Estimating the Heterogeneous Response to a Universal Child Benefit Programme: Do Education and Civil Status Play any Role?

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Abstract

This paper evaluates the heterogeneous effect of a universal child benefit of €2,500 introduced unexpectedly in Spain in 2007. We estimate the effect of the reform across different demographic groups. Our findings suggest that the cash benefit increased fertility in the very short-run by 8%, having a special impact on second order births. In addition, we find suggestive evidence that the policy increased fertility in absolute terms (rather than just having an effect on timing of births), as women at the end of their fertile period responded the most. Moreover, our results confirm that the policy had asymmetric effects across education and civil status groups. Due to the rigidity in intergenerational mobility, this heterogeneity in response may have distributional implications for the next generations.

Keywords: Pro-birth Policy, *Baby Check*, Fertility, Parity, Education, Civil Status, Distributional Impact.

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1. Introduction

Over the past decades; many developed countries have been facing declining birth rates which stand below the replacement level[1]. This situation together with a noticeable increase in longevity[2] is causing a demographic transition characterized by population aging. Such trend might undermine the enlargement or even the sustainability of the welfare state.

In order to mitigate this problem the majority of the developed countries has implemented several policies to boost fertility. Some of these reforms aim to conciliate work and family, such as increasing the duration of leave entitlements (Ruhm (1996)) and providing more quantity and quality of child care benefits (Cornelissen et al. (2015)). Thévenon (2011) offers an in-depth description of OCDE cross-country variances and similarities in the policy mix created to support families. Such study concludes that these two pro-birth policies, specially applied in Nordic countries, are pretty successful in encouraging fertility (Table 1, Appendix).

However, the most popular pro-birth policies are those aimed to directly compensate for the raise in expenditure derived from childbearing, such as cash transfers, (Kim (2014)). The effectiveness of financial benefits has been broadly analysed in the theoretical literature. Becker (1960) and Willis (1973) explain that the demand for children depends on their costs, which are diminished by economic transfers. Therefore, it seems natural to infer a positive link between financial aid to families and fertility decisions.

Empirically, Duclos et al. (2001) study the effects of a financial aid given to mothers in Quebec to encourage fertility decisions. The financial aid departs from $500 for the first child up to $8,000 for the third. They find that the cash transfers were relevant to encourage childbearing, especially for third order births. Sinclair et al. (2012) study the impact of a baby bonus in Australia introduced in 2004 by means of a structural time series model. They find an increase in fertility 10 months after the policy implementation. In addition, this positive impact lasts in the long run. In a similar vein Laroque and Salanié (2014), using the variation in the French tax code, conclude that tax incentives affect fertility decisions in France. More specifically, they find that an unconditional child benefit with a direct cost of 0.3% of GDP may raise total fertility about 0.3 points. Finally, González (2013) demonstrates that the Spanish universal child benefit -which we use in our analysis- led to an immediate 6% increase in conceptions. However, the author does not analyse whether this short-run increase in fertility is durable or whether it just corresponds to a shift in the timing of births.

[1] The average number of live births per woman required to keep the population size constant in the absence of inward or outward migration. Following Eurostat, a total fertility rate of around 2.1 live births per woman is considered to be the replacement level in developed countries.
[2] According to the World Health Organization, global average life expectancy increased by 5 years between 2000 and 2015.

However, there is another meaningful factor when evaluating pro-birth policies. Many studies suggest that a flat child benefit is likely to have a higher impact on low earning pre-birth families (Raute (2014)). This may have important distributional consequences on next generations. As De la Croix and Doepke (2003) explicitly say: *It is not overall population growth, but the distribution of fertility within the population which is important.* In other words, who is having children matters more than how many children there are overall because characteristics of the home environment such as parents' education or civil status are pretty relevant for children's achievements[3] (Coleman et al. (1966)).

Leibowitz (1974) considers that the genetic endowments of parents are passed to children. In the author's model, the ability of parents and their educational choices determine the level of schooling of the child. Similarly, Becker and Tomes (1986) suggest that parents with a level of education far above the mean will produce children who attain high levels of schooling. In addition, as parents care about the success of their children, high skill parents may invest more in the human capital of their children. In a survey of the literature, Haverman and Wolfe (1995) find that among the main factors determining children's educational choices, parents' human capital and especially the one of the mother is the most fundamental economic factor. In addition, the literature widely agrees on the effect of parent's education on children's health. For instance, Currie and Moretti (2003) use data on college openings in the woman's county of birth in the year in which she turned 17. The authors find that higher maternal education improves infant health, as measured by birth weight and gestational age. Currie and Lin (2007) also find that children of poor or less educated parents are in worse health on average than other children in the United States. The fact that education and health are two key predictors of important future outcomes such as earnings and long term equality (Currie (2011)) validates perfectly the need of understanding who reacts to a pro-birth policy when it is implemented.

Besides parents' education, another dimension that influences children's outcomes is the civil status of the mother. Mcclanahan (1994) finds that children who grow up with only one of their biological parents (nearly always the mother) are twice as likely to drop out of high school and 2.5 times as likely to become teen mothers. She also finds that children in one-parent families have lower grade point averages, lower college aspirations, and poorer attendance records. As adults, they have higher rates of divorce. These patterns persist even after adjusting for differences in race, parents' education, number of siblings, and residential location. In addition, a report by Save the Children (2015) informs that half of the households in Spain whose breadwinner is a single mother are in severe risk of poverty.

[3] A variable whose influence on children's outcomes is not that clear is the age of motherhood. Literature on the topic evidences that young motherhood has negative and relevant effects on children's achievements. However, Levine et al. (2005) explain that this negative effect may be due to background factors, more than young motherhood per se. For instance, they suggest that young maternity limits the human capital development of the mother, as educational attainment is likely to be reduced. This implies that fewer economic resources and skills will be transferred to the child.

Nevertheless, to the best of our knowledge, there is no paper analysing how a onetime policy affects to different socioeconomic groups. In fact, Hotz et al. (1997) highlight the difficultness in the evaluation of fertility policies as the financial incentives created are mostly universal.[4] Thus, the fact that all mothers are eligible eliminates the possibility of having a natural control group for counterfactual situation.

In this paper, we fill this lack in the literature by taking advantage of a natural experiment - the introduction of a fertility policy in Spain – which allows us to credibly replicate randomization[5]. This policy was unexpectedly implemented in 2007[6] by the Spanish government to boost fertility, the so called *baby check*, and it consisted in a lump-sum cash payment to all mothers giving birth on and after July 1, 2007 to compensate them for the costs of having children. Our empirical approach is to analyse the time series of births over time and look for a break around the cut-off using monthly births from 2007 to 2009. The fact that mothers could not have reacted with anticipation to the policy is an important strength of this paper, since it allows us to credibly identify the short-run effects of the Baby Check.

Our contribution is twofold: we first analyse the short and long-term demographic effects of the policy on fertility and then we study *who is having the children* because of the policy. The first analysis is relevant to understand the effectiveness of the policy, especially important in a country as Spain, with one of the lowest fertility rates and one of the highest life expectancies in comparison with other developed countries[7]. Moreover, the dependency rate in Spain is remarkably increasing over time (Figure 1, Appendix). Conde Ruiz and González (2010) predict that Spain will become the second country with the highest dependency rate by 2050. Therefore, the design of effective pro-birth policies may play an important role to palliate the consequences of such a demographic transition.

Regarding the second analysis, its relevance is related with equality concerns. Currie (2011) emphasizes that inequalities start even before we are born, and these initial differences are persistent over time. Thus, it is relevant in terms of policy design to evaluate the impact that the *Baby Check* may have had on the distribution of the following generations in terms of equality. Additionally, the *Baby Check*, eliminated in 2011, is again in the political agenda of some parties. Thus, an investigation of its previous impact may be useful in the design of new pro-birth policies to avoid possible unintended side effects.

[4] Milligan (2005) or Cohen, Dehejia and Romanov (2007) exploit the fact that the policy was not implemented at the same time or that it was not universal to analyse the impact of pro-birth policies on fertility by means of Difference in Difference.

[5] The source of this randomization is the sharp cut-off established for the benefit eligibility: There are some mothers who were randomly assigned to a control group (they did not receive the cash benefit as they became mothers before July, 1) and some that were assigned to the treatment group (they did receive the child benefit because they give birth on or after July, 1

[6] Law 15/2007, November 15.

[7] Conde-Ruiz and González (2010) and World Bank statistics point that Spain is the country that had the fourth higher life expectancy for women and the sixth for men in the year of the policy implementation. Nowadays, Spain is still the country with the fifth highest life expectancy (Behind Japan, Singapore, Switzerland and Australia)

Our findings confirm the results suggested by González (2013): the policy was effective in encouraging fertility, especially at an intensive margin. In addition, we present empirical evidence suggesting that it has a permanent effect on fertility, as women in the end of their fertile period adjusted their fertility fairly fast. The new financial aid might induce them to have a (an additional) child. This (additional) child is likely to represent an impact on their completed fertility, and not a shift in their time of conception.

Furthermore, our main results are related with the heterogeneity in responses. To understand how women react to the policy, we separate mothers by age group, setting 30 years old as the threshold between *young* and *mature*[8]. The main reason to take this approach is that Leung et al. (2016) find women having children before their 30s experiment the larger losses in lifetime earnings. Therefore, it is plausible to assume that the financial aid may incentive differently women among their ages. Following the literature, civil status and education are two important variables to predict children's future outcomes. We look whether mothers with low education react to the reform equally than mothers without education. Within the group of mothers that are younger than 30 years old, we find that uneducated mothers react significantly more to the financial aid in comparison to educated mothers. This suggests that young uneducated women experiment a larger income effect. However, within the group of more mature mothers, the ones with more education respond statistically more to the policy implementation. This may be explained by the smaller opportunity cost that they face if we assume that their careers are already well oriented and that they have more access to childcare services. In addition we find that for both age groups, single women react more than married women. As there is just one salary in these households, the income effect may also be larger for them. Our results are robust after controlling for seasonality in births.

The remainder of the paper is organized as follows: The next section describes the institutional setting. Section III presents the data. Section IV describes the methodology and the results. We conclude with section V.

2. The Universal Child Benefit

Birth rates in Spain have been persistently low over the past decades; meanwhile the dependency rate has been continuously increasing. Fertility policies are needed to slow the consequences of this demographic transition. According to OCDE data, Spain spends considerably less on family and childhood programs than the average of the OCDE-33 (Figure 2, Appendix).

[8] As we have mentioned, the literature does not agree on the impact of age per se. Therefore, we create interactions between this variable with civil status and education.

On July 3, 2007, the Spanish president José Luis Rodríguez Zapatero declared, during his "State of the Nation" that: *"In order to continue progressing, Spain needs more families with more children. And families need more aid to have more babies and more resources for their upbringing".*

With these twofold proclaimed goals of encouraging fertility and helping parents to cope with the extra expenditures associated with childbirth, the president announced a universal child benefit for all new mothers starting with those giving birth on the declaration day. This proposal became law in November (Law 35/2007), and it was popularly known as *Baby Check*.

The cash benefit consisted in one-time payment of €2,500 to be paid to all the eligible new mothers, starting with those giving birth on or after the announcement day[9], independently of their income or other socioeconomic characteristics. The only requirement to be eligible was to have resided legally in Spain the two years previous of the policy implementation[10]. In case of death of the mother, the subsidy would be paid to the father. As González (2013) suggests, this amount represented 4.4 months of gross salary for women working full time at the minimum wage (€570.6 in 2007).

In the Memory of the Spanish Tax Agency (2008), it is declared that there was close to full take-up of the cash transfer: more than 95% of all the mothers (including non-eligible mothers) received the transfer in the year 2008. This might respond to the wide media impact it had, jointly with the low administrative costs of applying to this cash benefit.

Finally, on March 2010 the new president, Mariano Rajoy, unexpectedly announced the elimination of the cash benefit for all the new mothers giving birth starting on January, 2011. The total spending of the policy in its 3 years was almost € 4,000,000,000[11].

3. Data

Our analysis draws on one data source: The Spanish Vital Statistics (Spanish National Statistics Institute). This database delivers information at a micro level on all births taking place monthly in Spain since the year 1975. Besides the date of birth of the child, it also provides information about the age of the mother. In 2007, this dataset was considerably improved, including new valuable variables such as mothers' education, mothers' nationality, civil status, and parity of the child.

Due to this data availability limitation, we start our analysis in the year 2007. We focus the investigation in the immediate response to the policy. The main reason is that in 2008 the economic

[9] The eligibility cut-off was moved to the July, 1 for administrative reasons
[10] We cannot differentiate eligible from non-eligible mothers
[11] Boletín Oficial de las Cortes

crisis hit Spain. From that moment, childbearing decisions may be made considering unemployment, wages, expectations, and other unobservable variables. However, the majority of births taking place in 2008 correspond to conceptions in 2007, when Spain was not still severely affected by the economic crisis.

Finally, we focus our attention in Spanish native women. Even that immigrant women represent an important percentage of births each year, their fertility behaviour may have different determinants in comparison with the ones of Spanish native women, (Laroque and Salanié (2014)). In addition, these factors may also differ depending on the country of origin. By time constraints, we leave this analysis for an extension of the current paper. We show some descriptive statistics in Table 1.

Table 1: Descriptive Statistics

	2007			2008		
	Mean	SD	Median	Mean	SD	Median
Monthly number of births	30,357.67	2,430.14	30,685	32,569.92	1,194.222	32,638
Age at first birth	30.39	0.07	30.37	30.41	0.0865	30.36
Births by women older than 35	21,940.5	1,688.22	22,081	23,162.08	853.54	23,338.5
Births by women younger than 35	8,417.167	786.339	8,622.5	9,407.83	406.88	9,490.5
Births by high skill	16,363.5	1,301.87	16,530	17,816.92	759.5745	17,728
Births by low skill	13,174.25	1,147.53	13,222	13,865.92	517.72	13,832.5
Births by married	22,066	1,768.31	22,556	22,734.92	932.732	22,777
Births by single	3,271.75	470.13	3,156	4,368.917	224.87	4,384

Note: The sample includes monthly births of Spanish women.

4. Methodology and Results

The fact that the announcement of the policy was unexpected makes implausible that women adjusted their fertility behaviour before the implementation of the reform. Thus, we have a natural experiment where some families have been randomly selected to receive the benefit and some have not around the cut-off (July, 2007). In this setting, we can credible compare households before and after the cut-off date. Thus, the empirical approach is to analyse the monthly births over time, and look for a break around the cut-off, controlling for seasonality.

We set the cut-off at the point of time where births are expected to start reacting to the policy. There are two sources of response: more people trying to conceive or a reduction in abortions. In the first case, medical literature suggests that in average couples need between three and six months to conceive when actively trying. This may lead to a gradual increase in the number of births since March, 2008 but especially from July, 2008 on. On the other hand, the majority of abortions happen within the first 13 weeks of gestation[12]. If abortions are reduced due to the policy, as González (2013) finds, we may see an increase in the number of births starting in January 2008. Based on this, we set the cut-off in January, 2008.

4.1 Demographic effects

Short run effects of the policy:

Effects on fertility

The universal child benefit we are evaluating had the declared intention of encouraging fertility. Equation 1, exploited by González (2013), allows us to test if its objective was achieved (in the very short run)

$$B_m=\alpha + \gamma_1 m + \gamma_2 m^2 + \beta Post + \lambda X_m + \sum_{i=2}^{12} \partial_i Dmonth_m + \varepsilon_m \quad (1)$$

Where B_m is the log number of births in month m[13], *Post* is a binary variable taking value 1 for all months starting in January 2008, and X is the number of days of month m. The month of birth is normalized to 0 for January 2008, taking value -1 for December 2007, 1 for February 2008 and so on and so forth. This variable accounts for any fertility trend, and it can change after the reform. In addition, second order polynomials are included to allow for nonlinear trends, and they are also allowed to vary after the policy implementation.[14] The polynomials are important because they add flexibility to the function. This helps to capture better the true relation between the policy and our dependent variable. Finally, $Dmonth_m$ denotes calendar month dummies. These are used to eliminate seasonality and proof the robustness of our estimations. β is the coefficient of interest; it captures the overall increase in the number of births after the child benefit introduction.

[12] Following the Ministerio de Sanidad, Servicios Sociales e Igualdad in 2007 the 88.15% of abortions happened in the first 13 weeks. This percentage is quite stable for all the period 2005-2014.
[13] We use monthly date as it is there is not daily date of births.
[14] We interact the linear trend captured by the variable *m* and the variable *Post* in order to allow for a change in the tendency after the policy. With the same aim, we also use the interaction between m^2 and Post.

The results are shown in Table 2, where the different specifications derive from narrowing the sample to months closer to the cut-off. Our preferred result, that limits the sample to 9 months before and after the cut-off (column (3)), suggests that the policy led to an immediate 8,4% increase in fertility. This result is robust after controlling for seasonality, as it is shown in the two last columns of Table 2. Our results confirm that the findings of González (2013) are robust after limiting the sample to Spanish native mothers. However, these coefficients should be interpreted with caution. They indicate an immediate response of fertility to a financial incentive, but this does not mean a positive permanent impact on fertility: Women could have decided to shift the time of birth because of the policy implementation, without modifying the number of children they would have in absence of the policy.

Table 2: Fertility Results

	(1) 12-24m	(2) 12-12m	(3) 9-9m	(4) 3-3m	(5) 12-24m	(6) 12-12m
Post	0.1312*** (0.0289)	0.0963*** (0.021)	0.0837*** (0.0248)	0.0159 (0.0544)	0.1495*** (0.0224)	0.1013*** (0.0305)
Years included	2007-2009	2007-2008	2007-2008	2007-2008	2007-2009	2007-2008
Number of months	36	24	18	6	36	24
Linear trend in m	Y	Y	Y	N	Y	Y
Quadratic trend in m	Y	Y	N	N	Y	Y
Number of days of the month	Y	Y	Y	Y	Y	Y
Calendar Month Dummies	N	N	N	N	Y	Y

Notes: The dependent variable is the log of monthly births in Spain. Robust standard errors are shown in parentheses.
***Significant at 1 percent level.
**Significant at 5 percent level
*Significant at 10 percent level.

Effect on Parities

The evidence presented so far shows that the reform had an immediate impact on fertility. However, this policy might have affected first births differently than second or higher order births. To analyse this, we replicate Equation (1) changing the dependent variable. In its place, we define three dependent variables that capture births by parity. The first one is the log of the number of first order births[15] by month. The second one is the log of the number of second order births by month. The third one captures the log of the number of third or higher order births by month.

[15] First order births are the births that are given by mother's without previous children. Second order births capture the number of births by mothers who already gave birth to their first child, and so on and so forth.

Table 3 shows the results of estimating these three equations. The majority of the coefficients for the parameter β are positive. However, they are just statistically significant for second order births *(intensive margin)*[16]. Hence, among parents who already had one child, the policy led to an immediate increase in fertility of 14%. This may help to reduce the gap between the Spanish birth rate and the replacement level. In fact, the birth rate increased noticeably in 2008 (1.45 children per woman). However, in 2009 it went back to the levels of 2007 (1.38 children per woman).

Table 3: Fertility Result for Birth Order

	(1) 12-24m	(2) 12-12m	(3) 9-9m	(4) 3-3m	(5) 12-24m	(6) 12-12m
1st order births	0.0931*** (0.0272)	0.0643** (0.0263)	0.0584* (0.0278)	-0.0147 (0.0609)	0.1488*** (0.0322)	0.0871 (0.0489)
2nd order births	0.2303*** (0.0463)	0.1867*** (0.0335)	0.1418*** (0.0240)	0.0733 (0.0536)	0.1814*** (0.0254)	0.1446*** (0.0224)
3th or higher order births	-0.012 (0.0449)	-0.0458 (0.048)	0.0105 (0.0447)	-0.0276 (0.0385)	0.0596** (0.0266)	0.0291 (0.0264)
Years included	2007-2009	2007-2008	2007-2008	2007-2008	2007-2009	2007-2008
N (Number of months)	36	24	18	6	36	24
Linear trend in m	Y	Y	Y	N	Y	Y
Quadratic trend in m	Y	Y	Y	N	Y	Y
Number of days of the month	Y	Y	Y	Y	Y	Y
Calendar dummies	N	N	N	N	Y	Y

Notes: The dependent variable is the log of monthly births in Spain conditional to the number of children mothers already have. Robust standard errors are shown in parentheses.
***Significant at 1 percent level.
**Significant at 5 percent level
*Significant at 10 percent level.

Long run effects of the policy

Up to now, we have focused our attention on the immediate response of fertility to the policy. However, it is relevant to analyse whether this increase in fertility is durable. With this aim, we evaluate the effect of the universal child benefit among different age groups. In addition, we analyse if there was a retiming in births as consequence of the policy implementation.

[16] It is remarkable that there is also evidence of a positive effect of the reform on mothers without previous children *(extensive margin)*. However, this result is not robust to the inclusion of seasonality.

Effects by age

Cohen et al. (2013) and Raute (2014) suggest that a temporary increase in fertility for women nearing the end of their lifetime fertility translates into a permanent increase in fertility because these women are likely to be completing their fertility rather than anticipating the time of birth.

In order to know if *mature* women were more affected by the policy than *younger* women, we specify equation (2). We set the threshold between being *young* or not at 35 years. Following a report by the American Society for Reproductive Medicine (2012), reproductive potential gradually declines in the 30s, particularly rapidly after age 35[17].

A priori, if we assume that the policy was expected to be durable, women closer to the end of their fertility years may react more and sooner to the financial aid, as their remaining time to conceive biologically is pretty limited and rapidly decreasing (Cohen et al. (2013)). This, together with the fact that women were still reacting to the policy when this came up to its end (González (2015)) may predict a larger effect of the *Baby Check* on *mature* women.

With the aim of corroborating this initial hypothesis, we specify equation (2).

$$B_{m,g}=\alpha_0 + \gamma_1 m + \gamma_2 m^2 + \alpha_1 Post + \alpha_2 Mature + \alpha_3 PostMature + \lambda X_m + \sum_{i=2}^{12} \partial_i Dmonth_m + \varepsilon_{m,g} \quad (2)$$

Where $B_{m,g}$, in Equation (2), is the log of monthly births by age group. *Post* is a dummy variable that takes the value 1 since January, 2008 and X is the number of days of month m. *Mature* is a binary variable that takes the value one when the age of the mother is, at least, 35 years old. $Post \cdot Mature$ is the interaction between the *Post* and *Mature*. It represents how mature women are affected by the reform in comparison with younger mothers. In this specification, we also control for linear and polynomial trends that are permitted to change after the introduction of the child benefit policy. As a robustness check, we control for seasonality by including month fix effects ($Dmonth_m$).

The results (Table 4) suggest that the policy implementation increased immediate fertility by 5% in our favourite specification (3). This positive impact was particularly substantial for mature women, who experienced an average immediate increase on fertility of around 9%. These results suggest that the financial incentive increased the completed fertility for this cohort, and thus, the *Baby Check* could have a permanent effect on total fertility.

[17]In our case, mothers who are 35 years old are in the percentile 75th of the distribution by age.

Table 4: Results by Age Group

	(1) 12-24m	(2) 12-12m	(3) 9-9m	(4) 3-3m	(5) 12-24m	(6) 12-12m
Post	0.0630*	0.0624**	0.0524*	0.0200	0.0960***	0.0934***
	(0.0324)	(0.0270)	(0.0276)	(0.0264)	(0.0318)	(0.0206)
Mature	-0.953***	-0.959***	-0.949***	-0.904***	-0.941***	-0.958***
	(0.0198)	(0.0122)	(0.0104)	(0.0189)	(0.0196)	(0.0109)
*Post*Mature*	0.0964***	0.0577***	0.0369**	-0.0198	0.0813***	0.0565***
	(0.0239)	(0.0172)	(0.0148)	(0.0272)	(0.0230)	(0.0151)
Years included	2007-2009	2007-2008	2007-2008	2007-2008	2007-2009	2007-2008
Number of months	36	24	18	6	36	24
Linear trend in m	Y	Y	Y	N	Y	Y
Quadratic trend in m	Y	Y	Y	N	Y	Y
Number of days of the month	Y	Y	Y	Y	Y	Y
Calendar Month Dummies	N	N	N	N	Y	y
Observations	72	48	36	12	72	48

Notes: The dependent variable is the log of monthly births in Spain separating between Young (mothers that are <35 years old) and Mature (Mothers that are ≥ 35 years old). Robust standard errors are shown in parentheses

***Significant at 1 percent level.

**Significant at 5 percent level

*Significant at 10 percent level.

Retiming of fertility

Anticipating childbearing does not imply an absolute increase in fertility: Women may react to the policy by changing the timing of birth but not the total amount of children they have. However, research on determinants of fertility has widely recognized the importance of timing in explaining total fertility rates. For instance, Ermisch (1990), or Kohler et al. (2002) conclude that postponing childbearing leads to a total decrease in fertility.

Nonetheless, this retiming of births due to policies highly depends on expectations. As Raute (2014) suggests, young people may not react to the policy if they consider that it is a durable reform as the opportunity cost they face for early motherhood is large. With the objective of evaluating if there was a retiming in first births, we specify equation (3):

$$\text{Age}_m = \alpha + \gamma_1 m + \gamma_2 m^2 + \beta \text{Post} + \lambda X_m + \sum_{i=2}^{12} \partial_i \text{Dmonth}_m + \varepsilon_m \quad (3)$$

Where the dependent variable Age_m is the average monthly age of mothers giving birth for the first time in month m and X is the number of days of month m. *Post* is a dummy variable that takes the value 1 since January, 2008. In this specification, we also control for linear and polynomial trends

that are permitted to change after the introduction of the child benefit policy. We also include calendar month dummies $Dmonth_m$ to control for seasonality in births. If the coefficient for the parameter β is statistically significant, the reform had an impact on the timing of first births.

Despite of the negative sign of the coefficients for the parameter β, shown in Table 5, we cannot say that the policy had an effect on the retiming of first births as these coefficients are not statistically significant.

Table 5: Retiming in Births

	(1) 12-24m	(2) 12-12m	(3) 9-9m	(4) 3-3m	(5) 12-24m	(6) 12-12m
Post	-0.0678	-0.025***	-0.137	-0.0319	-0.0464	-0.137***
	(0.0621)	(0.0663)	(0.0829)	(0.0480)	(0.0437)	(0.034)
Years included	2007-2009	2007-2008	2007-2008	2007-2008	2007-2009	2007-2008
N (Number of months)	36	24	18	6	36	24
Linear trend in m	Y	Y	Y	N	Y	Y
Quadratic trend in m	Y	Y	N	N	Y	Y
Number of days of the month	Y	Y	Y	Y	Y	Y
Calendar Month Dummies	N	N	N	N	Y	Y

Notes: The dependent variable is the monthly average age of first birth. Robust standard errors are shown in parentheses.
***Significant at 1 percent level.
**Significant at 5 percent level
*Significant at 10 percent level.

4.2 Heterogeneity in the response

In absence of perfect intergenerational mobility, the characteristics of the parents are relevant to explain children's achievements. To identify the effects that the *Baby Check* could have on the distribution of the following generations, we try to understand who responded the most to the policy implementation.

Age, education, and motherhood.

As we have already documented, there is evidence on the relevance of mothers' education as a predictor of children's achievements, both in terms of earnings and health (Curie (2011)).

From a theoretical point of view, Becker (1981) suggests that the family formation decision is based on the benefits and costs of having children. Thus, families demand children when the benefit of

having them exceeds their costs. Both, benefits[18] and costs are decreasing in time. Therefore, the demand for children is an intertemporal decision between having children in *t*, associated with higher utility but also higher costs, or in *t+1*, when both utility and costs are inferior. In this framework, families are indifferent between childbearing in moment *t* or in *t+1* when the marginal rate of intertemporal substitution equals the economic rate of substitution between periods, as it is indicated in Walker (1995).

There are three costs of fertility composing the shadow price of children: direct expenditures, the opportunity cost for the time caring at home[19], and the foregone return to human capital investment (there is a loss of future potential earnings, as the mother foregoes an increase in her stock of human capital). The two last costs vary among women with different age and education.

According to Happel et al. (1984), childbearing is more costly for educated women as they have a higher (actual or expected) income. However, this higher cost is importantly reduced over time. In fact, Bratti and Tatsiramos (2012) explain that educated women that already have their careers oriented do not face large opportunity costs: they do not have to substitute childbearing by labour market participation as they have better access to high quality child care services. Taking this into account, from now on, we will consider the interaction between these two variables as they may influence together the response to the financial incentive generated by the *Baby Check*.

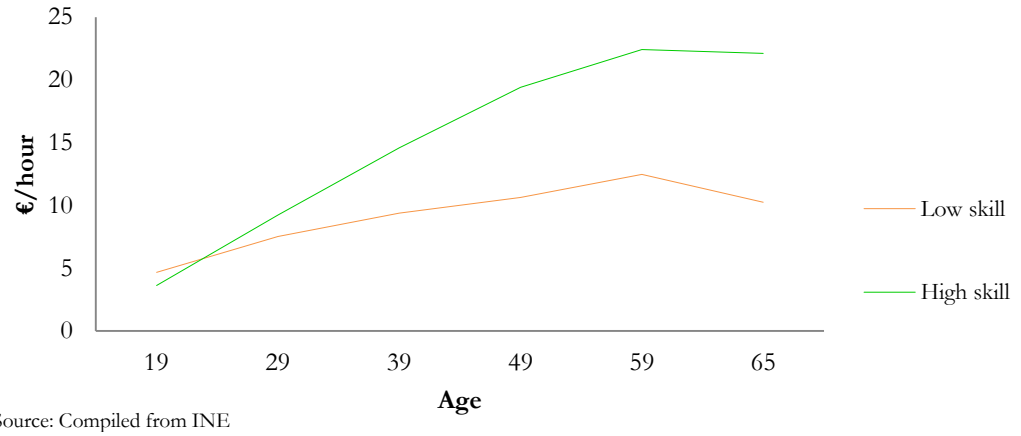
The findings of the previous authors are consistent with the negative relation usually established between education and age at first birth. Among others, Gustafson and Wetzels (2000) conclude that higher educated women have their children later than less educated women and they are also the ones that have postponed first births the most. This is explained by the larger opportunity cost derived from higher (expected) wages. Moreover, the progression of wages of more educated women tends to be steeper. This also justifies the later motherhood for this group: Cigno and Emisch (1989) and Cigno (1991) suggest that women with steeper earning profiles will have their children later. They also find that women in semi-skilled or manual occupations have earlier births than women in more skilled occupations. In fact, Figure 1 shows that native Spanish women with more education face a wage progression much steeper. In other words, women with greater initial human capital tend to have professions where the average wage per hour rises sharply with seniority.

This may offset the tendency to earlier motherhood for educated women, as it implies an important loss in actual and expected income, as well as a depreciation of their human capital. We do not expect a flat benefit of €2,500 to be enough for high educated mothers to change their behaviour, especially in comparison with low educated women. In other terms, the positive income effect that

[18] Assuming that a child always gives a positive value to its parents, the benefits of childbearing are higher the earlier childbearing happens in the life period.
[19] This cost increases with actual earnings

it is expected to have the *Baby Check* may not be large enough to compensate young educated women for the sizeable shadow price they face, Bratti and Tatsiramos (2012). However, the *Baby Check* represented 4.4 times the minimum wage at that time, an amount that can compensate the relatively low shadow price of childbearing that face uneducated young women, whose (expected) income is low.

Figure 1: Average Gross €/Hour in 2007



Source: Compiled from INE

Nevertheless, this higher cost of childbearing that face educated women may vary depending on the age. Leung et al. (2016) find that Danish educated women who have their first child after their 30s earn more in comparison with the educated Danish women with no children. As it has been mentioned, these educated and mature mothers do not have to substitute their participation in the labour market because they have a better access to childcare services and a more stable situation in the job market. Given that the utility of having children decreases with time, and that the costs may be relatively lower for educated women now, we may expect a different magnitude in the response.

Equation (4) is used to analyse if women with different education attainments react differently to the policy implementation. Consequently, in this specification, we compare *high skilled* versus *low skilled* women within two different age groups. We set the age threshold in 30 years old. Following Leung et al. (2016), having children starting on the thirties does not have large negative consequences in lifetime earnings, especially for educated women.

$$B_{m,ed,g}=\alpha + \gamma_1 m + \gamma_2 m^2 + \alpha_1 Post + \alpha_2 Educated + \alpha_3 PostEducated + \lambda X_m + \sum_{i=2}^{12} \partial_i Dmonth_m + \varepsilon_{m,\,ed,g} \quad (4)$$

Where $B_{m,ed,g}$ is the log of monthly births by educational achievement and age group of the mother. *Post* is a dummy variable that takes the value 1 since January, 2008 and X is the number of days of month m. *Educated* is a binary variable that takes the value 1 when the mother has upper secondary studies or tertiary education and 0 if she has no education, primary or first degree of secondary education. *Post·Educated* is the interaction between *Post* and *Educated*. It represents how educated mothers are affected by the reform in comparison with uneducated mothers. As usually, we control for linear and polynomial trends that are allowed to change after the policy implementation. In addition, we control for seasonality by using calendar month dummies.

Tables 6 and 7 come from the estimation of equation (4). The first one corresponds to mothers that are younger than 30 years old. The results suggest that *uneducated* women were statistically more affected than *educated* mothers. In fact, the fertility of *young educated* women experienced an immediate increase of 8% after the policy implementation, compared with a total average short run overall 13% increase in fertility.

Table 6: Results by Education for *Young* Mothers

	(1) 12-24m	(2) 12-12m	(3) 9-9m	(4) 3-3m	(5) 12-24m	(6) 12-12m
Post	0.1720***	0.1304***	0.1325***	0.0135	0.1916***	0.1696***
	(0.0359)	(0.0351)	(0.0354)	(0.0445)	(0.0403)	(0.0526)
Educated	-0.697***	-0.6973***	-0.7092***	-0.758***	-0.6973***	-0.6973***
	(0.0175)	(0.0172)	(0.0201)	(0.0493)	(0.0163)	(0.0164)
Post·Educated	-0.0754***	-0.0722***	-0.0458*	-0.000999	-0.0754***	-0.0722***
	(0.0212)	(0.0227)	(0.0251)	(0.0518)	(0.0187)	(0.0215)
Years included	2007-2009	2007-2008	2007-2008	2007-2008	2007-2009	2007-2008
Number of months	36	24	18	6	36	24
Linear trend in m	Y	Y	Y	N	Y	Y
Quadratic trend in m	Y	Y	N	N	Y	Y
Number of days of the month	Y	Y	Y	Y	Y	Y
Calendar month dummies	N	N	N	N	Y	Y

Notes: The dependent variable is the log of monthly births in Spain separating between young educated and young non educated women. Robust standard errors are shown in parentheses
***Significant at 1 percent level.
**Significant at 5 percent level
*Significant at 10 percent level.

Table 7 shows that, among mothers that were at least 30 years old, the policy supposed an 8% increase on immediate fertility. This effect was statistically larger for educated women, who reacted with an immediate 12% increase in fertility. As it is found by Leung et al. (2016), this group of mothers have already their career oriented and they do not face losses in income or human capital. In addition, they do not have to substitute their labour participation for family labour as they can easily conciliate both due to the fact that they have more access to childcare services, as it is suggested in Bratti et al. (2012).

Table 7: Results by Education for Mature Mothers

	(1) 12-24m	(2) 12-12m	(3) 9-9m	(4) 3-3m	(5) 12-24m	(6) 12-12m
Post	0.0808***	0.0692***	0.0801*	0.0806*	0.1030***	0.0646*
	(0.0278)	(0.0238)	(0.0419)	(0.0359)	(0.0339)	(0.0331)
Educated	0.6211***	0.6211***	0.6234***	0.6214***	0.6211***	0.6211***
	(0.0136)	(0.0137)	(0.0152)	(0.0179)	(0.0111)	(0.0109)
Post ·Educated	0.101***	0.0588***	0.0530**	0.0456*	0.1010***	0.0589***
	(0.0177)	(0.0172)	(0.0199)	(0.0198)	(0.0154)	(0.0146)
Years included	2007-2009	2007-2008	2007-2008	2007-2008	2007-2009	2007-2008
Number of months	36	24	18	6	36	24
Linear trend in m	Y	Y	Y	Y	Y	Y
Quadratic trend in m	Y	Y	N	N	Y	Y
Number of days of the month	Y	Y	Y	Y	Y	Y
Calendar month dummies	N	N	N	N	Y	Y

Notes: The dependent variable is the log of monthly births in Spain separating between old educated and old non-educated women. Standard errors are shown in parentheses
***Significant at 1 percent level.
**Significant at 5 percent level
*Significant at 10 percent level.

Up to now we have found that within *young* women the *Baby Check* affected statistically more to *uneducated* women and among more *mature* women, the impact was larger for *educated* women. The literature determines that, on average, the likelihood of success may be higher for children of these *educated mature* women. As the effect of this universal child benefit on long term inequality is a concern, we specify equation (5). We aim to distinguish which of these four groups reacted the most to the policy implementation. The benchmark is the group of *mature educated* mothers.

$$\begin{aligned} B_{m,ed,a} = {} & \alpha + \gamma_1 m + \gamma_2 m^2 + \alpha_1 Post \\ & + \alpha_2 YoungEducated + \alpha_3 YoungUnducated + \alpha_4 MatureUnducated \\ & + \alpha_5 PostYoungEducated + \alpha_6 PostYoungUnducated + \alpha_7 PostMatureUnducated \\ & + \lambda X_m + \sum_{i=2}^{12} \partial_i Dmonth_m + \varepsilon_{m,ed,a} \end{aligned} \quad (5)$$

The results are shown in Table 8. The focus is distinguished between young uneducated women and mature educated women, as were the ones that reacted the most in the aforementioned groups. We present some evidence that suggests that *young uneducated* women were statistically less affected by the policy than *mature educated* women. However, this result is quite weak, and only statistically significant at a 10%. Trying to clarify this, we

eliminate the age variable and we compare educated with uneducated women (Table 3, Appendix). The evidence is too weak to ensure that *educated* women were statistically different affected by the policy than *uneducated* women.

Table 8: Comparison among Different Age and Educational Groups

	(1) 12-24m	(2) 12-12m	(3) 9-9m	(4) 3-3m	(5) 12-24m	(6) 12-12m
Post	0.2282***	0.1535***	0.139***	0.0391	0.2491***	0.171***
	(0.0291)	(0.0247)	(0.0253)	(0.0227)	(0.0330)	(0.0334)
Young ·Uneducated	-0.8072***	-0.8072***	-0.813***	-0.811***	-0.8072***	-0.807***
	(0.0154)	(0.0149)	(0.0173)	(0.0367)	(0.0132)	(0.0129)
Young ·Educated	-1.5045***	-1.5045***	-1.522***	-1.569***	-1.5045***	-1.505***
	(0.0170)	(0.0174)	(0.0188)	(0.0379)	(0.0161)	(0.0157)
Mature· Uneducated	-0.6211***	-0.6211***	-0.623***	-0.621***	-0.6211***	-0.621***
	(0.0137)	(0.0140)	(0.0147)	(0.0400)	(0.0124)	(0.0120)
Post ·Young ·Uneducated	-0.1026***	-0.0485**	-0.0410*	-0.0259	-0.1026***	-0.0485***
	(0.0207)	(0.0191)	(0.0221)	(0.0383)	(0.0182)	(0.0170)
Post ·Young ·Educated	-0.1780***	-0.1207***	-0.0868***	-0.0269	-0.1780***	-0.0121***
	(0.0221)	(0.0224)	(0.0245)	(0.0395)	(0.0210)	(0.0200)
Post ·Mature· Uneducated	-0.1010***	-0.0588***	-0.0530***	-0.0456	-0.1010***	-0.0588***
	(0.0186)	(0.0175)	(0.0194)	(0.0405)	(0.0172)	(0.0155)
Years included	2007-2009	2007-2008	2007-2008	2007-2008	2007-2009	2007-2008
Number of months	36	24	18	6	36	24
Linear trend in m	Y	Y	Y	N	Y	Y
Quadratic trend in m	Y	Y	Y	N	Y	Y
Number of days of the month	Y	Y	Y	Y	Y	Y
Calendar month dummies	N	N	N	N	Y	Y

Notes: The dependent variable is the log of monthly births in Spain separating between educated and non-educated women. Robust tandard errors are shown in parentheses
***Significant at 1 percent level.
**Significant at 5 percent level
*Significant at 10 percent level.

Civil status effect

The civil status of the mother is a relevant variable to explain both the financial status of the household and the outcomes of the children. Norton and Glick (1979) indicate that the average single woman heading a household has significantly lower income than any other household headed by any other family group. This means that these families may experience a larger income effect as consequence of a cash benefit, but also that this may have negative consequences for children's outcomes. As a matter of fact, Save the Children (2015) reports that half of children that live only with their mothers are in serious risk of poverty in Spain. The impact that this initial poverty may

have on the outcomes of children is very serious, in terms of physical and mental health, and also regarding educational attainments. These inequalities that start when (or even before) we are born are very likely to persist over all the lifetime (Currie (2011)).

Due to the considerable impact that the civil status of the mother can have on future children's achievements, we aim to understand whether the reform affected more single or married women using equation (6).

$$B_{m,cs} = \alpha + \gamma_1 m + \gamma_2 m^2 + \alpha_1 Post + \alpha_2 Married + \alpha_3 PostMarried + \lambda X_m + \sum_{i=2}^{12} \partial_i Dmonth_m + \varepsilon_{m,cs} \quad (6)$$

Where $B_{m,cs}$ is the log of monthly births by civil status group. *Post* is a binary variable that takes the value 1 since January, 2008 and X is the number of days of month m. *Married* is a dummy variable that takes the value 1 when the mother is married (without including cohabiting partners) and 0 when she is single (without including divorced and widow mothers). *Post·Married* is the interaction between the variables *Post* and *Married*. It represents how married mothers are affected by the reform in comparison with single mothers. We allow for linear and polynomial trends that can change after the policy implementation. We also control for month fix effects in order to show the robustness of our estimators after controlling for seasonality.

Brewer et al. (2012) find that the welfare reforms made by the UK government affected mainly married couples. Baughman and Dickert-Conlin (2003) find that married couples were also more affected by the US *Aid to Families with Dependent Children*. In Table 9, we show the results of estimating equation (6). Our findings suggest that the immediate increase in fertility of single women explains the overall short-run increase in fertility. Our results are still robust after controlling for seasonality[20].

Despite the fact that this result does not go in the direction of the findings of other papers, it is consistent with the outcomes of González (2013): She indicates that a part of the immediate increase in fertility derived from this policy is due to a decrease in the number of abortions. Finer et al. (2005) suggest that one of the core reasons why women have abortions is the rejection to the idea of being a single mother, due, in part, to the financial constraints they face. Taking together, the financial aid may have had an important income effect in single women that were facing the decision of having or not an abortion.

[20] In the Appendix (Tables 3 and 4) we show the results of equation (6) after dividing for *young* and *mature* women. The conclusions are the same: the *Baby Check* led to an immediate increase in fertility, but this increase is derived from single and non from married women.

Table 9: Results by Civil Status

	(1) 12-24m	(2) 12-12m	(3) 9-9m	(4) 3-3m	(5) 12-24m	(6) 12-12m
Post	0.2583***	0.1961**	0.1746**	0.0758**	0.2947***	0.2312***
	(0.0748)	(0.0738)	(0.0830)	(0.0280)	(0.0840)	(0.1143)
Married	1.915***	1.915***	1.894***	1.753***	1.9150***	1.9150***
	(0.0280)	(0.0288)	(0.0361)	(0.0358)	(0.0277)	(0.0289)
Post · Married	-0.326***	-0.265***	-0.224***	-0.0779*	-0.3256***	-0.02651***
	(0.0322)	(0.0326)	(0.0393)	(0.0392)	(0.0318)	(0.0332)
Years included	2007-2009	2007-2008	2007-2008	2007-2008	2007-2009	2007-2008
Number of months	36	24	18	6	36	24
Linear trend in m	Y	Y	Y	N	Y	Y
Quadratic trend in m	Y	Y	N	N	Y	Y
Number of days of the month	Y	Y	Y	Y	Y	Y
Calendar Month Dummies	N	N	N	N	Y	Y

Notes: The dependent variable is the log of monthly births in Spain separating between single and married women. Robust standard errors are shown in parentheses
***Significant at 1 percent level.
**Significant at 5 percent level
*Significant at 10 percent level.

5. Conclusions

Understanding the financial incentives effects of family policies on fertility is especially relevant to mitigate the negative consequences of the demographic transition that the developed world is facing, as well as to avoid potential unintended side effects of the reforms.

This paper evaluates the effect of a universal child benefit of €2,500 introduced unexpectedly in Spain in 2007. Our findings suggest that this financial aid led to an immediate 8% increase in fertility, having a special impact on second order births. In addition, there is evidence suggesting a permanent increase in childbearing as women near the end of their fertile lifetime were the ones who responded the most. These women are likely to be completing their fertility cycle rather than shifting the timing of births. We also find that among women who were less than 30 years old, those with less education were more likely to have a(an additional) child. Within the group of more mature women, this universal financial aid impacted mainly women with more education achievements. Furthermore, our results suggest that single women responded in the short run significantly more than married women. These differences in the reactions may have serious consequences for future generations. Cervini-Plá et al. (2013) document a high degree of correlation between parents' and children's education and employment in Spain. In fact, they conclude that *(…) those born into a poor family will remain poor. Moreover, since these families are those with less mobility, these*

individuals are those most commonly kept in their situation of origin. In addition, a report of Save the Children (2015) finds that in Spain there are more than 1,000,000 households headed by single women, and more than one half of these households are in risk of poverty. As there is not a flexible intergenerational mobility (Hertz et al. (2007)), this policy may actively contribute to the enlargement of inequalities for future generations.

There is, thus, a concern for the design of pro-birth policies. Maybe the most interesting pro birth policy is the one that answers the question of Richard Ely: *What types of interventions are most likely to give children "a fair start in life"?* Of course, the analysis we did is a very short-run analysis. Thus, our results may be taken with caution.

Regarding the possible extensions of the paper, we aim to look at the long term effect of the *Baby Check* from the children's perspective. González (2013) finds that mothers spend more time at home after the policy. The effect of this larger artificial *maternal leave* may have had a positive impact on children's achievements. Thus, we would like to check if this larger period of time that mothers spend taking care of their children could have had a positive effect on children's achievements in school, as Carneiro et al. (2010) find for the case of Norway. In addition, it is also very interesting analyzing how foreign women react to the policy, taking into account the country of origin.

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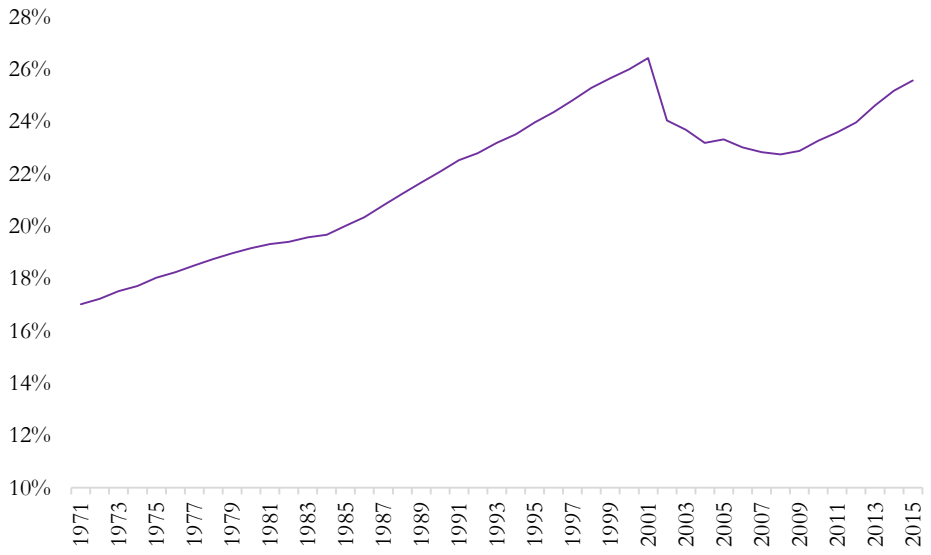
Appendix

Table 1: Fertility and Life Expectancy across Selected Countries.

Variable	Country/Year	2005	2007	2009	2011
Fertility rate	Denmark	1.80	1.84	1.84	1.75
	Finland	1.80	1.83	1.86	1.83
	Iceland	2.05	2.09	2.23	2.02
	Spain	**1.33**	**1.38**	**1.38**	**1.34**
	Sweden	1.77	1.88	1.94	1.90
Life expectancy	Denmark	77.6	77.7	78.3	79.1
	Finland	78.4	78.8	79.3	79.8
	Iceland	80.7	80.7	80.9	81.5
	Spain	**79.6**	**80.4**	**81.2**	**81.8**
	Sweden	79.9	80.3	80.7	81.0

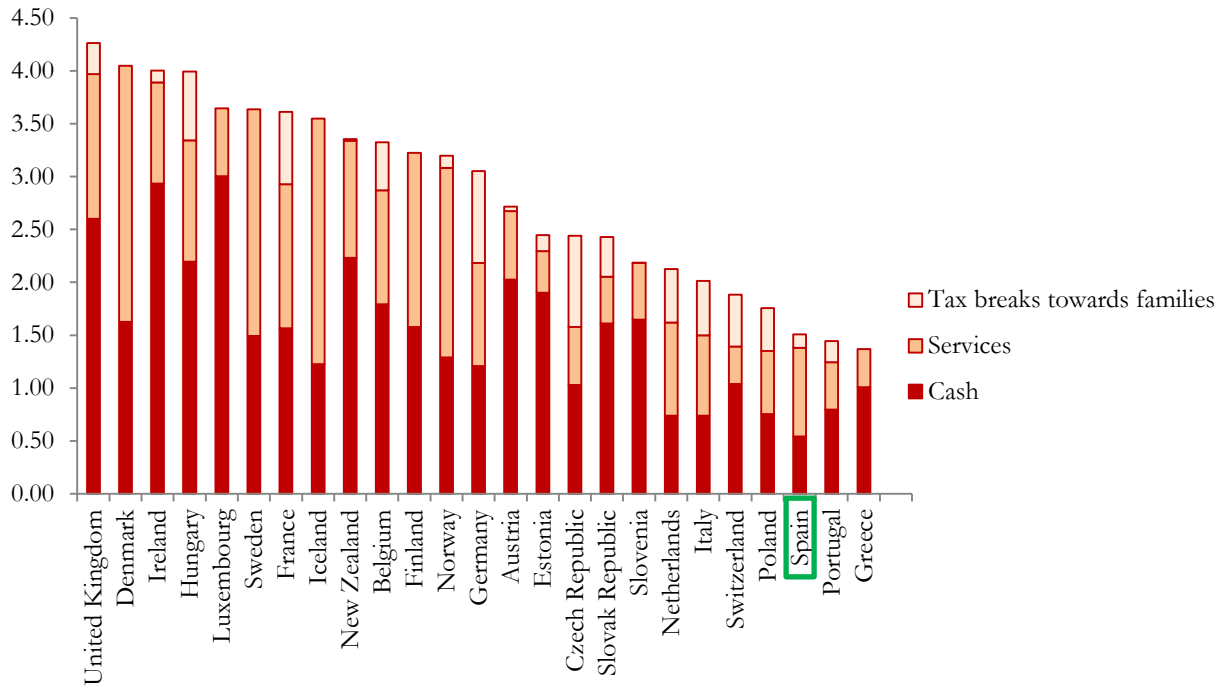
Note: The information is obtained from Eurostat statistics. Nordic countries have a fertility rate near to the replacement level.

Figure 1: Dependency Rate over Time



Note: The information is obtained from INE demographic statistics. The dependency rate measures the relation between people who are older than 65 years old with respect to people who are from 16 years old to 64 years old.

Figure 2: Public Spending On Family Benefits in Cash, Services and Tax Measures, In Per Cent of GDP, 2011



Note: The information is obtained from OCDE database

Table 2: Results by Education

	(1) 12-24m	(2) 12-12m	(3) 9-9m	(4) 3-3m	(5) 12-24m	(6) 12-12m
Post	0.0933***	0.0778***	0.0684***	-0.000404	0.118***	0.0859***
	(0.0246)	(0.0202)	(0.0240)	(0.0457)	(0.0301)	(0.0227)
Educated	0.217***	0.217***	0.218***	0.208***	0.217***	0.217***
	(0.0136)	(0.0133)	(0.0149)	(0.0422)	(0.0116)	(0.0114)
Post ·Educated	0.0719***	0.0332*	0.0324	0.0320	0.0719***	0.0332**
	(0.0176)	(0.0169)	(0.0198)	(0.0428)	(0.0152)	(0.0150)
Years included	2007-2009	2007-2008	2007-2008	2007-2008	2007-2009	2007-2008
Number of months	36	24	18	6	36	24
Linear trend in m	Y	Y	Y	Y	Y	Y
Quadratic trend in m	Y	Y	N	N	Y	Y
Number of days of the month	Y	Y	Y	Y	Y	Y
Calendar month dummies	N	N	N	N	Y	Y

Notes: The dependent variable is the log of monthly births in Spain separating between educated and non-educated women. Robust sandard errors are shown in parentheses
***Significant at 1 percent level.
**Significant at 5 percent level
*Significant at 10 percent level.

Table 3: Results by Civil Status Young

	(1) 12-24m	(2) 12-12m	(3) 9-9m	(4) 3-3m	(5) 12-24m	(6) 12-12m
Post	0.266***	0.186**	0.184*	0.0455	0.309***	0.256*
	(0.0915)	(0.0905)	(0.101)	(0.0336)	(0.1048)	(0.1416)
Married	1.054***	1.054***	1.027***	0.852***	1.054***	1.054***
	(0.0336)	(0.0346)	(0.0434)	(0.0466)	(0.0342)	(0.0359)
Post · Married	-0.364***	-0.292***	-0.232***	-0.0530	-0.364***	-0.292***
	(0.0393)	(0.0398)	(0.0462)	(0.0516)	(0.0394)	(0.0411)
Years included	2007-2009	2007-2008	2007-2008	2007-2008	2007-2009	2007-2008
Number of months	36	24	18	6	36	24
Linear trend in m	Y	Y	Y	N	Y	Y
Quadratic trend in m	Y	Y	N	N	Y	Y
Number of days of the month	Y	Y	Y	Y	Y	Y
Calendar Month dummies	N	N	N	N	Y	Y

Notes: The dependent variable is the log of monthly births in Spain separating between young single and young married women. Robust standard errors are shown in parentheses
***Significant at 1 percent level.
**Significant at 5 percent level
*Significant at 10 percent level.

Table 4: Results by Civil Status Old

	(1) 12-24m	(2) 12-12m	(3) 9-9m	(4) 3-3m	(5) 12-24m	(6) 12-12m
Post	0.312***	0.244***	0.202**	0.110***	0.331***	0.248**
	(0.0773)	(0.0744)	(0.0829)	(0.0240)	(0.0874)	(0.116)
Married	2.452***	2.452***	2.429***	2.297***	2.452***	2.452***
	(0.0270)	(0.0281)	(0.0341)	(0.0345)	(0.0260)	(0.0271)
Post · Married	-0.384***	-0.302***	-0.261***	-0.116**	-0.384***	-0.302***
	(0.0325)	(0.0324)	(0.0388)	(0.0364)	(0.0325)	(0.0322)
Years included	2007-2009	2007-2008	2007-2008	2007-2008	2007-2009	2007-2008
Number of months	36	24	18	6	36	24
Linear trend in m	Y	Y	Y	N	Y	Y
Quadratic trend in m	Y	Y	N	N	Y	Y
Number of days of the month	Y	Y	Y	Y	Y	Y
Calendar Month dummies	N	N	N	N	Y	Y

Notes: The dependent variable is the log of monthly births in Spain separating between young single and young married women. Robust standard errors are shown in parentheses
***Significant at 1 percent level.
**Significant at 5 percent level
*Significant at 10 percent level.