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 a few months before its introduction, parents appear to have behaved strategically in order to receive this benefit, with the number of births dipping sharply in the days 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 two weeks. Most of the effect was due to changes in the timing of inducement and cesarean section procedures. This birth timing event represents a considerable opportunity for health researchers to study the impact of planned birthdays and hospital management issues.

JEL Codes: H31, J13
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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*.

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

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 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 ten week lag between the policy announcement and its introduction would not have an impact on the number of pregnancies as those effected 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

[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*, 27 June 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 the July 25, 2004.

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), 490 babies were registered as having been born, a birthrate at the 10th 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, 978 babies were born. This was the *highest number of births* recorded throughout the 10,958-day period 1975-2004. The seventh-highest number of births on a single day was July 2, 2004 with 907.

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 the share of births delivered by both procedures rose in the month of the introduction, with cesarean sections (induced and non-induced) accounting for about half of the births shifted, and induced vaginal births accounting for about one-third of the births shifted. While there is also an effect for non-induced vaginal births these did not appear to shift in the days immediately around July 1, 2004. Analysing a subsample of birth records, we

[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.

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

[8] Milligan (2002) 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 2004 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. A final section concludes.

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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 2004 Baby Bonus, his household was blissfully unaware of any financial benefit to having a child.

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 (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. With some 243,000 babies born in Australia each year, this amounts to government payouts totaling $729 million per annum. If birth rates increased as a result, this payout could be larger still.

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. 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.[13] 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.[14] Figure A1 (in the appendix) confirms this utilizing Google searches for the term "baby bonus" in Australia.

[12] Authors' calculations, based on the 2004 Household, Income and Labour Dynamics in Australia survey (HILDA).
[13] 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."
[14] 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

The Baby Bonus we described here replaced a previous policy, which operated as a refundable tax offset.[15] 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.[16] 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.[17] If the income exceeded this amount, then the taxpayer would only receive a payment if his or her income fell. In this case, the benefit could potentially be quite large. In such a case, the size of the payment depended on the taxpayer's income level prior to having a baby (Inc_0), his or her average tax rate in that year (τ_0), and her income level for in year t (where $t \leq 5$). The yearly bonus in year t was then calculated as follows:

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

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. 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

Bonus in 13 fortnightly installments, unless special circumstances existed to warrant the payment of a lump sum.

[15] The 2004 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.

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

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

payment of $2,500 per year.[18] 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).[19] In its first year of operation, the new Baby Bonus was expected to cost the federal government $741 million (Australian Treasury 2004, 37).

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 second 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 and Welfare (AIHW).[20] Each source has its limitations: unregistered births do not appear

[18] As Leigh and Wolfers (2002) note, the first 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 second baby bonus that did not induce direct changes to marginal tax rates pre- and post-natality.

[19] 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.

[20] The ABS births data contains no other information apart from the number of births registered on each day. Requests for more detailed births information must be approved by state and territory birth registrars. To our surprise, the registrars in the three largest states refused to provide us with these data, rendering this analysis impossible. As a result, we are unable to draw any conclusions about the demographic characteristics of babies who were born immediately before or after the introduction of the Baby Bonus.

in the ABS data, while home births do not appear in the AIHW data.[21] By using both data sources, we hope to circumvent the problems that would arise from only analyzing one or the other.

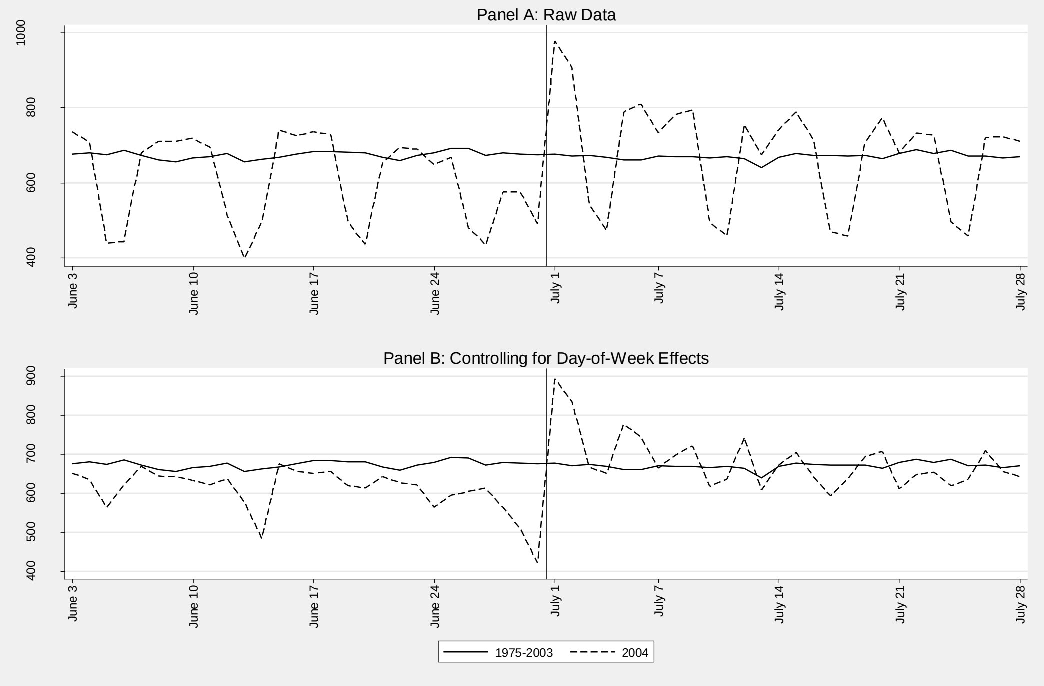
Since the ABS births data covers a longer time span, our central analysis uses these data. They include all recorded births from January 1, 1975 to December 31, 2004.[22] While the birth rate in Australia has declined over this period, the number of births has remained relatively constant (there were 232,678 births in 1975, and 243,216 births in 2003). 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 focuses on the period from June 3 to July 28, being the last 28 days of one financial year, and the first 28 days of the next financial year. The black line shows the average daily number of births for the years 1975-2003. Prior to the policy change, we observe no significant change in the number of births from the end of June to the beginning of July.

[21] 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 relatively short window, and for this purpose we find little difference between the two data sources.

[22] Due to reporting lags, the ABS births data is incomplete for December 2004. Since our analysis only uses data from June and July, this has no impact on our estimates.

Figure 1: The Introduction Effect



In Panel A, the dashed line shows the number of births over the same period in 2004. We observe a trough just prior to the beginning of the new financial year, at which point the number of births increased dramatically. As noted earlier, on June 30, 2004, 490 babies were registered as having been born, a birthrate at the 10th percentile of the births distribution over the entire period 1975-2004. On July 1, 2004, 978 babies were born.

As the 2004 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 729 children were born on weekdays and 517 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 regress the number of births on a vector of dummies for each day of the week, and remove this effect from the 2004 series.[23] This adjusted series is shown in Panel B of Figure 1.

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

$$Births_i = I_i^{BabyBonus} + I_i^{DayOfWeek} + I_i^{DayOfYear} + I_i^{Year} + \varepsilon_i \qquad (1)$$

Where *Births* is the number of children recorded as having been born on day *i*, and the indicator variables denote eligibility for the Baby Bonus (born on or after July 1, 2004), the day of the week (eg. Monday, Tuesday), the day of the year (eg. day number 182 is June 30, day number 183 is July 1), and the calendar year.[24] We estimate the regression both with the dependent variable as the number of births, and the log of the number of births.[25]

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.

[23] Specifically, we regress the number of births on indicator variables for each day of the week, predict the number of births using these indicator variables, set the mean of the predicted variables to zero, and subtract this from the actual number of births. These normalized day-of-week effects are Sunday -179, Monday 12, Tuesday 66, Wednesday 68, Thursday 85, Friday 72, Saturday -124.

[24] Since our focus is on effects that might be specific to 28 June, 29 June, and so on, we define a day of the year variable that is unaffected by leap years. In leap years and non-leap years, the day of year variable is 59 for February 28, and 61 for March 1. In leap years, the day of year variable takes the value of 60 for February 28.

[25] Since our analysis uses short windows, we compute Huber-White standard errors. An alternative is to compute Newey-West standard errors, forcing Stata to ignore the gaps between the windows. This approach produces slightly larger standard errors, though most of our estimates remain statistically significant at conventional levels.

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	206.527***	128.155***	98.294***	77.782***
	[38.970]	[25.884]	[20.809]	[16.841]
Observations	420	840	1260	1680
R-squared	0.93	0.92	0.9	0.9
Total number of births shifted	*723*	*897*	*1032*	*1089*
Panel B: Dependent variable is ln(number of births)				
Baby Bonus	0.302***	0.187***	0.146***	0.117***
	[0.053]	[0.037]	[0.031]	[0.026]
Observations	420	840	1260	1680
R-squared	0.94	0.93	0.91	0.91

Notes: Robust standard errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. All specifications include day of year, day of week, and year 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. *Total number of births shifted* is half the Baby Bonus coefficient, multiplied by the number of days in the window that fall on July 1 or later.

In Panel A of Table 1, we use the number of births as the dependent variable. With a window of seven days on either side, the coefficient on the Baby Bonus is 206, while a window of 28 days on either side produces a Baby Bonus coefficient of 78. All the estimates in Table 1 are statistically significant at the 1 percent level (and the 0.1 percent level, for that matter).

Since these coefficients are identified from the difference between the pre-Baby Bonus and post-Baby Bonus periods, the number of births per day that were "shifted" is equal to half the coefficient magnitude. The total number of births that were shifted is this number multiplied by the number of days in the window that fall on or after July 1. Our results suggest that over the period 28-days before and after the introduction of the Baby Bonus, 1089 births were "shifted" so that their parents could become eligible for the bonus. Most of this shifting (723 births) occurred within a week of the policy change.

In Panel B of Table 1, we use the log of the number of births as the dependent variable, with similar results. With a seven-day window, 30 percent more births per day occurred in the eligibility period. With a 28-day window, 12 percent more births per day occurred in the eligibility period. (We obtain qualitatively similar results from alternative specifications, for example interacting day-of-week effects with the year effects, or estimating the model in differences instead of levels.)

The results in Table 1 suggest that the effect of births being shifted into the new financial year occurred mostly, but not solely, during the first week. To test this, we estimate the effect of the Baby Bonus by focusing further out from the policy change. Table 2 shows the results of this exercise. Comparing the number of births 8-14 days before the policy change with the number of births 8-14 days after the policy change, we find a statistically significant effect. In the first column, the coefficient on the Baby Bonus is 50, suggesting that 174 births were shifted by more than two weeks as a result of the Baby Bonus. Our results for three and four weeks away from the policy change are positive, but not statistically significant. Using the log of the number of births (Panel B), we obtain similar results.

From a policy perspective, the results in Table 2 are the most troubling. As Figures 1 shows, most of the impact of the Baby Bonus occurred within a few days. To the extent that this involved falsification of hospital documentation, it posed no risk to the mother or child. Even to the extent that it 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. By contrast, the results in Table 2 suggest that at least 174 mothers moved their birth date outside the ±7 day window, potentially posing a significant risk to themselves and their children.

What is unclear from this exercise, however, is what the cause of the shift was. For example, 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 effects of 22-28 days. However, it is also possible that some births were delayed from June and brought into the first week of July. Our statistic methodology here cannot disentangle these two effects. However, it does identify disruption caused by the introduction of the Baby Bonus.

Table 2: Birth Rate Effects - Medium Run

	(1)	**(2)**	**(3)**
Window	**8-14 days**	**15-21 days**	**22-28 days**
Panel A: Dependent variable is number of births			
Baby Bonus	49.783***	38.571	16.246
	[17.679]	[26.889]	[17.146]
Observations	420	420	420
R-squared	0.93	0.9	0.91
Total number of births shifted	*174*	*135 (ns)*	*57 (ns)*
Panel B: Dependent variable is ln(number of births)			
Baby Bonus	0.072**	0.063	0.03
	[0.030]	[0.047]	[0.033]
Observations	420	420	420
R-squared	0.93	0.9	0.92

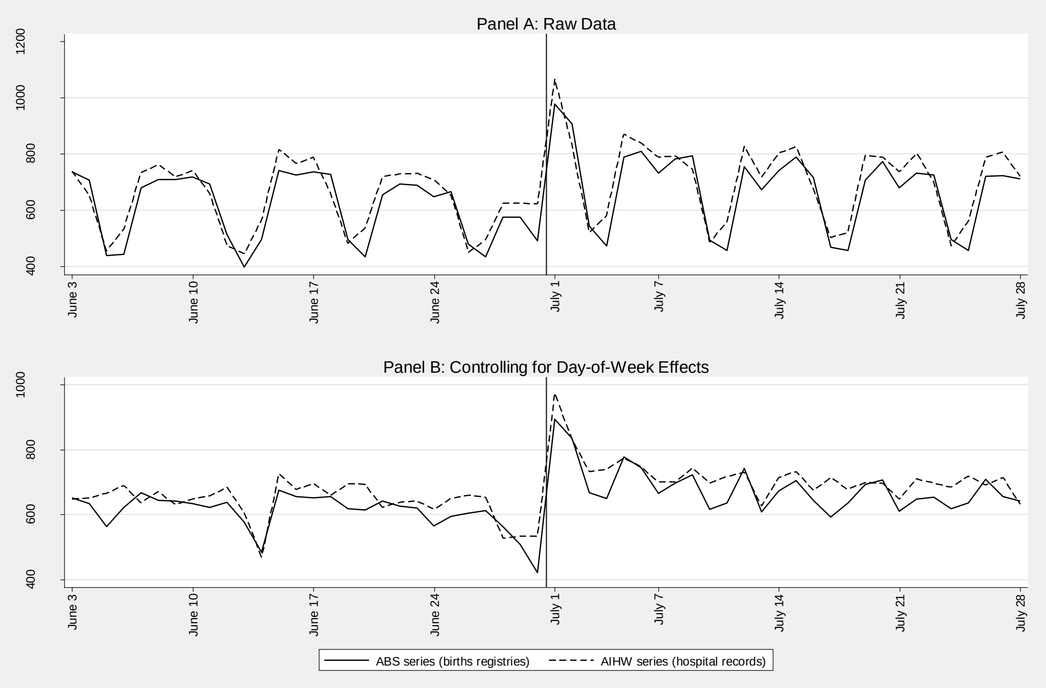
Notes: Robust standard errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. All specifications include day of year, day of week, and year fixed effects. *Window* denotes the number of days before and after the start of July. For example, the 8-14 day window covers the dates June 17-23 and 8-14 July. *Total number of births shifted* is half the Baby Bonus coefficient, multiplied by seven. ns=not significant.

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 birthdate (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. 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 returns a beta coefficient of 0.935 (T=20.31) and a statistically insignificant constant.

Figure 2: Comparing ABS and AIHW Births Data (2004)



[26] Perhaps more so given that planned caesareans would not likely be conducted at night; the time on June 30 that they likely would have been most pressure for falsification of hospital records.

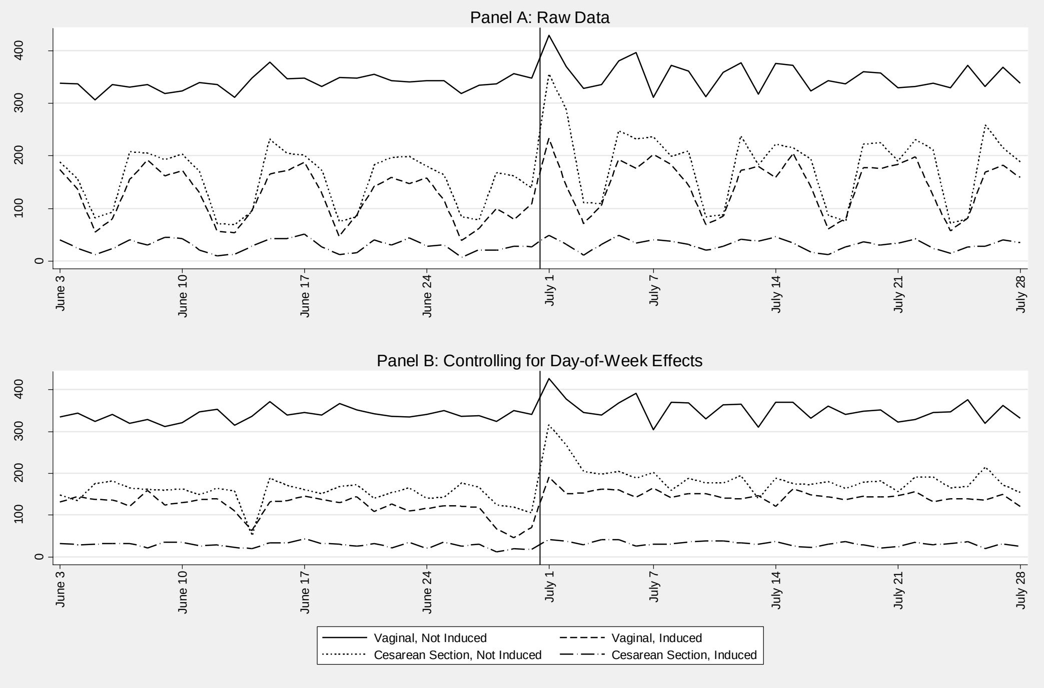
While the AIHW series has the disadvantage that it is not available over a long timespan, it has the advantage that it allows us to separate births into four delivery procedures. The procedures, and their corresponding share of 2004 births are:

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

Figure 3 decomposes births by birth procedure across June and July 2004. We show both the raw data (Panel A), and figures adjusting for the day-of-week effect (Panel B). 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]

[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 labour 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 requested postponing planned caesarians.

Figure 3: Comparing Birth Procedures (2004)



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 equation is simply:

$$Births_i = I_i^{BabyBonus} + I_i^{DayOfWeek} + \varepsilon_i \qquad (2)$$

Where *Births* is the number of infants recorded as having been born by a given procedure on day *i*. We estimate the regression with the dependent variable as the unlogged and logged number of births.

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 Section 3. 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, with induced vaginal and non-induced cesarean section accounting for most of the births that were moved. The 7-day insignificance of non-induced vaginal births suggests that birth shifting even within that window was a real effect rather than a reporting one. With the exception of induced cesarean sections, the *Number of births shifted* figure rises as the window is widened, indicating that a non-trivial share of the effect is outside the ±7 day window.

In the final row of Table 3, we sum the four *Number of births shifted* estimates for each window. 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 are similar to those in Table 1.

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*	12.952*	13.571**
	[14.651]	[9.088]	[6.768]	[5.563]
Observations	14	28	42	56
R-squared	0.67	0.38	0.31	0.28
Number of births shifted	*86 (ns)*	*115*	*136*	*190*
Panel B: Vaginal, induced				
Baby Bonus	66.143***	39.714***	35.143***	26.964***
	[11.587]	[8.553]	[6.481]	[5.476]
Observations	14	28	42	56
R-squared	0.94	0.86	0.86	0.86
Number of births shifted	*232*	*278*	*369*	*378*
Panel C: Cesarean section, induced				
Baby Bonus	12.143**	7.643***	4.619**	3.321*
	[3.700]	[2.547]	[2.144]	[1.691]
Observations	14	28	42	56
R-squared	0.85	0.76	0.71	0.73
Number of births shifted	*43*	*54*	*48*	*46*
Panel D: Cesarean section, not induced				
Baby Bonus	86.429***	51.214***	41.952***	36.214***
	[19.798]	[13.489]	[10.797]	[8.566]
Observations	14	28	42	56
R-squared	0.9	0.8	0.78	0.79
Number of births shifted	*303*	*358*	*440*	*507*
Sum of births shifted	**664**	**805**	**993**	**1121**

Notes: Robust 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. *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 shifted* is half the Baby Bonus coefficient, multiplied by the number of days in the window that fall on July 1 or later. *Sum of births shifted* is the sum of all *Number of births shifted* estimates within each 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: 42-55 percent within the ±7 day window, and by 16-22 percent within the ±28 day window. Non-induced vaginal procedures rose only a few percentage points, an effect that 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	0.035*	0.037**
	[0.040]	[0.025]	[0.019]	[0.016]
Observations	14	28	42	56
R-squared	0.67	0.37	0.31	0.28
Panel B: Vaginal, induced				
Baby Bonus	0.547***	0.333***	0.294***	0.224***
	[0.072]	[0.068]	[0.053]	[0.045]
Observations	14	28	42	56
R-squared	0.97	0.89	0.88	0.88
Panel C: Cesarean section, induced				
Baby Bonus	0.421***	0.298***	0.206**	0.156**
	[0.101]	[0.091]	[0.078]	[0.061]
Observations	14	28	42	56
R-squared	0.94	0.82	0.77	0.79
Panel D: Cesarean section, not induced				
Baby Bonus	0.448***	0.270***	0.238***	0.196***
	[0.055]	[0.058]	[0.055]	[0.045]
Observations	14	28	42	56
R-squared	0.98	0.91	0.87	0.87

Notes: Robust 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. *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.

In sum, the above results indicate that about half of the births that were "moved" by the Baby Bonus were delivered by cesarean section (induced and non-induced), about one-third by induced vaginal births, and the remainder by non-induced vaginal births. In proportionate terms, the largest increase was in induced vaginal births.

One corollary of the sharp increase in cesarean sections and inducements in July 2004 is that the share of all births delivered by these measures rose with the introduction of the Baby Bonus. Figure 4 plots the share of “non-natural” births over June and July 2004.

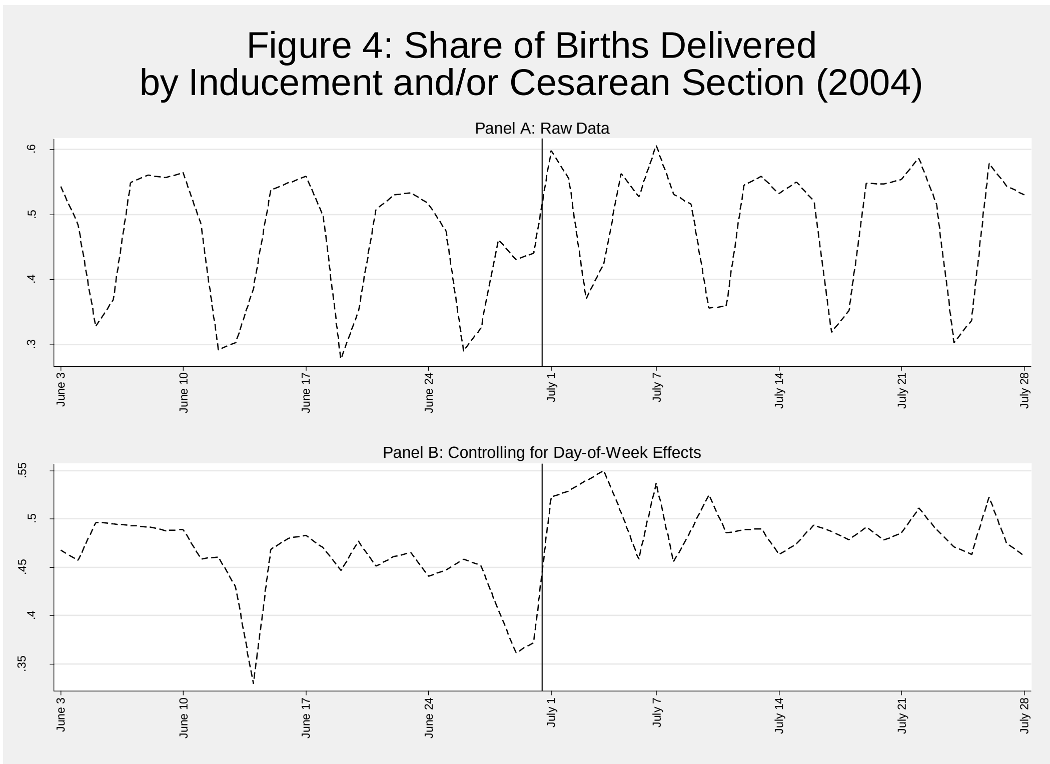


Figure 4: Share of Births Delivered by Inducement and/or Cesarean Section (2004)

5. Parental and Child Characteristics

Finally, 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.

The relationship between parents' SES and whether they moved their child's birthdate to qualify for the Baby Bonus depends upon two factors. 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.[28] 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 factor 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 factor 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 following the introduction of the Baby Bonus led to overcrowding, which increased the risk of death for newborns.

[28] 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).

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.[29] While using Western Australia only 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 over time is the same in these states as it is in the rest of Australia.

When focusing on parental SES and children's birthweight, our regressions take the form:

$$Outcome_{ij} = I_{ij}^{Baby\ Bonus} + I_{ij}^{DayOfWeek} + \varepsilon_{ij} \qquad (3)$$

[29] 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.

Where *Outcome* is a given outcome for parent/child *j* relating to a birth on day *i*. Standard errors are clustered by birth date.

When focusing on infant mortality, our regressions take the form:

$$IMR_i = I_i^{Baby\ Bonus} + I_i^{DayOfWeek} + \varepsilon_i \tag{4}$$

Where *IMR* is the infant mortality rate (deaths per 1000 births) for babies born on day *i*. 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 in which the death is classified as being caused by 'conditions originating in the perinatal period' (specifically, ICD-10 category P).

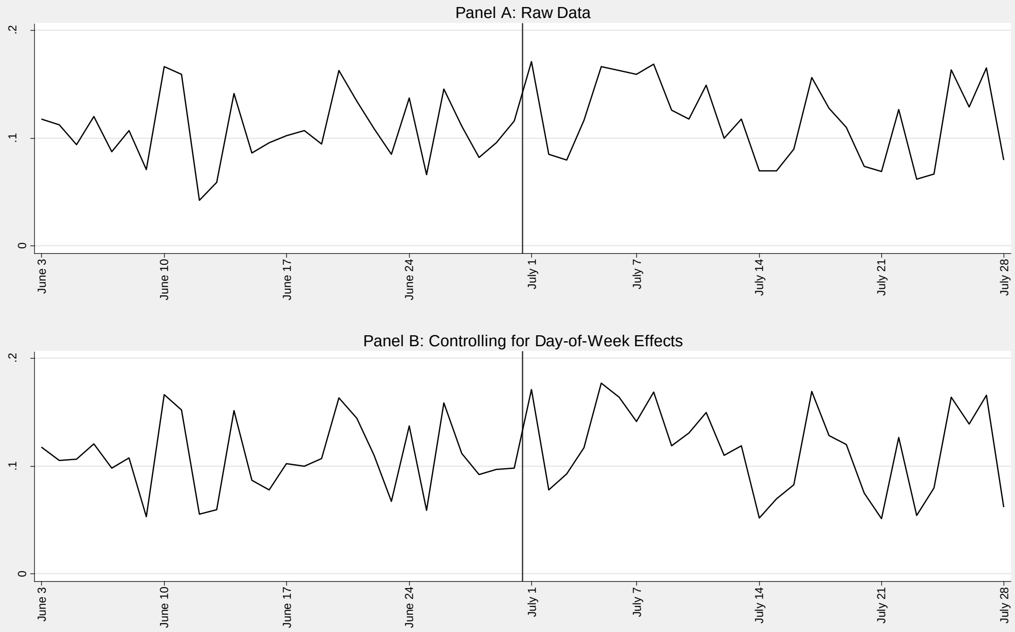
Table 5 presents the results from the parental regressions. Parents whose children were born shortly after the introduction of the Baby Bonus appear to be slightly older than parents of those born just prior to the introduction, though the coefficients are small and mostly statistically significant. We observe no systematic income differences. Overall, the results of Table 5 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 5: 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	0.124	0.135
	[0.125]	[0.123]	[0.100]	[0.094]
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	0.157	0.121
	[0.114]	[0.125]	[0.116]	[0.095]
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	0.132	0.128
	[0.097]	[0.116]	[0.100]	[0.089]
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: Robust 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. *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 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).

Table 6 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 5 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.

Figure 5: Proportion of Births That Are High Birth Weight (2004)



Note: Based on births in the Australian Capital Territory, Victoria, and Western Australia only.

Panels D and E of Table 6 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 find no evidence on the data available to us that the introduction of the Baby Bonus increased infant mortality.

Table 6: 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	4.19	-5.643
	[25.204]	[26.226]	[20.571]	[18.494]
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	-6.87	-2.892	-0.384	-0.113
	[6.359]	[3.806]	[3.007]	[2.273]
Observations	1040	2102	3174	4218
R-squared	0.01	0.01	0.01	0.01
Panel C: Dependent Variable is an Indicator for High Birth Weight (>4000g)				
Baby Bonus	0.030**	0.021**	0.007	0.009
	[0.011]	[0.010]	[0.010]	[0.008]
Observations	1040	2102	3174	4218
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	-0.789	-0.592
	[2.976]	[2.436]	[2.052]	[1.549]
Observations	14	28	42	56
R-squared	0.54	0.19	0.13	0.10
Panel E: Dependent Variable is the Infant Mortality Rate (per 1000 live births) for 5 Death-Reporting States				
Baby Bonus	-0.724	0.846	0.513	0.538
	[0.965]	[0.814]	[0.640]	[0.530]
Observations	14	28	42	56
R-squared	0.76	0.24	0.16	0.20

Notes: Robust 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. *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).

6. 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 1089 births were moved into the eligibility period, constituting 12 percent of the births in those months. While most of the births that were moved were within the window 7 days before and after the introduction of the policy, up to a quarter of the births that were moved were outside this window.

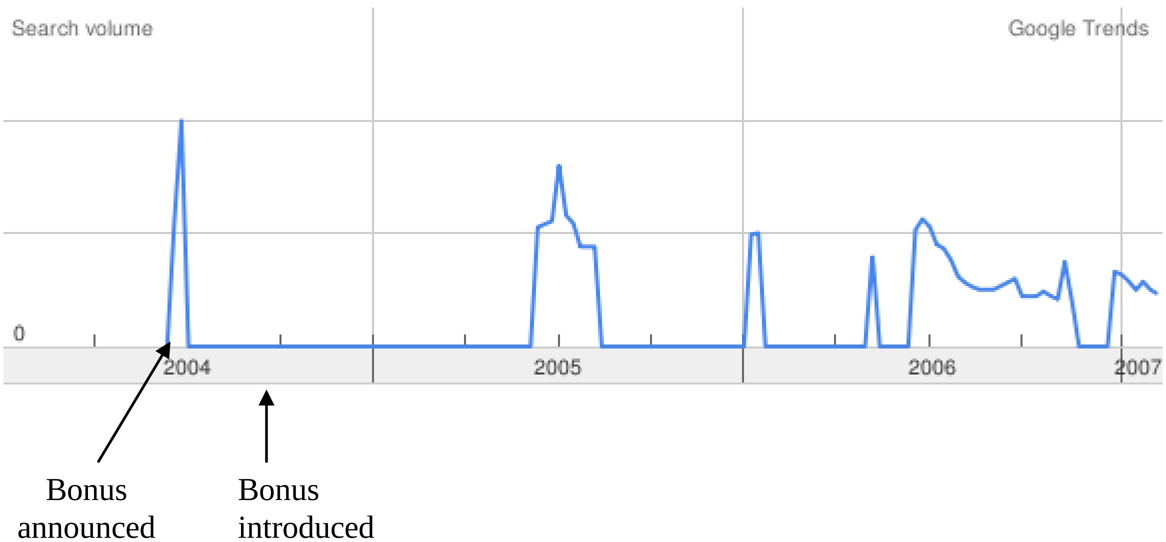
Analysis of birth procedures indicates that about half of the births that were moved by the Baby Bonus were delivered by cesarean section (induced and non-induced), about one-third by induced vaginal births, and the remainder by non-induced vaginal births. Consequently, the share of births delivered 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.

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.

7. Appendix

Figure A1: Google Trends on "baby bonus" (Australia only)



Source: http://www.google.com/trends

Note: The Google Trends database does not provide the actual volume of searches, so only a relative comparison of searches is possible. In addition, search volume below a given threshold is not recorded, so periods in which the search volume appears to be zero should be regarded as having relatively few searches.

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