The Effects of Financial Incentives on Fertility and Early Investments in Child Health

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Abstract
In a quasi-experimental setting, this paper investigates the effects of financial incentives, created by maternity leave benefits, on fertility, reproductive behavior and early investments in child health. I exploit a largely unanticipated policy change in the maternity leave benefits (MLB) law in Romania in 2004 which entailed a switch from proportional with pre-birth earnings to fixed, and very high, benefits. For most employed women the change entailed a significant increase in the potential benefits to be received after birth, but high earning women were disadvantaged by the change. I use Reproductive Health Survey data in a Double Difference design, and compare fertility outcomes of employed women to those of out of the labor force women. I analyze conception rates, probability of abortion, and maternal investments in the child's human capital. The main findings suggest that the substantial increase in financial incentives led to an increase in conception rates and a decrease in the probability of abortion, especially for women from poorer households, who benefited more from the policy change. All employed mothers appear to have worse prenatal behaviors, but poorer mothers have children with better health outcomes at birth, whereas richer mothers who were disadvantaged by the policy make more investments in early child health.

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1 Introduction

Maternity leave benefits (MLB), in the form of entitlements to protected and most often paid leave after the birth of children, are support schemes that are part of family policies and aim to subsidize childbearing and encourage fertility. All OECD countries with the exception of the US have in place governmentally funded policies granting paid maternity leave for employed parents and the median duration of the leave increased from 14 weeks in 1980 to 42 in 2011.[1] The design of these benefits, however, differs significantly across countries, with varying replacement rates of the labor income and lengths of time for receiving benefits after the birth of the child. The varying incentive structure reflects different policy objectives and can influence fertility decisions; for instance, a fixed entitlement favors low income families, whereas proportional transfers conditional on the pre-birth labor income encourage female labor market integration and a more rapid return to the labor force after the birth of the child. But despite their wide implementation, there is still relatively scarce evidence of the effects of maternity leave benefits policies on fertility, mostly due to difficulties in finding exogenous changes in these financial incentives and the opportunity cost of children.

In this paper, I exploit an unexpected turn in the legislative process, announced in 2003 and implemented in 2004 in Romania, which radically changed the way in which maternity leave benefits were awarded. This change significantly altered the opportunity cost of childbearing for employed mothers, but did not affect out of the labor force women. Prior to the policy change, MLB were awarded proportional to the mother's pre-birth income. The monthly benefit amounted to 85% of the taxable average earnings in the 10 months preceding childbirth, and was awarded for a maximum of two years. After an unexpected policy change, the benefits requested after January 1, 2004 were awarded as a fixed sum equal to 85% of the national average salary of *both* men and women. Benefits requested prior to that date were unaffected and continued to be calculated proportional to each mother's pre-birth earnings. Because of a very large gender gap in wages, calculating the fixed MLB in reference to the national average salary meant that more than 80% of employed women would potentially gain from the reform; moreover, the potential increase in nominal terms was very large, the fixed benefit being one third higher than the average (potential) proportional benefit in the previous regime.

As the reform drastically and unexpectedly changed the financial incentives of fertility, this policy change provides an excellent natural experiment to investigate several aspects related to fertility behavior and child health investments. As such, this paper contributes to the literature on the effects of maternity leave benefits on fertility by providing a comprehensive analysis of the behavioral responses of fertile age women to financial incentives. The analysis includes both conception behavior and abortions, which are not observed in register data, as well as prenatal and postnatal investments.

[1] Source: OECD Family Database.

This paper addresses the following three research questions. First, is there a change in the conception behavior induced by the prospect of increased financial incentives for most employed women? Second, conditional on a conception having occurred, does the policy change announcement alter the probability that the pregnancy ends in abortion as opposed to live birth? This is especially relevant in the Romanian context since abortion rates have been, historically, among the highest in Europe after the (re)liberalization of abortions in 1990. The abortion rate in Romania in 2002 was of 1174 abortions per 1000 live births, relative to an European average of 274 abortions per 1000 live births (source: World Health Organization), since abortions were regarded as a contraception method. In this setting, it is very important to understand if financial incentives can influence this aspect of reproductive behavior. Finally, conditional on a pregnancy being carried to term, is there any effect on the health outcomes at the birth of the child and on the early investments in child health?

The Becker model of fertility (Becker (1991)) predicts that the relative increase (decrease) in maternity leave benefits will lead to higher (lower) fertility levels, but also to a decreased (increased) quality of children. Quality of children can be influenced through prenatal investments, child health at birth and early investments in infant health. However, in addition to the price effect, the change in MLB also induces an income effect, which may affect investments in the quality of children in the opposite direction, leading to an ambiguous net effect on the quality of children. This paper aims to bring empirical evidence of the short term effects of maternity leave benefits on the quantity and quality of children, in a reduced form.

The analysis uses the 2004 wave of the Romanian Reproductive Health Survey (RHS-Ro), containing a representative sample of 4441 fertile aged women. The survey includes detailed information about all pregnancies, irrespective of how they ended, detailed information about health outcomes and investments in the youngest child, as well as socio-economic characteristics of the woman and the household in which she lives.

The primary strategy to identify the effects of the policy change announcement is a Difference in Difference design that exploits the fact that only employed women were affected by the policy change, and uses out of the labor force women as a control group, as they were, theoretically, unaffected by the policy. Although marginally insignificant, the findings indicate that in the post policy change announcement period, employed women had increased conception rates a and decreased abortion probabilities relative to housewife women. The effect is driven by women from poorer households, who benefited more from the policy change, with women from richer households having, if anything, an opposite behavior. All employed women appear to have worse prenatal behaviors, but women who benefited from the change have children with better health outcomes at birth. Women who were negatively affected by the policy change compensate by investing more in early infant health. Unfortunately, most treatment effects are statistically indistinguishable from 0 at conventional significance levels. The main robustness check I conduct is a mother fixed effects design, with the results pointing in the same direction as my main specification.

The paper expands the literature on the relationship between financial incentives and fertility, with the main contribution of a comprehensive analysis of reproductive behavior and early investments in child health as a response to changes in financial incentives. Previous studies analyzed one or few outcomes: using time series of births from vital statistics, they were constrained to analyzing only the effect on pregnancies carried to term Baughman and Dickert-Conlin (2003); Milligan (2005); González (2013), and in very few cases aggregate data on number of abortions González (2013). Due to the nature of the Romanian Reproductive Health Survey, I explore the entire spectrum of individual level decisions related to fertility: decision to conceive, decision to carry the pregnancy to term, and several important outcomes conditional on live birth, such as maternal behavior during pregnancy, child health at birth and early investments in child health. This is, to my knowledge, the first study to use reproductive health surveys to evaluate the effects of financial incentives on fertility outcomes in the context of quasi-natural experiments. Secondly, the Romanian policy reform modified only the level of the financial benefits attached to childbirth for employed mothers, and not the length of time for which they were awarded; some policies previously used for identification entailed a change in both the financial benefits and the length of time of protected employment Carneiro et al. (2011), making it more difficult to disentangle the effects attributable to monetary incentives. This paper investigates the exclusive role of pecuniary incentives in determining fertility and early investments in child health. Finally, the policy change exploited in this paper affects a very large share of the population, namely employed women, by a large margin and applies to births of any rank. Some previous work exploits changes which only affected a particular subgroup of the population, for example women at the third or higher birth Cohen et al. (2013) or women working in large firms Rossin (2011), thus limiting the external validity and the generalization of the findings.

The remainder of the paper proceeds as follows. Section 2 offers a review of the literature on financial incentives and fertility, presents the details of the Romanian policy reform affecting the maternity leave benefits and discusses the potential effects of the policy change, within the Becker framework of fertility demand. Section 3 describes the Romanian Reproductive Health Survey and the analysis sample. Section 4 presents the identification strategy, a Difference in Difference design that uses out of the labor force mothers as a control group, and discusses threats to identification. Section 5 presents the main results of the paper, for the three categories of outcomes of interest, and shows the heterogeneous effects with respect to household asset index, a proxy for treatment intensity. Section 6 discusses robustness checks and alternative specifications, including a mother fixed effects design. Section 7 concludes.

2 Background

2.1 Institutional details of the reform

Romania, European Union's seventh largest member state by population size but second to last by GDP per capita, provides excellent opportunities to study the determinants of early life environment and fertility. Between 1966 and 1989, the communist regime in place in Romania instituted family policies aimed at rapid population growth by imposing a strict, centrally planned, fertility control. This included an abortion and contraceptives ban, mandatory fertility controls for fertile age women, and supplementary taxes for childless families. After the fall of the regime, in December 1989, Romania liberalized fertility choices, removed the ban on abortions and transitioned to a system of family policies centered around proportional maternity leave benefits. Although paid at various rates, MLB were constantly awarded on a contribution-based manner proportional to pre-birth earnings (ranging from 65 to 85% of the pre-birth taxable income of the mother), and were paid for a period between one and two years.

Starting with April 2000, to receive the MLB, a mother needed to (i) be a tax contributor for at least 6 of the 12 months preceding the birth of the child, and (ii) needed to apply for the MLB after the birth of the child. The quantum of the benefit was calculated as 85% of the mother's earnings (taxable labor income) averaged over the 10 months preceding the birth of the child. There was no minimum or maximum cap set for the MLB, and the benefit was awarded for a period of two years, until the child reached the age of two years, or three years for children with disabilities. Although mothers could formally return to work before the child's second birthday without loosing the benefit, the very low availability of formal child care for children below the age of three entailed that mothers would most often stay at home for the entire duration of the protected leave - more on this issue will be discussed later in the next section.

In 2003, the average female monthly taxable earnings were 5,910,000 RON (approximately 180 USD), thus the average (potential) MLB was 5,023,500 RON (approximately 154 USD).[2] In March 2003, the Romanian Government concluded that the MLB entailed disproportional costs relative to other social security benefits, and issued an Emergency Ordinance[3] to modify the MLB so as to reduce public expen-

[2]The average taxable earnings, and hence the maternity leave benefits, for fertile age women and in particular for women becoming mothers may be different, but there is no official data on the labor income of these categories.

[3]An Emergency Ordinance is issued by the Government, which holds the executive power, and is an exceptional prerogative that intervenes in the legislative process, which is normally the responsibility of the Parliament. It comes into effect after it is published in the Official Monitor and after the Parliament has been notified for debate, de facto changing the laws it refers to. The two Chambers of the Parliament must convene to debate the Emergency Ordinance and emits a law of approval or rejection of the Emergency ordinance. However, until this acceptance or rejection law is passed by both the Chambers of the Parliament, the Emergency Ordinance produces legal effects. In Romania, Emergency Ordinances are a common procedure, and in the overwhelming majority they are approved by the Parliament, as the Government which proposes the Ordinance is necessarily formed from the party or alliance which has the majority in the Parliament.

ditures with these benefits. The Emergency Ordinance[4] set a maximum cap for all benefits, starting with January 1, 2004. The maximum sum to be paid as MLB was calculated as 85% of the official national average salary, leaving unchanged the MLB smaller than the cap. The Emergency Ordinance also extended the mandatory contribution period to 10 out the 12 months preceding the birth of the child.

The legislative changes were met with opposition from the civil society and the opposition parties in the Parliament; in a sudden change of strategy, the Government issued another Emergency Ordinance[5] not even one month later, on April 14, 2003, which significantly altered the way the MLB were awarded, citing equality of opportunity arguments. Mothers who would apply for MLB after January 1, 2004 would receive a fixed benefit, irrespective of their pre-birth earnings; this benefit was set at 85% of the official national average salary. The required contribution period for mothers applying after January 1, 2004 was set at 10 out the 12 months preceding the birth of the child. Mothers who were already receiving MLB and those who would apply for MLB until December 31, 2003 would not be affected: they would continue to receive the proportional benefits, calculated as 85% of their average pre-birth income, with no maximum cap, even after January 2004. They were *not* entitled to re-apply for the benefits and it was impossible for them to receive the fixed MLB. For these mothers, the required tax contribution period remained 6 out of the 12 months preceding the birth of the child. Since the new law was based on the application date to the MLB and not on the date of birth of the child, a mother could delay the application to the benefits until January 2004 *only* if she had not yet applied; this issue will be discussed in more detail shortly. The benefit was awarded for a maximum period of two years, until the child reached the age of two (or three for children with disabilities), irrespective of when the mother applied for the MLB.[6] The bill was passed into law in October 2003, and gained rapid popular support.

The official national average salary[7] to be used in the calculation of the MLB was 7,682,000 lei (235 USD); the fixed MLB level was set at 6,529,700 lei (200 USD). In November 2003, approximately 80% of employed women had an after-tax income smaller than the fixed MLB set for 2004, and the fixed MLB would be approximately 30% higher than the proportional MLB based on the average female taxable labor income in 2003. (Source: Statistics Romania). This suggests that the vast majority of employed women would receive a significantly higher MLB under the fixed scheme than under the proportional scheme if they applied for the benefit after January 2004.[8] It is important to bear in mind that mothers who would have applied for the

[4]EO 9/2003 issued on March 19, 2003.

[5]EO 23/2003

[6]The duration of awarding the MLB was modified in 2005 to be applied starting with 2006, when it would be discontinued when the mother returned to work; she would then receive an additional and fixed benefit entitled "reinsetion stimulent" to compensate for the loss of the MLB.

[7]The official national average salary was set yearly in the Law of Social Insurance Budget, and was the same for men and women. For 2004, the value of the official national average salary was published in December 2003.

[8]The fact that most working age mothers could potentially benefit from the policy change was acknowledged in the Parliamentary debates, with a Government representative declaring that an estimated 92% of the potential mothers would benefit from the reform.

MLB prior to January 2004 would continue to receive MLB proportional to their pre-birth earnings.

A key fact for the identification strategy is that the policy change was unexpected. I argue this is most likely the case with the second and final policy change, at least in terms of direction and magnitude. First, the Government initially attempted to limit the public expenditures on MLB by placing a cap on all paid benefits, but shortly after reconsidered the measure and awarded a fixed benefit for all new MLB requests. Second, this was the first time after the fall of communism that MLB would be paid as a fixed amount, and not proportional to pre-birth earnings of the mother. In fact, the quantum of the fixed benefit was so large relative to the average MLB previously paid, that in 2004 the total amount paid as MLB from the social security budget was 2.2 times larger than that paid in 2003 (source: National Bank of Romania Annual Report 2004). Third, in the central media the topic of changing MLB was only discussed in March-April 2003, when the Emergency Ordinances were passed, and again in October 2003 when the law was also changed.[9]

Women who had not earned taxable income in the previous 10 months, so out of the labor force women (most of whom are housewives) - are not eligible for the maternity leave benefits, but do qualify for a fixed child support allowance which is given for all children irrespective of the occupational status of the mother, which was substantially lower than the maternity leave benefit. Between 2002 and 2004, the child state allowance was 225,000 ROL (6,88 USD), and between 2004 and 2005 is was 240,000 ROL (between 7,40 USD and 8,30 USD) - therefore approximately 4 percent of the quantum of the fixed maternity benefit to which employed mothers were entitled to.

The monthly number of births in Romania for the period 2000-2010, together with a de-seasonalized series, which controls for month of the year dummies, and fitted values are presented in Figure 1. In the de-seasonalized data, there is a very steep downward trend in the pre-2004 period, whereas after 2004 there is almost no trend in the residuals. Formally testing for a discontinuous jump in the number of births after January 2004 (incidentally 9 months after the policy change announcement), after including a third order polynomial in month-year of births to account for smooth fertility trends and for seasonality through calendar month dummies, gives an estimate of 749 (s.e 191) births extra per month, which is approximately 4% of the pre-policy average monthly number of births. This is entirely driven by the

[9]"Adevarul", a nationwide daily newspaper with one of the highest circulations in written press, published related articles only between March 26, 2003 and April 14, 2003 with an average of two articles daily (Source: author's content analysis on the 2002 and 2003 "Adevarul" archive). A potential concern is related to differential access to information between high and low earning women, or different perceptions about the probability that the announced change would actually be implemented. However, this is not likely to pose threats to the identification since low earning women, who may be considered as having lower access to information, are actually not exclusively low educated women -in 2003, a very large share of low earning women were employed in the educational and health sector, which had amongst the lowest wages in the economy, and the highest educational level.

increase in the number of monthly births by employed women (an increase of 612, s.e. 112, which is approximately 10% of the monthly number of births of employed women), whereas there is no such discontinuous change in the monthly number of births by housewife women. This is indicative evidence that the MLB policy may have reversed the downward trend in natality.

Selection effects The changes came in effect for all benefits requested after January 1, 2004. Unfortunately, there is not a sharp discontinuity based on the date of the birth of the child, since the MLB can be requested at any time until the child turns two years. Although mothers giving birth after January 2004 would receive the fixed MBL with certainty (since one cannot apply for MLB before the birth of the child), there could also be mothers giving birth in 2003 that receive the fixed MLB and not the proportional ones. Mothers giving birth in 2003 after the final policy change announcement could strategically delay the application process until 2004, foregoing several months of proportional MLB to receive the fixed MLB for less than two years. This issue could be attenuated by the fact that some mothers giving birth in 2003 planned the birth of their child considering the 6 months tax contribution requirement, so delaying application until January would be impossible as they would no longer qualify for MLB.

In terms of potential selections into conception and into live birth which could occur due to the possibility of delaying application to MLB, the pregnancies conceived before the announcement that were carried to term or terminated before April 2003 should be unaffected by the policy change announcement. For the pregnancies conceived before the announcement that were above the legal abortion limit (the first trimester of pregnancy) at the time of the announcement, in April 2003, there should be no selection into conception among women, irrespective of their earnings, and there should be no selection into live birth induced by the announcement. Although these children would be born in 2003, prenatal and child investments may be affected as these mothers have the option of acting strategically regarding the application date to the MLB. For the pregnancies conceived before the announcement that were in the first trimester of pregnancy in April 2003, there should be no selection into conception among women, irrespective of their earnings. However, given that abortion on request was still an available option given the gestational stage of the pregnancy (for the pregnancies not already terminated until April 2003), the pregnancies that are carried to term could be a selected sample due to the policy change announcement. Both high earning and low earning mothers would have increased incentives to carry the pregnancy to term.[10] The pregnancies conceived

[10]High earning mothers would have decreased incentives to abort given that the opportunity cost of the already conceived child who would be born in 2003 (and would be entitled to the proportional MLB) is lower than that of a future-conceived child (who would be, presumably, entitled to the fix MLB), making it less beneficial to postpone childrearing. Low earning mothers, who are the potential gainers mothers of the reform, would have decreased incentives to abort given the opportunity to act strategically and delay the application to MLB until January 2004, to receive the fixed, higher benefit.

after the policy change announcement in April 2003, that would be carried to term in 2004 may be affected by both selection into conception and selection into carrying the pregnancy to term.

2.2 Theoretical considerations and empirical evidence

2.2.1 The Becker framework and expected effects

The relationship between fertility and income has long elicited academic interest; the seminal work of Becker (1960) and Becker and Lewis (1974) established that the demand for children responds to changes in the price of a marginal child, but there is a limited effect of income changes on fertility. The Becker model of fertility (Becker, 1991) assumes that a family maximizes a utility function which depends on the quantity of children, n, the quality of children, q, and an aggregate commodity that includes all other goods it consumes, Z, subject to a budget constraint dependent on the family income I. The central point in Becker's model is the interaction between quantity and quality of children, through the total amount spent on children: $p_c qn$, where p_c is the cost of a unit of quality; this makes the budget constraint non-linear in the commodities which enter the utility function. This interaction between quantity and quality, Becker argues, is the reason why the demand for children is highly responsive to price effects, and to a smaller extent to income effects, even if children have no close substitutes. In addition, the model introduces a fixed cost per child, p_n, which is independent of the quality of children, and expenditures on quality of children, p_q, which are independent of the number of children. Therefore, the family is faced with the following optimization problem:

$$max\ U(n, q, Z)\ s.t.\ p_c qn + p_n n + p_q q + \pi_z Z = I \tag{1}$$

Comparative statics indicate that a decrease in p_n, the fixed cost of n, would induce a substitution towards n and away from q and Z, as the shadow price of n would decrease relative to both q and Z. The interaction between n and q entails that the decrease in q further lowers the shadow price of n, while the increase in n increases the shadow price of q, which leads to even more substitution away from q and towards n.

In the Becker model, one of the main components of p_n is the negative cost of governmental child allowance, where an increase in the governmental child allowance would lower p_n. Given this interpretation of p_n, the Becker model can be used to make predictions about the consequences of the change in the maternity leave benefits induced by the policy reform in Romania analysed in this paper. An increase (decrease) in maternity leave benefits lowers (raises) the fixed cost, and hence the price, of a child, so according to the model it should lead to an increase (decrease) in the number of optimal children per family, n. Therefore, for women who would receive a relatively higher maternity benefit, there should be an increase in the conception rate and/or a decrease in the abortion rate, both leading to an increased fertility, with the opposite effect for women who would receive relatively lower benefits as a consequence of the policy.

At the same time, the Becker model predicts that the increased (decreased) maternity leave benefits would decrease (increase) the optimal quality of children. Quality of children may refer to any of the components that form the child's human capital (e.g. health, education, skills). However, numerous studies have shown that the accumulation of human capital is determined, or at least influenced, already from the prenatal period by fetal shocks (Almond and Currie, 2011; Almond et al., 2007)) and/or maternal investments during pregnancy (Nilsson, 2014; Almond and Mazumder, 2011). Prenatal investments usually refer to nutrition, medical care, and (abstinence from) the consumption of health damaging goods that affect fetal development, such as alcohol and tobacco. Hence, the decrease (increase) in the quality of children predicted by the model may be reflected in lower (higher) prenatal investments by the mothers, with the same reasoning applying to early investments in child health, which can also be included in the generic concept of quality of children. On the other hand, the change in maternity leave benefits may also generate income effects. The increase (decrease) of the benefits may therefore lead to an increase (decrease) in the consumption of other goods, which may enhance child quality (e.g. better nutrition, more prenatal medical care) or decrease child quality (increased consumption of health damaging goods such as alcohol and tobacco).

To summarize, the fertility demand framework makes clear predictions about the effect of the change in maternity leave benefits on fertility, but there is no clear prediction on the direction of the net effect on early investments in child health (i.e. on prenatal investment, child health at birth and investments in infant heaths) due to the opposing price and income effects. This paper aims to evaluate the net effects, with the reserve that with the data in hand I cannot analyse completed fertility, and so I may capture short term changes in fertility or timing effects, due to the relatively short time span between the policy and the time at which the data is recorded.

2.2.2 Previous empirical evidence

With the expansion of welfare polices, financial benefits related to childbirth have become a salient part of family policies, and there has been growing interest in providing empirical evidence to confirm the Becker hypothesis. Whether fertility responds to financial incentives, and to what extent, is still a very active topic of research, with studies showing mixed results. At the same time, there is also a growing interest in evaluating the effects of these financial incentives on both short term and long term outcomes, of both children and mothers.

Methodologically, evaluating the effects of financial incentives on fertility and other related outcomes has moved away from cross country comparisons towards quasi-natural experimental settings entailing changes in family or tax policies.[11]

[11] Cross-country evidence finds mixed, weak, or insignificant effects of child subsidies on fertility. Demeny (1986) reviews the mixed evidence on pro-fertility policies in France, Romania, Germany, and Hungary. Gauthier (2007) includes a review of studies that provide mixed conclusions as to

There is now a rather large body of evidence on the effects of tax incentive schemes on fertility and maternal labor supply, which typically finds small but significant effects of tax systems on fertility in the direction anticipated by Becker (Baughman and Dickert-Conlin, 2003, 2009; Brewer et al., 2012; Kearney, 2004; Rosenzweig, 1999; Robert, 1998).

Another strand of the literature, more closely related to the setting in this paper, investigates the effects of direct transfers related to birth (child benefits). González (2013) studies the impact of a universal child benefit on fertility and maternal labor supply, exploiting the unanticipated introduction of a sizable, one-off, benefit in Spain in 2007. Using a regression discontinuity design, she finds that the benefit significantly increased the number of live births, in part through a reduction in abortions. Cohen et al. (2013) use the variation in Israel's child subsidy awarded for the third child to identify the impact of changes in the price of a marginal child on fertility. They find a positive and significant price effect on overall fertility, with a 1% increase in the price of raising a marginal (third) child reducing the probability of pregnancy by 0.496%, and a benefit elasticity of 1.76%. Milligan (2005) exploits the introduction of a pro-natalist transfer policy that paid a lump sum to families having a child in the Canadian province of Quebec, with the size of the benefit being larger for higher order births; he finds a strong effect of the policy on fertility, with the average benefit elasticity of 0.107.

The empirical evidence of the effects of maternity leave benefits as a specific type of incentive conditional on childbirth has been, on the other hand, rather scarce and most of the quasi-natural experiments used to identify the effects of these benefits regard the changes in the non-monetary aspect pf MLB, such as the duration of protected leave. Rossin (2011) evaluates the impacts of unpaid maternity leave provisions instituted in 1993 in US on children's birth and infant health outcomes in the United States. Exploring the fact that only women in large companies were entitled to unpaid leave in a triple difference strategy, she find that maternity leave led to small increases in birth weight, decreases in the likelihood of a premature birth, and substantial decreases in infant mortality for children of college-educated and married mothers. However, the policy exploited in this paper does not have a financial benefits component, so it is not directly comparable to the Romanian context. Lalive and Zweimüller (2009) also explore changes in the parental leave provisions concerning only the duration of protected leave after childbirth (and not the financial component). Exploiting a major Austrian reform that increased the duration of parental leave from one year to two years for any child born on or after July 1, 1990, they find strong effects on both short run fertility and excess long run fertility; higher order fertility increases by about 5% for mothers that benefit from the longer maternity leave. Partially reversing the 1990 extension, a second 1996 reform reduces the spacing between births. Carneiro et al. (2011) study the

the effect of policies on fertility -either a small positive effect of policies on fertility is found in numerous studies, or no statistically significant effect; however, there is some evidence that the effect of policies tends to be on the timing of births rather than on completed fertility.

impact on the long run labor market outcomes of children of increasing paid and unpaid maternity leave benefits in Norway in 1977. The significant increases in the maternity benefits led to a 2.7 percentage points decline in high school dropout and a 5% increase in wages at age 30. However, the authors argue that the effects are driven by the increased amount of time spent by the mother with the child, and not an increase in the disposable income after the birth of the child, as is the case in the Romanian context. Dahl et al. (2013) investigate the subsequent Norwegian series of policy reforms which expanded paid leave further, from 18 to 35 weeks, without changing the length of job protection, and claim that these extensions were costly, had poor redistribution properties and had no measurable effect on a wide variety of outcomes, such as children's educational achievements, parental earnings and labor market participation, completed fertility, marriage or divorce. However, the authors argue that these extensions did not result in any change in the total family income, so their estimates capture the effects of parental time on child and family outcomes, and not income effects. In addition, a number of papers study the effect of MLB on other outcomes, including labor market outcomes of the mothers, spacing of births and long run outcomes of the children.[12]

The evidence on the effects of the financial incentives component of the maternity leave benefits is much more scarce. Raute (2014) uses the 2007 change in Germany, which entailed the move from a means-tested maternity leave benefits scheme in which only a small subset of mothers received the benefit at all, to a benefit proportional to the pre-birth income for all employed women. She finds that this change in maternity leave benefits led to an increase in fertility, especially for women in the middle and upper-end of the education and income distribution. In a cross-country comparison framework, Björklund (2006) exploits the expansion of benefits awarded by family policies. Specifically, he examines the evolution of completed fertility patterns for Swedish women born in 1925-1958 and makes comparisons to women in neighboring countries where the policies were not extended as much as in Sweden. The results suggest that parental leave benefits closely tied to the mother's previous labor market engagement raised the level of fertility, shortened the spacing of births, and induced fluctuations in the period fertility rates, but it did not change the negative relationship between women's educational level and completed fertility. With this paper, I contribute to this strand of the litetature by investigating the role of the financial component of the MLB in shaping the entire spectrum of individual level decisions related to fertility: decision to conceive, decision to carry the pregnancy to term, and several important outcomes conditional on live birth (maternal behavior during pregnancy, child health at birth and early investments in child health).

[12] E.g. Thevenon and Solaz (2013), Carneiro et al. (2011), Schönberg and Ludsteck (2014), Lalive and Zweimüller (2009), Ruhm (1998), Lalive et al. (2014).

3 Data

3.1 Romanian Reproductive Health Survey

The main dataset used in this analysis is the 2004 wave of the Romanian Reproductive Health Survey (RHS-Ro). The survey was ordered by the Romanian Ministry of Health and the World Bank, and was conducted by several reputed international organizations.[13] The structure of the survey and the questions are fairly similar to those in Demographic and Health Survey, albeit not as extensive. The data were collected between October and December 2004.

RHS-Ro includes a representative sample of 4441 women, aged 15-44, for whom it collects detailed records of all pregnancies, prenatal care indicators and early investments in child health for the most recent live birth of the woman, and detailed reproductive health information. In addition, it provides detailed socio-economic characteristics of the woman and the household in which she lives. The most important observable characteristics of the women in the sample are the date of birth, education, occupational status, marital status and household assets level. Descriptive statistics for the observable characteristics of all women included in the survey are presented in Table 1, column (1). Most women in the sample have medium education (63%). Occupational status is recorded as *"Employed"* (49%) or "Not Employed", the later containing 10 subcategories, the most numerous being *"Housewife"* (27%).[14]. For employed women, despite the rich set of socio-economic characteristics available, RHS-Ro does not directly record the woman's wage income. The household assets level is captured by a composite measure of the household's cumulative living standard, calculated using data on the ownership of selected assets (such as TV sets; sanitation facilities etc.). It is given as a continuous index measure based on which households are divided into 3 assets holding levels: "Low" (37%), "Medium" (51%) and "High" (12%).

These socio-economic characteristics are recorded only at the date of the survey, with no retrospective questions. A potential problem that arises is that the new MLB policy may have changed the labor supply of women by making it profitable to work (even for a low wage) for a limited period of time and benefit from the high fixed MLB. Since I observe the occupational status at the time of the survey, the employed category may include, alongside women who were employed at the time of the policy change announcement, women who were housewives in March 2003, but entered the labor force to be able to claim the fixed MLB after 10 months of tax contributions. To investigate whether this occurred, Figure 2 presents graphical evidence on the evolution of the occupational status of women at aggregate level between 2002 and

[13]RHS-Ro 2004 was designed to document the priority interventions required as part of the second phase of the Romanian health sector reform, financed by the Word Bank. The survey was conducted by the partnership between United Nations Population Fund , UNICEF, United States Agency for International Development, Center for Disease Control, World Health Organisation and the Romanian Institute for Mother and Child. This insures the high quality of the data collected.

[14]Studying; Job Seeking; Unemployed; Not requiring work; Sick Leave; Prenatal Leave; Maternity leave; Housewife; Unable to work; Other.

2005, both as quarterly stock and quarterly rates.[15] Neither the stock or the rate graphics indicate that there would be a significant increase in the number/rate of employed women that would coincide with the policy change announcement, nor with the date of its implementation. This would suggest a rigid adjustment of the female labor force participation, with out of the labor force women going through an even lengthier process of finding employment. Moreover, even before the policy change, being employed even for a low wage was incentive compatible from the MLB perspective, since it would entitle the women to receive MLB in addition to the child benefits awarded to all mothers, which, as discussed previously, were much smaller than the MLB. In addition, the survey period does not coincide with any abnormal peak in the stock or the rate of employed women, and the fact that we consider pregnancies occurring up until Q2 2004, as will be discussed in the next section, when women had had to be employed for at least 10 months to qualify for the fixed MLB, attenuates the concern that we include in the treatment group women who are employed at the time of the survey but were housewives at the time of the policy change announcement. There is also no significant change, apart from the seasonal fluctuations, in the evolution of the number of the housewives. This attenuates to a certain extent the concern that over the relatively short time span between the policy change announcement and the survey date, there were large changes in the labor force participation of housewife women, but the concern remains valid. I will address this issue further in the robustness checks. Figure 2 also excludes another potential effect of the MLB policy: that the very high financial incentives attached to childbirth conditional on taxed labor would determine some of the women employed in the informal sector (without paying tax contributions) to switch to the formal sector. This would have resulted in an immediate increase in the stock of employed women which, as discussed, is not observed.

Each woman in the sample is asked retrospective questions about *all* her pregnancies: how it ended (live birth, still birth, abortion, spontaneous miscarriage), the date when it ended (month and year) and stage at which it ended (gestational months or weeks). For live births, it also records gender of the child, any disabilities he/she has, and whether it is still alive. There are 9997 recorded pregnancies. Unfortunately, there is no information about the father of these children apart from the marital status of the woman. However, the socio-economic status of the woman and the rearing conditions of the child can, arguably, be well captured by the covariates that are recorded, namely the woman's education, occupation and especially the household assets index.

A common problem with reproductive data derived from retrospective questions is recall bias relating to the accuracy or completeness of retrieved data. Indeed, for 1655 pregnancies I cannot infer the date of conception because the termination

[15]The number of housewives is calculated as the difference between the stock of inactive females (defined as housewives, females in school and retirees) aged 20-44 and females 20 and above engaged in education. The stock of inactive females is recorded quarterly, whereas the stock of females engaged in any form of full time education is recorded with a yearly frequency.

month is unknown, but for almost all of these pregnancies, the year of conception, the termination mode and the stage of the pregnancy is known. Women not reporting termination month are significantly older at the time of the survey, and their age at pregnancy is significantly smaller than the age at pregnancy for the pregnancies which do have termination month reported, but there are no significant differences in their other observable characteristics (educational level, marital status, place of residence); this is consistent with recall bias. Thus, I impute the conception month, which would both preserve sample size and correctly account for the prevalence of abortion.[16]

For the last pregnancy that ended in live birth, RHS-Ro collects detailed information about prenatal investments, child health at birth and early investments in child health. The following data is available: status of pregnancy (intended/ unintended/ unwanted), smoking during pregnancy, alcohol consumption during pregnancy, prenatal supervision, prenatal vitamin supplements, birth weight of the child, postnatal visits, number of days in hospital after birth, information related to breastfeeding, infant vitamin supplements.[17] This rich information is usually not available in vital natality files or in other register data, which makes the RHS-Ro a very interesting resource to exploit when studying financial incentives and fertility outcomes.

3.2 Sample and descriptive statistics

In the main analysis, I consider the pregnancy as the observational unit, and consider pregnancies occurring until (and including) July 2004.[18] I restrict the sample in the main analysis to pregnancies (or births) occurring at most 15 months before the policy change announcement to obtain a symmetric 15 months window on each side of April 2003. I classify conceptions (births) occurring between January 2002 and March 2003 as occurring in the pre-announcement period, t_0, and conceptions (births) occurring between April 2003 and July 2004 as occurring in the post-announcement period, t_1. The narrow window reduces the probability that the effects are confounded by time trends in fertility and reproductive behavior, but includes sufficient repeated observations per month to allow controlling of seasonal effects, which are known to influence fertility patterns.

Columns (2)-(4) of Table 1 present the descriptive statistics for the observable

[16] I use a multinomial logistic regression for a nominal variable (the month of conception). The independent variables used are age at pregnancy, the number of children at that specific pregnancy, the number of previous abortions, educational dummies, marital status and urban dummy. In all regressions I include a dummy for pregnancies with imputed conception date. Excluding these pregnancies does not significantly change the estimated effects (results available on request).

[17] RHS-Ro also records detailed information about the last abortion (including questions on motive for abortion, place where it was performed, complication post-abortion, etc.), family planning practices, sexual behavior, reproductive health and healthcare utilization, STD knowledge and domestic violence.

[18] As most of the 2004 RHS-Ro interviews were conducted in October and November 2004 and there is a possibility that recently pregnant women would not be aware of the pregnancy, and therefore pregnancies occurring after July 2004 would be under-represented in the survey (which is confirmed by the total number of pregnancies by month of conception); moreover, these pregnancies would be in the first trimester at the time of the survey, hence abortion would still be available, and there would be no possibility to infer if the pregnancy would be carried to term or terminated.

characteristics of women who conceive, use abortion and respectively give birth in the analysis period, where the observational unit is the woman. Compared to all women in the sample, who are not necessarily mothers, women who conceive are younger and more likely to be married. Also, housewives are over-represented in the sample of women who conceive, in line with fertility models which link the number of children to the opportunity cost of tine of the woman. Although the distribution in terms of educational attainment is similar when comparing all women in the sample with those who conceive/abort/give birth in the analysis sample, those who carry the pregnancy to term are more likely to live in low or high assets level households.

Table 2 presents the descriptive statistics for the observable characteristics of women who conceive (Panel A), abort (Panel B) and give birth (Panel C), where the observational unit is the pregnancy, and the raw averages in the pre-announcement period and in the post-announcement period, belonging to all, employed and housewife mothers. Employed women who conceive after the policy change announcement are negatively selected relative to those conceiving before the announcement: a lower educational achievement level, less likely to be married, lower average household assets index. There is a lower probability that a pregnancy will be terminated using abortion, but not significantly so. This negative selection appears to be equally driven by the women who carry the pregnancy to term and those who use abortion. This pattern is consistent with the theoretical prediction that the increased financial incentives attached to childbirth increase the conception behaviour of the employed women who would benefit the most, i.e women with the lowest wages (which are expected to have the poorest observable characteristics), who then carry the pregnancy to term. For women who use abortion, in the post announcement there is a negative selection, with significantly more low education, low household assets level women. Although insignificant, there also appears to be a larger share of better off women (high education, high household assets level). For housewife women who conceive, the selection on observables is insignificant, although it appears to be mildly negative. This applies for both housewives who carry the pregnancy to term and those who terminate the pregnancy.

4 Identification strategy

4.1 Specification

In order to retrieve the causal effects of the policy change on the outcomes of interest I employ a *Double Difference* estimation strategy, adjusted to account for the fact that mothers could act strategically with respect to the date of application to the benefits. The underlying identification assumption is that that the changes over time for a specific group of non-participants provide a proper counterfactual for the participants, i.e. the parallel trend assumption. Assuming a suitable control group is available, which will be discussed shortly, the prefered specification for the Double Difference design is:

$$Y_{im} = \alpha + \beta_1 T_{im}^{Cert} + \beta_2 T_{im}^{Pot} + \beta_3 Treated_i + \beta_4 T_{im}^{Cert} * Treated_i + \beta_5 T_{im}^{Pot} * Treated_i + \gamma_1 m + \theta_q + \delta_1 X_{im} + \delta_2 X_i + \epsilon_{im} \quad (2)$$

where i indexes a pregnancy conceived/born in month-year m.

The first outcome studied is the occurrence of pregnancy; in this case, the outcome is the monthly aggregated number of conceptions per 1000 women; occurrence of pregnancy at individual level will also be studied in a woman fixed effects framework, whereas in a DD framework one cannot consider the outcome of conception at individual level. The second outcome is the probability of abortion; Y_{im} is 1 if the pregnancy ends in abortion and 0 otherwise. The third set of outcomes are conditional on live birth, capturing: i) prenatal maternal investments: alcohol and smoking during pregnancy indicator (1 if the mother ever smoked or consumed alcohol during pregnancy), month of first prenatal control (continuous variable) and prenatal vitamin supplements during indicator (1 if the mother reports having taken vitamin supplements during pregnancy); ii) child health at birth: low birth weight indicator (1 if birth weight of the child is less than 2500 grams), number of days of hospitalization at birth (continuous variable) and a postnatal control indicator (1 if the mother and child undertook a postnatal medical visit in the first month after birth); iii) early investments in child health: breastfeeding indicator (1 if the child was breastfed), number of months of breastfeeding (continuous variable) and infant vitamin supplements indicator (1 if the mother reports giving the infant recommended vitamin supplements).

T_{im}^{Cert} and T_{im}^{Pot} are mutually exclusive indicator variables capturing the time period of conception or birth. As such, for outcomes conditional on conception (occurrence of pregnancy and probability of abortion), T_{im}^{Cert} is 1 for the pregnancies conceived after April 2003 and T_{im}^{Pot} is 1 for the pregnancies conceived between January and March 2003 which were still in utero in the first trimester at the time of the announcement.[19] For outcomes conditional on live birth (prenatal investments, child health at birth and early investments in child health), T_{im}^{Cert} is 1 for the pregnancies delivered after January 2004 which would be certainly affected by the policy change, and T_{im}^{Pot} is 1 for the pregnancies delivered between April and December 2003, as the mothers could potentially delay the application to the MLB and receive the fixed sum after January 2004.

$Treated_i$ is 1 for pregnancies (births) of women in the treatment group, who were affected by the policy change, and 0 for pregnancies (births) women in the control group, who were not affected by the policy change. Treatment and control groups will be discussed shortly. m is a linear time trend standardized to be 0 in April 2003. θ_q are conception/birth quarter fixed effects. X_{im} is an individual and time specific vector of characteristics to control for the fertility history of the woman prior to the current pregnancy, specifically the number of previous abortions and the number of

[19]This important correction accounts for the fact that some of these pregnancies might have already been terminated prior to the announcement, and therefore not all pregnancies conceived in January-March 2003 are potentially treated.

live children at the time of the pregnancy i, and age at pregnancy i. X_i is a vector of individual characteristics of the mother, considered fixed, as they are only measured at the time of the survey: her educational level, marital status, household size, and a rural dummy.[20] ϵ_{im} is the individual error term. I estimate the regression using ordinary least squares and present robust standard errors to account for potential heteroskedasticity.[21]

4.2 Treatment and control groups

Treatment group Due to the design of the MLB policy in Romania, which conditions the receipt of the benefits on wage tax contributions prior to childbirth, only employed women are entitled to apply to MLB, whereas out of the labor force women are precluded from doing so. Employed women were affected by the policy change depending on their wage level (i.e. benefited from the change if their monthly wage income was below 7.6 mil. ROL (213 USD) and were disadvantaged if they had a monthly wage income above the threshold), as discussed in Section 2.2. However, despite the rich set of socio-economic characteristics available, RHS-Ro does not directly record the woman's wage income. As such, given that approximately 80% of women were potential gainers of the reform (as discussed in Section 2), I consider a baseline estimation in which the treatment group consists of all employed women. This makes the assumption of a uniform impact, and the coefficients would reflect the average effect on employed mothers.

However, since this average effect is likely composed of two opposing effects that may cancel each other out, I investigate the effects on subsamples determined by the household assets level; although technically employed women were favored or disadvantaged by the new provision of the MLB law depending on their pre-birth earnings, the "bite" of the policy reform may be better reflected by the household wealth. To this end, I use the household asset index to split the sample such that it matches the 80-20 division between gainers and loosers of the reform; this leads to a split into a group with non-high household asset index (women with low and medium household asset index, accounting for 80% of the sample) and a group with high household asset index (accounting for 20% of the sample of women). I estimate Equation 2 on these two subsamples.

Control group Due to the wage tax contribution requirement, out of the labor force mothers were, in theory, unaffected by this policy change and so they are a natural candidate category for the counterfactual group. The preferred subgroup of the out of the labor force women are the housewives (HW). Housewife

[20] Since these are only correlated with the actual characteristics at the time of the pregnancy, the fact that they are subsequent to the outcome itself may induce measurement error in our estimations. However, the results are not sensitive to their exclusion.

[21] These specifications use conceptions as the unit of observation, which would suggest clustering at the mother level. However, only 6% of the mothers who conceive in the time-window of the analysis have multiple pregnancies. However, if I do cluster at mother level, the estimated standard errors are very close to the robust standard errors.

women constitute an intuitive counterfactual for the employed women as they are non-participants in the treatment by law, and are more comparable in terms of observable characteristics to the employed women than the other out of the labor force categories (students and pensioners) in terms of the fertility cycle. Moreover, they are the second most numerous group by mothers occupational status after employed women. A potential problem with the selected control group is that women in this group may change their labor market status, and such an endogenous change of the individual labor force participation as a response to the reform would violate the identifying assumptions of the DiD by making the employed women incomparable over time. In addition to the claims made in Section 3.1 regarding the rigid adjustments of the labor market which seem to attenuate this problem, I try to address this issue in the Robustness section.

4.3 Parallel trends assumption

Figure 3 plots the average maternal characteristics by quarter of conception between 2000 Q1 to 2004 Q3, for Employed and Housewife mothers.[22] The figures show a fairly similar evolution in the composition of the observable characteristics of employed women and housewife women who conceive in each trimester before the policy change announcement, although there are, as expected, level differences in the anticipated direction. The fact that there is generally no clear diverging trend in the composition of observable characteristics between the treatment and the control group[23] provide evidence supporting the parallel trend assumption, which underlies the double difference identification strategy. Figure 4a plots the number of conceptions and share of conceptions ending in abortions for Employed and housewife women, by quarter of conception between 2000 Q1 to 2004 Q2, whereas 4b plots the average outcomes conditional on pregnancy which I analyze, by quarter of birth and occupational status of the mothers. Although more noisy than the maternal characteristics, they appear not to contradict the parallel trend assumption required by the double difference strategy. In addition, in the main regressions I control for a linear time trend and for quarter of conception fixed effects.

4.4 Early childcare and time at home after birth

An important issue for the identification strategy is whether the policy change also altered the duration of the temporary exit from the labor market of employed women after giving birth. The first evidence to support the fact that the majority of women take the full extent of the protected leave comes from aggregate data. According to Statistics Romania, in 2003 and 2004 only 2.1% of children between 0 and 3 years were enrolled in childcare facilities, and it slightly increased in 2005 and 2006 to

[22]Except for age at pregnancy, all other maternal characteristics are measured at the time of the RHS survey (Oct/Nov 2004), and not the time of the conception (the observation unit).

[23]With the exception of "married" status, which appears to be slightly diverging in the pre-announcement period and then re-converging after the policy change announcement in the post-announcement period.

2.4%. The low rate of the formal childcare is due to capacity constraints (in 2004, only 289 childcare facilities were operating on the entire territory of Romania) and is significantly below the European average of 30%. This is in contrast with the 70% enrollment rate in 2004 in kindergartens for children between 3 and 6 year old.

Additional evidence on the time at home after the birth of children at individual level comes from the Generations and Gender Survey, Wave 1 Romania, which was conducted in December 2005.[24] Although it is not possible to conduct an individual level analysis of the time spent at home/out of the labor force after the birth of a child, the data reveals that of the women active of the labor market that have a child younger than one year, 84% are on maternility leave, and rest of 16% are working. Of the women active on the labor market that have a child between one and two years old, 80.5% are on maternity leave and 19.5% are working. This suggests that the vast majority of women active on the labor market enjoy the full extent of the protected leave, but there exists a non-negliable share that do work while they would be entitled to maternity leave benefits. This would be problematic to the analysis in this study if earlier return to work would be a response to the policy, whereby the high earning mothers would return to the labor market because the fixed MLB would be smaller than a proportional MLB. Unfortunately, due to the timing of the survey (December 2005), women who would, theoretically, still be entitled to maternity leave benefits would have to have given birth the earliest in January 2004, which precisely corresponds with the implementation of the policy change studied in this paper -therefore I cannot observe the length of maternity leave for women who have given birth before December 2003. This makes it impossible to investigate whether the duration of the maternity leave changed for women who gave birth after January 2004 relative to those who gave birth before. However, indicative evidence comes from Figure 5, which plots the distribution of wages of all employed women in the sample and the distribution of wages of women who have children under the age of 2 (hence would have been entitled to be on MLB) and work. If the duration of the leave changed because of the smaller benefits for high earning mothers, it would be expected that most of the mothers who return to work before their child's second birtday would be the high earning women, with after tax wages above the fixed level of the MLB (6.5 million lei). However, the distributions in Figure 5 show that the women who retun to work before their child's second birthday are not concentrated among the highest earning employed women, but in fact are relatively more concentrated among low earners, those who gained from the policy change. As expected, there are also relatively more high earning mothers that return to work than in the overall distribution, but coupleted with the previous finding it could suggest that return to work prior to the child's second birthday is not related to the policy change, but to some unobserved preference for participation

[24]The Generations and Gender Survey is a longitudinal survey of 18-79 year olds conducted in 19 countries includin Romania, and it is designed to understand family and relationship dynamics. It covers a wide array of topics including fertility, partnership, economic activity, care duties for children and within the household.

in the labor market for some new mothers, that could have existed before 2004 as well.

Regarding informal care, Paunescu and Apostu (2012) show that 96.2% of children up to one year old were in the exclusive care of their parents; of children aged one to two years, 87.9% were in the care of their parents, 7.2% were in the care of their grandparents and the rest in formal childcare facilities. This also supports the claim that most mothers take the full extent of the protected leave after childbirth and do not use informal care to a great extent in the first two years of the child's life.

5 Results

5.1 Probability of conception

The first stage of the analysis is to examine whether there is a significant change in the number of pregnancies occurring to employed women, irrespective of how they end. Using the conception date of all pregnancies in the sample, I analyze whether there was a significant increase in the number of conceptions per 1000 women in the months following April 2003. Since the dependent variable is at aggregate level rather than individual level, no individual covariates can be used.

Table 3 presents the estimation results for the number of conceptions per 1000 women occurring each month. Columns (1)-(2) present the double difference estimation results, first with only a monthly time trend, then with quarter of conception fixed effects. Column (3) estimates the same Difference in Difference on the sample of women with non-high household assets index levels, whereas column (4) uses the restricted sample of women with high household assets index level.

Results indicate that there is an increase in the number of conceptions per 1000 women in the period after the announcement of the policy change of 0.46, but statistically insignificant (tval=1); this is mainly driven by the increase in the number of conceptions per 1000 women with non-high household assets level, of 0.65, but this is still insignificant. In contrast, the effect for high household assets level is an imprecise zero. The difference between the conception rate of women from non-high and high household assets is positive and rather large, but marginally insignificant due to the large standard errors.

I also investigate the compositional changes in the observable characteristics of women who conceive after the policy change announcement. I estimate Equation 2 where Y_{im} is, in turn, an observable maternal characteristic of interest: age at pregnancy, educational level (captured by three dummy variables for each broad level of education), marital status, non-high household assets level and place of residence (rural vs. urban). Table 4 Panel A presents the estimation results for the observable maternal characteristics of all mothers who conceive, irrespective of how the pregnancy ends. After controlling for time trends and seasonality in the Double Difference framework, the observable characteristics of employed women relative to housewife women do not change significantly. However, they appear to be older, less likely to be married and more likely to be from urban localities. The point estimate on the dummies related to household assets level, which proxy household wealth, are small and have very large standard errors, which precludes even a tentative interpretation of the selection in term of these observables.

Unfortunately, because of the lack of individual data on wages, I cannot calculate the benefit elasticity of the conception rate per 1000 women. Howerver, using aggregate data on female wages to impute the average potential MLB under the proporional benefit regime, I can approximate that the average benefit elasticity of the conception rate for the group of women with non-high households assets would be roughly 0.06. This elasticity is, however, uncomparable to those found in previous studies, since they only observe live births, and not conceptions at individual level. In order to be able to compare the effects found in this study with those in the previous literature, I estimate the effect of the policy change announcement on the number of live births per 1000 women, as does, for instance, Raute (2014). Estimating Equation 2 on the number live births per 1000 women, and using the estimated treatment effect for the sample of women with non-high households assets level gives an average benefit elasticity of 0.083. Raute (2014) calculates a benefit elasticity of live births per 1000 women of 0.11, Milligan (2005) puts the average benefit elasticity at 0.107, whereas Baughman and Dickert-Conlin (2003) estimates an elasticity of 0.06. Although these elasticities capture the effects of variuous types of financial incentives attached to childbirth, and not fixed MLB specifically (with the exception of Raute (2014)), and use very large administrative datasets, the fact that the approximated elasticity I calculate is well in their range indicates that the results I obtain in the context of the Romanian policy change are reasonable, despite not being significant.

5.2 Outcomes conditional on conception

Next, I estimate the impact of the policy change announcement on outcomes conditional on conception having occurred, namely how the pregnancy is terminated (abortion versus live birth). I model abortion prevalence at individual level: in Equation 2 the outcome variable is 1 if the pregnancy ends in abortion and 0 if it ends in live birth. Table 5 presents the estimation results, where the first column shows the simplest double difference specification and the following columns build up to the richest specification, and then presents the estimation results for the sub-samples defined by the household asset index. The results reveal a rather large but insignificant reduction in the probability that a pregnancy is terminated using abortion, of 4.7 percentage points (approximately 10% of the mean). Controlling for individual characteristics and quarter of conception fixed effects do not seem to affect the estimated treatment effect, but the standard errors remain large, which does not exclude a zero effect. However, when splitting the sample according to the household asset index, the treatment effect for the group with low and medium household asset index, i.e those who would have likely benefited most from the policy change, almost doubles, indicating a reduction of 8.8 percentage points (roughly 20% of the mean), and the relative size of standard errors significantly decreases, reaching a t-statistic of 1.30. In the sample restricted to high household assets levels, the treatment effect is of opposite sign (and almost equal magnitude), but with much larger standard errors, probably due to the much smaller sample size. These two effects go in the expected direction: the non-rich households responded to the increased financial incentives attached to childbirth by reducing the probability of abortion, once a pregnancy occurred, suggesting that financial constraints were an important determinant of the decision to abort. For richer households, the policy change had the opposite effect, increasing the probability of abortion (albeit one cannot exclude a zero effect here due to the very large standard errors), and again indicating that this type of reproductive behavior responds very quickly to financial incentives. The difference between the estimated treatment effects in the two subsamples based on household assets level is very high, with the non-high household asset level women having a 16 percentage points lower probability to terminate the pregnancy after the policy change announcement; this is, however, indistinguishable from 0 due to the large standard error on this difference.

To understand the mechanism behind the reduction in the probability of abortion, I use the information collected on the most recent recent abortion of each woman in the sample (which then constitutes a sub-sample of all abortions registered in the dataset) regarding the main reason for termination. There are three broad categories: heath reasons (maternal or fetal health status), socio-economics reasons, and the desire to limit fertility. The first thing to note is that for the pre-announcement window, detailed information is recorded for 165 abortions, whereas for the period post announcement, information for 183 abortions is recorded, despite the fact that the total number of abortions decreased in the post announcement period relative to the pre announcement period. Since the module focuses only on the last performed abortion, this suggests that in the pre-announcement period there were more abortions per woman.[25] I use a multinomial logit model to estimate Equation 2, where the dependent variable is the categorical variable recording the main reason for abortion. For the whole sample, the marginal effects of the main interaction term, $Treat * T^{cert}$, show that the probability of stating health reasons is higher by 8 percentage points (z=0.90), the probability of stating socio-economic reasons reasons is higher by 8 percentage points (z=0.68), and the probability of stating fertility limitation reasons lower by 16 percentage points (z=-1.37), very close to significance. For the sub sample of women with non-high household assets levels, the marginal effects at the mean indicate that the treated group in the post announcement period (i.e. the marginal effect of the $Treat * T^{cert}$ term) have a significantly lower probability of stating limiting fertility as the main reason, by 22

[25] In fact there were 293/165=1.77 abortions per woman in the pre-announcement period, and 282/183=1,54 abortions per woman in the post announcement period, both conditional on the women having at least one abortion.

percentage points (z=-1.74), and a higher, but insignificant probability of stating socio-economic reasons (17 percentage points, z=1.33) and health reasons (5 percentage points, z=0.58).[26],[27] This would suggest that the policy (announcement) changed the desired level of fertility.

None of the previous studies that investigated the effects of financial incentives on fertility have access to individual level abortion data, hence my results on the probability of abortion are not directly comparable to any of the previously obtained results. However, Raute (2014) uses aggregate quarterly data on abortion and finds that the increase in the potential MLB lead to a discontinuous decrease of roughly 3% of the mean in abortion rates of married women right after the announcement of the policy, and González (2013) finds that the one-off child benefit reduced abortions by 6 to 7%. The effects I find, namely a reduction of 8.8 percentage points, which represents approximately 20% of the mean, are therefore much larger. This may be due to the specific context of Romania, where abortions were extensively used as fertility-limiting methods, whereas the rates were much lower in Germany or Spain. Therefore, the reduced use of abortions would be a more effective and rapid means of increasing fertility in Romania, but not so much in the other two, more developed countries. Also, the individual level analysis may capture more accurately the policy affects than an aggregate level analysis.

In terms of selection on observables, Table 4 Panel B reveals that women who terminate the pregnancy are less likely to be married, less likely to live in households with non-high levels of assets (suggesting that women from poorer households are less likely to abort after the policy change) and less likely to be from a rural area, albeit insignificantly so. The treatment effects on the other observable characteristics is very small in comparison to their estimated standard errors, so they are not interpretable. Panel C shows that employed women who carry the pregnancy to term after the policy change are, relative to housewives, (insignificantly) older, more likely to be from non-high assets level households and more likely to be married, who were likely financially constraint before the policy change. Interestingly, these are appear to be opposing effects, suggesting we have identified the "switchers", the marginal mothers whose behavior is influenced by the policy.

5.3 Outcomes conditional on live birth

As the final stage of the analysis I study the effects of the policy change announcement on the outcomes of the pregnancies carried to term: 1) prenatal investments: smoking and/or alcohol consumption during pregnancy, month of the first prenatal control, vitamin supplements during pregnancy; 2) child health at birth: low birth weight, number of days in hospital after birth, probability of a postnatal medical control; and 3) early life investments in child health: breastfeeding, months of breastfeeding and infant vitamin supplements. These outcomes were chosen either

[26]For the sub-sample of women with high level of household assets index, the ML does not converge and so Equation 1 cannot be estimated with *mlogit*.

[27]The estimation results are available upon request.

because they are established in the health economic literature (e.g. the low birth weight indicator), or because medical research has shown they play an important role in determining child health.

It is important to remember that the RHS-Ro registers detailed information about the health at birth and multiple measures of investments in child health at birth for the last born child of each interviewed woman. Thus, the sample of births with information on child health and investments is a subset of the pregnancies recorded as being terminated with live births in the retrospective survey.

Table 6 Panel A presents the estimation results for the entire sample for the three sets of outcomes conditional on live birth: pre-birth investments in columns (1)-(3), health at birth of the child in columns (4)-(6) and for early investments in child health in column (7)-(9). All present the estimation results for the richest double difference specification. Panel B presents the results for the sub-sample of women with non-high household assets level and Panel C the results for the women with high household assets level. The last row of the table presents the p-value on the difference between the estimated coefficients on the interaction term of interest between non-high indicator from a fully interacted model.

Employed mothers who give birth after the introduction of the new MLB appear to have a 13 percentage points larger, but insignificant (s.e. = 0.084), probability to smoke or consume alcohol during pregnancy relative to housewife mothers, which may have detrimental effects on the health of the child. Although even more insignificant, the results show negative coefficients on the month of first prenatal control and for probability of the mother taking prenatal vitamin supplements.

Despite the apparent worsening behaviors during pregnancy, the negative treatment effect on the outcome "low birth weight" indicates a marginally insignificant ($t = 1.55$) improvement in the health at birth of children born after January 2004 (hence conceived after the announcement of the policy change) to employed mothers relative to housewife mothers, reflected in the reduction of the probability that the child is born with low birth.[28] This is in line with the findings in Rossin (2011), that finds that the introduction of unpaid maternity leave decreases the likelihood of low birth weight, potentially due to the decreased stress that the mother is subject to in the prenatal period. Despite this, they have a slightly higher probability of having a medical visit in the first month after birth and appear to stay longer in the hospital after birth -with the reserve that the effects are insignificant. Regarding the early investments in child health, results in Panel A indicate point estimates close to zero on the probability of breastfeeding probability of giving the infant vitamin supplements but a somewhat large (but insignificant) increase in the length of breastfeeding for the children that are breastfed.

In Panel B, results indicate that most of the effects on alcohol and tobacco consumption during pregnancy, probability of low birth weight, postnatal consult and days in hospital observed in Panel A are driven by the effects on women with non-

[28]Including dummy variables to capture the household assets level reveals a significant negative treatment effect on the probability of low birth weight.

high household assets level. In particular, there is a 13 percentage point (s.e. = 0.09) increase in the probability of alcohol and tobacco consumption during pregnancy, but also a 13 percentage points (s.e. = 0.088) decrease in the probability of low birth weight which suggests an (insignificant) improvement in the health outcomes at birth. Again, despite being large in magnitude, these treatment effects are statistically indistinguishable from 0 due to the large standard errors. Months of breastfeeding, on the other hand, has a virtually 0 estimated coefficient, as opposed to the large point estimate in Panel A. An interesting effect is that on the number of hospitalization days after birth, which increases by 2.3 days for children of women with non-high household assets levels. Although they may appear to suggest worsening health of the children, it may actually capture the increased financial resources of the mothers. In fact, the RHS-Ro records whether the mothers made any informal payments to doctors and nurses in relation to the birth of the child, and if yes, the sum that was paid. Women with non-high household assets are 5 percentage points (s.e. = 0.12) *more likely* to give such informal payments after January 2004, but they give on average 1,430 thousand ROL (s.e. = 1330) (2.3 USD, but 38% relative to the average informal payment) *more* than they did before January 2004, relative to housewives. At the same time, women with high household assets levels are 25 percentage points (s.e = 0.29) less likely to make informal payments, and when they do, these sums are 2387 thousand ROL (s.e. = 2667) (almost 7% of the mean) *smaller* after January 2004 than before, relative to housewives. The large point difference between these estimates suggests that an increase in the anticipated disposable income is positively related to the amount of informal payments, which in this setting equates to the quantity or quality of the medical care the infant receives at birth.

For the high household assets level women (in Panel C), as opposed to the non-high household assets level women, the treatment effect for the binary variable capturing health damaging behaviors (alcohol consumption and smoking) seems to be slightly larger in magnitude, suggesting a 19 percentage point increase, but still insignificant (s.e. = 0.128), and also have a large and negative estimate of the effect on prenatal vitamins durinh pregnancy, of 27% (s.e. = 0.178). Regarding the variables that reflect child health at birth, the estimated coefficients on low birth weight and on number of days in hospital after birth are small in magnitude and very insignificant; probability of postnatal consult is, however, rather large but still insignificant. Despite this, they have a 13 percentage point higher probability of breastfeeding the child, and a significantly longer period of breastfeeding, which may suggest compensatory investments on behalf of these women who might have been negatively affected by the policy change. The last row of the table, containing the p values of the differences between the treatment effects in the two sub-samples reveal that there is a statistically significant difference in the probability of low birth weight, with the non-high household assets level sub-sample having a significantly more negative treatment effect, which suggests that the children of poorer women have a significantly better health at birth as reflected by this indicator. In the same time, richer women breastfeed they children significantly more compared to non-high household assets level women, along the line of compensatory investments outlined earlier.

A heterogeneity analysis is presented in the Appenxid, with the results in the expected direction.

6 Robustness checks

6.1 Validity of the control group

The main challenge to the Double Difference identification strategy is the validity of the control group, mainly the parallel trend assumption. The problem that arises in the particular case of the policy change I am analyzing is the possibility that women in the control group took up the treatment by changing their occupational status as a response to the policy change. In particular, it is possible that some women reporting to be employed in October/November 2004 were out of the labor force in April 2003 but (re)entered the labor market (irrespective of the wage received), so that they could benefit from the fixed MLB after the mandatory contribution period; given that I use a cross-sectional dataset with no retrospective questions on the occupational status, I would include these women in the treatment group which would induce a selection bias in the double difference estimates.[29]

To address this, I restrict the sample of pregnancies to those conceived within three months around the announcement date. By doing this, I increase the probability that women whose occupational status is "employed" in October 2004 *and* who conceived just after the policy change announcement were also employed at the time of the conception; this is due to the fact that even if housewife women would be able to enter the labor force that rapidly, they would not fulfill the 10 months mandatory contribution criterion until the birth of the child if they would conceive in the first three months after the announcement date, and so would not be able to benefit from the fixed MLB. At the same time, women registered as housewives in October 2004 and who conceived around the announcement date were likely to have been housewives at the time of the conception. Hence, the certainly treated group are the employed women conceiving in April, May and June 2003. I still have to acknowledge the fact that employed women conceiving in January-March 2003 were in the first trimester of pregnancy at the time of the policy change announcement, hence both abortion was still available and there existed the possibility of strategic delay of the application process to MLB. This renders this group unsuitable as a valid baseline level for the treatment group in the pre-treatment period.

[29]If among the women conceiving after the policy change announcement, employed at the time of the survey, there are relatively more former housewives than before the policy change announcement, and by the logic presented above they would be less likely to abort (since they entered the labor force precisely to gain access to the MLB after giving birth), then there could be an upward bias in a Difference-in-Difference specification.

To circumvent this problem and the issue of seasonality in fertility outcomes, I use the conceptions occurring in April-June in the previous years (2000, 2001, 2002) as the baseline pre-treatment levels, with employed women as the control group and housewives as a control group. The results, presented in Appendix, generally have the same direction and are close in magnitude to the main results.

In addition, in Appendix I present an alternative strategy in which I make use of an aditional dataset to draw the control group-employed women from the neighbouring (and largely Romanian) Republic of Moldova, using a similar Reproductive health Survey. The results largely point in the same direction as my main specification.

6.2 Cut-off date variations: T^{Cert} coverage

The first attempt to change the law regarding maternity leave benefits was made public in March 2003, with the first emergency ordinance. However, the final change came in April 2003, with the second emergency ordinance, but the law was modified in October 2003. Although ex post there were no more changes between April and October, ex ante the public expectations might have been different, and there is the possibility that the April 2003 changes were not perceived as final, given the previous radical change of the attitude of the government towards the MLB. However, as the ordinance was voted into law in late October, this may be a more precise signal for the population, and perceived as final. Therefore, I re-estimate the main specifications using October as the policy change announcement date instead of April.

Table 7 presents the estimation results for the number of conceptions per 1000 women, which are in line with the main results: an increased, albeit insignificant, conception rate per 1000 women. The difference between conception rates of women from households with non-high and high assets level accentuates, with women from poorer households having a relatively large increase, of 0.73 conceptions per 1000 women, and women from richer households a decrease in the conception rate of 0.39 conceptions per 1000 women. This is in line with our prior, as by October women had the possibility of changing their fertility behavior according to the incentives, given that conception may not be immediate.

The results in Table 8, which estimate the probability of abortion using the October threshold also reveal the same pattern of results as in the main estimation, with a reduction in the probability of abortion in the entire sample. Employed women from non-high assets level households have a decreased probability of abortion, whereas women from high assets level households have an increased probability of abortion after change was voted into law, with the point difference between these two sub-samples being even larger than in the main specification.

Table 9 estimates the policy effect for the outcomes conditional on live birth using the October threshold. However, in this case, I only modify the potentially exposed group, since there is no uncertainly regarding births that would certainly receive the fixed benefits. As with the previous outcomes, the estimation results are similar to those in the main specification, and some may even become larger and more significant. As such, women from households with high assets levels are significantly more likely to consume alcohol or smoke during pregnancy, and less likely to take prenatal vitamins, whereas these effects are much more diminished for women from poorer households. In the same time, the health at birth of children belonging to women from poorer households significantly improves, whereas it insignificantly worsens for better-off women. This appears to be compensated for by higher post-natal investments in child health at birth, with better off mothers being more likely to breastfeed and conditional on this, they breastfeed their children longer, an effect not observable for women from households with non-high assets levels. For the other outcomes studied, the pattern when using the October threshold is the same as in the main regression, but they remain insignificant.

6.3 Woman fixed effects

In the main analysis I evaluated the impact of the policy change announcement on the probability of conception by using the aggregate number of conceptions occurring before and after April 2003. An alternative way to analyse the fertility effects of the change in financial incentives attached to MLB is to use individual level observations of occurrence of pregnancy in a pre-post difference design coupled with individual fixed effects, on the entire sample of women in the RHS-Ro. Similarly, one can analyze the within-mother use of abortion, and whether the announcement of the policy change alters the probability that a pregnancy is carried to term relative to the prior pregnancy. Such a fixed effect specification would also be justified if there are reasons to suspect that the OLS estimates in the main analysus suffer from omitted variable bias, and in particular unobserved heterogeneity bias.

I estimate the analogue of Equation 2 in the panel data, individual fixed effects design:

$$Y_{it} = \alpha + \beta_1 T_t^{Cert} + \beta_2 Treatment_i * T_t^{Cert} + X_{it} + \gamma_1 m + \theta_q + \theta_i + \epsilon_{it} \qquad (3)$$

Where Y_{it} is an individual level outcome for mother i time period t. To investigate the probability of conception, Y_{it} 1 if woman i becomes pregnant in 15 month period, both before and after the policy change announcement, and 0 otherwise. To study within mother probability of abortion, I use the sample of women with at least two pregnancies and Y_{it} is 1 if the pregnancy is terminated using abortion and 0 otherwise. T_t^{Cert} is 1 for the April 2003-July 2004 period and 0 for the January 2002-March 2003 period. $Treatment_i$ is 1 if the woman is employed and 0 if she is housewife. X_{it} are individual characteristics at the beginning of period t, specifically age, number of previous pregnancies, and number of previous abortions, which can be inferred from the fertility history of each woman in the sample. $m \; and \; \theta_q$ would be a linear time trend and a conception quarter fixed effects, but they cannot be used when analyzing the probability of conception. θ_i are the woman fixed effects which capture all time invariant unobservable individual characteristics of the woman, but also all characteristics which are only observed at the time of the survey for which I cannot infer the level at the beginning of the periods of interest (education, marital status, household assets level, region of residence). β_2 is the treatment effect.

Table 10 Panel A presents the estimation results for the probability of conception. Although insignificant, the results indicate that employed women are 16 percentage more likely to conceive after the policy change announcement relative to housewives. The results on the sub-samples based on the household assets level index have even higher standard errors, twice as large as the estimated treatment effect, which makes any interpretation of the size of the coefficient irrelevant.[30]

Table 10 Panel B presents the estimation results which analyze whether the announcement of the policy change alters the probability that a pregnancy is carried to term. They show that pregnancies conceived by employed mothers after the policy change announcement relative to the previous pregnancy have a 20 percentage points (t = 1.44) lower probability of being terminated on request using abortion, with the effect mostly driven by women from households with non-high assets levels, which have a 27 percentage points (t = 1.48) lower probability of terminating a pregnancy conceived after the announcement relative to her previous pregnancy. Although these results are on a selected sample of women, with at least two pregnancies, they are in line with those obtained in the main specification.[31]

The results of these individual fixed effects specifications must, however, be interpreted with caution. Fixed effects estimators are particularly susceptible to measurement error bias, which is more likely to be exacerbated, due to the relatively small number of switches from which the fixed effects estimators are identified of; if there is measurement error, the proportion of misclassified observations will be larger. Secondly, the identifying assumption of the individual fixed effects strategy is that the unobserved heterogeneity that is correlated with the outcome of interest is constant over time, such that it will be differenced away when including the individual fixed effects. However, if there is time-variant unobserved heterogeneity or unobserved characteristics correlated with the outcomes which are non-constant over time (e.g. marital status or occupational status), the fixed effects estimators will be more biased than the OLS estimators.

6.4 Specification tests

I conduct several robustness checks to test the sensitivity of the results to various specifications. First, I exclude March 2003 from the analysis, since the first

[30]Excluding the controls for the number of previous abortions and the number of previous live births at the time of the pregnancy analyzed (as they may be regarded as lags of the dependent variable) does not change the point estimates nor the standard errors for the outcome "probability of conception".

[31]Excluding the controls for the number of previous abortions and the number of previous live births at the time of the pregnancy analyzed (as they may be regarded as lags of the dependent variable) does not change the magnitude of the estimated effects, although it does affect the standard errors from 0.18 to 0.22 for the non-high household assets group and from 0.62 to 0.45 for high household assets group.

Emergency Ordinance was issued in March 2003 and may have induced uncertainty regarding the level of MLB. Second, I also exclude April 2003. Third, I use conception month fixed effects instead of quarter of conception fixed effects, a more demanding specification. Forth, I include a quadratic trend. And fifth, I include a split trend after the policy change announcement. Results are presented in Table 11, where I show only the treatment effect, i.e. the interaction between $Treat * T^{Cert}$. The results are robust to these alternative specifications.

6.5 Efficiency of the estimator

So far, the main results and the robustness checks have all been estimated using OLS with robust estimates for the standard errors, but econometric theory shows that in the presence of heteroskedasticity, the OLS estimator is less efficient than, for example, the feasible generalized least squares (FGLS) estimator. However, the gain in efficiency of the FGLS over the OLS comes at the cost of stronger distributional assumptions about the variance of the error term. This issue can then be addressed by using a weighted least squares estimator (WLS), whereby standard errors are robust to misspecification of the error variance. (Cameron and Trivedi, 2005). Given that the main results, based on the OLS estimators, are marginally insignificant, for example on the probability of abortion, a potential avenue to increase the precision of the estimates is to use such an estimator. Tables 12, 13 and 14 present the main estimated treatment effects using WLS estimators. As expected, efficiency gains exist, but are relatively small for most outcomes. For the probability of abortion, however, the treatment effect becomes significant for the non-high households assets index group: the employed women who most likely benefited from the policy change have a 10 percentage ponts lower probability of terminating a pregnancy using abortion on request, indicating an increase in fertility. Similarly, the probability of breastfeeding becomes significant and is positive in the group of women with high household asset index, whereas the length of breastfeeding remains positive and significant for the same group, suggesting increased investments in infant health for the women who most likely were disadvantaged by the policy change.

7 Discussion and conclusions

This paper investigates the effects of financial incentives on fertility behavior and early investments in child health in a quasi-experimental setting: it uses a largely unanticipated and substantial change in the maternity leave benefits to which employed mothers were entitled, with most of them benefiting from the reform. The policy reform, which entailed the switch from proportional (equal to 85% of the mother's pre-birth earnings) to fixed benefits, was unexpected; moreover, the level of the fixed benefits was larger than the wage income of most employed women, hence potentially benefiting a very large share of the population. Using data from the Romanian Reproductive Health Survey collected one and a half years after the policy change announcement, I am able to explore the entire spectrum of individual level decisions related to fertility: decision to conceive, decision to carry the pregnancy to term, and several important outcomes conditional on live birth (maternal behavior during pregnancy, child health at birth and early investments in child health). I employ a double difference identification design in which employed women are the treatment group and out of the labor force women are the control group. Although insignificant, the main findings are suggestive of the fact that the substantial increase in the financial incentives led to an increase in conception rates and a decrease in the probability of abortion, especially for women from poorer households, who benefited more from the policy change. Despite not observing any significant changes in the observable characteristics of women who conceive following the announcement, all mothers appear to have worse prenatal behaviors. However, poorer mothers have children with better health outcomes at birth, and richer mothers and make more investments in early child health. For most outcomes I cannot exclude a zero effect of the policy, due to imprecisely estimated effects.

The majority of the main results, as well as the robustness tests and heterogeneity investigations, are relatively large in magnitude and in the expected direction, but have large standard errors which render them statistically indistinguishable from 0. This can stem from two mutually exclusive causes: there are significant and large effects but due to particularities of the dataset I am using, the analysis lacks power to precisely estimate the effects; or there are in fact no (or very small) effects of financial incentives on fertility and early investments in child health. I argue that most likely the first scenario plays an important role, although I cannot completely rule out the second.

Among the characteristics of the dataset that may lead to imprecisely estimated effects is the number of observations, i.e. individuals, in the dataset. An insufficient number of observations in the dataset would lead to a small sample size problem and a lack of power to detect effects of a certain size. Taking the example of the probability of abortion: assuming that the estimated coefficient on the treatment effect is unbiased, in the non-high household assets level, at an estimated effect of -0,088, for a significant effect to be detected the standard error would need to be 0,045, as opposed to 0,068. Since standard errors are proportional to $1/\sqrt{n}$, this would require the sample size to be increased by a factor of $(0,068/0,045)^2 = 2,28$, so at least 1830 observations, compared to the 801 observations available in my sample. This makes the sample size scenario a plausible explanation for the absence of significant results. Another data-related problem that may render the results imprecise is the short time interval between the actual policy change and the survey date: individuals may strongly react to the financial incentives by changing their fertility behavior, but conception may not occur immediately.[32] Hence the 7 months window that I can observe in the RHS data after the actual policy change, and 15 months from the announcement date, is insufficient to be able to estimate precise

[32] Medical evidence shows that each month that she tries, a healthy, fertile 30-year-old woman has only a 20% chance of getting pregnant.

effects on fertility. In the same time, the noisy estimates may be caused by outliers in the treatment group. Since I do not observe wage income, I cannot delimit gainers and losers of the reform perfectly and effects in the two samples may cancel each other out; using the household assets index classification only approximates these two groups, and miss-classifications based on this criterion has the same downward bias due to the opposing effects mentioned when analyzing the entire sample; moreover, there may be outliers or influential observations that render the results insignificant. And finally, the lack of precision may be caused by an imperfect control group and an inefficient estimator. I argue that these data issues are likely the cause of the imprecisely estimated effects. First, as was shown when discussing the main results, the point estimates I uncover are well within the range of the previously estimated effects for the outcomes that were studied before. Secondly, I show that using a more efficient (but rather uncommon) estimator, such as FGLS, does reduce the estimated standard errors -with some outcomes becoming significant at conventional levels (such as the probability of abortion for the group that most likely benefited from the reform). Thirdly, the effects are in line with the predictions of the Becker model. And lastly, as presented in the discussion of the institutional setting, the analysis of a longer time series of births at national level, shows that the 2004 reform of the MLB had, at least temporarily, reversed the downward trend in natality by discontinuously and significantly increasing the number of births for employed women.

On the other hand, the hypothesis of a true 0 effect of financial incentives on fertility and early investments in child health is could stem from a Ricardian equivalence, where individuals are forward looking and recognize that the significant increase in MLB will affect the Government's budget constrain, which could lead to future permanent increases in taxes; since the MLB would be paid for a determinate, short period of time, whereas investments in children would be long term, they do not change their fertility behavior or their investments in early child health. Although I have found no anecdotal evidence in favor of this hypothesis, I cannot completely rule it out.

In addition to the contribution to the academic debate, understanding how financial incentives affect reproductive behavior and abortion usage is important from a policy perspective in the context of a generalized downward trend in fertility in the developed countries and the large financial commitments on behalf of governments required to support this component of family policies. Understanding the effects of such benefits on the prenatal maternal behavior and on early investments in child health is particularly important given the mounting evidence that early-life conditions can have consequences on individual outcomes throughout the life cycle. This paper provides some preliminary evidence on the role of the monetary incentives that are part of the maternity leave benefits, but more research is needed confirm to the magnitude of the effects and understand the underlying mechanisms.

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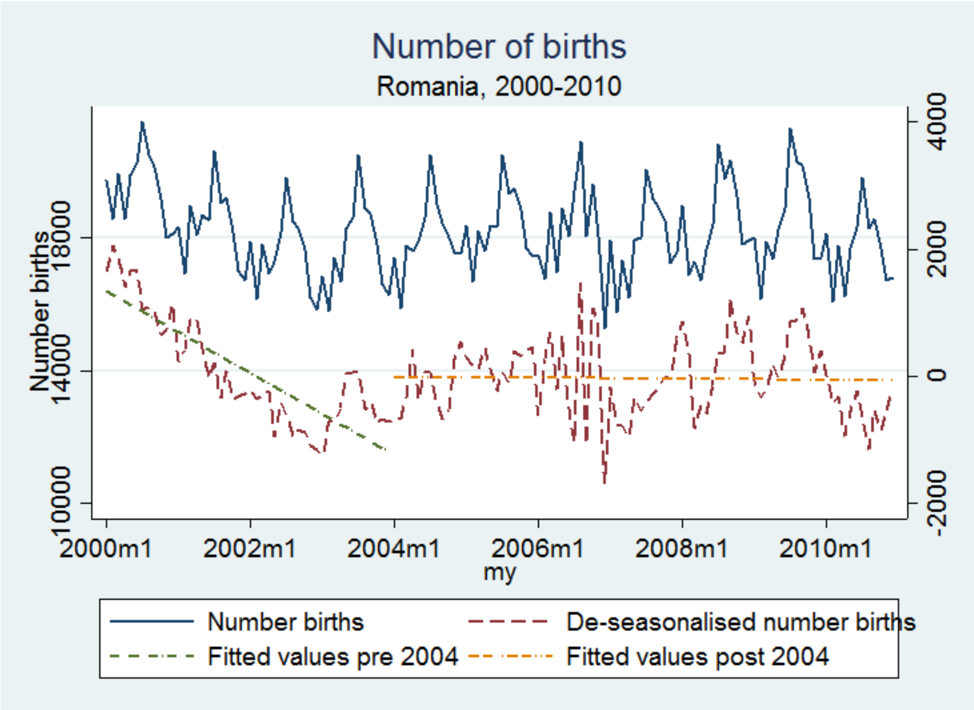
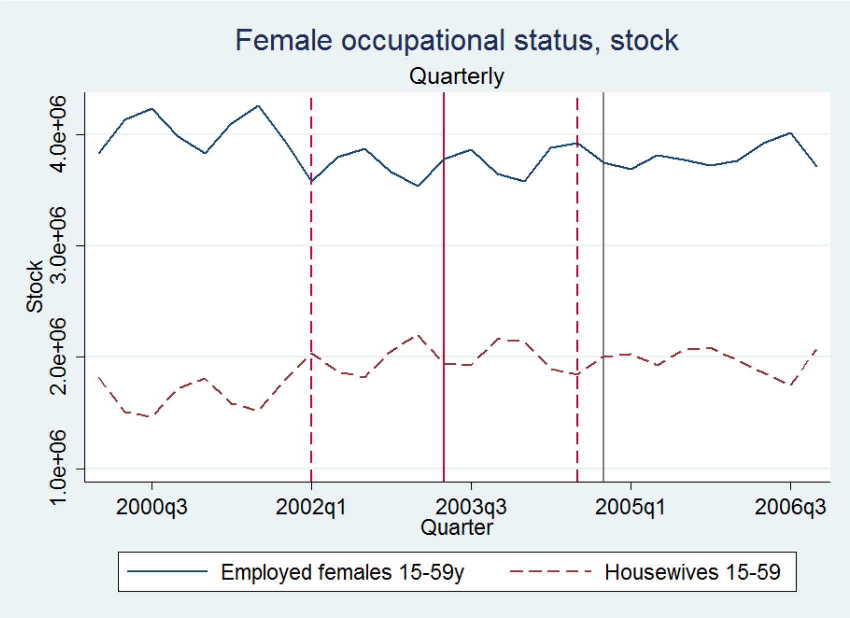
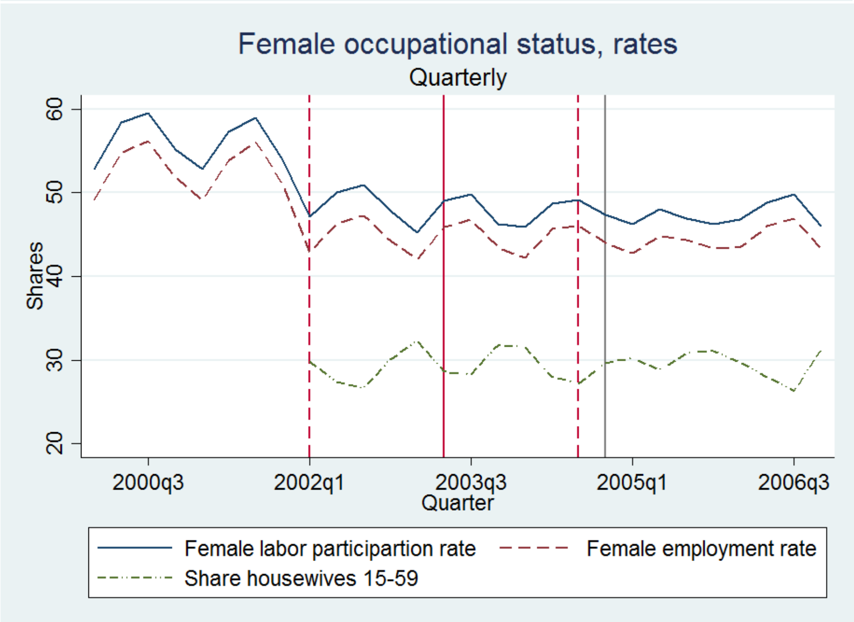
Figure 1: Number of births



Notes: Series of monthly number of live birth on left axis. De-seasonalized monthly series of live births (residual series after controlling for month dummy variables) on the right axis.

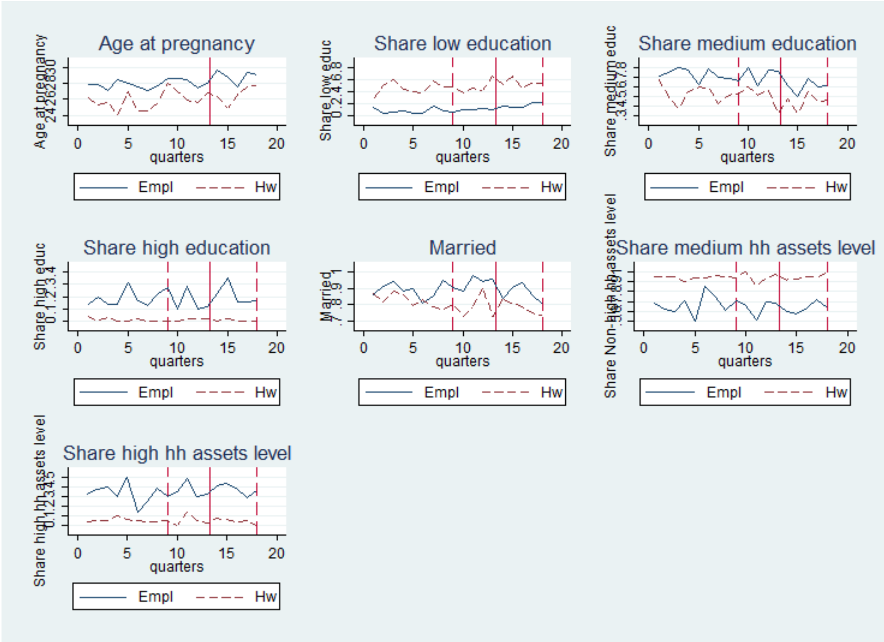
Figure 2: Occupational status of women





Notes: Quarterly stock women (unbroken line) and housewives (dashed line), 2000-2006. Vertical dotted lines delimit analysis sample. Vertical unbroken line marks the policy change announcement month. vertical gray unbroken line marks the time when RHS-Ro survey was conducted. Source: Statistics Romania.

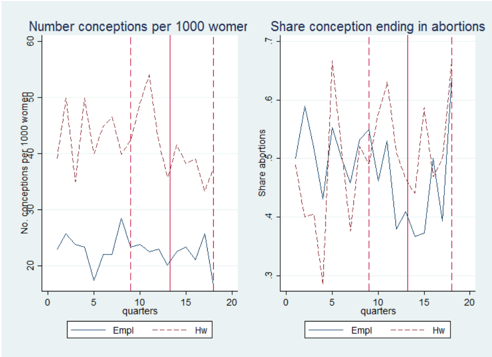
Figure 3: Average maternal characteristics by quarter of conception



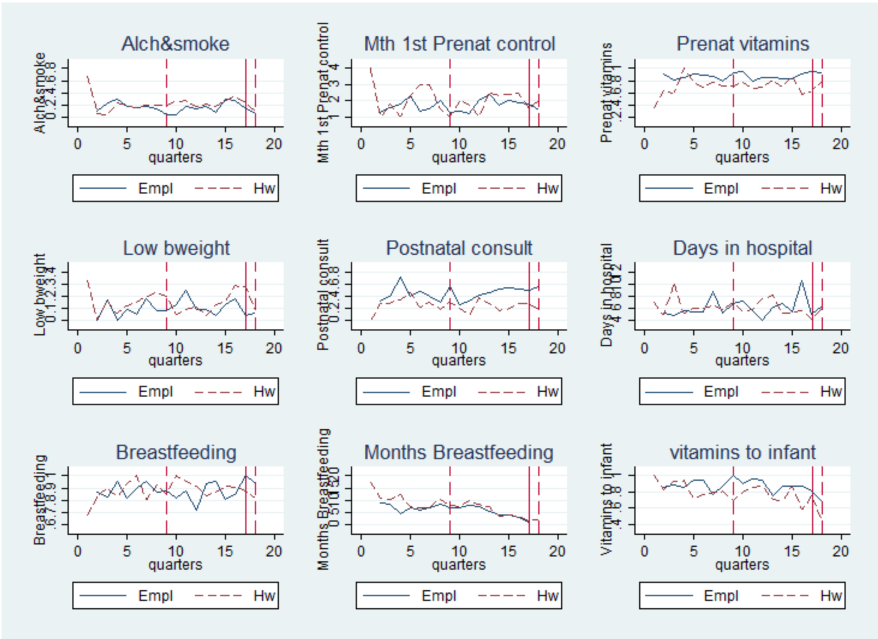
Notes: Average maternal characteristics by quarter of conception. Vertical dotted lines delimit analysis sample. Vertical unbroken line marks the policy change announcement month.

Figure 4: Average outcomes of interest by quarter of conception or birth

(a) Number of conceptions and probability of abortion by quarter of conception

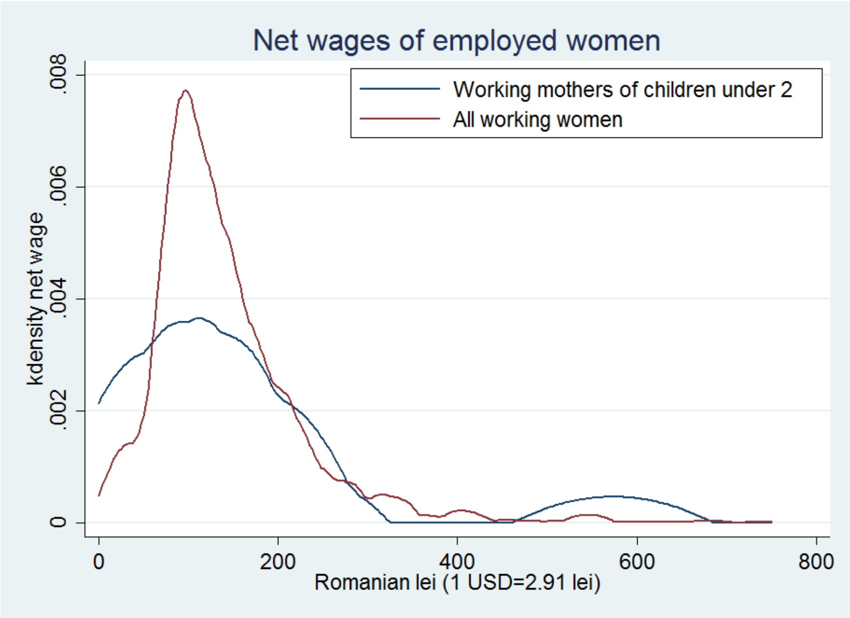


(b) Early investments in child health by quarter of conception



Notes: Average number of conceptions per 1000 women and average abortion rates by are quarter of conception. Average outcomes conditional on live birth are by quarter of birth. Vertical dotted lines delimit analysis sample. Vertical unbroken line marks the policy change month.

Figure 5: Net wages of employed women, GGS data



Notes: The distribution of net wages (thousand lei) of employed women (all and wimen with children under 2 who are working). Source: Generations and Gender Survey 2005.

Table 1: Descriptive statistics -observable characteristics of women in the RHS-Ro survey

Variable	(1) Survey	(2) Analysis sample	(3)	(4)
Age	30,278	27,813	28,563	26,840
	(8,018)	(5,640)	(5,820)	(5,315)
Low education	0,237	0,282	0,303	0,287
	(0,425)	(0,450)	(0,460)	(0,453)
Medium education	0,639	0,611	0,618	0,583
	(0,480)	(0,488)	(0,487)	(0,494)
High education	0,123	0,107	0,079	0,130
	(0,328)	(0,310)	(0,270)	(0,337)
Employed	0,491	0,483	0,437	0,520
	(0,499)	(0,500)	(0,497)	(0,500)
Housewife	0,271	0,428	0,431	0,429
	(0,444)	(0,495)	(0,496)	(0,495)
Other	0,237	0,089	0,132	0,051
	(0,425)	(0,285)	(0,339)	(0,221)
Married	0,664	0,818	0,804	0,826
	(0,472)	(0,386)	(0,397)	(0,379)
Low hh assets	0,368	0,439	0,477	0,425
	(0,482)	(0,497)	(0,500)	(0,495)
Medium hh assets	0,508	0,337	0,319	0,342
	(0,499)	(0,473)	(0,466)	(0,475)
High hh assets	0,123	0,224	0,204	0,233
	(0,328)	(0,417)	(0,404)	(0,423)
Rural	0,440	0,554	0,565	0,549
	(0,496)	(0,497)	(0,496)	(0,498)
Observations	4441	884	455	506

Notes: Descriptive statistics (standard error in parentheses) for selected observable characteristics: (1) all women included in the RHS survey; (2) women included in the analysis sample, who conceive between January 2002-July 2004; (3) women included in the analysis sample, who terminate a pregnancy using abortion between January 2002-July 2004; (4) women included in the analysis sample, who give birth between January 2002-July 2004.
Maternal education is coded using 10 educational categories, which I group in three levels: low (no schooling; primary education; secondary education), medium (upper secondary; professional education; high school education; post-high school education) and high (short term university degree; long term university degree; post-graduate degree). Marital status is recorded using six categories, which are then grouped into two broad groups: married (legally married or cohabiting) and unmarried (never married, divorced, widowed, separated).

Table 2: Descriptive statistics -observable characteristics of women who conceive

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		All			Employed			Housewives	
VARIABLES	t0	t1	t1-t0	t0	t1	t1-t0	t0	t1	t1-t0
Panel A: All conceptions									
Age at pregn	27.329	27.679	0.351	28.215	28.972	0.756	26.625	26.744	0.119
Sh. Low educ	0.287	0.366	0.079***	0.098	0.178	0.081***	0.468	0.545	0.076*
Sh. Med. educ	0.627	0.530	-0.097***	0.728	0.607	-0.120***	0.520	0.451	-0.069
Sh. High educ	0.086	0.104	0.018	0.175	0.215	0.040	0.011	0.004	-0.007
Married	0.838	0.797	-0.041*	0.931	0.874	-0.056**	0.788	0.785	-0.004
Sh. Low SES	0.422	0.492	0.070**	0.175	0.251	0.076**	0.662	0.720	0.058
Sh. Med. SES	0.492	0.404	-0.088***	0.650	0.534	-0.116***	0.327	0.276	-0.051
Sh. High SES	0.086	0.104	0.018	0.175	0.215	0.040	0.011	0.004	-0.007
Share abortions	0.526	0.514	-0.012	0.467	0.441	-0.026	0.543	0.545	0.002
Observations	557	549		246	247		269	246	
Panel B: Abortions									
Age at pregn	27.942	28.461	0.519	29.096	29.991	0.895	27.253	27.821	0.567
Sh. Low educ	0.307	0.397	0.090**	0.113	0.202	0.089*	0.473	0.545	0.072
Sh. Med educ	0.638	0.528	-0.110***	0.757	0.615	-0.142**	0.527	0.455	-0.072
Sh. High educ	0.055	0.074	0.020	0.130	0.183	0.053	0.000	0.000	0.000
Married	0.829	0.801	-0.028	0.904	0.835	-0.069	0.829	0.851	0.022
Sh. Low SES	0.447	0.539	0.092**	0.217	0.275	0.058	0.658	0.731	0.074
Sh. Med SES	0.498	0.387	-0.112***	0.652	0.541	-0.111*	0.342	0.269	-0.051
Sh. High SES	0.055	0.074	0.020	0.130	0.183	0.053	0.000	0.000	0.000
Observations	293	282		115	109		146	134	
Panel C: Live births									
Age at pregn	26.648	26.854	0.206	27.443	28.167	0.724	25.878	25.455	-0.423
Sh. Low educ	0.265	0.333	0.068*	0.084	0.159	0.075*	0.463	0.545	0.081
Sh. Med. educ	0.614	0.532	-0.082*	0.702	0.601	-0.101*	0.512	0.446	-0.066
Sh. High educ	0.121	0.135	0.014	0.214	0.239	0.025	0.024	0.009	-0.015
Married	0.848	0.794	-0.054	0.954	0.906	-0.048	0.740	0.705	-0.034
Sh. Low SES	0.394	0.442	0.048	0.137	0.232	0.094**	0.667	0.705	0.039
Sh. Med. SES	0.485	0.423	-0.062	0.649	0.529	-0.120**	0.309	0.286	-0.051
Sh. High SES	0.121	0.135	0.014	0.214	0.239	0.025	0.024	0.009	-0.015
Observations	264	267		131	138		123	112	

Notes: Descriptive statistics for selected observable characteristics of all women who conceive (Panel A), women who conceive and terminate the pregnancy using abortion (Panel B), and women who conceive and carry the pregnancy to term (Panel C) in the selected time window: t0=Janyary 2000-March 2003, t1=April 2003-July 2004.

Table 3: Probability of conception: conceptions per 1000 women

VARIABLES	(1) DD	(2) DD	(3) DD Non-high	(4) DD High
$Treat * T^{Cert}$	0.464	0.464	0.656	0.045
	(0.485)	(0.497)	(0.650)	(0.719)
$Treat$	-0.450	-0.450	-2.214***	3.412***
	(0.367)	(0.376)	(0.518)	(0.457)
T^{Cert}	0.013	0.048	-0.225	0.647
	(0.598)	(0.635)	(0.727)	(0.924)
Observations	62	62	62	62
R-squared	0.160	0.167	0.441	0.666
Time trend	Yes	Yes	Yes	Yes
Quarter FE	No	Yes	Yes	Yes

Notes: Dependent variable: monthly number of conceptions per 1000 females. $Treat$ is 1 for employed women and 0 for housewives. T^{Cert} is 1 for conceptions occurring after April 2003. Controls: linear time trend, quarter of conception fixed effects. Regressions include T^{Pot} that is 1 for conceptions occurring between January-March 2003, and the interaction term $Treat * T^{Pot}$."Non-high" refers to households with low and medium household assets levels. "High" refers to households with high household assets level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4: Selection on observable characteristics

VARIABLES	(1) Age at pregnancy	(2) Low educ.	(3) Medium educ.	(4) High educ,	(5) Married	(6) Nonhigh hh assets	(7) Rural
Panel A: All conceptions							
$Treat * T^{Cert}$	0.538	-0.029	0.002	0.024	-0.035	-0.019	-0.083
	(0.721)	(0.056)	(0.064)	(0.038)	(0.047)	(0.050)	(0.059)
$Treat$	1.663***	-0.338***	0.156***	0.173***	0.125***	-0.278***	-0.338***
	(0.513)	(0.039)	(0.046)	(0.028)	(0.032)	(0.036)	(0.045)
T^{Cert}	-1.030	0.049	-0.106	0.063	0.012	-0.025	0.070
	(0.949)	(0.079)	(0.085)	(0.043)	(0.064)	(0.057)	(0.073)
Observations	1,008	1,008	1,008	1,008	1,008	1,008	1,008
R-squared	0.044	0.171	0.062	0.127	0.038	0.162	0.191
Panel B: Abortions							
$Treat * T^{Cert}$	-0.238	0.012	-0.043	0.031	-0.082	-0.088	-0.126
	(1.030)	(0.080)	(0.090)	(0.053)	(0.066)	(0.070)	(0.085)
$Treat$	2.445***	-0.349***	0.193***	0.156***	0.065	-0.296***	-0.348***
	(0.724)	(0.055)	(0.064)	(0.037)	(0.045)	(0.050)	(0.064)
T^{Cert}	-0.277	-0.053	0.024	0.029	0.045	0.058	0.109
	(1.275)	(0.112)	(0.118)	(0.053)	(0.080)	(0.073)	(0.095)
Observations	504	504	504	504	504	504	504
R-squared	0.053	0.154	0.069	0.118	0.018	0.199	0.188
Panel C: Live births							
$Treat * T^{Cert}$	1.429	-0.063	0.047	0.016	0.010	0.050	-0.053
	(1.039)	(0.081)	(0.093)	(0.058)	(0.069)	(0.073)	(0.086)
$Treat$	1.173	-0.324***	0.119*	0.205***	0.191***	-0.291***	-0.358***
	(0.739)	(0.057)	(0.067)	(0.043)	(0.048)	(0.054)	(0.065)
T^{Cert}	-0.686	0.164	-0.246*	0.082	-0.002	-0.119	0.093
	(1.421)	(0.113)	(0.126)	(0.075)	(0.101)	(0.089)	(0.112)
Observations	504	504	504	504	504	504	504
R-squared	0.069	0.193	0.065	0.122	0.088	0.128	0.167

Notes: Dependent variable: observable maternal characteristic. Controls: linear time trend, quarter of conception fixed effects. $Treat$ is 1 for employed women and 0 for housewives. In Panel A and B, T^{Cert} is 1 for pregnancies conceived between April 2003 and July 2004. Regressions include T^{Pot} that is 1 for conceptions occurring between January-March 2003. In Panel C, T^{Cert} is 1 for pregnancies delivered between January 2004 and July 2004. T^{Pot} is 1 for pregnancies delivered between April-December 2003, and the interaction term $Treat * T^{Pot}$. "Non-high" refers to households with low and medium household assets levels. "High" refers to households with high household assets level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 5: Conditional on conception: Probability of abortion

VARIABLES	(1) DD	(2) DD	(3) DD	(4) DD Non-high	(5) DD High
$Treat * T^{Cert}$	-0.031	-0.046	-0.047	-0.088	0.081
	(0.065)	(0.059)	(0.059)	(0.068)	(0.178)
$Treat$	-0.069	0.013	0.016	0.017	0.045
	(0.048)	(0.046)	(0.046)	(0.052)	(0.121)
T^{Cert}	-0.194**	-0.171**	-0.194***	-0.158*	-0.411**
	(0.080)	(0.071)	(0.075)	(0.081)	(0.202)
Observations	1,008	1,008	1,008	801	207
R-squared	0.023	0.214	0.217	0.228	0.265
Time trend	Yes	Yes	Yes	Yes	Yes
Quarter FE	No	No	Yes	Yes	Yes
Ind. cov.	No	Yes	Yes	Yes	Yes

Notes: Dependent variable: 1 if pregnancy terminated using abortion. $Treat$ is 1 for employed women and 0 for housewives. T^{Cert} is 1 for conceptions occurring after April 2003. Controls: linear time trend, quarter of conception fixed effects; individual controls(number of previous abortions at pregnancy i, age at pregnancy i, educational level, marital status, rural dummy, number of members in household). Regressions include T^{Pot} that is 1 for conceptions occurring between January-March 2003, and the interaction term $Treat * T^{Pot}$. "Non-high" refers to households with low and medium household assets levels. "High" refers to households with high household assets level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 6: Outcomes conditional on live birth

VARIABLES	(1) Alcohol& smoking	(2) Prenatal vitamins	(3) Mth 1st control	(4) Low bweight	(5) Postnatal consult	(6) Days in hospital	(7) Infant vitamins	(8) Breast feeding	(9) Months breastf.
Panel A: All women									
$Treat * T^{Cert}$	0.136	-0.060	-0.012	-0.112	0.107	1.497	0.002	-0.015	1.219
	(0.084)	(0.083)	(0.315)	(0.072)	(0.099)	(1.255)	(0.076)	(0.065)	(1.104)
$Treat$	-0.065	0.065	-0.407*	0.038	0.063	0.182	0.023	-0.022	-0.920
	(0.050)	(0.061)	(0.225)	(0.045)	(0.063)	(0.715)	(0.049)	(0.044)	(0.947)
T^{Cert}	-0.025	0.191	0.921	0.029	0.171	-2.050	-0.043	-0.149	-6.660***
	(0.144)	(0.146)	(0.600)	(0.123)	(0.146)	(1.579)	(0.112)	(0.099)	(2.114)
Observations	520	486	486	520	520	517	520	520	344
R-squared	0.122	0.108	0.186	0.026	0.100	0.020	0.061	0.024	0.206
Panel B: Non-high hh assets index									
$Treat * T^{Cert}$	0.131	0.014	-0.141	-0.130	0.136	2.336	-0.003	-0.046	-0.001
	(0.094)	(0.096)	(0.373)	(0.088)	(0.113)	(1.680)	(0.091)	(0.083)	(1.501)
$Treat$	-0.125**	0.024	-0.412	0.034	0.085	-0.031	0.031	-0.017	-0.182
	(0.053)	(0.070)	(0.259)	(0.053)	(0.067)	(0.666)	(0.055)	(0.053)	(1.135)
T^{Cert}	-0.014	0.192	0.822	0.178	0.188	-0.966	-0.089	-0.156	-6.020**
	(0.157)	(0.171)	(0.723)	(0.135)	(0.150)	(1.271)	(0.132)	(0.118)	(2.521)
Observations	399	366	366	399	399	396	399	399	249
R-squared	0.132	0.117	0.139	0.032	0.063	0.028	0.059	0.043	0.206
Panel C: High hh assets index									
$Treat * T^{Cert}$	0.197	-0.279	0.097	0.069	0.387	0.482	-0.033	0.131	6.096***
	(0.128)	(0.178)	(0.807)	(0.058)	(0.308)	(2.115)	(0.061)	(0.085)	(2.079)
$Treat$	0.197**	0.179	-0.019	0.008	-0.349	0.154	-0.021	-0.110	-4.664**
	(0.091)	(0.168)	(0.453)	(0.038)	(0.232)	(1.770)	(0.053)	(0.068)	(1.945)
T^{Cert}	-0.006	0.153	1.534	-0.704**	-0.407	-8.429	0.126	-0.245	-13.514***
	(0.331)	(0.279)	(1.081)	(0.299)	(0.505)	(7.316)	(0.162)	(0.152)	(3.898)
Observations	121	120	120	121	121	121	121	121	95
R-squared	0.211	0.089	0.206	0.283	0.129	0.196	0.059	0.159	0.446
pval diff	0.672	0.137	0.782	0.060	0.430	0.484	0.781	0.132	0.015
Time trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ind cov	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Dependent variable: (1) 1 if mother consumed alcohol or smoked during pregnancy; (2) 1 if mother took prenatal vitamins; (3) month of first prenatal control; (4) 1 if child is born with ¡2500 g; (5) 1 if mother and child had a postnatal control; (6) number of days in hospital after birth; (7) 1 if infant was given vitamins; (8) 1 if infant was breastfed; (9) number of months of breastfeeding. $Treat$ is 1 for employed women and 0 for housewives. T^{Cert} is 1 for births occurring after January 2004. Controls: linear time trend, quarter of conception fixed effects; individual controls(number of previous abortions at pregnancy i, age at pregnancy i, educational level, marital status, rural dummy, number of members in household). Regressions include T^{Pot} that is 1 for births occurring between April-December 2003, and the interaction term $Treat * T^{Pot}$. "Non-high" refers to households with low and medium household assets levels. "High" refers to households with high household assets level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 7: Probability of conception: conceptions per 1000 women, October threshold

VARIABLES	(1) DD	(2) DD	(3) DD Non-high	(4) DD High
$Treat * T^{Cert}$	0.383	0.383	0.738	-0.395
	(0.552)	(0.558)	(0.766)	(0.686)
$Treat$	-0.450	-0.450	-2.214***	3.412***
	(0.372)	(0.376)	(0.511)	(0.449)
T^{Cert}	-0.244	0.652	0.693	0.560
	(0.857)	(1.094)	(1.497)	(1.253)
Observations	62	62	62	62
R-squared	0.135	0.165	0.446	0.666
Time trend	Yes	Yes	Yes	Yes
Quarter FE	No	Yes	Yes	Yes

Notes: Dependent variable: monthly number of conceptions per 1000 females. $Treat$ is 1 for employed women and 0 for housewives. T^{Cert} is 1 for conceptions occurring after October 2003. Controls: linear time trend, quarter of conception fixed effects. Regressions include T^{Pot} that is 1 for conceptions occurring between January-September 2003, and the interaction term $Treat * T^{Pot}$. "Non-high" refers to households with low and medium household assets levels. "High" refers to households with high household assets level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 8: Conditional on conception: Probability of abortion, October threshold

VARIABLES	(1) DD	(2) DD	(3) DD	(4) DD Non-high	(5) DD High
$Treat * T^{Cert}$	-0.016	-0.031	-0.027	-0.075	0.161
	(0.073)	(0.067)	(0.067)	(0.076)	(0.242)
$Treat$	-0.064	0.002	0.015	0.015	0.033
	(0.044)	(0.043)	(0.043)	(0.048)	(0.106)
T^{Cert}	0.260***	0.188**	0.337***	0.312***	0.313
	(0.088)	(0.079)	(0.091)	(0.098)	(0.306)
Observations	1,008	1,008	1,008	801	207
R-squared	0.019	0.207	0.217	0.228	0.267
Time trend	Yes	Yes	Yes	Yes	Yes
Quarter FE	No	No	Yes	Yes	Yes
Ind. cov.	No	Yes	Yes	Yes	Yes

Notes: Dependent variable: 1 if pregnancy terminated using abortion. $Treat$ is 1 for employed women and 0 for housewives. T^{Cert} is 1 for conceptions occurring after October 2003. Controls: linear time trend, quarter of conception fixed effects; individual controls(number of previous abortions at pregnancy i, age at pregnancy i, educational level, marital status, rural dummy, number of members in household). Regressions include T^{Pot} that is 1 for conceptions occurring between January-September 2003, and the interaction term $Treat * T^{Pot}$. "Non-high" refers to households with low and medium household assets levels. "High" refers to households with high household assets level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 9: Outcomes conditional on live birth- October threshold

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	Alcohol& smoking	Prenatal vitamins	Mth 1st control	Low bweight	Postnatal consult	Days in hospital	Infant vitamins	Breast feeding	Months breastf.
Panel A: All women									
$Treat * T^{Cert}$	0.109	-0.049	-0.017	-0.115*	0.118	1.777	0.004	-0.019	1.186
	(0.081)	(0.077)	(0.297)	(0.067)	(0.094)	(1.193)	(0.071)	(0.063)	(0.980)
$Treat$	-0.038	0.052	-0.416**	0.040	0.055	-0.084	0.022	-0.017	-0.955
	(0.044)	(0.052)	(0.198)	(0.039)	(0.055)	(0.678)	(0.041)	(0.038)	(0.783)
T^{Cert}	-0.065	0.131	-0.006	0.122*	0.034	-0.704	-0.049	-0.003	-4.803***
	(0.089)	(0.092)	(0.361)	(0.071)	(0.101)	(1.183)	(0.076)	(0.068)	(1.202)
Observations	520	486	486	520	520	517	520	520	344
R-squared	0.123	0.108	0.178	0.027	0.104	0.024	0.064	0.021	0.213
Panel B: Non-high hh assets index									
$Treat * T^{Cert}$	0.087	0.024	-0.106	-0.139*	0.162	2.559	-0.006	-0.089	0.053
	(0.092)	(0.089)	(0.351)	(0.082)	(0.108)	(1.644)	(0.086)	(0.079)	(1.314)
$Treat$	-0.082*	0.014	-0.453**	0.043	0.062	-0.241	0.035	0.025	-0.388
	(0.048)	(0.060)	(0.227)	(0.046)	(0.060)	(0.760)	(0.047)	(0.043)	(0.926)
T^{Cert}	-0.087	0.130	-0.070	0.186**	0.034	-0.336	-0.076	-0.051	-3.854***
	(0.099)	(0.107)	(0.417)	(0.082)	(0.108)	(1.439)	(0.088)	(0.076)	(1.459)
Observations	399	366	366	399	399	396	399	399	249
R-squared	0.130	0.116	0.130	0.037	0.059	0.029	0.062	0.036	0.216
Panel C: High hh assets index									
$Treat * T^{Cert}$	0.230*	-0.267*	0.101	0.012	0.119	-0.341	-0.020	0.195**	4.159**
	(0.121)	(0.138)	(0.744)	(0.055)	(0.279)	(1.838)	(0.058)	(0.088)	(1.618)
$Treat$	0.167**	0.171	0.009	0.059	-0.103	0.857	-0.033	-0.197**	-2.896**
	(0.070)	(0.120)	(0.460)	(0.042)	(0.182)	(1.347)	(0.046)	(0.076)	(1.448)
T^{Cert}	-0.049	0.196	0.141	-0.140	-0.081	-1.599	0.094	0.087	-9.225***
	(0.152)	(0.162)	(0.831)	(0.086)	(0.319)	(2.102)	(0.068)	(0.121)	(1.998)
Observations	121	120	120	121	121	121	121	121	95
R-squared	0.215	0.103	0.184	0.170	0.140	0.193	0.062	0.237	0.436
pval diff	0.335	0.071	0.796	0.125	0.882	0.232	0.889	0.015	0.046
Time trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ind cov	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Dependent variable: (1) 1 if mother consumed alcohol or smoked during pregnancy; (2) 1 if mother took prenatal vitamins; (3) month of first prenatal control; (4) 1 if child is born with ¡2500 g; (5) 1 if mother and child had a postnatal control; (6) number of days in hospital after birth; (7) 1 if infant was given vitamins; (8) 1 if infant was breastfed; (9) number of months of breastfeeding. $Treat$ is 1 for employed women and 0 for housewives. T^{Cert} is 1 for births occurring after January 2004. Controls: linear time trend, quarter of conception fixed effects; individual controls(number of previous abortions at pregnancy i, age at pregnancy i, educational level, marital status, rural dummy, number of members in household). Regressions include T^{Pot} that is 1 for births occurring between October-December 2003, and the interaction term $Treat{*}T^{Pot}$. "Non-high" refers to households with low and medium household assets levels. "High" refers to households with high household assets level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 10: Robustness -Mother fixed effects

VARIABLES	(1) Baseline	(2) Non-high	(3) High
Panel A: Conception			
$Treat * T^{Cert}$	0.016	0.013	0.012
	(0.016)	(0.019)	(0.040)
T^{Cert}	-0.018	-0.019	-0.010
	(0.014)	(0.015)	(0.038)
Observations	6,774	4,748	2,026
R-squared	0.561	0.559	0.563
Ind cov	Yes	Yes	Yes
Panel B: Abortion			
$Treat * T^{Cert}$	-0.208	-0.276	-0.121
	(0.144)	(0.186)	(0.625)
T^{Cert}	-0.203	-0.186	-0.432
	(0.151)	(0.162)	(0.576)
Observations	529	445	84
R-squared	0.812	0.821	0.827
Ind cov	Yes	Yes	Yes

Notes: Panel A: dependent variable is 1 if women conceived in period "t" (t=0 for January 2000-March 2003; t=1 for April 2003-July 2004). Controls: number of previous abortions at pregnancy i,number of children at pregnancy i, whether the woman was already pregnant at the begining of the period. Panel B: sample of women with at leat 2 pregnancies in the interval January 2000-July 2004. Dependent variable is 1 if pregnancy i was terminated using abortion. $Treat$ is 1 for employed women and 0 for housewives. T^{Cert} is 1 for conceptions occurring after April 2003. Controls: number of previous abortions at pregnancy i, number of children at pregnancy i. "Non-high" refers to households with low and medium household assets levels. "High" refers to households with high household assets level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 11: Sensitivity to specification

		(1)	(2)	(3)	(4)	(5)	(6)
I	Concept per1000	0.464	0.464	0.435	0.464	0.464	0.464
		(0.480)	(0.482)	(0.496)	(0.466)	(0.484)	(0.485)
II	Prob abortion	-0.047	-0.052	-0.048	-0.046	-0.046	-0.046
		(0.059)	(0.059)	(0.060)	(0.060)	(0.059)	(0.059)
III	Alcohol, smoke	0.136	0.128	0.132	0.134	0.136	0.136
		(0.084)	(0.084)	(0.087)	(0.086)	(0.084)	(0.084)
	Prenatal vitamins	-0.060	-0.062	-0.037	-0.069	-0.060	-0.060
		(0.083)	(0.084)	(0.088)	(0.086)	(0.084)	(0.083)
	Mth 1st control	-0.012	-0.004	-0.109	0.079	-0.020	-0.012
		(0.315)	(0.316)	(0.334)	(0.320)	(0.314)	(0.315)
	Low birth weight	-0.112	-0.123*	-0.120	-0.117	-0.112	-0.112
		(0.072)	(0.072)	(0.077)	(0.074)	(0.072)	(0.072)
	Postnatal consult	0.107	0.095	0.065	0.089	0.107	0.106
		(0.099)	(0.100)	(0.105)	(0.101)	(0.100)	(0.100)
	Days in hosp	1.497	1.482	1.781	1.592	1.497	1.505
		(1.255)	(1.276)	(1.389)	(1.296)	(1.256)	(1.259)
	Vitamins to infant	0.002	0.010	0.025	-0.016	0.002	-0.000
		(0.076)	(0.075)	(0.082)	(0.075)	(0.076)	(0.075)
	Breastfeeding	-0.015	-0.019	-0.006	0.003	-0.015	-0.013
		(0.065)	(0.066)	(0.067)	(0.066)	(0.065)	(0.065)
	Mths breastfeed	1.219	1.145	1.359	1.084	1.165	1.092
		(1.104)	(1.120)	(1.182)	(1.126)	(1.081)	(1.091)

Notes: (1) Baseline specification; (2)Exclude March 2003; (3) Exclude March and April 2003; (4) Conception month fixed effects; (5) Quadratic trend; (6) Split trend.
Panel I: Regression dos not control for any individual level characteristics. Panel II and III: Regressions include individual controls(number of previous abortions at pregnancy i, age at pregnancy i, educational level, marital status, rural dummy, number of members in household). Panel I and II: T^{Cert} is 1 for conceptions occurring after April 2003. Regressions include T^{Pot} that is 1 for conceptions occurring between January-March 2003, and the interaction term $Treat * T^{Pot}$. Panel III: T^{Cert} is 1 for births occurring after January 2004. Regressions include T^{Pot} that is 1 for births occurring between April-December 2003, and the interaction term $Treat * T^{Pot}$. "Non-high" refers to households with low and medium household assets levels. "High" refers to households with high household assets level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 12: Probability of conception: conceptions per 1000 women, Weighted Least Squares

VARIABLES	(1) DD	(2) DD	(3) DD Non-high	(4) DD High
$Treat * T^{Cert}$	0.460	0.460	0.673	-0.043
	(0.471)	(0.480)	(0.663)	(0.697)
$Treat$	-0.454	-0.454	-2.233***	3.411***
	(0.366)	(0.366)	(0.527)	(0.458)
T^{Cert}	0.048	0.087	-0.010	0.894
	(0.538)	(0.589)	(0.790)	(1.131)
Observations	62	62	62	62
R-squared	0.171	0.181	0.404	0.586

Notes: Dependent variable: monthly number of conceptions per 1000 females. $Treat$ is 1 for employed women and 0 for housewives. T^{Cert} is 1 for conceptions occurring after April 2003. Controls: linear time trend, quarter of conception fixed effects. Regressions include T^{Pot} that is 1 for conceptions occurring between January-March 2003, and the interaction term $Treat*T^{Pot}$. "Non-high" refers to households with low and medium household assets levels. "High" refers to households with high household assets level. Estimation using Weighted Least Squares. *** p<0.01, ** p<0.05, * p<0.1

Table 13: Conditional on conception: Probability of abortion, Weighted Least Squares

	(1)	(2)	(3)	(4)	(5)
VARIABLES	DD	DD	DD	DD Non-high	DD High
$Treat * T^{Cert}$	-0.050	-0.069	-0.069	-0.103*	0.001
	(0.071)	(0.050)	(0.050)	(0.058)	(0.159)
$Treat$	-0.164***	0.004	0.005	0.006	0.087
	(0.049)	(0.037)	(0.037)	(0.041)	(0.077)
T^{Cert}	-0.123	-0.146***	-0.163***	-0.140**	-0.361**
	(0.084)	(0.054)	(0.061)	(0.065)	(0.181)
Observations	1,008	1,008	1,008	801	207
R-squared	0.049	0.367	0.369	0.373	0.404

Notes: Dependent variable: 1 if pregnancy terminated using abortion. $Treat$ is 1 for employed women and 0 for housewives. T^{Cert} is 1 for conceptions occurring after April 2003. Controls: linear time trend, quarter of conception fixed effects; individual controls(number of previous abortions at pregnancy i, age at pregnancy i, educational level, marital status, rural dummy, number of members in household). Regressions include T^{Pot} that is 1 for conceptions occurring between January-March 2003, and the interaction term $Treat * T^{Pot}$. ”Non-high” refers to households with low and medium household assets levels. ”High” refers to households with high household assets level. Estimation using Weighted Least Squares. *** p<0.01, ** p<0.05, * p<0.1

Table 14: Outcomes conditional on live birth- Weighted Least Squares

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	Alcohol& smoking	Prenatal vitamins	Mth 1st control	Low bweight	Postnatal consult	Days in hospital	Infant vitamins	Breast feeding	Months breastf.
Panel A: All women									
$Treat * T^{Cert}$	0.097	-0.095	0.067	-0.090	0.123	1.954	-0.016	0.001	1.012
	(0.078)	(0.077)	(0.289)	(0.069)	(0.099)	(1.452)	(0.071)	(0.063)	(1.036)
$Treat$	-0.061	0.074	-0.495**	0.039	0.047	-0.865	0.014	-0.019	-1.038
	(0.045)	(0.058)	(0.210)	(0.046)	(0.062)	(0.996)	(0.049)	(0.042)	(0.947)
$Treat$	-0.030	0.188	1.080**	-0.011	0.170	-0.196	-0.059	-0.125	-5.832***
	(0.134)	(0.137)	(0.501)	(0.125)	(0.143)	(1.432)	(0.101)	(0.096)	(2.217)
Observations	520	486	486	520	520	517	520	520	344
R-squared	0.103	0.089	0.235	0.026	0.103	0.080	0.044	0.017	0.209
Panel B: Non-high hh assets index									
$Treat * T^{Cert}$	0.090	-0.013	0.038	-0.115	0.145	2.166	-0.015	-0.023	-0.296
	(0.088)	(0.088)	(0.344)	(0.087)	(0.114)	(1.658)	(0.090)	(0.079)	(1.388)
$Treat$	-0.123***	0.031	-0.562**	0.038	0.067	-0.878	0.016	-0.013	-0.308
	(0.047)	(0.066)	(0.247)	(0.053)	(0.066)	(1.047)	(0.054)	(0.050)	(1.085)
T^{Cert}	-0.043	0.180	0.842	0.148	0.177	-0.441	-0.128	-0.138	-4.865**
Observations	399	366	366	399	399	396	399	399	249
R-squared	0.118	0.108	0.203	0.032	0.063	0.081	0.042	0.031	0.184
Panel C: High hh assets index									
$Treat * T^{Cert}$	0.054	-0.285	-0.042	0.055	0.410	2.118	-0.038	0.133*	5.984***
	(0.055)	(0.176)	(0.686)	(0.055)	(0.306)	(1.897)	(0.066)	(0.078)	(2.081)
$Treat$	0.018	0.172	-0.072	0.016	-0.353	-1.712	-0.016	-0.114	-5.164***
	(0.037)	(0.163)	(0.486)	(0.037)	(0.229)	(1.484)	(0.059)	(0.069)	(1.915)
T^{Cert}	-0.633**	0.196	1.682*	-0.646**	-0.394	-2.199	0.097	-0.224	-11.026***
	(0.286)	(0.308)	(0.898)	(0.286)	(0.510)	(3.804)	(0.152)	(0.136)	(4.034)
Observations	121	120	120	121	121	121	121	121	95
R-squared	0.265	0.096	0.150	0.272	0.137	0.169	0.065	0.130	0.504
Time trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ind cov	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Dependent variable: (1) 1 if mother consumed alcohol or smoked during pregnancy; (2) 1 if mother took prenatal vitamins; (3) month of first prenatal control; (4) 1 if child is born with ¡2500 g; (5) 1 if mother and child had a postnatal control; (6) number of days in hospital after birth; (7) 1 if infant was given vitamins; (8) 1 if infant was breastfed; (9) number of months of breastfeeding. $Treat$ is 1 for employed women and 0 for housewives. T^{Cert} is 1 for births occurring after January 2004. Controls: linear time trend, quarter of conception fixed effects; individual controls(number of previous abortions at pregnancy i, age at pregnancy i, educational level, marital status, rural dummy, number of members in household). Regressions include T^{Pot} that is 1 for births occurring between October-December 2003, and the interaction term $Treat{*}T^{Pot}$. "Non-high" refers to households with low and medium household assets levels. "High" refers to households with high household assets level. Estimation using Weighted Least Squares. *** p<0.01, ** p<0.05, * p<0.1