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Abstract

In May 2004, the Australian government announced a “Baby Bonus” policy, paying women an initial A$3,000 per new child. We use household panel data from the Household, Income and Labour Dynamics in Australia Survey (N = 14,932) and a simultaneous equations approach to analyze the effects of this bonus on fertility intentions and ultimately births. The results indicate that opportunity costs influence intentions and births in predictable ways. Fertility intentions rose after the announcement of the Baby Bonus, and the birth rate is estimated to have risen modestly as a result. The marginal cost to the government for an additional birth is estimated to be at least A$124,000.

After the post-war baby boom, fertility declined in most developed countries. The total fertility rate in Australia, for example, after peaking at 3.5 children per woman in 1961, has fallen almost continuously since, dropping below replacement levels in 1976 and reaching a level of 1.75 in 2003 (see ABS, 2008a). Below-replacement level fertility rates are a concern in many industrialized countries because of negative consequences associated with a shrinking workforce and a rapidly aging population (Morgan & Taylor, 2006). These conditions have generated public debate in Australia about both the causes of fertility decline and appropriate policy responses to reverse the trend (Australian Treasury, 2002; Gray, Qu, & Weston, 2008). This debate led to the introduction, in 2004, of the Maternity Payment, now known as the Baby Bonus, whereby mothers received an amount starting at A$3,000 per new child as of July 1 of that year, with major increases scheduled for July 1, 2006 (to A$4,000), and July 1, 2008 (to A$5,000). The pro-natalist purpose of the Baby Bonus was clear from remarks made by the federal Treasurer, Peter Costello, who, when speaking to the press following the introduction of the initiative, urged Australian couples to have "one [baby] for your husband and one for your wife and one for the country" (quoted in *The Australian* newspaper, May 13, 2004).

The political context for the introduction of the Baby Bonus made it controversial. The major opposition party at the time, the Australian Labor Party (ALP), had been campaigning for a paid maternity leave system, and viewed the bonus as an alternative use of government funding for the same purpose of increasing fertility. In fact, since gaining control of the government late in 2007, the ALP has indicated that it will retain the Baby Bonus for non-employed mothers, but is considering the introduction of a payment for maternity leave from employment. Although Australia is not the first low-fertility country to offer a cash incentive to encourage women to have more children, the Baby Bonus is distinctive because of the size and inclusiveness of the payment (i.e., it is not based upon household income, the number of previous children born, or the mother's age).

The Baby Bonus serves as a natural experiment in terms of fertility policies, which we exploit in this paper. Specifically, we use a household panel data set that includes information on both new births and fertility intentions to answer a series of questions of interest to many policy-makers and researchers. First, did the Baby Bonus raise fertility intentions and thereby increase births? Second, were the effects of the bonus concentrated among women who already had children and among low-income groups, as economic theory predicts? Third, if the bonus had a positive effect, what was the estimated marginal cost per new child generated by the bonus? Finally, were the effects of the bonus temporary or relatively permanent?

FERTILITY, CASH PAYMENTS AND THE BABY BONUS

The Australian Baby Bonus began as a straightforward cash payment of A$3,000, paid in a lump sum to families following childbirth or the adoption of a child up to two years of age. It was paid for claims made within 26 weeks of the child's birth (or the child coming into the care of the claimant), and paid regardless of household income, mother's employment status, or previous children. Further, the payment was not included in taxable income. Due to concerns regarding youthful parents, it was revised in 2007 such that the bonus was spread over 13 bi-weekly payments for mothers under the age of 18, a change that was extended to all mothers in 2009. As mentioned earlier, the amount rose to A$4,000 as of July 1, 2006, and to A$5,000 on July 1, 2008. As of January 1, 2009, the payments will be means tested to exclude high income families (those earning more than $150,000 per year), although that change, announced as part of the May 2008 Federal Budget, should not affect our analysis of data gathered prior to that date.

A potential complication is that a birth subsidy program was in operation prior to the introduction of the new Maternity Payment. In effect the Maternity Payment replaced two existing payments – the Maternity Allowance and a "baby bonus" administered through the Australian Tax Office. The Maternity Allowance was a relatively modest payment (a maximum of A$842.64 per child at the time the scheme came to an end) which was restricted to women who qualified for Family Tax Benefit payments, and thus by extension lived in households with at most modest incomes. The bonus administered through the tax system, on the other hand, while potentially much more generous (with a maximum sum of up to $12,500 per child available over a 5-year period following a birth), seems to have not been widely utilized (Gans & Leigh, forthcoming). Low utilization rates were probably due to the system functioning as a complicated and delayed tax rebate system. Indeed, the most substantial payments were reserved for women with relatively high employment income in the year prior to birth, who subsequently remained out of the workforce for a total of five years.

Should we expect the newer Maternity Payment (i.e., Baby Bonus) to function differently and increase fertility? Economic theory supports this possibility, postulating a direct relationship between the costs associated with having children and fertility outcomes (Becker, 1960). Implicit in this model is the idea that state support can influence fertility rates by reducing the cost of children, either through the provision of services or by increasing household income through cash transfers (Gauthier & Hatzius, 1997).

Empirical evidence is also supportive, suggesting that pro-natalist social and economic policies introduced in many OECD nations have slowed if not entirely halted long-run declines in fertility (McDonald, 2006). Included here are cash payments, though where they have been tried in the OECD, the amounts were typically quite small, and certainly far too small to compensate for the increase in annual household expenditure that results from having a new child (D'Addio & Mira D'Ercole, 2005).

Studies focussing on the fertility effects of cash benefits have also been conducted at various levels. Macro-level studies use nations as the unit of analysis, and the majority have shown small though positive effects (see Gauthier, 2007, for a review). National studies utilize time-series data, and tend to find positive effects (e.g., Bonoli, 2008; Buttner & Lutz, 1990; Manski & Mayshar, 2003). One of the most careful studies to date, by Milligan (2005), compared two provinces in Canada for a period when one, Quebec, had implemented a cash payment for new biological children. Milligan found a strong impact of cash benefits on fertility, particularly for third or higher order children. However, there are two possible reasons for parity to mediate benefit effects. First, the marginal cost of a child declines as the number of children rises. Milligan estimates marginal costs for Canadians at the time of his study as C$7,935 for the first child, C$6,348 for the second, and C$5,324 for the third. Since these costs decline, a fixed cash payment per new child should generate positive effects for first order births, but stronger effects for second and particularly higher order births. Second, the Quebec system was more generous for higher order births. First births generated a payment of only C$500, with the amount doubling for second births, and more than tripling (to a maximum of C$8,000 at one point) for third and higher order births, thereby providing even stronger incentives for higher-order births. The Australian Baby Bonus is a fixed amount per child, so for the first reason we might find parity-specific effects for the scheme.

More generally, Baby Bonus effects should be mediated by the opportunity cost for bearing and rearing children, with reduced costs enhancing the Baby Bonus effect. For example, a fixed dollar payment should be of greater value in circumstances where income is or is expected to be low. That would be the case for low-income families, for women with low levels of education, and possibly for younger women (although family income per se may be positively correlated with fertility intentions; e.g., see Milligan 2005). Similar logic suggests that women already out of the labor market would confront a lower opportunity cost for childbearing, since no loss of immediate earnings is involved, while women who are married and have an additional source of income should also face a lower opportunity cost. School enrollment should also function to increase the opportunity cost of children, since the ability to sustain enrollment and achieve graduation may be compromised by pregnancy and childbirth.

Milligan (2005) finds results that are largely consistent with the opportunity cost approach, but with the important exception that high income is associated with a larger fertility effect for cash payments. The relationship between the fertility effects of the bonus and income in Australia is also an open question, although for a slightly different reason, flowing from the fact that the Baby Bonus does not count as taxable income. Australia has a progressive income tax system, with marginal rates ranging from zero for incomes below A$6,000 to 45% for income above $150,000 per year (as of the 2007-2008 tax year). Therefore, the pre-tax value of the payment ranges from A$3,000 for low-income individuals to over A$5,400 for high-income individuals. To the extent the Baby Bonus functions as an alternative to other sources of income, the payments may then be viewed as regressive, and we might not find low-income women more often taking advantage of the Baby Bonus as expected.

It is also possible that any positive fertility effects from the Baby Bonus will be temporary. One reason to suspect this is the potential for compression effects, whereby payments affect the timing rather than the total number of children, with a bunching of births around an announcement and a countervailing subsequent drop-off that leaves the ultimate number of births unchanged. Ermisch (1988), for example, finds evidence of compression effects from cash benefits in a study of British fertility. Second, there might be announcement effects such that the media or peers are initially excited about the new cash payment, with interest waning over time and behavior returning to previous patterns. Related to the second possibility, Gans and Leigh (forthcoming) found evidence of an immediate timing effect from the Baby Bonus, with more children born on the first day it went into effect (July 1, 2004) than on any other day in a 30-year period. That timing effect was, however, associated with women who were already pregnant when the bonus was announced in May of that year, and thus reflected the timing of scheduled caesarean births and induced labor around the introduction of the bonus. For announcement effects on fertility, we require data covering a longer time period.

As an empirical matter, compression and announcement effects can be distinguished to some extent. A pure announcement effect generates an increase in the birth rate followed by a return to previous levels. A pure compression effect also generates an initial increase but with the birth rate eventually falling to a level below that previously existing. Either effect could be mediated by parity with, for example, an initial burst in second births followed by a decline in the same behavior.

Of course, a contrary pattern of delayed effects might also be discovered, with fertility intentions and births rising over a period of a year or a few years after the announcement of the bonus. There are several reasons delays might occur. First, news of the bonus might travel slowly. This possibility seems unlikely, however, given the relatively immediate and large effect of the announcement on the timing of childbirth during 2004. Second, individuals and couples might wish to put financial or social supports in place prior to taking advantage of the bonus. Supports such as marriage, a secure job, home ownership, or savings, could take time to acquire, hence generating a delayed effect from the bonus. Finally, the scheduled increase of the bonus amount to A$4,000 on July 1, 2006, might have generated delays, given it represented a 33% increase in the financial incentives involved.

Conceptually, the Baby Bonus should have motivated a rise in fertility intentions. Over time, those increased intentions should have led to an increased rate of childbearing. That is, the government was hoping that individuals would respond to the bonus by seeking to bring more children into the world, thereby generating an increase in the birth rate. To model this behavior requires longitudinal data, with information on fertility intentions pre-dating pregnancy and childbirth, as in Schoen, Astone, Kim, Nathanson and Fields (1999). That study finds a positive correlation between intentions at one point in time and births 10 to 69 months later. Indeed, in models with a variety of controls, intentions explain a greater proportion of the variance in births than all other control variables combined. The controls are similar to those discussed earlier (e.g., parity, age, income, education, marital status, employment, and school enrollment status), and they conclude that "[f]ertility intentions and expectations are not the avenue through which background and life-cycle variables influence fertility" (Schoen et al., 1999, p. 798). This conclusion is based in part on the results of a single equation birth model with both fertility intentions and a battery of controls entered as independent variables. However, if fertility intentions are causally dependent on other control variables, then the single equation model is misspecified. Intentions should instead be modelled as endogenous. Further, this point goes to the core of economic theorizing around these issues; the opportunity cost arguments of economic theory assume that behavior is driven by rational choices, and those choices should be reflected in intentions that are both responsive to differences in opportunity costs and logically prior to births.

We therefore apply a simultaneous equations approach in what follows, with fertility intentions treated as endogenous in a model predicting births. The advantage of such an approach is particularly strong here, as it allows us to test the effects of the Baby Bonus on fertility intentions and, via that route, on births.

METHOD

The data used here are drawn from the first six waves of the Household, Income and Labour Dynamics in Australia (HILDA) Survey. Described in more detail in Wooden and Watson (2007), the HILDA Survey is a household panel survey with a focus on work, income, and family. It commenced in 2001 with a national probability sample of Australian households. Personal interviews were completed at 7,682 of the 11,693 households identified as in scope for wave 1, and while non-response is considerable, the characteristics of the responding sample appear to match the broader population quite well.

The members of these participating households form the basis of the panel pursued in the subsequent waves of interviews, which are conducted approximately one year apart. Interviews are conducted with all adults (defined as persons aged 15 years or older on the 30th June preceding the interview date) who are members of the original sample, as well as any other adults who, in later waves, are residing with an original sample member. Annual re-interview rates (the proportion of respondents from one wave who are successfully interviewed the next) are reasonably high, rising from 87% in wave 2 to over 94% in wave 6.

For the analyses, we use women in each wave of survey administration aged at least 17 and at most 50 years. These restrictions exclude groups with very low birth rates. A total of 17,066 responses were obtained for the first four waves of survey administration. The further exclusion of non-responses on fertility intentions and other independent variables, described below, results in a total of 16,694 observations. Our working sample also requires that birth information is provided either during the next wave of survey administration (for an immediate indicator) or two waves beyond the collection of intentions data (for a delayed birth indicator). The prior approach yields a working sample with 14,932 observations, while the latter provides 14,258 observations. These observations are associated with 4,799 and 4,495 unique respondents, respectively.

Dependent Variables

The relevant question for fertility intentions asks "how likely are you to have [a child / more children] in the future," with responses ranging on an eleven-point 0 to 10 scale with the extreme values labeled "very unlikely" and "very likely." One advantageous feature of this variable is that it is collected at a known point in time, so we can clearly divide the fertility expectations data into that collected prior to and after the date the Baby Bonus was announced. For analytical purposes, we treat the intentions variable as continuous. This strategy was selected mainly to make the interpretation of results straightforward; having distinct coefficients for 10 of the 11 fertility expectations categories (as would occur with an ordered probit or ordered logit) becomes particularly unwieldy once multiple regressors are introduced. Unfortunately, the questions used to measure fertility intentions in wave 5 are not directly comparable to those from the other five waves of survey administration. We therefore exclude intentions data from that wave.

As in the Schoen et al. (1999) study, the timing of birth is an important consideration in data construction, since the fertility intentions data should pre-date conception. The HILDA Survey data inform us as to whether a woman bore a child between waves of survey administration. Information is not available on the precise birth date of the child, so we utilize information on survey administration dates. The period between these dates can be as short as 6 months (for administration in February in one wave and August in the next), or as long as 18 months (for administration in August in one wave and February in the next). However, close to 80% of all interviews are conducted within an 11 to 13 month span (Watson, 2008, p. 115).

Given the timing complexities, we use two alternative indicators of births. An immediate births variable captures births in the survey wave immediately following that for the collection of intentions data. However, this indicator will include many women who were already pregnant when the intentions data were collected. To see why, assume for a moment that births are distributed randomly across months of the year, and that all surveys are administered precisely 12 months apart. Under these assumptions, a full three quarters of respondents who gave birth between survey waves were pregnant during the earlier wave of survey administration. It is possible that unintended pregnancy might raise the fertility intentions of some women, perhaps as a device to mitigate cognitive dissonance. If this is true, then the phenomenon undermines the validity of an approach that treats intentions as exogenous in the prediction of births.

To rectify this problem, a delayed birth indicator uses birth information for the period between one and two survey waves after the collection of the intentions data. Given the vast majority of surveys are administered between 11 and 13 months apart, this indicator will include few women who were pregnant when the intentions data were collected. However, the variable also introduces a lag into the analysis. Under the same strong assumptions regarding birth timing and survey administration as above, the variable misses all births related to conception during the three months after collection of the intentions data. Given these deficiencies, both birth indicators are used for the analysis, although the delayed indicator is perhaps preferred given it will contain fewer errors.

The birth variables are both constructed as dummies, with unity for a birth event and zero for no birth event between waves of survey administration. Multiple births are treated identically to single births because there is no obvious causal mechanism for fertility intentions to influence the rate of multiple births.

Independent Variables

The means and standard deviations of the independent variables (using data from the first four waves) are provided in Table 1. The Baby Bonus indicator is a dummy variable which is zero for surveys administered prior to May 12, 2004, and unity for surveys administered thereafter. The remaining variables mainly derive from our discussion of opportunity costs, and are similar to those found in Schoen et al. (1999). There are two dummy controls for parity, with the first taking a value of unity for the presence of one dependent child at the time of survey administration and the second taking a value of unity if at least two dependent children were present.

Table 1. *Independent Variables: Descriptive Statistics*

Variable Name and Description	Mean	St. Dev.
Baby Bonus – 1 = survey administered after May 12, 2004; 0 = survey administered on or before May 12, 2004.	.242	.428
Parity1 – 1 = one child in previous survey; 0 = otherwise.	.140	.347
Parity2 –1 = at least two children in previous survey; 0 = otherwise.	.513	.500
Younger – 1 = under 25 years of age; 0 = 25 years of age or older.	.171	.376
Older – 1 = at least 35 years of age; 0 = less than 35 years of age.	.540	.500
Low income – 1 = annual disposable household income in prior tax year AU$25,000 or less; 0 = income more than $25,000 (2000-2001 dollars).	.137	.344
High income – 1 = annual disposable household income in prior tax year AU$80,000 or more; 0 = income less than $80,000 (2000-2001 dollars).	.178	.382
Low education – 1 = high school diploma or less; 0 = otherwise.	.506	.500
High education – 1 = at least a bachelor’s degree; 0 =otherwise.	.251	.433
Single – 1 = not married nor living in a committed relationship for at least six months; 0 = otherwise.	.329	.470
Stay at home – 1 = coupled woman out of the labor market; 0 = otherwise.	.178	.383
Enrolled – 1 = currently enrolled in school at least part-time; 0 = otherwise.	.007	.084

Note: Data from HILDA Survey waves 1 through 5; N=14,932.

Other relevant controls include dummies for younger women (under the age of 25 years), for younger women (under the age of 25 years), older women (over the age of 34 years), low income, high income, low education (completion of high school or less), high education (at least a bachelors' degree), single status (excludes married or living in a committed relationship for at least six months), whether the woman was out of the labor force and coupled, and whether the woman was enrolled in school at the time of survey administration. The income dummy approach mirrors that in Schoen et al. (1999), but given our reliance on longitudinal data, figures are corrected for inflation using an average of four quarters of CPI data spanning each tax year (ABS, 2008b). The 2000-01 tax year serves as the base, with the low and high income thresholds set at A$25,000 and A$80,000, respectively, for that tax year.

From our earlier discussion, we expect that fertility intentions will be inversely correlated with parity, age, education, single status, and school enrollment, but positively associated with being out of the labor force and coupled and with family income. These variables are later used as controls to test for the effects of the Baby Bonus on fertility intentions. They are also interacted with the Baby Bonus variable to test for mediating effects.

Analytical Methods

The main analysis involves the maximum likelihood estimation of a probit model predicting births, with fertility intentions included as an endogenous regressor and the Baby Bonus variable employed as one of the instruments identifying intentions (using the IVPROBIT procedure available in Stata version 10). Put more simply, we effectively estimate a two equations model with the Baby Bonus predicting intentions in a linear regression, which, in turn, predicts births in a binomial probit regression (but note the equations are actually estimated simultaneously). The longitudinal character of the HILDA Survey data is exploited through the use of either a single-period lag (for immediate births) or a two-period lag (for delayed births). The fertility intentions data and independent variables for waves one through four are from an unbalanced panel. This approach both helps to maintain the largest sample size possible, and serves to maintain a single age distribution (17-50 years) for the intentions variable. Use of a balanced panel would necessarily involve incrementing the age of the sample by one year for each wave of survey data utilized, thus confounding the influence of respondent age with the effects of the Baby Bonus. Because respondents are not independent (they are correlated across years), standard errors and tests of statistical significance are corrected by clustering on the cross-wave identification number for each respondent.

If perfect foresight exists, then the Baby Bonus variable can be entered along with all other independent variables as predictors of intentions, with intentions serving as the sole predictor of births. We begin with this simple model. However, that model depends upon the strong assumption that both unintended births as well as intended births that do not eventuate are random events. It seems more reasonable to believe that some controls will themselves be correlated with either unintended births or the failure to achieve intended births. For example, young individuals may engage in riskier behavior and be associated with a higher birth rate than intentions would predict, although a contrary effect may exist given the concentration of abortions among Australian women in their early 20s (Australian Office for Women, 2007, Table 3.15). Although we cannot detect abortions in the data, as an empirical matter this behavior reduces the number of births. Conversely, although fertility declines with age, recent advances in fertility treatments might lead older respondents to overestimate the probability that attempted conception will result in birth, generating a lower birth rate than intentions would predict. Further, single persons may engage in sexual activity less frequently, hence experience unintended births less frequently due to the failure of contraception. Alternatively, single persons might be more likely to respond to contraception failure with abortion; again we have no means to detect such behavior. We therefore replicate the model after including the variables for younger and for older respondents, and for single respondents, as having potential direct effects on both fertility intentions and on births.

To test for the mediating effects of control variables on the Baby Bonus, another replication is performed with the addition of interactions between the control variables and the Baby Bonus variable in the intentions equation. Because many of the control variables are themselves correlated, and to achieve a more parsimonious specification, a Wald test is performed to ascertain whether insignificant interaction terms are jointly insignificant. Given insignificance, the remaining interactions are included in the reported specification, and again a Wald test is performed to gauge their joint significance. Note that this approach involves a specification search which, according to classical statistical theory, implies that the reported significance levels are not valid. Nonetheless, since we require significance in the regressions with all interaction terms included, validity is arguably not a problem. Note also that the interaction terms are not entered directly into the probit for births, since there is no obvious reason to expect the Baby Bonus to influence either unintended births or the failure to realize intended births.

A more general specification test involves the calculation of a chi-squared statistic for the reduction in the pseudo log likelihood explained by the variables in the two equations. Additionally, a Wald test is performed to check the exogeneity of intentions in the prediction of births. A significant test result means that the errors in both the birth and fertility intentions measures are correlated, hence indicating that the simultaneous equations model is appropriate. However, correlated errors can also appear if an omitted variable influences both births and intentions. We are particularly concerned about the latter possibility when utilizing the immediate birth indicator, since knowledge of pregnancy at the time of survey administration could both influence measured intentions at that time and be closely connected to subsequent births.

If the Baby Bonus has a significant positive effect on measured intentions, which, in turn, exert a positive and significant effect on births, then we can estimate the marginal cost of an additional child associated with the bonus. Since the intentions regression is linear, and the Baby Bonus represented by a dummy variable, the relevant coefficient represents the estimated effect of the bonus on intentions. That figure is multiplied by the marginal estimated effect of intentions on the probability of birth from the probit to yield the estimated change in the probability of birth due to the bonus. That figure is divided by the estimated birth probability for the entire sample prior to the introduction of the bonus to obtain a proportional increase in the birth probability. The A$3000 amount of the bonus can then be divided by that proportion to estimate the marginal cost per new birth resulting from the Baby Bonus. This estimate uses the logic that most babies would have been born regardless of the bonus, yet mothers receive the bonus regardless of whether it motivated the birth or not.

A subsidiary analysis explores the longer term effects of the Baby Bonus on fertility intentions, using data from waves 2, 4 and 6 of the HILDA Survey. Wave 6 data were collected mainly during November and December of 2006, or around two and a half years after the announcement of the Baby Bonus. Waves 1 and 3 are excluded to focus on data spaced two years apart, so that any instability in the estimates resulting from the absence of wave 5 intentions data is not confounded with differences in the time periods involved.

To test for announcement, compression or delayed effects, the basic intentions model includes control variables as before and a Baby Bonus dummy variable for all surveys administered after the announcement of the bonus, but with the addition of a spline for wave 6 responses. The use of a spline implies that we can add the coefficients on the Baby Bonus and the spline to estimate the longer-range effects of the bonus. If the bonus only generated announcement effects, then the Baby Bonus coefficient should be positive and significant, while the spline should attract a negative significant coefficient of the same absolute size. In this case the effects of the Baby Bonus are positive but temporary, with longer range behavior remaining unchanged. Delayed effects, on the other hand, would appear as a positive significant coefficient on the spline. A positive significant Baby Bonus coefficient, with an insignificant spline, would indicate that the bonus generated an immediate and permanent increase in fertility intentions.

As a further check for compression and announcement effects, interaction terms are introduced for the Baby Bonus and parity variables, and a Wald test is employed to test for any additional explanatory power. Interaction terms are next added for the wave 6 spline and the parity variables, and again a Wald test is employed to test for additional explanatory power. If there are compression or announcement effects through parity, then the initial interactions should be positive, with counteracting negative effects for interactions with the wave 6 spline.

RESULTS

Results for analyses using the immediate birth indicator are reported in Table 2. Beginning with the base model, which assumes that intentions alone predict births, both the Baby Bonus coefficient in the intentions regression and the intentions coefficient in the probit for births are positive and significant, supporting the claim that the bonus had a positive impact on fertility. Parity effects on intentions are negative and significant, with women who have already borne at least two children reporting lower estimated intentions than those with only one child. As expected, younger women report significantly higher, and older women

Table 2. *Results of Estimation of Probit Model of Births with Endogenous Fertility Intentions, using Immediate Birth Indicator*

	Base Model (I)		Model I + Birth Controls (II)		Model II + Interaction (III)	
Variable	β	Robust SE	β	Robust SE	β	Robust SE
Probit for birth predictors						
Fertility intentions	.106**	.008	.056**	.022	.057**	.022
Younger			-.225**	.080	-.227**	.080
Older			-.620**	.101	-.617**	.101
Single			-.602**	.060	-.602**	.060
Linear regression for fertility intentions						
Baby Bonus	.114**	.038	.114**	.038	.055	.039
Parity1	-.730**	.134	-.744**	.135	-.844**	.142
Parity2	-3.129**	.112	-3.13**	.112	-3.134**	.112
Younger	1.899**	.118	1.818**	.119	1.818**	.119
Older	-3.196**	.096	-3.258**	.095	-3.258**	.095
Low income	-.183*	.087	-.184*	.086	-.182*	.086
High income	.121	.072	.113	.071	.114	.071
Low education	-.041	.078	-.040	.078	-.040	.078
High education	.557**	.098	.557**	.098	.556**	.098
Single	-.667**	.079	-.730**	.078	-.731**	.078
Stay at home	.406**	.085	.398**	.085	.398**	.085
Enrolled	-.096	.310	-.086	.308	-.090	.307
Baby Bonus x Parity1					.404**	.142
Chi-squared statistic for overall explanatory power	197.8**		355.6**		355.4**	
Wald chi-squared statistic for exogeneity of fertility intentions	98.17**		46.59**		46.23**	

Note: Source is HILDA Survey data on intentions and controls from waves 1 through 4, and lagged, matched birth information from waves 2 through 5. All probit and linear equations include a constant term (results not reported). The standard errors have been adjusted for the within-person clustering of observations. N = 14,932.

* $p < .05$. ** $p < .01$.

significantly lower, intentions. Low levels of income are associated with significantly reduced fertility intentions, as also expected. High levels of education are associated with significantly higher, and being single with significantly lower fertility intentions, while partnered women who are out of the labor market are related to significantly higher fertility intentions. Only the education result is unexpected.

We next augment the base model by including age and marital status variables as controls in the probit for births. The chi-squared statistic for the additional explanatory power of these variables is significant (229.16, 3 d.f., $p<.01$), supporting their inclusion. All three coefficients are negative and significant, suggesting either a low prevalence of unintended births among the groups or a high incidence of intended births that are not realized. From our earlier discussion, it seems likely that the effects for young and single respondents reflect a relative tendency to avoid unintended births, while the effect for older respondents may be due to overestimation of the ability to realize fertility intentions. Regardless of the reasons, the effects of including these controls on the general pattern of results are minor; significant variables in the basic model retain significance and sign. However, the coefficient for fertility intentions in the birth probit declines by almost half.

Adding a full set of interactions for the Baby Bonus and control variables in the intentions equation yields one significant interaction term ($p < .05$). The remaining nine interaction coefficients are jointly insignificant (chi-squared = 14.34, 9 d.f., $p = .111$), so excluded, resulting in the model with the significant interaction reported in the final two columns of Table 2. Patterns of significance and sign largely remain as in the previous specifications, but with the direct Baby Bonus effect on intentions losing significance and the significant and positive Parity1 interaction coefficient suggesting that the Baby Bonus operates mainly through increases in intentions to bear second children. Indeed, the interaction coefficient is over three times the size of any of the direct Baby Bonus coefficients.

Turning to specification tests, the chi-squared statistics for the overall explanatory power of the regressions are significant ($p < .01$) in each case. In addition, the Wald chi-squared statistic for the exogeneity of intentions in the prediction of births is significant ($p < .01$) for all models, implying that errors are correlated across the birth and intentions regressions.

Parallel results using the delayed birth indicator are reported in Table 3. Considering the basic model, the estimated effect of intentions on births, and of the Baby Bonus on intentions, are again positive and significant. Other results in the intentions equation are similar to those reported in Table 2, except the negative low income coefficient loses significance.

Entering the age and single status dummies into the probit for births is again associated with significant direct effects, and a decline in the positive, significant intentions effect on births. The chi-squared statistic for the joint significance of the age and single status dummies in the probit is again significant (chi-squared = 207.89, 3 d.f., $p < .01$). Other significant results in the intentions regression remain stable, with little change in size.

Adding all of the interaction terms here reveals two significant additional coefficients, for Parity1 and Parity2. The individually insignificant coefficients are also jointly insignificant (chi-squared = 11.66, 9 d.f., $p = .167$), so can be ignored. The final column of Table 3 provides results after adding the significant interactions. Most results remain largely unchanged in this specification with the notable exception that the direct Baby Bonus

Table 3. *Results of Estimation of Probit Model of Births with Endogenous Fertility Intentions, using Delayed Birth Indicator*

	Base Model (I)		Model I + Birth Controls (II)		Model II + Interactions (III)	
Variable	β	Robust SE	β	Robust SE	β	Robust SE
Probit for birth predictors						
Fertility intentions	.123**	.007	.075**	.021	.075**	.021
Younger			-.246**	.078	-.246**	.078
Older			-.662**	.110	-.661**	.110
Single			-.542**	.061	-.542**	.061
Linear regression for fertility intentions						
Baby Bonus	.083*	.038	.082*	.038	.023	.070
Parity1	-.626**	.139	-.645**	.140	-.750**	.147
Parity2	-3.097**	.116	-3.104**	.116	3.126**	.118
Younger	1.872**	.124	1.862**	.123	1.863**	.123
Older	-3.259**	.098	-3.263**	.097	-3.263**	.097
Low income	-.159	.088	-.165	.088	-.162	.088
High income	.087	.074	.088	.074	.089	.074
Low education	-.052	.080	-.049	.080	-.049	.080
High education	.582**	.102	.580**	.101	.580**	.101
Single	-.717**	.081	-.723**	.080	-.724**	.080
Stay at home	.411**	.087	.404**	.087	.404**	.087
Enrolled	-.118	.314	-.104	.314	-.110	.314
Baby Bonus x Parity1					.416*	.161
Baby Bonus x Parity2					.088	.085
Chi-squared statistic for overall explanatory power	320.8*		411.3**		410.7**	
Wald chi-squared statistic for exogeneity of fertility intentions	0.37		4.42*		4.38*	

Note: Source is HILDA Survey data on intentions and controls from waves 1 through 4, and lagged, matched birth information from waves 2 through 5. All probit and linear equations include a constant term (results not reported). The standard errors have been adjusted for the within-person clustering of observations. N = 14,258.

* $p < .05$. ** $p < .01$.

coefficient loses significance. The Parity1 and Parity2 interactions meanwhile are both positive, but with only the prior achieving significance. These results again suggest the Baby Bonus mainly motivated an increase in second and perhaps higher-order births.

As with the models using the immediate births indicator, the chi-squared statistics for the overall explanatory power of the equation are each significant. More striking is the dramatic decline across Tables 2 and 3 in the chi-squared statistic for the exogeneity of fertility intentions. For each of the three models, the corresponding figures in Table 2 are higher by a factor of around 10. This finding is consistent with the possibility that many respondents reporting immediate births were pregnant when intentions data were collected, with pregnancy altering at least some reported fertility intentions. Given this divergence, the models using the delayed birth indicator are preferred.

Model II is preferred among those reported in Table 3. On the one hand, the joint significance of the age and single status dummies in the birth probit from Model II suggests it is superior to Model I. On the other hand, simulations for the Baby Bonus effect derived from Model III, assuming all direct and indirect effects are accounted for, involve reliance on two insignificant coefficients (i.e., the direct effect of the Baby Bonus and its interaction with Parity2).

Estimates of the marginal cost per child are derived from Model II results reported in Table 3. The marginal effect of intentions on births is estimated to be .00344 (not shown). Multiplying that term by the Baby Bonus coefficient from the intentions regressions yields an additional birth probability of .000282 which, divided by the mean estimated probability of birth for the pre-Baby Bonus sample (.0391), yields a Baby Bonus effect on births of .00721, for a 0.721% increase in the birth rate. Dividing the A$3,000 bonus by .00721 yields a marginal cost per additional child attributable to the bonus of just over A$416,000.

Replicating the exercise for Models I and III, including all direct and indirect effects for the latter, yields smaller estimates of about A$210,000 and A$268,000, respectively.

To gauge the longer term effects of the bonus, fertility intentions regression results for waves 2, 4 and 6, are presented in Table 4. The basic model in the first numeric column includes the same independent variables as the earlier base model for intentions, but with the addition of the wave 6 spline to capture announcement, compression or delayed Baby Bonus effects. The coefficient on the Baby Bonus is a positive and significant .125, while the spline attracts a positive and significant coefficient of .241, with a combined positive effect from the bonus of .366 on fertility intentions by wave 6. These results are consistent with delayed effects, but inconsistent with either announcement or compression effects. Adding interaction terms for the Baby Bonus and parity variables, as shown in the right-hand column of Table 4, results in a significant improvement in the fit of the equation (chi-squared = 8.38, 2 d.f., $p < .01$). As was found in Model III with delayed births and significant interactions, the direct Baby Bonus coefficient fails to achieve significance, while the coefficient for its interaction with Parity1 is positive and significant. Nonetheless, the spline retains significance across the two equations reported in the table, and is of virtually the same size in each. Note also that the explanatory power of the regression is reasonable, with an R-squared statistic above .5.

To test more directly for compression or announcement effects, the spline is interacted with the parity variables. This strategy, however, results in additional coefficients that are individually and jointly (chi-squared = 1.74, 2 d.f., $p = .176$) insignificant. Therefore, the data yields no evidence of either announcement or compression effects flowing from the Baby Bonus.

The strong evidence for delayed effects, however, suggests that the marginal cost figures should be re-estimated after accounting for these. Given one reason to expect delayed

Table 4. *Results of Linear Regression Analysis Predicting Fertility Intentions Over an Extended Period*

	Base Model		Model with Baby Bonus x Parity Interactions	
Independent Variable	β	Robust SE	β	Robust SE
Baby Bonus	.125**	.044	.076	.078
Wave 6 spline	.241**	.051	.240**	.051
Parity1	-.640**	.124	-1.024**	.167
Parity2	-3.253**	.106	-3.218**	.121
Younger	1.678**	.104	1.674**	.104
Older	-3.251**	.090	-3.252**	.090
Low income	-.118	.090	-.109	.090
High income	.173*	.070	.174*	.070
Low education	-.068	.075	-.068	.075
High education	.527**	.093	.527**	.093
Single	-.843**	.075	-.841**	.075
Stay at home	.379**	.089	.379**	.088
Enrolled	-.206	.323	-.211	.323
Baby Bonus x Parity1			.597**	.174
Baby Bonus x Parity2			-.064	.095
F-statistic for overall explanatory power	10651**		928.1**	
R-squared	.578		.579	

Note: Source is HILDA Survey data on intentions and controls from waves 2, 4 and 6. Both equations include a constant term (results not reported). The standard errors have been adjusted for the within-person clustering of observations. N = 11,316.

* p < .05. ** p < .01.

effects lies in the expansion of payments to A$4,000, the calculations are based on that amount. Assuming the estimated marginal effect of intentions on births from Model II in Table 3 (.00344) holds over this extended period, and again using the pre-bonus birth probability as the base (.0391), the combined figure of .366 for the bonus effect on intentions yields an estimated increase in the birth rate of .0322 (3.22%) and a marginal cost per additional child of just over A$124,000.

Discussion

The results presented here are generally both sensible and fit with theoretical predictions. Echoing the results of earlier studies, we find that the Australian Baby Bonus exerted a small though positive and significant effect on fertility. Further, the effect seemed to be stronger for second and possibly higher-order children, also consistent with theory and previous findings. In addition, the fertility intentions data provide no evidence that bonus effects were temporary; the evidence is inconsistent with either announcement or compression effects, instead fitting the possibility that many women exhibited a delayed response to the incentives. Nonetheless, the small size of the effect yields a marginal cost per additional child figure of at least A$124,000, though the cost may well lie above A$400,000 per child. These cost estimates are sufficiently high that policy-makers may wish to reconsider cash benefits relative to alternative policies for enhancing fertility.

A major analytical advance of the study is the deployment of a simultaneous equations model with various factors influencing fertility intentions which, in turn, predict births. The findings support the theoretical argument that background factors and resources – including cash incentives – operate to alter intentions which result in behavioral changes. These findings are in contrast to the conclusion reached by Schoen et al. (1999) that background factors do not operate mainly through intentions to influence fertility. That finding, however, was not based on a simultaneous equations approach.

The simultaneous equations approach allowed us to pinpoint the effects of background factors on fertility intentions, and indirectly on births, while capturing direct birth effects as well. Background effects on intentions are mainly reasonable, and explain much of the variation in fertility intentions (see Table 4). Fertility intentions are lower for women who already have children, decline with age, are lower for single women, and are higher for coupled women out of the labor force, although we also find that intentions are positively related to both income and education.

Our approach also provides evidence concerning the factors associated with unintended fertility behavior. The results suggest that young and single women are less likely to have unintended births, a finding that could be attributed to a lesser frequency of sexual activity and contraception failure, higher rates of contraception usage, or higher rates of abortion in response to unintended pregnancy. On the flip side of the age coin, the evidence suggests that older women sometimes overestimate their ability to conceive a child, a result which suggests that media reports of advances in fertility treatments are misleading for some older women.

The potential for overestimation suggests to us that not all behavior around fertility is rational. Indeed, the finding of a close correlation between errors in the intentions and birth regressions in the models using immediate births data is consistent with the possibility that many women respond to unintended pregnancy by adjusting their preferences and stated fertility intentions. That logic fits the notion that individuals alter their beliefs and emotions in response to cognitive dissonance, and would be difficult to explain as rational economic behavior.

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