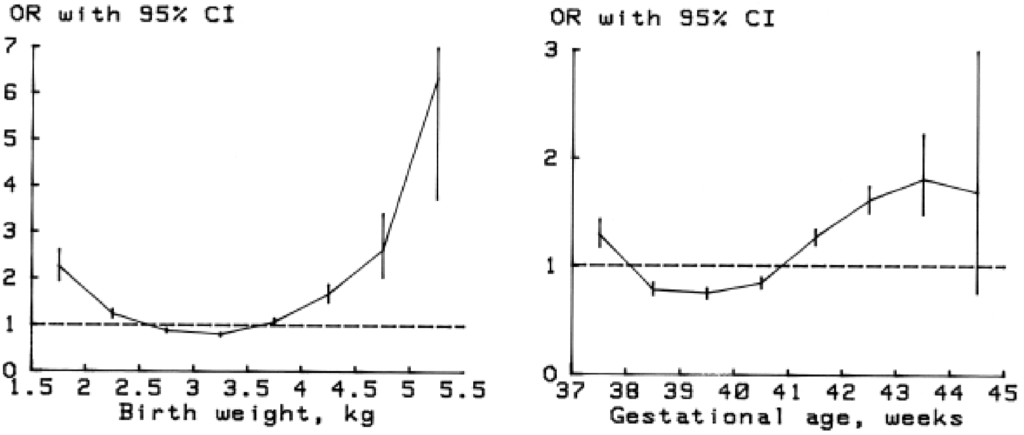


**Figure 7: Child Health, Birth Weight and Gestational Age**
***Chart shows the odds ratio (OR) for an Apgar score below 7, plotted against birth weight (left panel) and gestational age (right panel).***
***Source: Thorngren-Jerneck and Herbst (2001)***

Nonetheless, while the evidence on birth weight suggests that the introduction of the Baby Bonus might have put some children at risk, we do not find strong evidence that this had more dire consequences. Panels D and E of Table 7 focus on infant mortality. In both the Western Australian sample and the five death-reporting states sample, we find no statistically significant relationship between the introduction of the Baby Bonus and the infant mortality rate. Of the eight coefficients, four are positive and four are negative. While it is possible that more complete data might show an effect, we cannot conclude on the data available to us that the introduction of the Baby Bonus increased infant mortality.

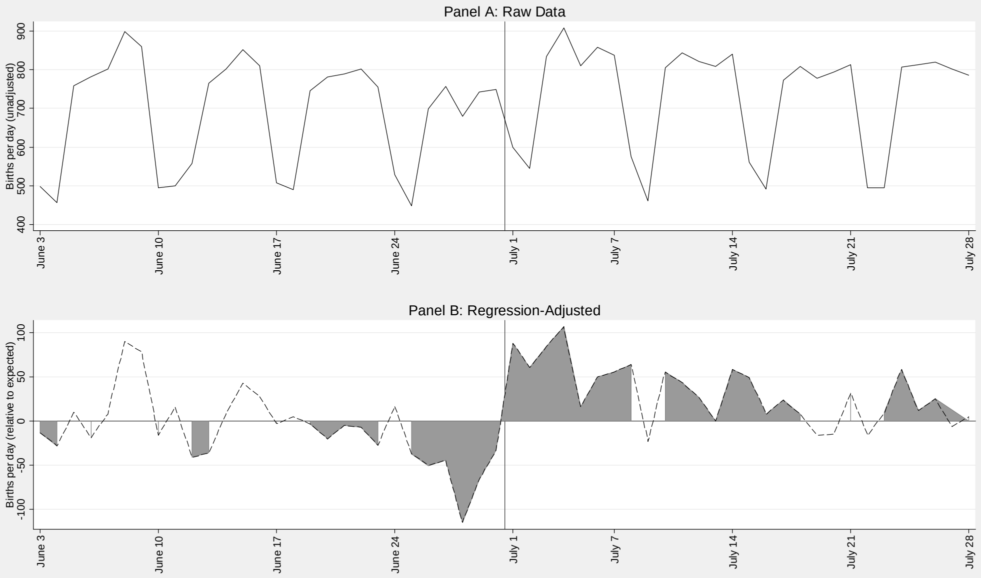
## 6. Coda: The 2006 Increase in the Baby Bonus

In 2004, at the time of announcing the Baby Bonus, the Australian government promised that it would be increased in the future. These increases would take two forms: an indexed increase every March and September, and a large increase to $4000 on July 1,

2006. Consequently, expectant parents in mid-2006 were faced with similar incentives to those in mid-2004. A child born on June 30, 2006 received a Baby Bonus payment of $3166 (the value of the Baby Bonus after indexation), while a child born on July 1, 2006 received a Baby Bonus of $4000.[32] While this increment was not as significant as the initial introduction of the bonus, it still represented a non-trivial amount of money; equivalent to three-quarters of a week's post-tax income for the median household.

In Figure 8, we plot daily births for June-July 2006, around the point at which the Baby Bonus increased. Again, we observe a fall in births in late-June, and a rise in early-July.[33]

Figure 8: The Introduction Effect in 2006



Panel A shows daily birth counts. Panel B shows births relative to expected, accounting for year, day of week, day of year, and public holidays.
In Panel B, shading shows days of unusually low births in June and unusually high births in July.

In Table 8, we re-estimate (2) and (3) using data from 1975-2003 and 2005-06.[34] Our results show a substantial Baby Bonus effect around July 1, 2006. The coefficient on

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[32] A first draft of this paper was released on June 18, 2006, and received considerable coverage in the Australian media. At that time, we unsuccessfully urged the government to phase in the 2006 increase. There is a slim chance that the publicity accorded to our study caused more births to be moved in 2006 (by drawing more attention to the sudden increase).

[33] As a falsification exercise, we also looked at the pattern of births around July 1, 2005 (when there was no change in the Baby Bonus), and found no significant changes.

the 28-day window is 49.1, which suggests that 687 births were moved from June 2006 to July 2006, or about 4 percent the children who would otherwise have been born in June. Of these, about two-thirds were moved from the last week of June 2006 to the first week of July 2006. The narrowness of this window suggests that it is unlikely that our results are driven by the timing of conceptions. However, unlike our 2004 results, we cannot entirely rule out the possibility of a conception effect in 2006.[35]

**Table 8: Birth Rate Effects for the 2006 Increase**

| **Window** | **(1)** **±7 days** | **(2)** **±14 days** | **(3)** **±21 days** | **(4)** **±28 days** |
|---|---|---|---|---|
| **Panel A: Dependent variable is number of births** | | | | |
| Baby Bonus 2006 | 123.729*** | 87.988*** | 66.426*** | 49.055*** |
| | [14.693] | [10.841] | [9.131] | [8.125] |
| Observations | 434 | 868 | 1302 | 1736 |
| R-squared | 0.97 | 0.95 | 0.94 | 0.94 |
| *Number of births moved* | *433* | *616* | *697* | *687* |
| **Panel B: Dependent variable is ln(number of births)** | | | | |
| Baby Bonus 2006 | 0.176*** | 0.123*** | 0.095*** | 0.072*** |
| | [0.022] | [0.016] | [0.014] | [0.012] |
| Observations | 434 | 868 | 1302 | 1736 |
| R-squared | 0.97 | 0.96 | 0.95 | 0.94 |
| *Share of births moved* | *9%* | *6%* | *5%* | *4%* |

Notes: Standard errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. Sample is daily births within the relevant window from 1975-2003 and 2005-06. All specifications include day of year, public holiday, and year×day of week fixed effects. *Window* denotes the number of days before and after the start of July. For example, the ±7 day window covers the last seven days of June and the first seven days of July. *Number of births moved* is $W\beta/2$, where $W$ is the number of days in the window. *Share of births moved* is $\exp(\beta/2)-1$.

This second Australian experiment allows us to draw some more general conclusions about the responsiveness of birth timing to financial incentives. Table 9 presents results from the 2004 and 2006 Baby Bonus changes, plus those of Dickert-Conlin and Chandra (1999) for the United States. Focusing only on the week before and after the change, the three studies together show that benefits worth 1-5 percent of annual

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[34] We obtain very similar results if we also include data for 2004, and estimate the model with indicators for both the 2004 introduction and the 2006 increase.

[35] We also tested for changes in births around the smaller increments in the Baby Bonus (on March 20 and September 20), but found no consistent pattern.

income caused 9-16 percent of births to be moved. However, Table 9 does suggest that the share of births moved is slightly larger as a result of the US Child Tax Credit than the Australian Baby Bonus. Several factors might account for this difference: one is that the foreseeability of the end of the US financial year has a small effect on conceptions as well as births.[36] The second possibility is that it is easier to hasten a birth (the incentive created by the US policy) than to delay it (the incentive created by the Australian rule). Finally, there is a different mix of private and public healthcare. As noted earlier, in Australia, high socioeconomic status was correlated with more delay. One reason for this may be that higher socioeconomic status was associated with parents having private health coverage (a situation closer to that in the US).

**Table 9: Birth Timing and Financial Incentives**

| Policy | Incentive | Could policy also affect conceptions? | Value (dollars) | Value (% of median annual household disposable income) | Share of births moved |
|---|---|---|---|---|---|
| 2004 Baby Bonus introduction | Delay | No | A$3000 (ignoring previous policy) | 5.4% | 16.2% |
| 2006 Baby Bonus increase | Delay | Yes | A$834 | 1.4% | 9.2% |
| US Child Tax Credit changes from 1979-1993 | Hasten | Yes | US$401 (mean credit, 1996 dollars) | 1.1% | 13.6% |

Notes: US estimates from Dickert-Conlin and Chandra (1999), Table 5. For all studies, effect sizes are based on a ±7 day window, to allow comparability with Dickert-Conlin and Chandra (1999). Effect sizes for the 2004 and 2006 Baby Bonus changes are from Tables 1 and 8. Median household annual disposable income is from the Household, Income and Labour Dynamics in Australia survey (HILDA), and detailed income tabulations from the US Current Population Survey (available at www.census.gov).

## 7. Conclusion and Future Directions

In May 2004, the Australian government announced that it would provide a $3000 Baby Bonus to children born on or after July 1, 2004. This delay between the announcement and the introduction of the policy led to what we term an “introduction effect.” We estimate that over 1000 births were moved into the eligibility period, constituting 6 percent of the babies who would have been born in the month before the

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[36] As all public finance economists know, the best way to maximize one’s US child tax benefit is to conceive by the end of March.

policy change. About three-quarters of the reduction in births in June occurred in the last week, and about three-quarters of the rise in births in July occurred in the first week. But around one-quarter of the births that were moved appear to have been moved by more than one week.

Analysis of birth procedures indicates that virtually all the drop in births in June was due to a fall in cesarean section and inducement procedures. Of the rise in births in July, half were cesarean sections, three-tenths were vaginal non-induced births, and two-tenths were vaginal induced births. Consequently, the share of births delivered by cesarean section or induction rose substantially in July 2004. Analysis of a subsample of unit record birth data indicates that those babies born in early-July were significantly more likely to be of high birth weight than babies born in late-June. Both these findings indicate that the shift in birth timing observed in the aggregate data is a real phenomenon, and not merely due to the misreporting of birth dates.

Two years later, a further increase in the Baby Bonus, implemented suddenly on July 1, 2006, had a similar effect on births; causing over 600 births (or 4 percent of those who would have been born in June 2006) to be moved to July 2006. This allows us to compare two Australian natural experiments with one another, and with evidence from sudden end-of-year changes in the US Child Tax Credit.

There are two important implications of the analysis here. First, these results suggest that when policymakers are announcing a new policy, they should think not only about the behavioral distortions of the policy over the long-run, but also of the possibility that the introduction of the policy may itself cause distortions in the short-run. Such effects are likely to be largest when a sharp policy discontinuity is announced in advance. The event studied here provides a clean example of the potential magnitudes involved.

Second, we have identified a very significant disruption to normal operating procedures for maternity hospitals and staff in Australia. This disruption appeared to impact both planned and unplanned birth procedures. Although we do not find evidence of a rise in infant mortality, the overall health effects of this are not known. However, with more data, this event provides an opportunity for health researchers and economists to study the impact of a large disruption in a well-developed, modern medical system.

The results of such studies have the potential to inform debates regarding the efficacy of planned birthdays and issues of under-staffing in hospitals.

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