The Effects of a Maternity Leave Reform on Children's Abilities and Maternal Outcomes in Chile

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This paper studies a change in paid maternity leave entitlements in Chile. We exploit a reform that increased paid leave from 12 to 24 weeks for mothers of children born on July 25, 2011 or later. We estimate the effects of reform exposure on different children and maternal outcomes finding significant and positive effects on children's cognitive abilities, especially for those with less educated mothers. There is an increase in the probability of breastfeeding at least six months and breastfeeding durations. Maternal stress exhibits a significant reduction, and there is an increase in the probability of being employed for mothers after maternity leave.
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I. Introduction

Early childhood has taken a central role in human capital literature over the past decade. Investments during this stage are fundamental to the skill accumulation process especially for cognitive skills (Cunha and Heckman, 2008; Cunha, Heckman and Schennach, 2010), and they explain a broad range of adult outcomes (Heckman, Stixrud and Urzua, 2006; Almond and Currie, 2011). Parental care (or time spent with the child) is usually thought of as a significant input to the human capital production function, and maternity leave is one policy that can promote parental time investments (Dahl et al., 2016).

Maternity leave legislation varies widely across countries regarding the duration of the leave, job protection, and income replacement. Over the past 20 years, there has been a global trend

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towards longer paid leave periods (meeting or exceeding the International Labour Organization (ILO) standard of 14 weeks), higher levels of income replacement, and less reliance on employer liability (ILO, 2014). Although it is commonly believed that longer leaves should have a positive effect on children's skill development, the empirical evidence is still scarce and shows mixed results.

This paper estimates the effects of a maternity leave reform on children's cognitive and non-cognitive abilities and maternal outcomes in Chile. We exploit a legislative change that increased paid leave from 12 to 24 weeks for mothers of children born on July 25th, 2011 or later. The implementation of the reform provides exogenous variation in paid maternity leave entitlements that allows us to consistently estimate the effects of reform exposure on different outcomes by Ordinary Least Squares (OLS). This is relevant for identification purposes since the timing of returning to work after childbirth (i.e., the length of maternity leave) is a mother's decision (Klerman and Leibowitz, 1997). As such, it is likely to be influenced by some unobserved factors correlated with child development.

We use data from the Early Childhood Longitudinal Survey (*Encuesta Longitudinal de La Primera Infancia*, ELPI), which is a nationally representative survey of children designed to study early childhood and support public policy evaluation (Behrman, Bravo and Urzúa, 2010). The sample includes data on working mothers' children born between years 2006 and 2011 who were evaluated during the second half of 2012, i.e. they were tested at varying ages (between seven months and six years). The outcomes analyzed include children's cognitive and non-cognitive skills as measured by the Child Learning and Development Test (*Test de Aprendizaje y Desarrollo Infantil*, TADI) that evaluates four dimensions: cognitive, language, motor, and socioemotional (Edwards and Pardo, 2013). This test corresponds to a direct measure of children's abilities unlike school test scores or dropout rates.

Our results show that mothers exposed to the reform significantly extended their paid maternity leave by an average of four weeks (it does not crowd out unpaid leave) and estimates reveal positive gains in children's cognitive skills by 0.2 standard deviation. These results are stronger for children of mothers with low education levels. While it has been reported that most interventions that boost IQ in the short-run fade out, there is evidence that the effects of very early interventions (before age three) may persist into adulthood.[1] As mentioned above, the exposure to the reform occurred during the first six months of life, so this is applicable.

We analyze potential mechanisms through which the reform operates. In addition to time

[1]See Elango et al. (2015) for a review of early childhood programs. Early childhood Interventions often target especially disadvantaged subpopulations. We analyze a broader treatment given to children of working mothers regardless of their socioeconomic status.

spent with the child (as proxied by maternity leave duration), we analyze breastfeeding since it may increase children's abilities through a biological channel (Kramer et al., 2008; Belfield and Kelly, 2012; Victora et al., 2015). We find a significant increase in breastfeeding duration and the probability of breastfeeding for at least six months for those exposed to the reform compared to those not exposed. We also analyze maternal stress (as measured by the Parenting Stress Index, PSI) as an additional mechanism (Abidin, 1995). We find a decrease in the PSI suggesting that maternity leave legislation also improves maternal health. Lastly, we also look at maternal outcomes such as the probability of being employed and wages. While we do not find any effects of the reform on wages, there is a small but positive and significant effect on the probability of being employed one year after birth.

Our identification strategy is similar to that of Baker and Milligan (2015) who exploit a legislative change in the duration of maternity leave in Canada that increased the amount of time mothers stayed at home in the first year. However, our findings differ from theirs. While they do not find any effects on non-cognitive skills and a small but negative effect on cognitive ones, we find positive and statistically significant effects on cognitive skills. Possibly the counterfactual of maternity leave in Chile and Canada might be different (such as the quality of public and private child care as discussed later). There are also differences in the outcome variables analyzed and the children's age of exposure in the Chilean and Canadian reforms. Their measure of non-cognitive skill is solely based on parental report, and thus it is exposed to a non-classical measurement error since parents who spend more time with their children can be more aware of their skill development. Meanwhile, we use formal psychological tests that were constructed and standardized with a Chilean sample and thus are more precise measures of abilities.[2]

Other recent articles analyzing the effects of maternity leave on children outcomes do not find any significant effects except for Carneiro, Løken and Salvanes (2015). They examine the effects of maternity leave expansions in Norway, from 0 to 18 paid weeks (in the late 1970s), on dropout rates and wages at 30 years old finding positive and significant results. Interestingly, Dahl et al. (2016) analyze successive expansions in Norway from 18 up to 35 paid weeks and find null effects on exam scores at 9th grade and on high school graduation. They provide regression discontinuity evidence using birth date as a running variable exploiting the eligibility for increased leave for those born after some specific cutoff date. Thus, the interpretation of their results is the average treatment effect for those right after vs. those right before the cutoff date. This is relevant for the analysis

[2]Another difference in our study is the age at which the expansion of maternity leave affected the children. While the reform analyzed by Baker and Milligan was for children between 6 and 12 months of age, ours occurred during the first six months. As they note, their results could be driven by the development of stranger and separation anxiety, which is usually observed during the second six months of life. In contrast, our results are not exposed to this issue.

since the validity of their approach holds for kids "older" than those affected by the reform analyzed by Carneiro, Løken and Salvanes (2015). Lastly, Danzer and Lavy (2017) investigate the effects of a policy change that increased maternity leave from 12 to 24 months in Austria finding no effects on PISA scores in the pooled sample. They do find a positive effect on children with educated mothers opposed to Carneiro, Løken and Salvanes (2015) who find larger results for children with less educated mothers, similar to our results.[3]

Our paper contributes to the literature in three ways. First, it analyzes a maternity leave expansion for young children (maternity leave expansion from 12 to 24 weeks) as opposed to many articles that study increases in parental leave at older ages. Second, it uses a comprehensive set of outcome variables for children and mothers. In particular, we have access to direct measures of children's abilities rather than indirect measures such as test scores and dropout rates. We also have a measure of maternal stress (PSI) and breastfeeding length, which allows us to explore the channels through which the reform operates. Third, this is the first paper (to the best of our knowledge) studying a maternity leave expansion for a middle-income developing country, where the alternatives to maternal care could be of lower quality compared to developed countries.

The remainder of the paper is organized as follows. Section II describes the reform that extended maternity leave in Chile. Section III describes the data and section IV our identification strategy. Section V presents the main results and section VI discusses the mechanisms through which the reform operates. Finally, section VII concludes.

II. Institutional Background

A. Maternity Leave Legislation

Maternity leave in Chile was first enacted in 1925. Before the 2011 reform, legal maternity leave consisted of 12 mandatory weeks with full income replacement.[4] Job protection continued until one year after completion of the leave. In October 17, 2011 Law No. 20,545 introduced a new 12-week parental leave that operates under the same conditions and constitutes an extension of the former 12-week maternity leave.[5] However, women are allowed to return to work on a part-time

[3]The recent literature exploiting changes in maternity leave entitlements is mainly focused on developed countries (especially European countries). For example, Baker and Milligan (2010); Dustmann and Schönberg (2012); Liu and Skans (2010); and Rasmussen (2010) find no evidence of overall positive effects of maternity leave extensions on cognitive skills in Canada, Germany, Sweden, and Denmark respectively. However, Liu and Skans (2010) find positive effects for children of well-educated mothers.

[4]The benefit limit is about USD $2,640 at an exchange rate of USD/CLP 0.0016 and is slightly above the 95^{th} percentile of the female wage distribution for those 25 to 55 years old in 2011 (calculations based on the National Socio-Economic Characterization Survey, CASEN, 2011).

[5]Just as the previous leave, this new parental leave covers formal workers who have a signed employment contract and contribute to the social security system. It may also include independent workers, but under requirements which are difficult

arrangement after the 12 mandatory weeks, and they are allowed to transfer a fraction of their extended leave to the father.[6] Thus, the extension of the leave varies.[7]

The reform entitled mothers who gave birth on or after July 25, 2011 up to a total of 24 weeks of leave. These mothers are part of what we call the fully-exposed group because they qualify for the entire 12 additional weeks of leave granted by the reform. In contrast, mothers who gave birth between May 2, 2011 and July 25, 2011 were entitled to a fraction of the 12 weeks granted by the reform and they comprise what we call the partially-exposed group. Finally, mothers who gave birth before May 2, 2011 were not exposed at all to the new parental leave, and they comprise the non-exposed group. Formally, let e_i be the number of eligible weeks of leave due to the reform (on top of the regular leave) with a maximum of 12. Exposure is determined by the rule $e_i = \max\left(\min\left(12 - a_i, 12\right), 0\right)$ where a_i is the age of the child in weeks on July 25, 2011. This means that the amount of additional weeks of leave granted to a mother is a deterministic function of the date day she gave birth. Figure 1 illustrates this relationship.

A valid concern in this setting is the exogeneity of the enactment date. We argue in favor of this hypothesis since extending the leave was a longstanding idea. Some legislative proposals were submitted to the Congress over the past decades, but none were approved.[8] The legislative process took almost eight months. It began on February 28, 2011, the law was approved on October 6, 2011, and enacted on October 17, 2011. With all of this, the date of the reform is unlikely to be correlated to child development through any mechanism other than leave duration. Thus, this reform generates suitable exogenous variation to estimate the effects of maternity leave on child development as detailed in the following sections.

There are two features of this reform that make it especially suitable for our analysis. First, Baker and Milligan (2015) argue that stranger anxiety and separation anxiety are both observed between 6 and 12 months of age, and these factors could offset the effects of additional maternal care. Our reform is not exposed to this issue. As far as we know, this is one of the earliest

to meet in the short run. To become eligible, independent workers must have at least one year of participation in the social security system and at least six (continuous or discontinuous) contributions during the year prior to prenatal leave. Before this reform, independent workers did not have strong incentives to enter the social security system. This policy change might induce some women to start contributing, anticipating the benefits of the subsidy. However, the time span we study is too short for this behavior to be observed.

[6] Just as in Norway (Dahl et al., 2016), very few fathers (2.5% in our sample) make use of the parental leave so it actually works as an extended maternity leave.

[7] Mothers are allowed to return to work on a half workday basis after the 12 mandatory weeks of leave. If the part-time leave is chosen, the length of the extension increases to 18 weeks (instead of 12), and the subsidy is reduced by half. This means that, after the reform, female workers can take a maximum of 24 weeks in case of full-time parental leave or 30 weeks in case of part-time parental leave (12 weeks full-time plus 18 part-time). Mothers can also transfer some leave time to the father, up to 6 weeks in case of full time or up to 12 weeks for part-time leave. In any case, the weeks used by the father must be the last weeks of the leave. The paternal subsidy is calculated based on his income up to the same limit of USD $2,640. By this, even though the first 12 weeks of leave remain inalienable, the additional 12 (or 18) weeks are not completely mandatory.

[8] For more information see the "*Trámite de Proyectos*" (processing of projects) section on the Senate's website, `www.senado.cl`.

treatments studied in the maternity leave literature, covering the period between the third and sixth month of life. A second feature is that the paid leave in Chilean legislation considers full income replacement. Therefore our analysis is less likely to be affected by income effects.[9]

B. *Sick Child Leave*

There is also paid sick child leave for working mothers of young children. When a child under one year old requires home care due to serious illness, the working mother has the right to take a paid leave for the period determined by the respective health provider. The child health condition must be accredited with a medical certificate granted or ratified by the services responsible for the child's medical care.

It has been suggested that some mothers might use this leave as a mechanism to extend their paid maternity leave beyond the legal entitlement (SUSESO, 2012; Congress, 2012). To assess the interaction of our results with changes in sick child leave patterns, we construct an alternative measure of maternity leave that includes self-reported weeks of sick child leave and estimate the impact of the reform on this variable. It is important to note that there were no changes in sick child leave policy. As we discuss later, the incidence of sick child leave in the alternative measure of maternity leave is small.

C. *Alternative Care*

The recent literature on maternity leave expansions analyzes settings with different alternatives to maternal care, which may affect the interpretation of the results. While some studies analyze settings where formal care was the main alternative (Liu and Skans, 2010; Rasmussen, 2010), others study maternity leave in a context where informal care was the predominant arrangement (Carneiro, Løken and Salvanes, 2015; Dahl et al., 2016; Danzer and Lavy, 2017). In Chile, the main alternative to maternal care in 2011 was informal care. However, the availability of formal care was sizable including 18% of children aged 0 to 2 (OECD, 2013). This rate is higher than that of other countries where informal care was the main alternative to maternal care at the time of the reform. For example, in Norway the coverage of formal care was 1-2% in 1977 (Carneiro, Løken and Salvanes, 2015), and in Austria Danzer and Lavy (2017) calculate an enrollment rate of 2.5%.

Moreover, the effects of an extension of maternity leave benefits not only depend on the type of alternative care but also on its quality. Chilean formal child care centers have been rated as

[9] As we commented in a previous footnote, the subsidy limit of \$2,640 corresponds to the 95^{th} percentile of the income distribution of Chilean women aged 25 to 55 in 2011. It is also close to the 99^{th} percentile of maternal income distribution in our samples.

low quality, especially with respect to the production of cognitive skills (Alarcón et al., 2015). For instance, Strasser, Lissi and Silva (2009) find that more than half of the time in the classroom is spent in non-instructional activities such as snack and recess, and the distribution of instructional time is not consistent with current findings regarding the types of activities that are successful for child development. From an international comparative perspective, Villalón et al. (2002) conclude that Chilean child care centers exhibit only the minimum level of quality as measured by the Early Childhood Environment Rating Scale.

Therefore, the alternative to maternal care in Chile is either informal or low-quality formal care. This must be considered when interpreting the results. A low-quality counterfactual care may imply higher effects of maternity leave reforms than a high-quality one. We elaborate on this in the discussion section.

III. Data

We use the Early Childhood Longitudinal Survey (ELPI), a nationally representative survey of young children oriented at designing and evaluating different public policy programs in Chile (Behrman, Bravo and Urzúa, 2010). The first wave, conducted in 2010, consists of approximately 15,000 children born between January 1, 2006, and August 31, 2009. The second wave, conducted in 2012, includes these children plus a refreshment sample of approximately 3,000 children born between September 1, 2009, and December 31, 2011. This survey contains a comprehensive data set including family background, child care, and maternal labor market outcomes. It also includes test scores from a formal psychological evaluation of children's development. However, the survey only includes one child per household, so information on siblings is very limited.

Using the 2012 wave of ELPI we are able to identify children who were exposed to the reform. Furthermore, we have a measure of total maternity leave taken by mothers who worked during their pregnancies. There is a specific subsection of the survey in which there are questions about the extension of maternity leave prior and after childbirth.[10] This question is simply: *how many weeks of maternity leave did the mother of the child take?* Note the question makes no reference to paid or unpaid leave, so we interpret it as a total leave measure. However, as a robustness check, we also consider the impact of the reform on alternative measures of maternity leave that include inactivity periods in the labor market after childbirth or the duration of sick child leave. We will return to this point in the results section. Finally, a small fraction of the sample (near 3%) chose

[10] Mothers who were unemployed or out of the labor force during pregnancy were not asked this set of questions because they were not eligible for any maternity leave entitlement (neither the mandatory 12 weeks nor the extension).

part-time parental leave and for those cases we considered the sum of full-time and part-time weeks as our measure of total leave.[11]

Regarding children's outcomes, the second wave of the ELPI contains cognitive and non-cognitive skills as measured by the Child Learning and Development Test (*Test de Aprendizaje y Desarrollo Infantil*, TADI) which is a standardized test developed in Chile that evaluates the development and learning of children between seven months and six years old.[12] Children were evaluated during the second half of 2012.

This test evaluates four dimensions of development, each with its own score: cognitive, language, motor, and socioemotional abilities. In our analysis, we use the corresponding age-adjusted T-scores and also the total T-score (the mean of the four-dimensional scores).[13] T-scores are distributed with mean $\mu = 50$ and standard deviation $\sigma = 10$, but for the sake of interpretation we standardized them by subtracting the mean and dividing by the standard deviation, so they have zero mean and unit variance.

TADI scores have some advantages as skill measures. First, the socioemotional score is a more precise measure of non-cognitive skills than the parent-reported indexes typically used in the literature. It is not exposed to the same amount of systematic measurement error. This allows for a more precise estimation of the non-cognitive effect of maternity leave. Second, this scale was constructed and standardized with a Chilean representative sample, so it includes the cultural background.

To assess the mechanisms through which the reform manifests, we use information on different variables related to child development. In particular, the ELPI survey has a set of questions about breastfeeding, which we use to construct three variables: the proportion of mothers breastfeeding their child for at least three and then six months, and the duration of breastfeeding in months. The survey also contains the maternal Parenting Stress Index Short Form (PSI), which assesses three areas of stress in the parent-child relationship: child characteristics, parent characteristics, and stress stemming from situational or demographic conditions (Abidin, 1995). [14] We use the

[11]We could have alternatively specified our duration measure as the sum of full time weeks plus half the amount of part-time weeks, or we could have just used full time weeks only. Our results are not sensitive to any of these changes in specification, partly because less than 3% of mothers in our sample made use of part-time leave. Similar results using alternative specifications are available upon request.

[12]This test was constructed by the Centre for Advanced Research in Education (CIAE) at the University of Chile in conjunction with the Centre for Studies in Development and Psychosocial Stimulation (CEDEP), under guidance from a multidisciplinary group of national and international experts. Its construction was based on the Early Learning and Development Standards (ELDS), an exhaustive literature review, and a set of development and learning evaluation instruments (Edwards and Pardo, 2013; Kagan and Britto, 2005).

[13]The TADI scale is divided into 13 age groups. T-scores indicate the relative position of children within their corresponding age group. For further information see Edwards and Pardo (2013).

[14]The PSI Short Form is a brief version of the Parenting Stress Index. It contains 36 of the 120 items in the full-length version, combining rating scale and multiple selection questions. Each item is scored on a five-point scale, yielding a global stress score based on three dimensions: parental distress, parent-child dysfunctional interaction, and having a difficult child.

PSI percentile scores as our measure of maternal stress.

Regarding maternal outcomes, the survey contains information on current employment status (at the time of the interview) and wages. We assess the effects of the reform on these outcomes since there is mixed evidence on the impacts of maternity leave expansions on maternal labor market outcomes (Ruhm, 1998; Baker and Milligan, 2008*a*; Lalive and Zweimüller, 2009). The survey also includes self-reported information about the use of the sick child leave for different age ranges (0-3, 3-6, and 6-12 months). This information may shed light on the cost-benefit analysis since a reduction of sick child leave may help to compensate the costs of longer maternity leaves.

Our analysis focuses on children seven months to six years old at the time of evaluation (TADI was created for this age range), but in the robustness checks we perform the analysis on a subsample of younger children as well. The final sample size consists of 3,458 observations. We present some descriptive statistics for the sample of eligible mothers in Table 1. Roughly one third of them are highly educated (more than a high school diploma), 19% are single mothers, 5% are students, and 79% were employed at the time of the survey. As can be seen, 91% were working at the point of childbirth, and 96% of those working had a formal employment contract. These high figures are expected since questions about maternity leave were asked of mothers who were working during pregnancy. The father was present in 72% of the households, and 96% of these fathers were employed. The mean duration of self-reported maternity leave is 17 weeks for exposed women versus 13.5 weeks for non-exposed ones.

IV. Empirical Strategy

We consider the following structural equation

$$y_i = x_i'\gamma + \beta m_i + u_i \tag{1}$$

where y_i denotes a developmental outcome of child i, x_i a column vector of covariates, and m_i represents maternal care (time spent with the mother). As discussed by Bernal and Keane (2010), Baker and Milligan (2015), Carneiro, Løken and Salvanes (2015) and Danzer and Lavy (2017), mothers' choice of m_i might depend on some unobserved factors that affect child development. If mothers of children with lower initial skill endowments take longer leaves as a compensation mechanism, there will be a reverse causal relation. In this case the OLS estimator of β will be inconsistent and downwardly biased.

We use each dimensional score separately and the total score in our analysis.

Therefore, we exploit exposure to the reform as a source of exogenous variation in maternal care (m_i). As described in Section II, exposure to the extended leave is determined by the difference between birth date and the date of the reform. This variable is plausibly exogenous with respect to the human capital accumulation process. Besides, the reform should induce some mothers to choose a longer leave. Hence we can consistently estimate the effects of reform exposure on child development and maternal outcomes in a reduced-form approach. We also analyze potential mechanisms that could explain our results, estimating the impact of the reform on reported maternity leave, breastfeeding duration, and maternal stress.

As explained in Section II, the number of eligible weeks due to the reform (on top of the 12 mandatory weeks) is defined by the deterministic rule $e_i = \max(\min(12 - a_i, 12), 0)$, where a_i is the age of the child in weeks on July 25, 2011. Hence, we have variation in exposure and our treatment variable ranges from 0 to 12. For the sake of interpretation in our regression analysis, we define treatment as $z_i = e_i/12$. Therefore it ranges from 0 to 1, where $z_i = 1$ indicates a fully-exposed mother, $z_i = 0$ a non-exposed mother, and $z_i \in (0, 1)$ a partially-exposed one. Another alternative is to define a binary treatment (exposed and non-exposed). We present results with a binary variable in the online appendix. They are robust to the definition of treatment.

Hence, we estimate the following equation.

$$y_i = x_i'\gamma + \beta z_i + u_i \tag{2}$$

where the column vector of covariates, x_i, includes child and mother's age, child birth order, number of people in the household, and indicators of maternal education level, region, child's sex, living in an urban area, father's presence, mother being a student, mother's marital status, if the child was a multiple birth, date of evaluation, and child's birth month. We are able to control for birth month since we have children born in different years.[15]

The identifying assumption of our reduced-form approach is that the date of the reform is uncorrelated with child development. We argue in favor of this hypothesis since the length of the legislative process and the longstanding intention to extend the leave make this correlation less likely to exist. Indeed, the political process delayed the enactment of the law. During the Congress sessions, the discussion focused on the maximum amount of the subsidy, which ended up in a three-month episode that delayed the enactment of the reform (Congress, 2012).[16] This dilation

[15] In appendix A we present two bin scatter plots to depict the idea of the identification strategy. In Panel A of Figure A1, we plot the evolution of maternity leave durations and in Panel B the evolution of age-adjusted cognitive test scores (our main outcome).

[16] The maximum subsidy amount was set at USD $2,640 as previously established.

was arguably not related to child development.[17]

Another threat to our identification strategy might occur if there were changes in the fertility patterns related to the reform. If certain mothers postponed their pregnancies anticipating the reform, and if they were systematically different from the rest in some unobserved aspect related to child development, the identifying assumption would not hold. However, the argument above applies to this case too. The date of the law's enactment was not readily predictable, so it seems plausible to assume its exogeneity. In Appendix B we present evidence on the evolution of weekly births and mean gestational durations suggesting that there is not any bunching around the date. We also show the distribution of maternal education, household income, and labor histories by month, and also do not find any bunching either.

We also estimate equation (2) separately for variables that can be interpreted as mechanisms through which the reform works in conjunction with time with the child. First, we estimate the effects of the reform on breastfeeding duration and the probability of breastfeeding at least three and then six months to explore a nutritional channel. Then we assess the reform effects on a set of maternal outcomes including stress (PSI), log-wage, and the likelihood of being employed at the time of the survey. Lastly, we assess if the reform had any impact on the use of sick child leave for various age intervals (0-3 months, 3-6 months, and 6-12 months). The specification of vector x_i varies depending on the response variable and is described in the notes of each results table.

V. Results

A. *Child Development*

Table 2 shows the results for the standardized TADI T-scores. We find a positive effect of 0.18 standard deviations on total TADI scores (significant at the 5% level), driven by cognitive and motor dimensions with effects of 0.24 and 0.20 standard deviations respectively (both significant at the 1% level). We do not find any evidence of an impact on the language and socioemotional dimensions.

As mentioned above, the mean of TADI T-scores for every dimension is $\mu = 50$ with $\sigma = 10$, so the estimated effects are equivalent to 4%-5% of the mean, in line with the magnitudes of the estimated impacts of other early childhood interventions (Behrman, Cheng and Todd, 2004; Krueger, 1998). These results are robust to alternative specifications of the exposure variable. In Appendix C we present the results using a binary specification for exposure finding similar results.

[17] This exogeneity argument has been invoked, for example, for the case of California's Paid Family Leave reform. See Huang and Yang (2015) for details.

See Table C2 for details.

Our results are consistent with those of other studies in the maternal employment literature. Using time-diary data and exploiting exogenous variation induced by shifts in the opportunity cost of time, Villena and Ríos (2012) obtain positive LIML estimates of one order of magnitude greater than their OLS estimates of maternal care on child development.[18] Bernal (2008) and Bernal and Keane (2010) estimate structural models of maternal work and child care decisions. They find a statistically significant negative impact of maternal work on child development, which is consistent with our finding of a positive impact of maternity leave. Bernal and Keane (2011) use exogenous variation in welfare policy rules, also finding negative effects of child care use (maternal employment) on child development.[19]

Next, we assess the presence of heterogeneous effects in two dimensions: maternal education and child's sex. In columns (1) and (2) of Panel A in Table 3 we estimate separate models for more (more than high school) and less educated (high school or less) mothers. The results suggest a higher impact of the reform for children with less educated mothers on cognitive, motor, and language skills. Interestingly, while the pooled model suggested no impact on the language dimension, we find a positive effect of 0.2 standard deviations (significant at 10%) for children of less educated mothers. However, there is not enough statistical power to reject the null hypothesis that coefficients are equal across groups.[20]

Panel B explores if there are heterogeneous impacts by child's sex, repeating the previous exercise for this variable. The cognitive coefficient remains statistically significant for both genders but the motor coefficient is only significant for girls. Again, we cannot reject the null hypothesis that the coefficients are equal across groups.

Robustness Analysis. — To better control for time patterns in a difference-in-difference setup, we define an alternative exposure variable that assumes the 2011 pattern of exposure (non-exposed, partially and fully-exposed) as in Figure (1) for children born in every calendar year. Hence, in this falsification exercise exposure is based solely on month of birth, regardless of year. The exact

[18]Note that they study the relationship between maternal care and child development in a different framework, yet find similar results to ours. They use the Child Development Supplement data, a complementary survey of the Panel Study of Income Dynamics. It is a longitudinal study of children who are 0 to 12 years old in the first round. Successive rounds are conducted in five-year intervals. Their sources of exogenous variation in maternal time are shifts in the opportunity cost of time induced by changes in the average cost of child care, housekeeping costs, average offered wages for the mothers, and welfare programs (subsidies).

[19]Other articles studying the developmental effects of maternal employment during early childhood are Blau and Grossberg (1992), Hill and O'Neill (1994), Waldfogel, Han and Brooks-Gunn (2002), Brooks-Gunn, Han and Waldfogel (2002), Ruhm (2004) and James-Burdumy (2005). Perhaps because it often fails to account for endogeneity, this literature has produced mixed evidence (Bernal, 2008).

[20]In Appendix D we present similar results for paternal education. Since there is a strong correlation between maternal and paternal education in our sample (0.61) the results and interpretation are the same as those discussed for maternal education.

definition of this alternative exposure in this setting is $\tilde{e}_i = \max\left(\min\left(12 - \tilde{a}_i, 12\right), 0\right)$ where now $\tilde{a}_i$ represents age in weeks on July 25^{th} in the corresponding birth year.

We estimate the following equation

$$y_i = x_i'\alpha + \beta\tilde{z}_i + \gamma post_i + \delta\left(\tilde{z}_i \times post_i\right) + u_i \tag{3}$$

where $\tilde{z}_i = \frac{\tilde{e}_i}{12} \in [0,1]$ and $post_t$ is a dummy variable indicating that child i was born in 2011, the year of the reform. Thus, we should not expect any significant effect for children born before the reform ($\beta = 0$). On the other hand, the interaction of this alternative exposure variable with the "post" dummy should capture the true effect ($\delta > 0$) since it corresponds to the effect of exposure for children affected by the reform.

Table 4 shows the corresponding results. As can be seen, the interaction coefficient for cognitive skills is close to that of Table 2 and significant at 5% (column 1). Also, the post coefficient is not significant. Thus, after controlling for the same covariate vector as those in Table 2 which includes child age and month of birth among others plus the exposure pattern, the result on cognitive skills persists. On the other hand, the interaction coefficient for motor skills is non-significant, and the post coefficient in this case is positive and significant (column 4).

Even though in all specifications we control for child's age (trend) and month of birth (seasonality), we perform a robustness check considering younger children so that we are comparing children who are closer in terms of age, at the cost of losing observations and statistical power. Table 5 presents the results for reduced form of all outcomes with a sample of children three-and-a-half years old (42 months) or less. Despite a considerable reduction of the sample size (about 60%), the point estimate for cognitive outcome is very close to that of Table 2 and is significant at the 5% level. The effect on motor skills is negligible and negative as in Table 4. Hence, given that the results on motor skills are not robust, we focus on cognitive skills in the following robustness check.

Table 6 shows the results of estimating equation (2) for cognitive outcomes using different specifications of the covariate vector. In column (1) we only control for child's age. Next, we add controls for other child characteristics (namely sex and birth weight) in column (2). In column (3) we also add maternal characteristics (mother's age and indicators for the mother being a student, and her schooling level and marital status). Finally, in column (4), we add other family background control variables (number of people in the household and indicators of father's presence, region, and urban area), mimicking the specification in column (1) of Table 2. As observed, the point estimate shows little change across columns.

B. Maternal Labor Market Outcomes

We estimate linear models to assess the impact of the reform on maternal labor market outcomes. Column (1) of Table 7 shows the estimates for the wage equation at current job at the time of the survey (conducted in the second half of 2012). We find no evidence of a wage effect of maternity leave in our sample. The dependent variable in column (2) is an indicator for the mother being employed at the time of the survey. The point estimate is positive and significant at 10%, indicating an increase in the probability of being employed by 5.8 percentage points.

Our maternal employment result is consistent with the findings of Ruhm (1998) regarding women's employment, although he finds a negative impact in their relative wages. In contrast, Lalive and Zweimüller (2009) find that the Austrian reform induced a short run decrease in women's employment and earnings, but produced no long run effects. An increase in maternal employment could be explained by job continuity. The possibility of returning to her pre-childbirth job (assured by job protection) allows the mother to continue accumulating specific human capital, thus reducing her incentives to quit the labor market. Baker and Milligan (2008*a*) find evidence that maternity leave entitlements increase job continuity with the prebirth employer in Canada.[21]

C. Use of Sick Child Leave

As described in Section II, there is a paid sick child leave for working mothers with children younger than one year of age in the case of serious illness. We estimate the effects of the reform on the probability of using this leave. The results may shed some light on the cost-effectiveness of the program. A decrease in sick child leave use (induced by the reform) should be taken into account when quantifying the net cost of the program.

Table 8 shows the estimated coefficients of a linear probability model for the use of the sick child leave. The dependent variable is an indicator of using this leave while the child was 0 to 3 months old in column (1), 3 to 6 months old in column (2), and 6 to 12 months old in column (3). As the reform extended the maternity leave entitlement from 12 to 24 weeks (3 to 6 months), it is plausible to expect a negative effect of the reform in this age range. As can be seen in column (2), the estimated coefficient for exposure to the reform when the child was 3 to 6 months old is negative

[21] In Chile, Perticara and Sanhueza (2010) find that women face a 40% higher chance of leaving employment when the child is three months old, but this disappears after the child is one year old. Our result differs from theirs. We find that the longer the maternity leave is, the higher the probability of being employed at the time of the survey, which is at least 7 months after birth.

and significant at 1%. It implies a 12.5 percentage point decline in the probability of using the sick child leave that translates into a reduction of 50% in our sample. The estimates in columns (1) and (3) are not statistically significant. This finding is consistent with administrative records that show a 50% decline in the number of sick child leave licenses one year after the implementation of the reform (SUSESO, 2012).

This result helps to quantify the net costs of the reform. Part of the cost of the new maternity subsidy is compensated by a decline in payments for the sick child leave subsidy. The reduction in the probability of using the sick child leave might be partially explained by substitution between types of leave. In the reform's absence, some mothers may have used the sick child leave in order to postpone their return to work. After the reform, this is no longer necessary as mothers are entitled to a longer maternity leave. We will return to this point in Section VI.D.

Next section analyzes mechanisms affecting child development such as time spent with the child, breastfeeding, and maternal stress.

VI. Potential Mechanisms and Discussion

So far, we have shown that exposure to the reform is positively correlated with cognitive TADI T-scores. In this section, we analyze potential mechanisms through which the reform operates. As in Carneiro, Løken and Salvanes (2015), these results are not separately decisive. However, together they suggest a coherent story.

A. Time Spent with the Child

As discussed in Section IV, we expect the reform to increase the time that mothers spend at home with their children. To assess the effects of the reform on maternal care (proxied by maternity leave) we regress maternity leave duration on exposure and a vector of control covariates. As mentioned before, exposure is measured as the ratio of eligible weeks over the maximum number of weeks so it varies continuously from 0 (non-exposed) to 1 (fully-exposed). Values of this variable between 0 and 1 reflect partial exposure to the reform. Table 9 presents results for three measures of total leave. Column (1) shows the result for the self-reported leave. Column (2) shows results for the self-reported leave plus the number of weeks out of labor force after the leave (if any) and column (3) shows the results when using self-reported leave plus the number of weeks of work missed due to sick child leave.[22]

[22]To construct the measure in column (2) we add the number of weeks of reported leave and the number of weeks out of the labor force, right after the end of maternity leave. We capped them at 52 weeks, which is the largest leave in the sample. For

As can be seen, the coefficient of the treatment variable has the expected sign; the reform induced some mothers to extend their leave. The estimated impact of the reform on leave duration is an increase of roughly one month when using self-reported leave (4.13 weeks). When using alternative measures, the results do not change very much, showing increases of 4.08 and 3.72 weeks in columns (2) and (3) respectively. The three estimates are statistically significant at 1%. The magnitude of these effects is comparable to those in Dahl et al. (2016) where the leave was increase by 3 weeks. Table C1 in Appendix C shows similar results for a binary specification of treatment ($z_i = 1$ for exposed mothers and $z_i = 0$ for non-exposed mothers).

As mentioned in Section II, an impact of 12 additional weeks was not expected because of the non-mandatory nature of the reform. Mothers are allowed to come back to work in a part-time role after the 12 mandatory weeks, and they can transfer up to six full-time weeks to the father. Additionally, some substitution of paid for unpaid leave may occur. Since the question in the survey about length does not specify the nature of the leave, there might be a sizable share of always takers who would have extended their legal maternity leave without the reform (for example, by direct negotiation with their employers).

B. Breastfeeding

There is a widely held view of breastfeeding as a promoter of cognitive development. For example, in a population-based birth cohort study Victora et al. (2015) find a positive association between the duration of breastfeeding and IQ, educational attainment and income at age 30. On the same ground, Belfield and Kelly (2012) find a positive association between the incidence of breastfeeding at birth and health status at 9 months old, nutritional status (less likely to be obese), and cognitive outcomes at 24 months. Experimental evidence supports these findings. Kramer et al. (2008) conduct a large-scale RCT in Belarus, the Promotion of Breastfeeding Intervention Trial (PROBIT). Their intention-to-treat estimates suggest that the promotion of exclusive breastfeeding leads to a substantial increase in exclusive breastfeeding and IQ measures.

The literature points to early childhood health and nutritional conditions as important determinants of adult outcomes (Almond and Currie, 2011). For example, Bütikofer, Løken and Salvanes (2016) find that the long run effect of access to mother and child health care centers — which provided nutritional advice and promoted breastfeeding — on educational attainment and income in Norway operates through better nutrition during the first year of life. Moreover, besides

sick child leave we have the number of absences but not their duration. We obtain the average duration of sick child leave by year from administrative records and use it to construct the alternative measure in column (3).

the nutritional channel, breastfeeding can also improve the quality of mother-child interactions. For example, Papp (2013) finds a positive association between breastfeeding and maternal sensitivity.

Furthermore, it seems plausible to expect an increase in average breastfeeding duration due to increased average maternity leave duration. There is some empirical evidence supporting this hypothesis. Baker and Milligan (2008*b*) find an increase in breastfeeding durations induced by the Canadian reform although they do not find any impact of breastfeeding on health outcomes.

Thus, in this section we explore the effects of the Chilean reform on breastfeeding. The ELPI survey contains questions on breastfeeding duration and allows us to identify mothers who have finished breastfeeding and those who are still breastfeeding to address right-censoring.

We start our analysis with a comparison of the survivor functions of breastfeeding for children whose mothers were not exposed to the reform ($z_i = 0$) and those who were partially or fully exposed ($z_i \in (0, 1]$).[23] In Figure 2 we plot the Kaplan-Meier estimate of the survivor function for both groups. This nonparametric estimator, also known as the product-limit estimator, is consistent under independent censoring (Kaplan and Meier, 1958). We plot the estimates for durations $t \leq 20$ because exposed children are younger than non-exposed ones and there is no data for this group after 20 months. The survivor functions seem to be equal until 3 months of breastfeeding. From this point on the probability of survival is greater at any duration for the exposed group. The difference is more pronounced from $t = 6$ onwards. This evidence suggests that the reform increased months of breastfeeding.

Next, we introduce covariates into the analysis, which requires adding some structure. We estimate a Cox proportional hazards model. This semiparametric estimator consistently estimates a proportional hazards function with independent censoring (Cox, 1972). The hazard rate for individual i is modeled as

$$h(t|z_i, x_i) = h_0(t) \exp\left(\phi z_i + x_i'\omega\right) \quad (4)$$

where z_i is our treatment variable, x_i is a vector of covariates and no assumption is made about the functional form of the baseline hazard $h_0(t)$.[24] Following Jayachandran and Kuziemko (2011), we exclude multiple births from our analysis. Column (1) in Table 10 shows the results for this model.

[23]The survivor function, defined as $S(t) \equiv Pr\,(T > t)$, is the probability of survival past time t. By survival we mean remaining in the "breastfeeding" state.

[24]The hazard rate, defined as $h(t) \equiv \lim_{\Delta t \to 0} \frac{Pr(t+\Delta t\, >\, T\, >\, t\ |\ T>t)}{\Delta t}$, is the instantaneous probability of leaving the "breastfeeding" state conditional on survival to time t. The specification of a proportional hazards model implies that, for two individuals $i \neq j$, the ratio of the hazard rates is a constant: $\frac{h(t|z_i,\mathbf{x}_i)}{h(t|z_j,\mathbf{x}_j)} = \frac{\exp\left(\phi z_i + \mathbf{x}_i'\omega\right)}{\exp\left(\phi z_j + \mathbf{x}_j'\omega\right)}$. In other words, one individual's hazard rate is a multiple of another's.

The estimated hazard ratio ($\exp(\hat{\phi})$) is 0.77, meaning that the hazard rate for exposed children is 0.77 times that of non-exposed children, i.e. having a lower probability of ending breastfeeding.[25]

Complementing the previous finding, we estimate linear probability models (LPM) for the probability of breastfeeding for at least 3 and then 6 months. Results are shown in columns (2) and (3) of Table 10 respectively. While we find no evidence for an increase in the probability of breastfeeding for at least 3 months, we estimate a 7 percentage point increase in the probability of breastfeeding for at least 6 months (statistically significant at 10%). We show similar results using a binary treatment in Table C4 in Appendix C.

This evidence points in the same direction as our previous analysis with the Kaplan-Meier estimator, implying that the Chilean reform caused an increase in breastfeeding durations. In addition to the biological channel, an increase in breastfeeding duration implies an increase in time spent with the child. This result is similar to those of Berger, Hill and Waldfogel (2005) for the US and Baker and Milligan (2008*b*) for Canada. This mechanism could partially explain the positive cognitive effects of maternity leave found in the previous section.[26]

C. Maternal Mental Health

We estimate the impact of the reform on maternal mental health as measured by the Parenting Stress Index (PSI). Maternal mental health has been reported to affect child development and time investments in children (Patel et al., 2004; Frech and Kimbro, 2011). Table 11 shows the results for the total percentile score and for each domain separately. We find that a longer leave reduces maternal stress according to this measure and the effect is driven by a reduction of stress related to the "difficult child" aspect of the PSI. High scores in this domain suggest stress related to regulating the child's behavior. This result is consistent with other findings indicating that a longer maternity leave might have a long-lasting positive effect on maternal mental health (Avendano et al., 2015; Aitken et al., 2015; Dagher, McGovern and Dowd, 2014; Chatterji and Markowitz, 2005, 2012).

D. Discussion

Our main results are consistent with those of Carneiro, Løken and Salvanes (2015) who study a Norwegian reform that extended the 12 weeks of unpaid leave to four months of paid and 12

[25]The exponentiated coefficient is the hazard ratio for a one unit change in the regressor. Particularly, in the case of our treatment, we have $\frac{h(t|z_i=1,\mathbf{x}_i=\mathbf{x})}{h(t|z_i=0,\mathbf{x}_i=\mathbf{x})} = \frac{h_0(t)\exp(\phi+\mathbf{x}'\omega)}{h_0(t)\exp(\mathbf{x}'\omega)} = \exp(\phi)$. We reject the null hypothesis $H_0 : \phi = 0$ at the 5% significance level. This test is asymptotically equivalent to $H_0 : \exp(\phi) = 1$, but is preferred for its better small-sample properties.

[26]Table 10 also suggests that children with low birth weights are breastfed for shorter periods, which is consistent with other results in the literature (Millman and Cooksey, 1987; Datar, Kilburn and Loughran, 2010).

months of unpaid leave. Their analysis suggests a decline in high school dropout rates and an increase in wages at age 30, both stronger for children with low maternal education levels.[27] The Chilean reform is similar in two important aspects. Children were exposed to an extension of the paid leave during their first six months of life, and the main alternative to staying home with their mother was informal care. Indeed, according to the ELPI survey 92% of mothers who did not stay at home between the child's first and third months used an informal child care alternative. Among those who did not stay at home between the fourth and sixth months, 80% turned to informal child care arrangements. Similarly, Carneiro, Løken and Salvanes (2015) show that at the time of the Norwegian reform, the coverage of formal child care was very low, about 1-2% for children between 0-2 years old in 1977.

Substitution of informal care by prolonged maternal care might have a greater impact than the substitution of formal care. This would be true if the quality of informal care was lower than that of formal care.[28] Less educated mothers are expected to have lower incomes and thus are more likely to turn to informal care. Moreover, even if less educated mothers chose formal arrangements with the same probability as the more educated, they could be expected to rely more on public child care centers, which could be of lower quality.[29] All in all, it seems reasonable to think of child care quality as a normal good.[30] This argument could explain the heterogeneity of our results regarding maternal education.

The evidence shown in Table 12 is consistent with the last argument. We show the proportion of non-exposed mothers who chose a public child care center, conditional on having used a formal child care arrangement when the child was 3-6 months old. As can be seen, mothers with lower education were 18 percentage points more likely to use a public center. If public centers are of lower quality, then their children could benefit more from prolonged maternal care.

Our main result regarding cognitive abilities is in opposition to that of Baker and Milligan (2015). As mentioned above, they find a negative cognitive effect of the Canadian reform. There is at least one difference that could explain this contrast: the age at which children were exposed to

[27] Similarly, although in a different setting, Bütikofer, Løken and Salvanes (2016) find that the long run impacts of access to mother and child health care centers on educational attainment and income in Norway were stronger for children with low socioeconomic backgrounds, particularly those with less educated fathers.

[28] It is a plausible assumption that in formal child care centers, care is provided by trained professionals and children are exposed to greater peer interaction.

[29] For example, Noboa and Urzúa (2012) argue that the negative impact of public child care center attendance on adult interactions "*may be related to the low quality of individual care provided by a limited number of teachers and caregivers at public child care centers.*"

[30] For example, in their study of the demand for child care quality, Blau and Hagy (1998) find that "*center care is the only 'normal' mode of child care, although the estimated income elasticities are small*". However, they measure household income as spouse's earnings. They also study the effects of maternal wages on the demand for quality and find a positive wage elasticity of mode choice for center based care. They conclude that the "*use of more formal modes of care is more sensitive to the wage rate than other non-parental care.*"

the extension of maternal care. The Chilean reform affected children at very young ages, thus they were not exposed to stranger anxiety and separation anxiety, which could offset positive effects of prolonged maternal care.[31] On the other hand, our findings regarding non-cognitive skills are in line with Baker and Milligan (2015) who find nonsignificant results.

Cost-effectiveness. — Quantifying the benefits of the policy by performing a cost-benefit analysis can be a very complicated task. Instead, we compare the cost-effectiveness of the maternity leave expansion with that of the full-school day program (JEC) implemented in Chile during the 2000s. To do so, we focus on the effects of maternity leave reform on cognitive scores (TADI) and the effects of the full-school day program on reading test scores. This comparison assumes that there is a one-to-one mapping from cognitive scores to reading scores, and a similar behavior in their fade-out patterns.

Using administrative records (SUSESO, 2012), we find that the unit cost of the maternity leave reform is US$1,278 in 2010 dollars. This cost is net of savings produced by the decline in sick child leave use. This gives a cost of US$527 per 0.1 standard deviations (SD) increase in cognitive score.[32] Now, as discussed by Contreras and Rau (2012), the full-day school program costs about $636 per 0.1 SD increase in standardized test scores (in 2010 dollars). Therefore, the cost-effectiveness of the maternity leave reform may be comparable to (if not better than) that of full-day school program on test scores.

VII. Conclusions

This study estimates the causal effect of a maternity leave reform on children's abilities and maternal outcomes. Our identification strategy exploits the exogenous variation induced by a reform that extended the legal maternity leave differentially affecting children born before and after its implementation. Using a rich data set that includes cognitive and non-cognitive ability tests we assess the effects of maternity leave on total, cognitive, language, motor, and socioemotional ability as measured by TADI T-scores.

Our main findings indicate that the short term effects of maternity leave are positive and significant for cognitive skills, especially for children whose mothers are less educated. We find no effects of exposure to the reform on non-cognitive skills.

In addition to time spent with the child, we identify breastfeeding and maternal mental health

[31] This is explained in more depth by Baker and Milligan (2015).

[32] It is obtained by dividing the unit cost of US$1,278 by the effect on cognitive skills of 0.2425 SD, and then multiplying the result by 0.1.

as channels through which the reform impacts children. We estimate duration models for breastfeeding length and linear probability models for the probability of breastfeeding for at least six months, finding positive and significant effects for those who were exposed to the reform. When looking at maternal outcomes, we find a reduction of mother's stress as measured by the Parenting Stress Index and a slightly positive effect on the probability of being employed at the time of the survey but no effect on wages.

Our findings complement the mixed evidence reported in the literature in three ways. First, it is one of the few investigations using maternity leave reforms that affected children at young ages (between 3 and 6 months old). As we discussed, the effects of interventions at such ages may persist in the long-run. Second, it uses a comprehensive set of outcome variables for children, including direct measures of abilities, and maternal outcomes that complements outcomes commonly analyzed in the literature. Lastly, this is the first article studying a maternity leave expansion for a middle-income developing country, which may imply different counterfactuals regarding child care quality compared to developed countries.

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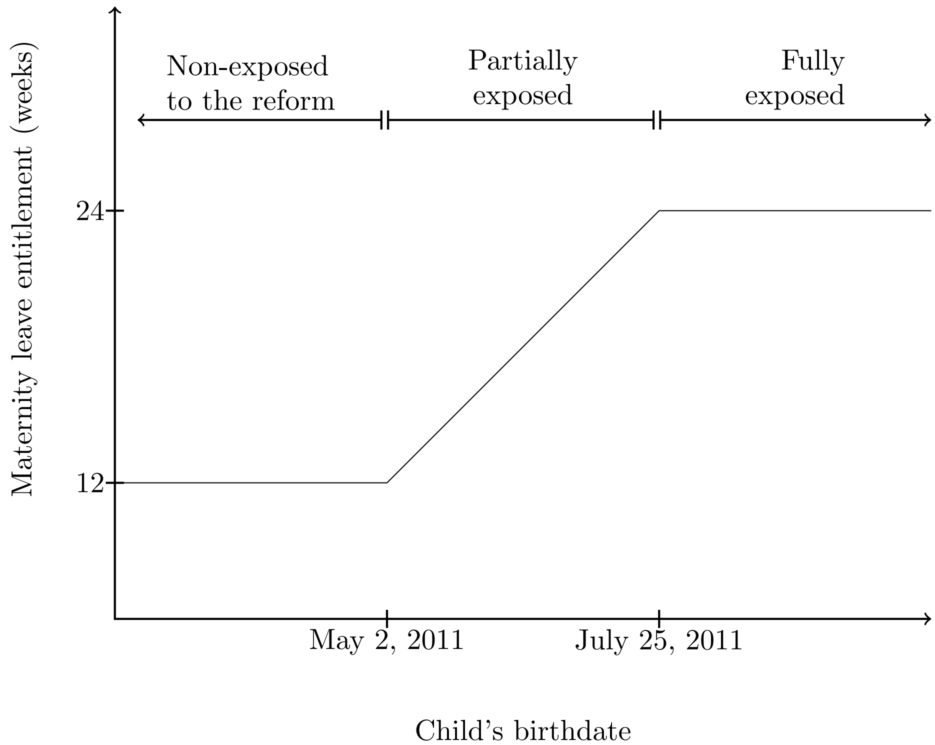
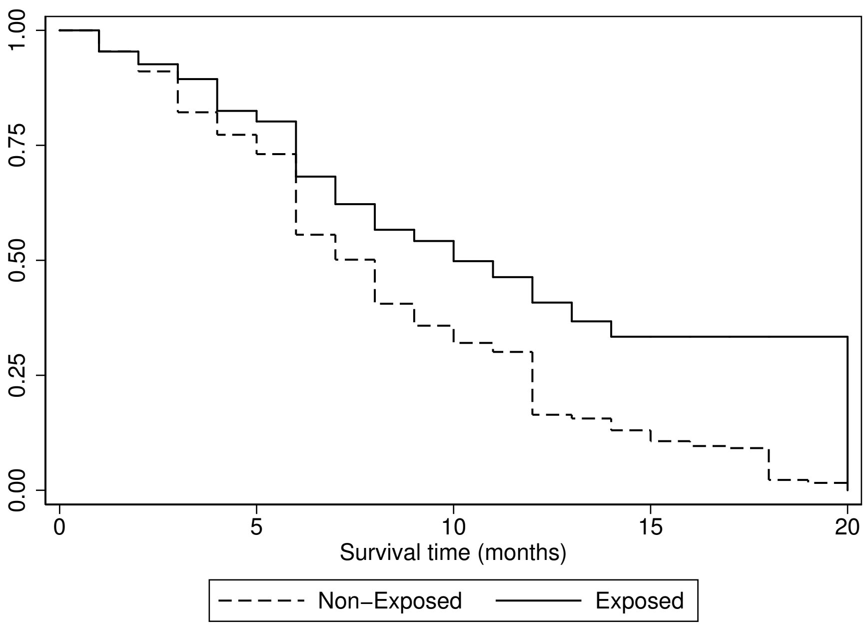
Figure 1. : Maternity leave entitlement



Notes: Maternity leave entitlement in weeks for three periods: before May 2, 2011 (non-exposed), between May 2 and July 25, 2011 (partially exposed), and after July 22, 2011 (fully exposed).

Figure 2. : Survival functions for breastfeeding duration by exposure status



Notes: Kaplan-Meier estimates of the survivor function for breastfeeding duration for mothers exposed ($Z > 0$) and not exposed ($Z = 0$) to the reform.

Table 1—: Descriptive Statistics

	Obs.	Mean	St. Dev.	Min	Max
Maternity Leave Duration					
Exposed	237	17	8.07	0	48
Non-exposed	3,221	13.5	8.06	0	54
All	3,458	13.7	8.1	0	54
Response Variables					
TADI, total	3,387	52.6	8.67	23	81
TADI, cognitive	3,402	52.8	11.1	23	81
TADI, motor	3,401	53.2	11.9	23	81
TADI, language	3,405	52.1	11	23	81
TADI, socioemotional	3,406	52.4	11.6	23	81
Used SCL, 0-3 months	980	.201	.401	0	1
Used SCL, 3-6 months	1,737	.253	.435	0	1
Used SCL, 6-12 months	2,448	.259	.438	0	1
Parenting Stress Index (PSI)	2,894	42	33.5	1	99
Mother employed at survey time	3,450	.79	.407	0	1
Mother's wage (CLP, thousands)	3,049	330	286	0	4,000
Child Characteristics					
Age (months)	3,458	45.7	16.6	7	72
Gender (male)	3,458	.509	.5	0	1
Birthweight (kg.)	3,233	3.36	.479	2	4.98
Family Background					
Mother's age (years)	3,450	33	6.15	16	71
Mother employed at childbirth	3,453	.911	.284	0	1
Mother unemployed at childbirth	3,453	.0107	.103	0	1
Mother not in labor force at childbirth	3,453	.0779	.268	0	1
Mother had an employment contract at childbirth	3,037	.965	.184	0	1
Mother is married	3,450	.424	.494	0	1
Mother is cohabiting	3,450	.318	.466	0	1
Mother is widowed	3,450	.00464	.068	0	1
Mother is single	3,450	.191	.393	0	1
Mother is separated	3,450	.062	.241	0	1
Mother is student	3,450	.0528	.224	0	1
Mother with high school or less	3,442	.669	.471	0	1
Mother with more than high school	3,442	.331	.471	0	1
Father is present	3,458	.723	.447	0	1
- Father employed	2,501	.961	.194	0	1
- Father unemployed	2,501	.0272	.163	0	1
- Father not in the labor force	2,501	.012	.109	0	1
Home is located in urban area	3,458	.938	.241	0	1
Number of people in the household	3,458	4.55	1.52	2	23
Household income (CLP, thousands)	3,440	826	721	10	8,140

Notes: SCL refers to sick child leave

Table 2—: Child Development

	TADI				
	(1)	(2)	(3)	(4)	(5)
	Total	Cognitive	Language	Motor	Socioemotional
Exposure (z)	0.181**	0.243***	0.116	0.199***	-0.010
	(0.075)	(0.078)	(0.086)	(0.075)	(0.082)
Child's age (months)	0.006***	0.006***	0.002	0.008***	0.002*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Mother's age (years)	0.001	0.004	0.000	-0.005	0.002
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Child's sex (male)	-0.181***	-0.075**	-0.151***	-0.130***	-0.201***
	(0.032)	(0.031)	(0.032)	(0.032)	(0.034)
Birthweight (kg.)	0.040	0.045	-0.006	0.069**	0.020
	(0.034)	(0.033)	(0.034)	(0.034)	(0.037)
Urban area	0.085	0.165**	0.094	-0.054	0.082
	(0.070)	(0.065)	(0.070)	(0.072)	(0.082)
Number of people in the household	-0.025**	-0.010	-0.020*	-0.023*	-0.020
	(0.012)	(0.011)	(0.011)	(0.012)	(0.013)
Mother is student	0.085	0.043	0.021	0.022	0.179**
	(0.077)	(0.078)	(0.076)	(0.077)	(0.087)
Household income (thousands of pesos)	0.000***	0.000***	0.000***	0.000**	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Multiple birth	-0.149	-0.069	-0.126	-0.186*	-0.063
	(0.105)	(0.102)	(0.103)	(0.109)	(0.104)
Order of birth	-0.079***	-0.092***	-0.073***	-0.009	-0.069***
	(0.023)	(0.022)	(0.023)	(0.024)	(0.024)
Constant	-0.305	-0.401	-0.055	-0.123	-0.411
	(0.431)	(0.461)	(0.435)	(0.380)	(0.426)
Observations	3,136	3,151	3,154	3,150	3,155

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Control variables also include indicators of geographical region, maternal education level and marital status, father's presence, birth month, and test date.

Table 3—: Child Development Heterogeneity

Panel A: Maternal Education

	(1)	(2)
	High school or less	More than high school
TADI total	0.265***	0.084
	(0.096)	(0.120)
	[2,099]	[1,037]
TADI cognitive	0.333***	0.119
	(0.096)	(0.132)
	[2,109]	[1,042]
TADI language	0.203*	0.016
	(0.105)	(0.146)
	[2,112]	[1,042]
TADI motor	0.270***	0.111
	(0.097)	(0.123)
	[2,110]	[1,040]
TADI socioemotional	-0.006	0.003
	(0.102)	(0.133)
	[2,113]	[1,042]

Panel B: Child's Gender

	(1)	(2)
	Female child	Male child
TADI total	0.167	0.168
	(0.102)	(0.113)
	[1,545]	[1,591]
TADI cognitive	0.221**	0.221*
	(0.104)	(0.117)
	[1,552]	[1,599]
TADI language	0.160	0.033
	(0.115)	(0.129)
	[1,554]	[1,600]
TADI motor	0.241**	0.170
	(0.105)	(0.106)
	[1,550]	[1,600]
TADI socioemotional	-0.115	0.083
	(0.112)	(0.121)
	[1,553]	[1,602]

Notes: SUEST robust standard errors in parentheses, sample sizes in square brackets. *** p<0.01, ** p<0.05, * p<0.1. Control variables include child's and mother's age, birthweight, number of people in the household, and indicators of region, urban area, father's presence, mother's marital status, child's birth month , and test date. In Panel A we also control for child's gender. In Panel B we also control for mother's education level.

Table 4—: Child Development, Alternative Exposure

	TADI				
	(1) Total	(2) Cognitive	(3) Language	(4) Motor	(5) Socioemotional
Alternative exposure × post	0.066	0.228**	0.072	-0.051	-0.046
	(0.098)	(0.102)	(0.108)	(0.099)	(0.110)
Alternative exposure	-0.218	-0.213	-0.092	-0.238	-0.171
	(0.318)	(0.299)	(0.319)	(0.324)	(0.347)
Post	0.140*	0.019	0.054	0.302***	0.045
	(0.081)	(0.083)	(0.081)	(0.081)	(0.091)
Child's age (months)	0.007***	0.006***	0.002	0.010***	0.003*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Mother's age (years)	0.001	0.004	0.000	-0.005	0.002
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Child's sex (male)	-0.179***	-0.074**	-0.150***	-0.127***	-0.200***
	(0.032)	(0.031)	(0.032)	(0.032)	(0.034)
Birthweight (kg.)	0.040	0.046	-0.006	0.068**	0.020
	(0.034)	(0.033)	(0.034)	(0.034)	(0.037)
Urban area	0.082	0.163**	0.093	-0.059	0.080
	(0.070)	(0.065)	(0.070)	(0.072)	(0.082)
Number of people in the household	-0.025**	-0.011	-0.020*	-0.025**	-0.020
	(0.012)	(0.011)	(0.011)	(0.012)	(0.013)
Mother is student	0.090	0.043	0.023	0.032	0.180**
	(0.077)	(0.078)	(0.076)	(0.077)	(0.087)
Household income (thousands of pesos)	0.000***	0.000***	0.000***	0.000**	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Multiple birth	-0.142	-0.066	-0.123	-0.172	-0.059
	(0.106)	(0.103)	(0.103)	(0.109)	(0.104)
Order of birth	-0.078***	-0.092***	-0.073***	-0.008	-0.068***
	(0.023)	(0.022)	(0.023)	(0.024)	(0.024)
Constant	-0.343	-0.397	-0.068	-0.215	-0.418
	(0.435)	(0.464)	(0.438)	(0.384)	(0.430)
Observations	3,136	3,151	3,154	3,150	3,155

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. In this falsification exercise, exposure is based solely on birth month regardless of year. Thus, it assumes the 2011 pattern of exposure for every year. We interact this fake-exposure variable with a dummy indicating the post-reform period. We expect the placebo reform to have an effect only for children born in the year of the actual reform as measured by the coefficient of this interaction. Control variables also include indicators of geographical region, maternal education level and marital status, father's presence, birth month, and test date.

Table 5—: Child Development for Young Children

	TADI				
	(1) Total	(2) Cognitive	(3) Language	(4) Motor	(5) Socioemotional
Exposure (z)	0.032	0.253**	0.011	-0.006	-0.167
	(0.099)	(0.105)	(0.109)	(0.095)	(0.105)
Child's age (months)	0.001	0.007*	0.001	0.000	-0.004
	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)
Mother's age (years)	-0.002	-0.004	-0.006	-0.004	0.007
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Child's sex (male)	-0.159***	-0.094**	-0.100**	-0.081*	-0.222***
	(0.046)	(0.046)	(0.047)	(0.043)	(0.048)
Birthweight (kg.)	0.115**	0.105**	0.048	0.133***	0.076
	(0.049)	(0.049)	(0.048)	(0.045)	(0.052)
Urban area	0.073	0.170*	0.014	-0.002	0.021
	(0.091)	(0.094)	(0.097)	(0.091)	(0.107)
Number of people in the household	-0.035**	-0.018	-0.032**	-0.047***	-0.006
	(0.015)	(0.015)	(0.015)	(0.014)	(0.017)
Mother is student	0.148	0.169*	0.027	0.019	0.237**
	(0.096)	(0.094)	(0.104)	(0.090)	(0.103)
Household income (thousands of pesos)	0.000**	0.000**	0.000*	0.000**	0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Multiple birth	-0.289**	-0.133	-0.259**	-0.256*	-0.211*
	(0.139)	(0.126)	(0.121)	(0.141)	(0.126)
Order of birth	-0.040	-0.077**	-0.030	0.042	-0.067*
	(0.034)	(0.035)	(0.034)	(0.032)	(0.036)
Constant	0.626	0.264	0.656	0.861*	0.237
	(0.478)	(0.455)	(0.483)	(0.450)	(0.478)
Observations	1,292	1,299	1,298	1,298	1,300

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Control variables also include indicators of geographical region, maternal education level and marital status, father's presence, birth month, and test date.

Table 6—: Different Specifications of Covariate Vector

	TADI			
	(1) Cognitive	(2) Cognitive	(3) Cognitive	(4) Cognitive
Exposure (z)	0.219***	0.244***	0.235***	0.243***
	(0.072)	(0.078)	(0.078)	(0.078)
Child's characteristics	No	Yes	Yes	Yes
Mother's characteristics	No	No	Yes	Yes
Other family background variables	No	No	No	Yes
Observations	3,402	3,181	3,166	3,151

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. We control only for child's age in column (1). In column (2) we also include child's gender, birth weight, order of birth, child's birth month and an indicator of multiple births. In column (3) we also control for mother's age, maternal education and marital status, and an indicator of the mother being a student. Finally, control variables in column (4) also include number of people in the household, household income, and indicators of household geographical region, living in an urban area, father's presence, and test date.

Table 7—: Labor Market Outcomes

	Labor Market Outcomes	
	(1) Log wage	(2) Employment
Exposure (z)	-0.064	0.058*
	(0.056)	(0.035)
Child's age (months)	-0.002***	-0.000
	(0.001)	(0.001)
Mother's age (years)	0.014***	0.005***
	(0.002)	(0.001)
Child's sex (male)	-0.014	-0.020
	(0.023)	(0.014)
Urban area	0.016	0.057*
	(0.050)	(0.032)
Number of people in the household	-0.027***	0.006
	(0.007)	(0.006)
Mother is student	0.031	-0.037
	(0.062)	(0.034)
Number of children younger than 2		-0.051***
		(0.015)
Constant	4.887***	-0.025
	(0.343)	(0.185)
Observations	2,225	3,442

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Control variables also include geographical region dummies, and maternal education and marital status dummies.

Table 8—: Use of the Sick Child Leave

	Mother used SCL when the child was		
	(1)	(2)	(3)
	0-3 months	3-6 months	6-12 months
Exposure (z)	-0.004	-0.125***	0.003
	(0.042)	(0.040)	(0.043)
Child's age (months)	0.003***	0.001	0.001
	(0.001)	(0.001)	(0.001)
Mother's age (years)	-0.001	-0.002	-0.004**
	(0.002)	(0.002)	(0.002)
Child's sex (male)	0.005	0.009	0.020
	(0.026)	(0.021)	(0.018)
Urban area	-0.005	0.045	-0.002
	(0.055)	(0.046)	(0.042)
Number of children of the mother	0.055*	-0.005	0.016
	(0.031)	(0.021)	(0.017)
Household income (thousands of pesos)	-0.000	-0.000	-0.000**
	(0.000)	(0.000)	(0.000)
Multiple birth	0.029	0.100	0.037
	(0.074)	(0.070)	(0.061)
Order of birth	-0.066**	-0.023	-0.001
	(0.032)	(0.022)	(0.019)
Constant	-0.207	-0.117	-0.043
	(0.166)	(0.121)	(0.109)
Observations	971	1,724	2,427

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Control variables also include geographical region, health insurance type, and education level dummies.

Table 9—: Effects on Maternity Leave Duration

	Maternity Leave Duration (weeks)		
	(1)	(2)	(3)
	Reported leave	Reported + inactivity	Reported + sick child leave
Exposure (z)	4.134***	4.078***	3.725***
	(0.659)	(0.647)	(0.730)
Mother's age (years)	0.040	0.036	0.010
	(0.027)	(0.027)	(0.030)
Child's sex (male)	-0.502*	-0.395	-0.476
	(0.278)	(0.282)	(0.316)
Birthweight (kg.)	-0.176	-0.362	-0.004
	(0.300)	(0.309)	(0.330)
Urban area	-0.180	-0.552	0.045
	(0.592)	(0.625)	(0.665)
Number of people in the household	0.173*	0.169*	0.121
	(0.096)	(0.094)	(0.109)
Mother is student	-1.573**	-1.406*	-1.422
	(0.704)	(0.728)	(0.924)
Household income (thousands of pesos)	-0.000	-0.000*	-0.000
	(0.000)	(0.000)	(0.000)
Multiple birth	1.401	0.872	3.240**
	(1.141)	(1.148)	(1.573)
Order of birth	-0.066	0.063	-0.013
	(0.193)	(0.198)	(0.219)
Constant	9.959***	11.001***	9.759***
	(3.284)	(3.221)	(3.548)
Observations	3,203	3,144	3,203

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Control variables also include indicators of geographical region, maternal education level and marital status, father's presence, child's birth month, and test date.

Table 10—: Breastfeeding

	Duration (months)	Probability	
	(1) Hazard Rate (exponentiated coefficients)	(2) At least 3 months (coefficients)	(3) At least 6 months (coefficients)
Exposure (z)	0.773**	0.003	0.066*
	(0.090)	(0.026)	(0.038)
Birthweight (kg.)	0.922*	0.004	0.039**
	(0.040)	(0.012)	(0.019)
Child's sex (male)	1.027	-0.003	-0.016
	(0.038)	(0.010)	(0.016)
Order of birth	0.969	0.006	-0.007
	(0.026)	(0.006)	(0.011)
Gestational duration	0.970**	0.011***	0.009*
	(0.012)	(0.004)	(0.006)
Household per capita income (thousands of pesos)	1.001***	0.000	-0.000
	(0.000)	(0.000)	(0.000)
Mother's age (years)	0.995	0.001	0.002
	(0.004)	(0.001)	(0.002)
Labor market inactive	0.980	0.015	-0.018
	(0.052)	(0.014)	(0.024)
Constant		0.184	-0.232
		(0.271)	(0.300)
Dummies for mother's risky behaviors	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes
Dummies for mother's civil status and schooling level	Yes	Yes	Yes
Observations	2,660	2,660	2,660

Notes: Hazard ratios (exponentiated coefficients) reported in column (1). Robust standard errors in parentheses in columns (2)-(3). Delta-method standard errors in parentheses in column (1). *** p<0.01, ** p<0.05, * p<0.1. Control variables also include dummies for consumption of alcohol during pregnancy, childcare arrangement when the child was 0-3 months old, the mother being a dependent or independent worker, maternal education and marital status, child's birth month, and household geographical region.

Table 11—: Maternal stress (Parenting Stress Index)

	PSI (percentile score)			
	(1)	(2)	(3)	(4)
	Total	Parental distress	Parent-child dysfunctional interaction	Difficult child
Exposure (z)	-9.433***	-0.508	-3.940	-17.285***
	(3.135)	(3.226)	(2.685)	(2.800)
Child's age (months)	0.053	-0.038	0.104***	0.063
	(0.046)	(0.043)	(0.039)	(0.044)
Mother's age (years)	-0.108	-0.035	0.168	-0.165
	(0.126)	(0.119)	(0.107)	(0.119)
Child's sex (male)	3.423***	1.778	1.881*	3.460***
	(1.253)	(1.189)	(1.078)	(1.203)
Urban area	1.585	0.496	2.365	1.410
	(2.892)	(2.736)	(2.484)	(2.792)
Number of people in household	0.248	0.005	0.713*	0.208
	(0.451)	(0.458)	(0.400)	(0.432)
Mother is student	-5.833*	-6.890**	-3.996	-2.721
	(3.058)	(2.873)	(2.495)	(2.986)
Birthweight (kg.)	0.039	-0.519	-0.627	0.688
	(1.306)	(1.250)	(1.140)	(1.250)
Household income (thousands of pesos)	-0.005***	-0.006***	-0.005***	-0.003***
	(0.001)	(0.001)	(0.001)	(0.001)
Multiple birth	-0.447	-1.336	-3.216	1.186
	(4.780)	(4.625)	(3.731)	(4.389)
Order of birth	0.164	0.850	0.082	-1.454*
	(0.926)	(0.888)	(0.792)	(0.875)
Constant	55.429***	81.859***	27.675**	43.340**
	(16.311)	(16.644)	(12.059)	(18.456)
Observations	2,701	2,856	2,784	2,760

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Control variables also include indicators of geographical region, father's presence, maternal education level and marital status, and child's birth month.

Table 12—: Childcare Center Dependence

	High school or less	More than high school	Difference
Public center (proportion)	0.51	0.33	0.18***
	[193]	[89]	(0.061)

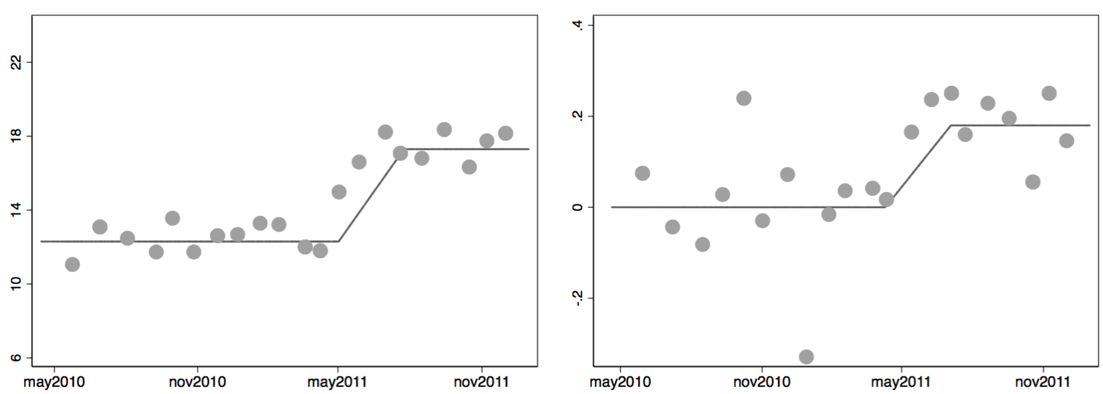
Notes: Proportion of mothers who chose a public childcare center while the child was 3-6 months, conditional on having chosen a formal childcare arrangement and not being exposed to the reform. Sample size in square brackets and standard error of the difference of proportions in parentheses. *** p<0.01, ** p<0.05, * p<0.1 (for the difference of proportions.)

Online Appendix

A. Evolution of maternity leave durations and cognitive scores

To depict the idea of the identification strategy, we present the evolution of maternity leave durations and cognitive test scores before and after the reform's implementation. In Panel (a) of Figure A1, we present a binned scatter plot of maternity leave durations where each dot corresponds to the average mean of a given bin. As in Bleakley (2010) the solid line plots the reform exposure pattern which consists of three regions. The two horizontal lines (from May 2010 to May 2011 and from August 2011 to December 2011) correspond to the sample averages of maternity leave durations for the non-exposure and full-exposure periods respectively. The line spanning from May 2011 to August 2011 connects the two averages just mentioned corresponding to the partial exposure period. Panel (b) presents a binned scatter plot for cognitive test scores, age-adjusted by linear regressions. We control for influential observations using Cook's distance (Cook, 1979). Each dot represents the average test score for a given bin, and the solid line the exposure pattern, constructed similarly to that of Panel (a). While noisy, the evolution of cognitive test scores follows a similar pattern to that of maternity leave duration. This does not occur with the other outcomes where no effects are found in the reduced-form analysis.

Figure A1. : Evolution of maternity leave durations and cognitive scores



(a) Maternity leave duration

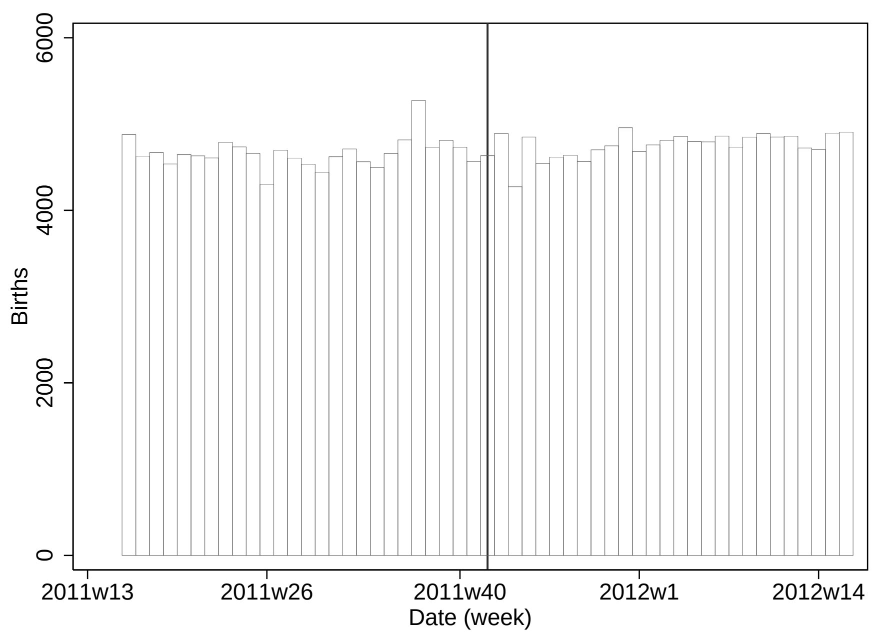
(b) Cognitive test score

Notes: Each dot represents the average number of maternity leave weeks (Panel a) or cognitive test scores (Panel b) for a given bin. The solid line represent the reform exposure pattern. See text for details.

B. Exogeneity of the reform

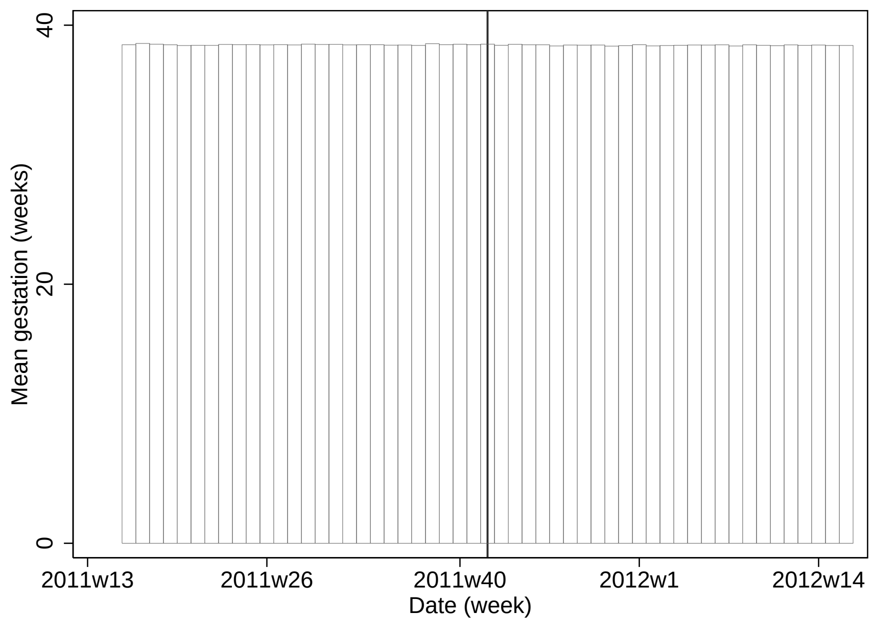
In this appendix, we show some evidence suggesting no manipulation of fertility timing decisions due to the reform. Figure B1 shows no strange behavior of weekly births around the date of enactment of the reform. Figure B2 shows a similar result for gestational durations. If mothers decided to postpone childbirths anticipating the benefits of the reform, we should find an increase in mean gestational duration after the reform. It is not the case. We also look at average schooling levels and mean household income of mothers giving birth in different months. Figures B3 and B4 reveal no evident patterns. Finally, we study the behavior of monthly employment contract signing in the period 2004-2011. Contract signing shows a cyclical pattern, but Figure B6 reveals no particular change in October 2011 (for example as compared to 2010).

Figure B1. : Weekly births before and after enactment



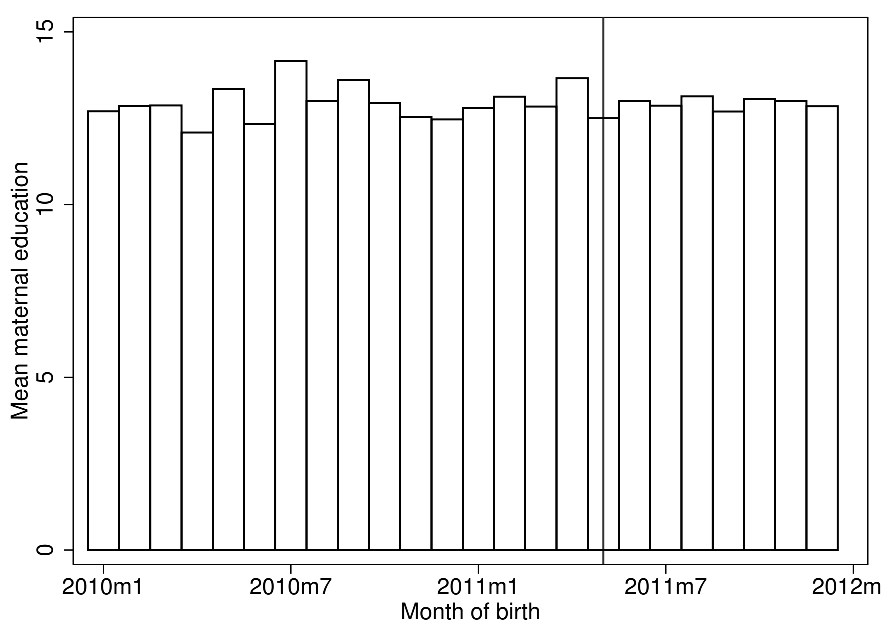
Notes: Weekly births before and after the enactment of the law. The vertical line is the enactment date October 17, 2011 (week 42). *Source:* Birth census data (DEIS)

Figure B2. : Mean gestational duration before and after enactment (birth census data)



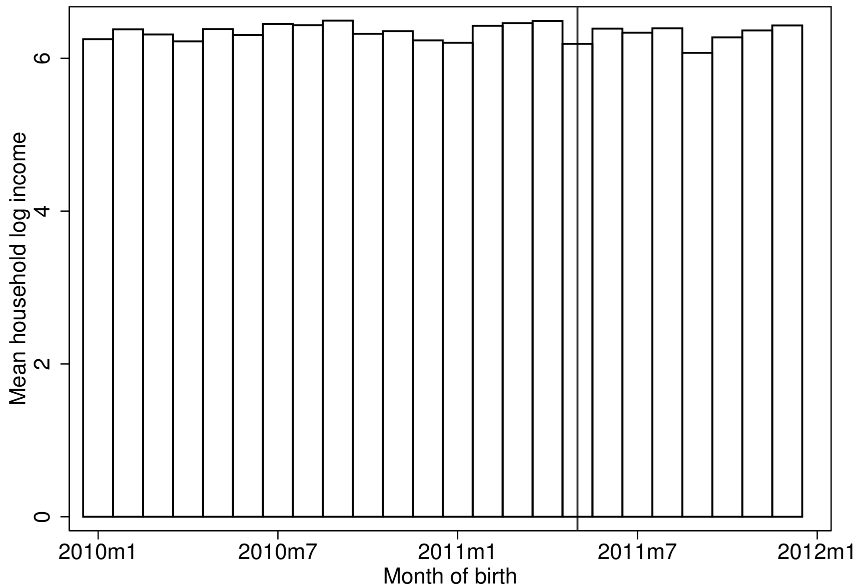
Notes: Weekly mean gestational duration before and after the enactment of the law. The vertical line is the enactment date October 17, 2011 (week 42). *Source:* Birth census data (DEIS)

Figure B3. : Mother's education and month of birth of the child



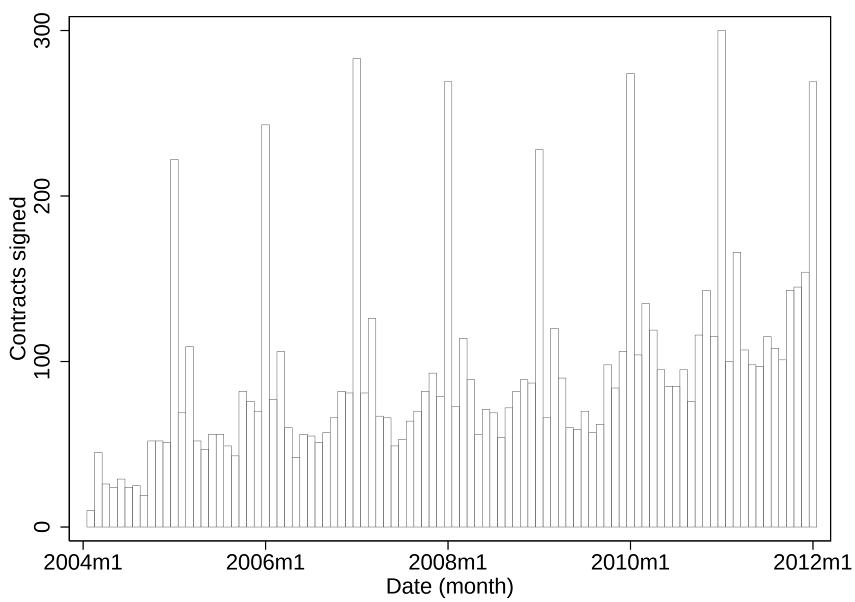
Notes: Mean schooling of mothers giving birth each month. The vertical line is May 2011. *Source:* ELPI data.

Figure B4. : Household income and month of birth of the child



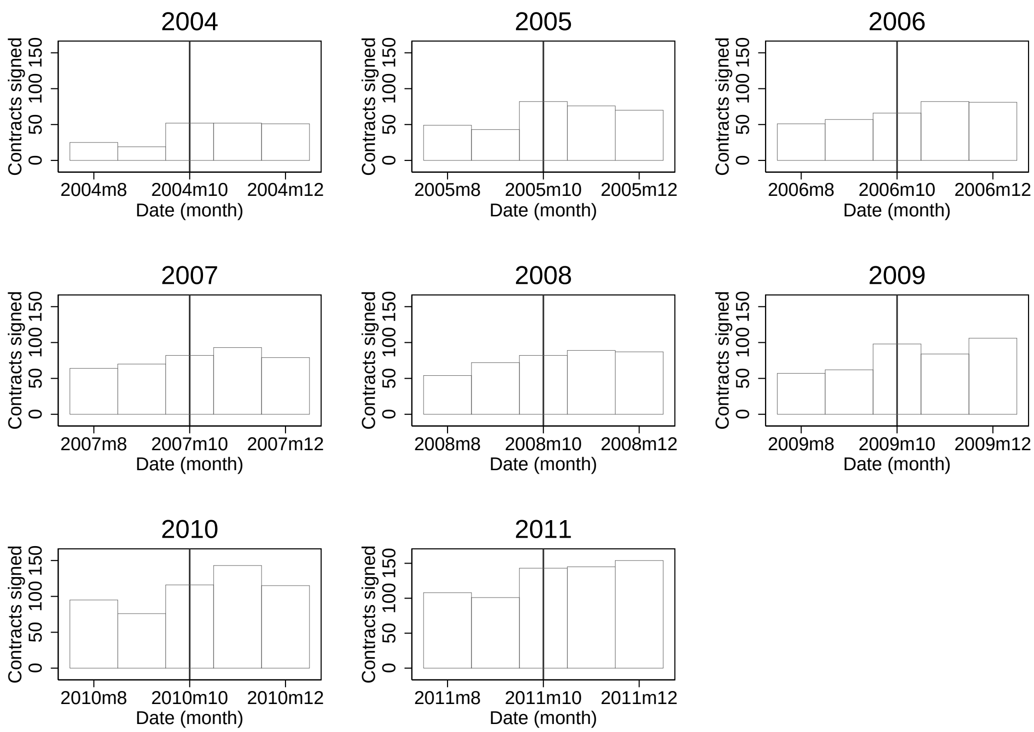
Notes: Mean household income of mothers giving birth each month. The vertical line is May 2011. *Source:* ELPI data.

Figure B5. : Monthly beginning of labor stories: 2004-2011 (ELPI data)



Notes: Monthly beginning of labor stories: 2004-2011 (ELPI data) *Source:* ELPI data.

Figure B6. : Monthly beginning of labor stories (zoom): 2004-2011 (ELPI data)



Notes: Monthly beginning of labor stories: 2004-2011 (ELPI data) *Source:* ELPI data.

C. Binary treatment

Table C1—: Effects on Maternity Leave

	Maternity Leave Duration (weeks)		
	(1)	(2)	(3)
	Reported leave	Reported + inactivity	Reported + sick child leave
Exposure (z)	3.497***	3.497***	3.105***
	(0.569)	(0.553)	(0.630)
Mother's age (years)	0.039	0.035	0.009
	(0.027)	(0.027)	(0.030)
Child's sex (male)	-0.507*	-0.401	-0.481
	(0.279)	(0.283)	(0.316)
Birthweight (kg.)	-0.170	-0.353	0.001
	(0.300)	(0.310)	(0.330)
Urban area	-0.173	-0.544	0.051
	(0.594)	(0.627)	(0.666)
Number of people in the household	0.173*	0.169*	0.121
	(0.097)	(0.095)	(0.109)
Mother is student	-1.517**	-1.347*	-1.375
	(0.704)	(0.729)	(0.925)
Household income (thousands of pesos)	-0.000	-0.000*	-0.000
	(0.000)	(0.000)	(0.000)
Multiple birth	1.449	0.918	3.285**
	(1.138)	(1.144)	(1.568)
Order of birth	-0.063	0.066	-0.009
	(0.193)	(0.198)	(0.220)
Constant	9.981***	11.009***	9.781***
	(3.291)	(3.227)	(3.554)
Observations	3,203	3,144	3,203

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Control variables also include indicators of geographical region, maternal education level and marital status, father's presence, child's birth month, and test date.

Table C2—: Child Development

	TADI				
	(1)	(2)	(3)	(4)	(5)
	Total	Cognitive	Language	Motor	Socioemotional
Exposure (z)	0.223***	0.258***	0.129*	0.250***	0.043
	(0.067)	(0.070)	(0.075)	(0.066)	(0.074)
Child's age (months)	0.007***	0.007***	0.002	0.009***	0.003*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Mother's age (years)	0.000	0.004	0.000	-0.005	0.002
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Child's sex (male)	-0.180***	-0.075**	-0.151***	-0.129***	-0.201***
	(0.032)	(0.031)	(0.032)	(0.032)	(0.034)
Birthweight (kg.)	0.040	0.046	-0.006	0.069**	0.020
	(0.034)	(0.033)	(0.034)	(0.034)	(0.037)
Urban area	0.085	0.165**	0.094	-0.054	0.082
	(0.070)	(0.065)	(0.070)	(0.072)	(0.081)
Number of people in the household	-0.025**	-0.010	-0.020*	-0.023*	-0.020
	(0.012)	(0.011)	(0.011)	(0.012)	(0.013)
Mother is student	0.091	0.049	0.024	0.029	0.181**
	(0.077)	(0.078)	(0.076)	(0.077)	(0.087)
Household income (thousands of pesos)	0.000***	0.000***	0.000***	0.000**	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Multiple birth	-0.147	-0.066	-0.124	-0.184*	-0.063
	(0.105)	(0.102)	(0.103)	(0.109)	(0.104)
Order of birth	-0.079***	-0.092***	-0.073***	-0.009	-0.069***
	(0.023)	(0.022)	(0.023)	(0.024)	(0.024)
Constant	-0.332	-0.420	-0.066	-0.154	-0.433
	(0.431)	(0.461)	(0.435)	(0.381)	(0.425)
Observations	3,136	3,151	3,154	3,150	3,155

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Control variables also include indicators of geographical region, maternal education level and marital status, father's presence, birth month, and test date.

Table C3—: Maternal stress (Parenting Stress Index)

	PSI (percentile score)			
	(1)	(2)	(3)	(4)
	Total	Parental distress	Parent-child dysfunctional interaction	Difficult child
Exposure (z)	-8.786***	-1.530	-3.531	-15.501***
	(2.908)	(2.945)	(2.497)	(2.633)
Child's age (months)	0.048	-0.047	0.103***	0.057
	(0.046)	(0.044)	(0.039)	(0.044)
Mother's age (years)	-0.108	-0.035	0.168	-0.162
	(0.126)	(0.119)	(0.107)	(0.119)
Child's sex (male)	3.422***	1.769	1.882*	3.455***
	(1.252)	(1.189)	(1.078)	(1.203)
Urban area	1.645	0.523	2.388	1.520
	(2.899)	(2.737)	(2.490)	(2.799)
Number of people in the household	0.252	0.005	0.714*	0.211
	(0.451)	(0.458)	(0.400)	(0.432)
Mother is student	-5.975*	-6.942**	-4.052	-2.969
	(3.059)	(2.874)	(2.496)	(2.990)
Birthweight (kg.)	-0.000	-0.524	-0.643	0.638
	(1.307)	(1.250)	(1.139)	(1.251)
Household income (thousands of pesos)	-0.005***	-0.006***	-0.005***	-0.003***
	(0.001)	(0.001)	(0.001)	(0.001)
Multiple birth	-0.527	-1.308	-3.260	1.028
	(4.781)	(4.623)	(3.731)	(4.391)
Order of birth	0.157	0.852	0.078	-1.477*
	(0.926)	(0.888)	(0.791)	(0.874)
Constant	55.742***	82.327***	27.715**	43.515**
	(16.318)	(16.652)	(12.051)	(18.461)
Observations	2,701	2,856	2,784	2,760

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Control variables also include indicators of geographical region, father's presence, mother's education level and marital status, and child's birth month.

Table C4—: Breastfeeding

	Duration (months)	Probability	
	(1)	(2)	(3)
	Hazard Rate	At least 3 months	At least 6 months
	(exponentiated coefficients)	(coefficients)	(coefficients)
Exposure (z)	0.782**	0.009	0.082**
	(0.077)	(0.021)	(0.032)
Birthweight (kg.)	0.922*	0.004	0.039**
	(0.040)	(0.012)	(0.019)
Child's sex (male)	1.028	-0.003	-0.015
	(0.038)	(0.010)	(0.016)
Order of birth	0.969	0.006	-0.007
	(0.026)	(0.006)	(0.011)
Gestational duration	0.970**	0.011***	0.010*
	(0.012)	(0.004)	(0.006)
Household per capita income (thousands of pesos)	1.001***	0.000	-0.000
	(0.000)	(0.000)	(0.000)
Mother's age (years)	0.995	0.001	0.002
	(0.004)	(0.001)	(0.002)
Labor market Inactive	0.979	0.015	-0.018
	(0.052)	(0.014)	(0.024)
Constant		0.183	-0.236
		(0.271)	(0.299)
Dummies for mother's risky beahaviors	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes
Dummies for mother's civil status and schooling level	Yes	Yes	Yes
Observations	2,660	2,660	2,660

Notes: Hazard ratios (exponentiated coefficients) reported in column (1). Robust standard errors in parentheses in columns (2)-(3). Delta-method standard errors in parentheses in column (1). *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Control variables also include dummies for consumption of alcohol during pregnancy, childcare arrangement when the child was 0-3 months old, the mother being a dependent or independent worker, maternal education and marital status, child's birth month, and household geographical region.

D. Child Development and Paternal Education

In this section, we present additional heterogeneity results. In particular, we estimate separate models for high (more than high school) and less educated (high school or less) fathers. Similar to the case of maternal education, we find a higher impact of the reform on children with less educated fathers on cognitive and motor skills. However, there is not enough statistical power to reject the null hypotheses that coefficients are equal across groups. Also, the correlation between father and mother years of schooling in our sample is high (0.61). Thus, the interpretation of the results does not differ from those of maternal education due to positive assortative matching.

Table D1—: Child Development and Paternal Education

	(1)	(2)
	High school or less	More than high school
TADI total	0.255**	0.192
	(0.111)	(0.147)
	[1,525]	[720]
TADI cognitive	0.363***	0.279*
	(0.113)	(0.156)
	[1,531]	[725]
TADI language	0.149	0.120
	(0.126)	(0.174)
	[1,530]	[727]
TADI motor	0.235**	0.205
	(0.109)	(0.160)
	[1,530]	[725]
TADI socioemotional	0.021	-0.021
	(0.113)	(0.171)
	[1,531]	[727]

Notes: SUEST robust standard errors in parentheses, sample sizes in square brackets. *** p<0.01, ** p<0.05, * p<0.1. Control variables not shown are child's and mother's age, birth-weight, number of people in the household, and indicators of geographical region, urban area, child's gender, maternal education, mother's marital status, child's birth month, and evaluation date. In panel B we also control for father's presence.