Does Subsidized Childcare Matter for Maternal Labor Supply? A Modified Regression Discontinuity Analysis

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Abstract: We use a variant of the regression discontinuity method to estimate the effect of subsidized childcare availability on mothers' labor supply when other factors (maternity leave, preferences regarding separation) change simultaneously. We separate the age and calendar-specific childcare effect from those that are only age-specific by comparing treatment and control groups by holding children's age constant, and combine RD with difference-in-differences to account for seasonal effects. Our estimates suggest that childcare availability has a significant positive impact of around 18%. The results highlight the importance of methods that control for endogeneity and concurrent age-related effects, and a comprehensive policy approach.

Keywords: Subsidized Childcare Availability, Maternal Labor Supply, Regression Discontinuity

JEL codes: J13, J22

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I. INTRODUCTION

Encouraging higher labor market participation of women, especially mothers of young children, is an important policy goal in most countries.[1] Many factors affect a woman's ability and willingness to work after having a child, including parental leave, tax benefits, childcare availability, labor market opportunities, and societal attitudes. The possible range of policy tools is correspondingly varied, but recent consensus among policymakers is that the expansion of subsidized childcare is an important component.[2] To find the most effective mix of policies - and to forecast the benefits of investment in childcare expansion - it is important to estimate the impact of childcare (and other factors) on mothers' labor supply precisely.

Due to the inherent endogeneity of subsidized childcare availability with respect to regional and individual characteristics[3], Regression Discontinuity (RD) seems to be the most promising method for measuring a causal effect, as enrollment cutoffs create a quasi-experimental setup. To our knowledge, so far only one study used RD to measure the childcare effect (Fitzpatrick 2010), because the applicability of RD is limited by the fact that other factors often change simultaneously.[4] In Hungary, for example, subsidized kindergarten[5]

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[1] It is key to sustainable growth, lowering budget deficits, and gender equality (Bloom et al. 2009), demographic policy (Apps and Rees 2001), and satisfying increased skill demand (Krusell et al. 2000).

[2] In the US and Canada, universal subsidized pre-kindergarten was introduced in several places (Fitzpatrick 2010, Lefebvre and Merrigan 2008), the EU set targets for increasing childcare availability (EU 2002).

[3] For instance, in better developed regions, labor market opportunities are better, so participation rates are higher. At the same time, demand for childcare institutions is higher, and more resources are available for building childcare capacities.

[4] In many countries, parental leave ends around the child's 3rd birthday: 30 months after birth in Bulgaria, 36 months in the Czech Republic, Estonia, Finland, Lithuania, Norway, Russia, Slovakia and Spain, 42.2 months in Germany and 45.3 months in France (Multilinks

becomes available to mothers when their child turns 3, which corresponds to the end of parental leave and (according to surveys) a strong shift in the population's views regarding when mothers should return to work (Blaskó 2011).

We propose a modified version of the RD method that can be used in cases where changes in various factors are tied to different underlying running variables. While eligibility for kindergarten depends on both age and calendar date (whether the child turns 3 prior to the cutoff date), parental leave and separation preferences depend only on the age of the child. We can therefore identify the effect of childcare availability and separate it from the effect of factors that are not dependent on the calendar date by holding child age, rather than the date of observation, constant in our samples of treatment and control groups. This method requires the use of repeated cross-section or panel data, but enables better measurement of the childcare effect by extending the usability of RD to cases not previously possible.

Most common in the previous literature are studies that use regional or time variation in childcare prices to identify the impact on labor supply.[6] However, unobserved characteristics in the error term - mainly individual and regional - make childcare availability endogenous in the labor supply equation (e.g. migration between settlements, or the economic development of settlements), and most of these introduce an upward bias. The evidence from these studies varies not only because of differences in methodology (Blau 2003) and data, but also differences in the age of the children analyzed, and cross-country differences in institutional

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Database, 2011). The availability of childcare changes at age 3 all over Europe, when children are eligible for kindergarten.

[5] In Hungary, kindergarten refers to childcare for 3-6 year olds, and nursery school to childcare for 1-3 year olds.

[6] Most study Europe and North America, while studies on developing countries are rare (Lokshin (2004)).

and hard-to-observe preferential factors.[7] Several structural studies support the existence of a negative effect of childcare costs on participation or employment (Lokshin 2004, Borra 2010, Kimmel 1992, Conelly 1992, Haan and Wrohlich 2011, Del Boca 2002), others find little or no significant effect (Chevalier and Viitanen 2002, Chone et al. 2003, Ribar 1995).

More recent research noted that these issues make it difficult to provide causal estimates of the impact of childcare based on regional and time variation alone, and looked to exogenous sources of variation for identification. Several studies make use of policy changes and use difference in differences methods. However, policy decisions about subsidized childcare supply may be endogenous as well if they depend on local childcare demand and related political pressures, which is likely the case. Some policy change-based studies find a significant positive impact of childcare expansion on the labor supply of mothers (Baker et al. 2008, Lefebvre and Merrigan 2008), while others find no significant impact (Cascio 2009, Lundin et al. 2008). Baker et al. (2008) note that the estimated elasticities from policy change based studies (Berger and Black 1992, Gelbach 2002, Herbst 2008, Cascio 2009) are at the lower end of the range of estimates based on structural models reported by Blau (2003).

In an RD approach, there is a unique discontinuity that can be exploited: identification comes from the difference in the eligibility of otherwise identical children born just before and after the cutoff. Randomness of the children's birth dates ensures that there is exogenous variation in childcare. RD also requires fewer assumptions, but a crucial condition is that no other factors change discontinuously at the same cutoff point. On the other hand, the external

[7] Regarding the wide variation among countries in separation preferences, see the International Social Survey Programme: http://zacat.gesis.org/webview/index.jsp?object=http://zacat.gesis.org/obj/fStudy/ZA3880%20

validity of these measures may be limited, as local average treatment effects are measured. Fitzpatrick (2010) uses an RD framework to measure the childcare effect, based on three US states that recently introduced universal prekindergarten programs (for 4-year-olds) with birth date-based eligibility cutoffs. The results suggest that universal childcare availability had negligible effect on the labor supply of most women.

The validity of this approach depends on the assumption that the groups do not differ significantly besides their treatment status. However, since they are observed and compared when their children have different ages on average, RD requires that no other age-related factor may change discontinuously at the cutoff. This often poses a problem, as childcare policy may be linked to other policies that affect the labor supply of mothers simultaneously. Additionally, the institutional framework may influence (or have been shaped by) preferences regarding separation from the child. In such cases, estimating the effect of childcare via standard RD would therefore give us the magnitude of the joint effect of these factors.

We modify the standard RD setup in order to address this violation of the requirement that no other factor may be discontinuous at the cutoff point. We define treatment and control groups based on whether the mother's child is eligible for kindergarten or not (turns 3 before or after the cutoff date). We then utilize a sampling design that holds the age of child at observation constant. As a result, discontinuities related to child age affect the treatment and the control groups similarly, and do not bias the estimation of the childcare effect. Contrary to the standard RD setup, this sampling design makes it possible for us to separate calendar date and age effects.

In this setup, the groups differ by the season in which they are observed and that their child was born. This means that estimates may be affected by selection bias if parental characteristics or child development differs by season of birth of children, or if labor market opportunities differ by season. We control for this by including mothers with 4-5-year-old children, grouped based on the same cutoff date, as comparison groups, combining RD with difference in differences estimation. These comparison groups should be affected by the same seasonal effects, but not by the treatment effect, allowing us to separate out seasonal factors.

Our estimates suggest that subsidized childcare availability has a significant positive effect on mothers' labor supply. Increasing subsidized childcare availability for children around 3 years old by 10% increases the participation rate by 1.8%. The improvement of childcare opportunities explains about a third of the 31 percentage point increase in mothers' labor market participation seen around age 3 of children. Parental leave is unlikely to explain the rest of the increase, suggesting that further factors, such as separation preferences, play a role. The policy implication of our paper is therefore that childcare expansion should be considered as a component of a set of coordinated steps aimed at achieving an increase in the labor market participation of mothers.

II. BACKGROUND AND FRAMEWORK

Before discussing the methodology and estimation results, we describe important details of the institutional setting in Hungary that are relevant to our analysis and a simple theoretical model. We focus on the childcare system and two other sources of change around age 3 of children that affect our estimation: parental leave and preferences regarding separation.

II.1. FACTORS DETERMINING MOTHERS' LABOR SUPPLY IN HUNGARY

II.1.1. THE CHILDCARE SYSTEM

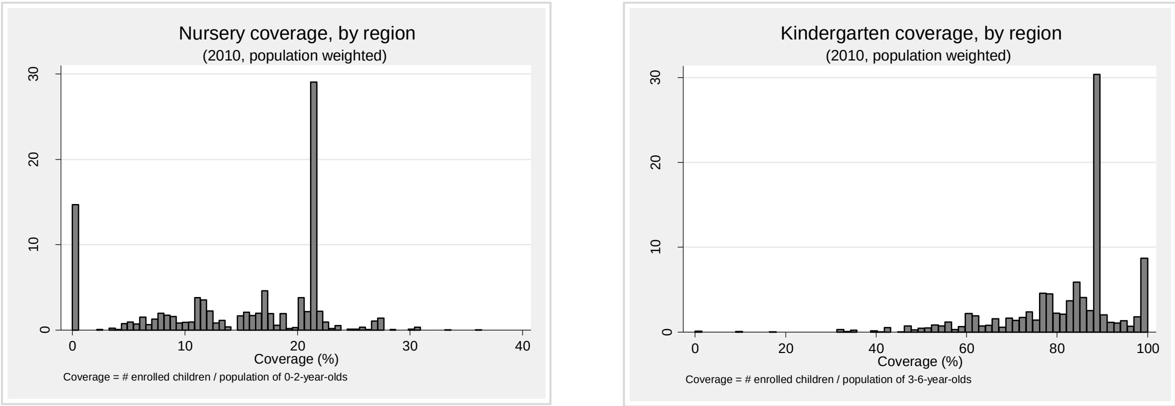
The system of formal childcare institutions in Hungary consists of various possibilities. State-owned and -financed *nursery schools* accept children up to 3 years of age.[8] The childcare services of these institutions are free of charge, but parents pay for the meals and make minor material contributions. Nursery school coverage is relatively low: a mere 11% of children under age 3 were attending nursery school in 2009 (EU-SILC), and approximately 9% of settlements had a nursery school. For children above 3, state-owned kindergarten becomes available (depending on their birth date), which costs about the same. Kindergarten coverage is significantly higher, around 80% in 2009 (EU-SILC), and relatively high compared to other EU countries. This means that when a child becomes eligible for kindergarten, the mother's low cost childcare opportunities expand significantly, as their child is likely to be accepted into subsidized childcare.

Figure 1 shows our own calculations of state-owned nursery and kindergarten coverage rates based on the Hungarian regional dataset (T-STAR) used in our empirical analysis. Coverage is calculated as the number of children enrolled divided by the number of children of the given age group in the region.[9] These statistics also highlight the low availability of nurseries, and significant regional variability in subsidized childcare coverage.

[8] State-owned institutions refer to those run by the federal or local government.

[9] The construction of the regions is based on township-level data aggregated according to kindergarten commuting statistics, as described in the Data section.

FIGURE 1: DISTRIBUTION OF NURSERY AND KINDERGARTEN COVERAGE RATES BY REGION



Notes: *Based on T-STAR Hungarian regional data, 2010. Coverage rate: the number children enrolled within each region, divided by the number of children of relevant age (0-2 for nursery, 3-6 for kindergarten) in each region. Region refers to townships merged based on kindergarten commuting data, there are 530 regions in the sample.*

The kindergarten school year begins in September. Officially, the main eligibility rule for subsidized kindergartens is that children who turn 3 prior to September 1st may be enrolled. Additionally, kindergartens may accept children who turn 3 after the cutoff date and enroll them continuously, as long as they have available places left.[10] This means that the September 1st cutoff, in fact, may not be strictly enforced, which affects our estimation. To account for this uncertainty in the cutoff date, we separate mothers into three groups whose situation differs in terms of when (at what age of the child) they are likely to be able to enroll their child into subsidized kindergarten, and compare the situation of all three groups. This means that in the RD estimation setup, we explore several alternative cutoffs (September 1, January 1, and the interval cutoff September 1-January 1) and compare the results.

[10] Anecdotal evidence and interviews with kindergarten directors suggest that a few institutions have an additional January 1st enrollment wave, while others allow children to attend unofficially starting in September, even though they do not receive the state quota after them until they turn 3. Since 2010, a law change has allowed 2.5 year olds to be admitted from September as well if space is available. Although this extension was not in effect during the period in our analysis, it may have been motivated by these already existing practices.

Figure 2 depicts the three groups of mothers based on child birthdates and their expected kindergarten enrollment dates. We show groups based on 5 month birth date intervals, corresponding to our main estimation results (though we also carry out our analysis with 3 and 4 month windows). Group 1, with children born between April and August, can enroll their children on September 1st, an average of 2.5 months after their birthday. We refer to this measure as the average waiting time (AWT). The situation of Group 2, with children born between September and January,[11] is less clear. If kindergarten places are already filled by Group 1 and these mothers have to wait until next September to enroll their children, their average waiting time would be 8.5 months. However, it is more likely that there are still places available after September, so they can enroll immediately after the children's 3rd birthday, with an average waiting time of 0 months.[12] Group 3, with children born January-May, are only able to enroll their children next September, as there are not likely to be kindergarten places left after January. This suggests that they have to wait 5.5 months after their child's birthday on average to enroll.

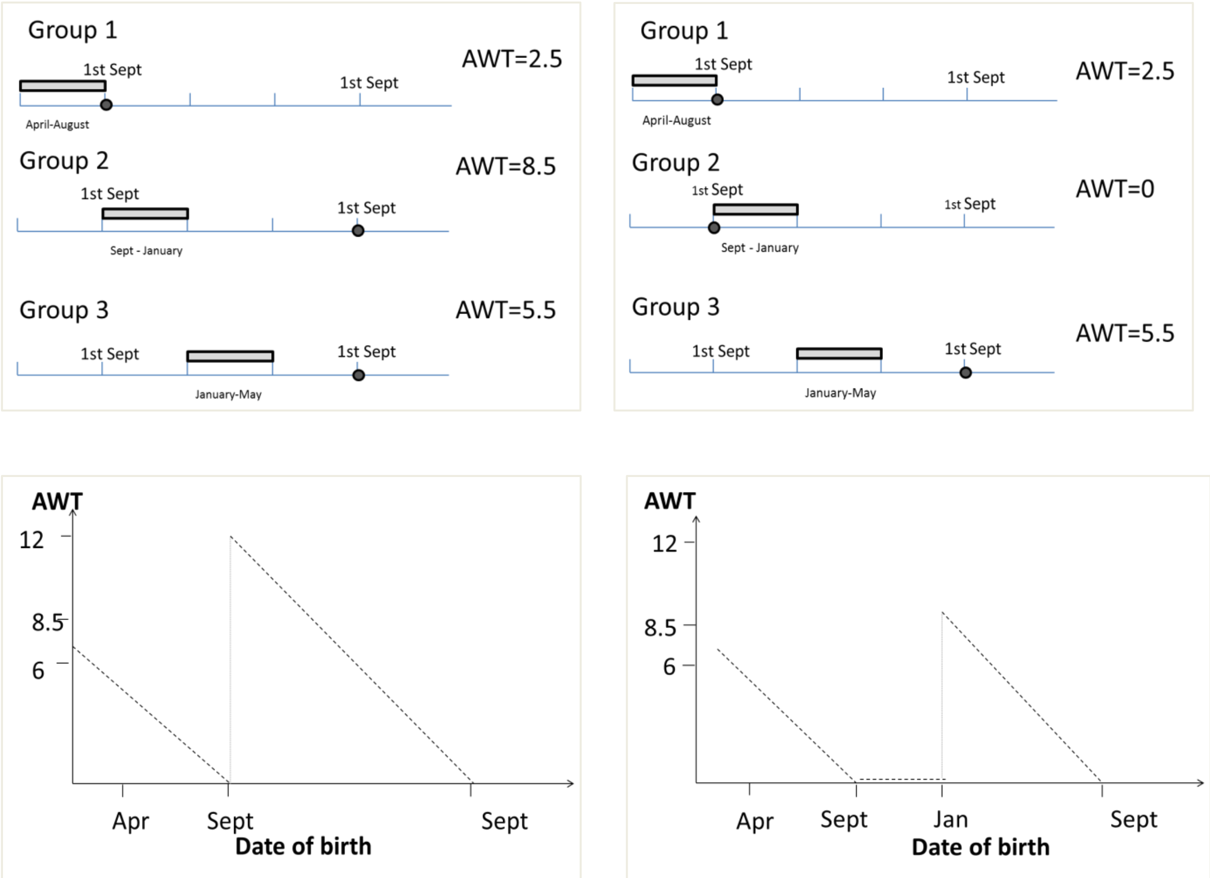
[11] Groups are defined based on 5 month windows on either side of the cutoff. So in our September 1 cutoff estimation, Group 2 is defined as September-January, while in the January 1 cutoff estimates, it is defined as August-December. To save space, we only show figures for the first Group 2 definition, though they do not differ significantly in either case.

[12] If they are allowed to enroll before their 3rd birthday, AWT is negative (-2.5 months), while if they have to wait until a January 1st enrollment wave, it would be 2.5 months, the same as Group 1.

FIGURE 2: TIMING OF EXPECTED KINDERGARTEN ENROLLMENT BY CHILD BIRTH DATE

2.a. Strictly enforced law

2.b. Weakly enforced law



Notes: Strictly enforced law refers to the September 1st cutoff date for enrollment. Weakly enforced law means that children born after September 1st are allowed to enroll as they turn 3, until all kindergarten places are filled. Groups are defined based on children's birth months. AWT refers to the average time (in months) that children have to wait to be enrolled in kindergarten relative to their 3rd birthday.

Overall, we can say that *if all kindergarten spots are filled in September* (2.a.), then there is no additional enrollment for those born after September 1st until the next year, and mothers in Group 1 have the shortest waiting time. The September 1st cutoff is then the most significant one, as the largest gap in opportunities occurs between Group 1 and Group 2. However, *if places are not filled in September* (2.b.), then children born after September 1st are able to enroll continuously until January 1st (or until all places are filled), and Group 2 may actually be in a slightly more favorable situation than Group 1 in terms of waiting time. The largest gap in childcare opportunities then occurs between Group 2 and 3, and the January 1st cutoff is the most significant. We do not impose any assumptions regarding which of these is the case prior to our estimation: we compare all three groups (use various cutoffs) and allow the statistics and estimates to tell us what actual enrollment practices are.

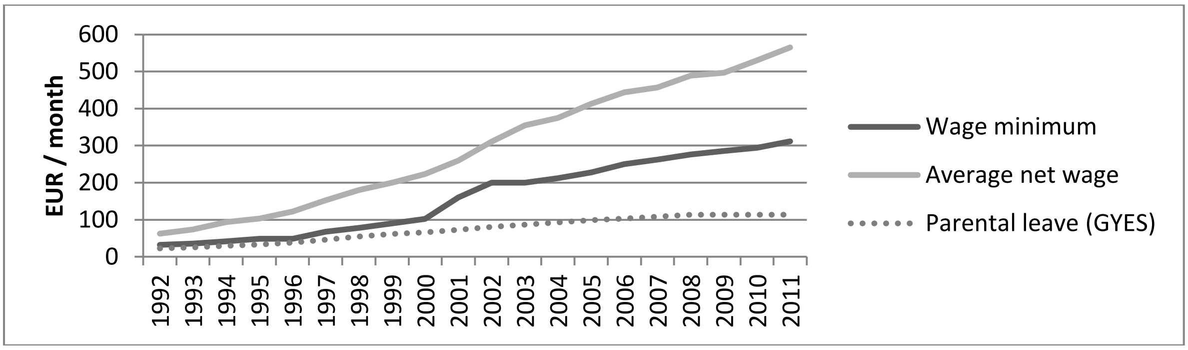
As a final note, from 1993, the system of formal childcare was amended with the possibility of opening family daycare service centers, though these are not very common. In addition to formal childcare arrangements, it is also possible to ensure childcare informally. Informal care may be provided by a babysitter, family member, or neighbor. Although our focus is on the effect of the provision of subsidized formal childcare, we control for the availability of family daycare services and the presence of potential informal childcare providers via the estimation method: the randomness of children's birth dates ensures that these should be equally available, on average, for all groups. Enrollment cutoff dates do not apply to private-owned kindergartens or informal caregivers, so our estimates only capture the effect of differences in subsidized state childcare availability between the groups.

II.1.2. Parental leave

In order to understand the labor supply decision of mothers with young children, it is important to review the parental leave system, which also impacts mothers' participation when children are young. For our purposes, flat-rate parental leave is of special interest among the available benefits, because it is given to families when the youngest child is under 3 years old, and it is terminated afterwards. Flat-rate parental leave was means-tested, but became universal in 1999 in Hungary: it can be received by anyone, with any high or low previous income, whether they were insured previously or not. One parent in each family is entitled to it, however, the overwhelming majority (98.1%) is taken by mothers according to the Hungarian Labor Force Survey (H-LFS) data.

The sum of this benefit in the final year (when child is between 2 and 3) equals the old-age pension minimum. Parental leave also provides basic health insurance and social security payments. Figure 3 depicts the evolution of average net wages, the mandatory wage minimum, and parental leave payments over time. The amount of the parental leave is low relative to the average wage, however, it may still have an impact on the labor supply decision of mothers with low expected wages or employment probabilities. Furthermore, the length of parental leave may be taken as an institutional signal regarding the "proper", socially accepted time for separation from the child, affecting mothers' preferences (discussed next). Since parental leave ends at age 3, it is relevant to our estimation, which is based on the discontinuous change in subsidized childcare availability at that age.

FIGURE 3: AVERAGE WAGE, WAGE MINIMUM, AND PARENTAL LEAVE (1992-2010)



Source: CSO and The Hungarian Labor Market – Review and Analysis (2011)

II.1.3. OTHER AGE-RELATED FACTORS: SEPARATION PREFERENCES

In addition to the two main institutional factors (subsidized childcare availability and parental leave), we also have to consider the role of other age-related factors that change around age 3 of the child. Preferences regarding separation from the child change during this period. Since parents become less attached as the child grows older, a comparison of treatment and control groups before and after the cutoff in the RD setup may lead to a bias, depending on the rate of change in preferences and narrowness of the RD sample frame.

In the case of Hungary, there is reason to suspect that these preferences change discontinuously at age 3 of the child. A survey by Blaskó (2011) suggests that the majority, 56.4% of people believe age 3 is the earliest acceptable time for a mother to leave the child and return to work, while 19.6% responded age 2, and 19.7% gave a later age than 3. This suggests that there may be a correlation between the institutional setting and societal preferences that the 3rd birthday is an important deadline (Hasková et al. 2012). Whether this is due to the institutional framework being interpreted as a signal by mothers that they should send the child to childcare and return to work, employers assuming that mothers will be absent less often after this age, or other factors, it leads to a discontinuity at age 3 that needs to be addressed in the estimation setup in order for it to be separated from the childcare effect.

II.2. Theoretical Framework

Blau (2003) provides a simple theoretical framework used to model how childcare price subsidy affects the labor supply decision of mothers with young children. We adopt this model to motivate our empirical methodology and pinpoint the main estimation issues relevant in our case. The analysis is based on a cost of working model, which does not take into account childcare as an input into the child's development. Childcare enters the decision process only as a cost of working - a pre-condition of it - as a means of taking care of the child while the mother works. Thus, the quality of childcare institutions is not taken into account, and is assumed to be homogenous. Although they differ in many ways, we also do not differentiate between kindergarten and nursery school, because – if available – both of them fulfill the requirement of safeguarding the child while the mother works. We assume that childcare is available for everyone at some market price. However, mothers face a significantly lower cost of childcare if subsidized institutional childcare is available to them.

The decision model is based on a traditional view of family decisions. The labor supply decision and the wage of the husband are exogenously given, and it is taken into account in the decision of the mother. Thus, the mother is the only agent in the model. For the sake of simplicity, we assume that working has no fixed costs and the wage rate (w) is constant, independent of the hours worked. If the mother decides to work h hours a month, she receives wh amount of salary. Additionally, she has y nonwage income, which includes the husband's salary. The mother spends the income on consumption goods (c), and she also pays for h hours of childcare, with a market price p. Total income left after paying for childcare is I, and ℓ is the amount of leisure time of the mother.[13] The labor supply decision in the model is presented as a choice of hours. It encompasses a discrete choice of participation, which is assumed in our econometric specification. In our case, it makes sense to include the labor supply decision as a discrete choice, because part-time work is rare in Hungary.

Figure 4.a. depicts the mother's labor supply. With zero hours of work, the mother receives income y, independent of the type of childcare available to her. The dotted line shows the budget constraint when there is no subsidized childcare available. In this case, with each hour of work, the mother gains the wage rate, minus the market price of non-subsidized childcare (nanny), which is $(w-p)$. The solid line shows the mother's labor supply if subsidized childcare becomes available to her. The cost of childcare decreases by s, and the mother's budget constraint rotates upward, as an additional hour of work now provides a gain of $(w-p+s)$. The mother's optimal labor supply is given by the tangency point of her budget constraint and an indifference curve in each case. The effect of a decrease in childcare costs –

[13] This means that: $c+ph=I+ph=y+wh$. The mother's budget constraint is: $c=I=y+(w-p)h$. If there is state-subsidized childcare available, the slope of the budget constraint changes: $c=I=y+(w-p+s)h$. The mother's time constraint is: $h+\ell=1$.

an increase in the availability of subsidized childcare - results in a labor supply increase, that is, h increases to h'.

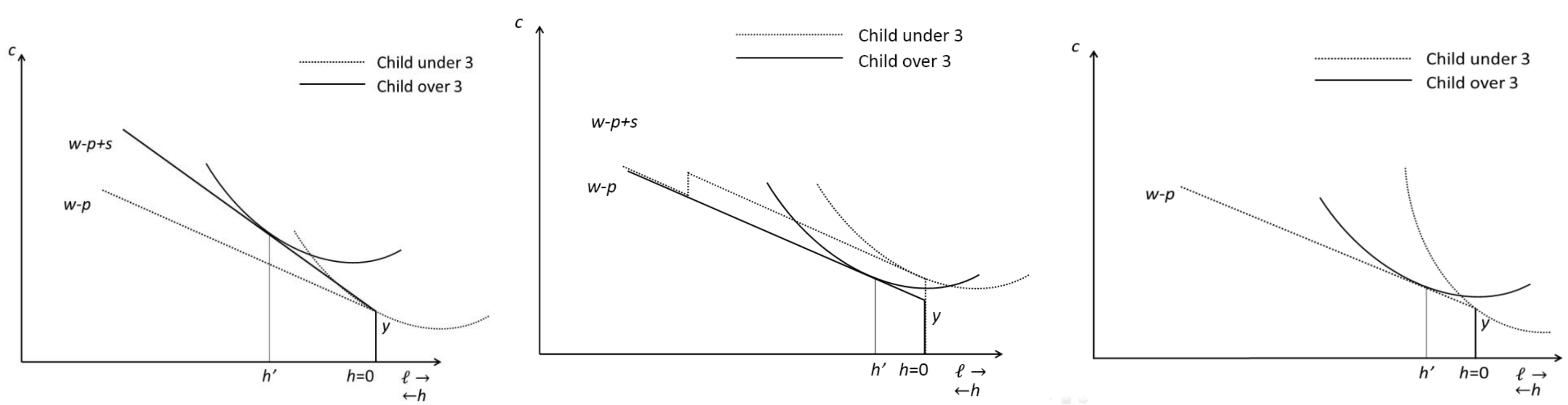
However, the other changes described above take place around age 3 of the child, which may confound the issue in the case of Hungary. First, one is that mothers are eligible for the flat-rate parental leave up to the child's 3rd birthday. Figure 4.b. depicts the labor supply of the mother when she is eligible for parental leave (child under age 3), and when not. The termination of parental leave also increases labor supply. Second, the mother's decision is also affected by her preferences regarding separation from the child (or other preferences, such as employers', or societal). Figure 4.c. shows the effect of a sudden change in separation preferences at age 3 of the child. As the child grows older, the mother requires less compensation for an extra hour spent working. The indifference curve becomes flatter, which will also lead to an increase in labor supply.

FIGURE 4: LABOR SUPPLY DECISION OF MOTHERS AROUND AGE 3 OF THE CHILD

(a) The effect of a childcare subsidy

(b) The effect of the end of parental leave

(c) The effect of a change in separation preferences



III. METHODOLOGY

Our methodology is based on the kindergarten enrollment cutoff date in an RD setup, however, we propose a modification of RD in order to tailor the estimation to a case where some of the standard RD assumptions do not hold. RD is usually estimated on a cross section of data, and the date of observation is the same for the treatment and the control group. As a consequence, effects related to age cannot be separated from those related to date of birth. The identification strategy is based on the fact that the treatment and control groups are divided by one cutoff point in time, so they are rather similar in age, birth date, and observation date, as well as every other characteristic except that only one group received the treatment.

A key condition for identification is that nothing other than the treatment changes discontinuously around the cutoff point. These two requirements can be somewhat relaxed by using our proposed setup. The method differs from standard RD in that here we use repeated cross section samples, one for the treatment and one for the control group, such that the mothers are observed one quarter after their child's third birthday, so their average child age is held constant among them. This allows for the separation of the calendar date-specific treatment effect from other factors that are age-dependent. We describe how the measurement strategy would look in the standard RD framework then introduce the differences in the modified RD design.

III.1 STANDARD RD

The standard regression discontinuity design is based on the following discontinuity:

$$Prob[D_i = 1|b_i] = \begin{cases} p_{yr}^{k} \ if \ b_i < b_0 \\ p_{yr}^{n} \ if \ b_0 \leq b_i \end{cases}, \text{ where } p_{yr}^{n} \neq p_{yr}^{k}.$$

D_i is a dummy variable, which indicates whether the child uses subsidized childcare, b_i is the month of the third birthday of the mother's youngest child,[14] and b_0 is the cutoff date. p_{yr}^n is nursery school coverage, p_{yr}^k is kindergarten coverage in region r, in the given period, y. As in many regions p_{yr}^n is near 0, and p_{yr}^k is near 100, this is a fuzzy RD setup. In our dataset, D_i, the actual subsidized childcare usage in not observed. Instead, in the regressions, we use the respective regional coverage measures, p_{yr}^n and p_{yr}^k to calculate aggregate usage by treatment group, region, and year. This introduces a measurement bias, but using an instrument solves this problem. The instrumental variable can then be defined as follows:

$$T_i = \begin{cases} 1 \; if \;\; b_i < b_0 \\ 0 \; if \;\; b_0 \leq b_i \end{cases}$$

Standard RD is based on individual level two-stage least squares regressions of the following form. The first stage is:

$$C_{yri} = \beta_1 T_i + \alpha_y + \gamma_r + X_i' \pi_{11} + S_{yr}' \pi_{12} + \xi_{1yri} \quad (1)$$

Where

$$C_{yri} = p_{yr}^n (1 - T_i) + p_{yr}^k T_i$$

is the regionally aggregated childcare usage in the relevant treatment group, in the given year, y. We can think of C_{yri} as the probability of individual i is able to use subsidized childcare. The

[14] We use the numbering 1-12 for birth months. In the case of a January cutoff, $b_i = 0$ for the preceding December, $b_i = -1$ for November, etc.

parameter β_1 reflects the first-stage effect of T_i on C_{yri}. The effects are measured adjusting for a set of individual (X_i) and regional covariates (S_{yr}). The subscripts indicate yearly (y), regional (r), and individual (i) variation. α_y represents year fixed effects, and γ_r region fixed effects.

The second stage equation is:

$$L_i = \beta_2 \widehat{C_{yri}} + \alpha_y + \gamma_r + X_i' \pi_{21} + S_{yr}' \pi_{22} + \xi_{2yri} \quad (2)$$

where individual-level labor supply L_i is a dummy variable, indicating whether the mother participates in the labor market at the date of observation. $\widehat{C_{yri}}$ is the fitted value of C_{yri}, obtained from the first stage regression.

The corresponding reduced form equation is:

$$L_i = \beta_R T_i + \alpha_y + \gamma_r + X_i' \pi_{R1} + S_{yr}' \pi_{R2} + \xi_{Ryri} \quad (3)$$

β_R captures the reduced-form effect of T_i on L_i, the individual labor supply.

In the case of Hungary, estimating using standard RD would capture the effect of not only increased childcare availability, but other factors as well: the end of parental leave, and other age-related factors such as changes in preferences regarding separation from the child at the 3rd birthday. In the standard RD setup, both groups are observed at the same time, in the quarter after the cutoff. This means that children in the treatment group are older on average (over 3) than children in the control group (just turning 3). Therefore parental leave ended in the previous quarter for the treatment group, while it is just ending for the control group, and, since children of the treatment group are older, their mothers may be more willing to separate from them and go back to work. Thus, we do not use this standard design in our regressions, only for comparison purposes (shown in Table 4).

III.2 MODIFIED RD

In order to separate the effect of childcare availability from these other effects, we use a modified RD design. We define the treatment and the control groups similarly to the standard RD case, but we observe each mother the same length of time, on average, after their child's 3rd birthday:

$$T_i = \begin{cases} 1 \quad if \quad b_i < b_0 \quad and \quad 3 \le a_i < 4 \\ 0 \quad if \quad b_0 + l \le b_i \quad and \quad 3 \le a_i < 4 \end{cases}$$

where a_i is the age of the youngest child, and l is the length of the interval cutoff,[15] in months. In case of a point cutoff, $l = 0$, while in case of an interval cutoff, $l > 0$. It is important to emphasize that we always examine the youngest child, as only mothers who don't have a smaller child are likely to be affected by subsidized childcare availability for their 3-year-old.

As in the previous setup, T_i is strongly correlated with subsidized childcare usage, as members of the treatment group have a significantly higher probability of having subsidized childcare available to them through their eligibility for kindergarten. At the same time, note that there is random assignment between the two groups, as the birth date does not correlate with any other factor that is relevant for labor market decisions. T_i is not correlated with any observed or unobserved individual or regional level characteristics, with the exception of possible seasonality effects. The average time spent from parental leave termination (the 3rd birthday) is the same in the two groups, so its effect is the same, on average, in the treatment and the control group.

[15] We use the interval cutoff to run regressions for mothers whose child was born before 1st September (treatment group) and after 1st January (control group). In our preferred regression point cutoff is used.

Moreover, since the children in the two groups are of the same age – all observed in the period after they turn 3 - separation preferences should also be the same for the two groups. Our estimation will no longer depend on these two factors, so we can isolate the childcare effect.

In this setup, the treatment and the control groups differ in birth date[16] and observation date, which may introduce seasonal bias of various forms. First, Bound and Jaeger (1996) claim that quarter of birth may be associated with various factors. They quote Kestenbaum (1987), who find that parents with higher incomes tend to have spring babies. Second, child development may differ by season of birth, which may influence the mother's willingness to separate from the child. Currie and Schwandt (2013) show that even after controlling for maternal characteristics, health status and weight at birth depends on the season of birth. Third, labor demand varies seasonally as well, and labor demand, in turn, determines the actual and expected probability of employment, which strongly affects labor supply.

In order to address this problem, we expand both treatment and control groups with reasonably close labor market substitutes, mothers of children aged 4-5 years (separated into groups based on the same cutoff date), and include difference in differences (DID) in the regression. These comparison groups should be affected by the same seasonal effects, but not by the treatment effect, allowing us to separate out seasonal factors. Any difference between them should be the result of the factors mentioned above. We denote our original sample of mothers of 3-year-old children $M^3 = 1$, and the comparison sample of mothers of 4-5-year-old children $M^{4-5} = 1$. As

[16] We have quite a small sample, thus we cannot afford narrowing the time window, thus the date of birth is quite different for the treatment and the control group.

noted above, M^{4-5} subsample is divided into two groups as in the case of the M^3 subsample, based on the month of childbirth and the date of the interview. We define the instrument for the M^{4-5} subsample as follows:

$$T_i = \begin{cases} 1 & if \quad b_i < b_0 \quad and \quad 4 \le a_i < 6 \\ 0 & if \quad b_0 + l \le b_i \quad and \quad 4 \le a_i < 6 \end{cases}$$

We construct a variable indicating the original and the comparison sample:

$$m_i = \begin{cases} 1 & if\ M^3 = 1 \\ 0 & if\ M^{4-5} = 1 \end{cases}$$

We then run the 2SLS regression on the extended sample, and include m_i and the interaction of m_i and T_i. The first stage is:

$$C_{yri} = \beta_1 T_i m_i + \alpha_y + \gamma_r + X_i' \pi_{11} + S_{yr}' \pi_{12} + \pi_{13} T_i + \pi_{14} m_i + \xi_{1yri} \qquad (4)$$

Where

$$C_{yri} = p_{yr}^n (1 - T_i) + p_{yr}^k T_i$$

The second stage is:

$$L_i = \beta_2 \widehat{C_{yri}} m_i + \alpha_y + \gamma_r + X_i' \pi_{21} + S_{yr}' \pi_{22} + \pi_{23} \widehat{C_{yri}} + \pi_{24} m_i + \xi_{2yri} \quad (5)$$

And the reduced form equation is:

$$L_i = \beta_R T_i m_i + \alpha_y + \gamma_r + X_i' \pi_{R1} + S_{yr}' \pi_{R2} + \pi_{R3} T_i + \pi_{R4} m_i + \xi_{Ryri} \qquad (6)$$

The variables are the same as before. In this setup, the parameter β_2 shows the effect of C_{yri} (childcare coverage) on L_i (labor supply), net of any seasonal effects, while β_R is the reduced form effect of T_i on L_i, free of the seasonal effects.

In our main results, the treatment group includes mothers whose child turned 3 in the 5 months prior to the cutoff, and the control group includes mothers whose child turned 3 in the 5 months after the cutoff. Mothers in both groups are observed on average 4 months after their child turns 3. As a robustness check, we narrow the time window, and carry out the estimation with 3 and 4 month groups as well. As the actual enrollment practices are unclear, we carry out the analysis for three different cutoff specifications (September 1, January 1, September1-January 1), corresponding to comparisons of Group 1, 2, and 3. A comparison of the results can tell us which cutoff is actually valid, i.e. whether those in Group 2 are allowed to enroll their children in kindergarten on (or near) their 3rd birthday, or only in the next September.

IV. DATA AND CHARACTERISTICS

The primary source of the data used to estimate the effect of childcare availability on the labor market activity of mothers is the Hungarian Labor Force Survey (H-LFS). This is a rotating panel dataset, which consists of individual-level data of all members of the households. Approximately 17% of the households are rotated in each quarter; the maximum number of periods for observation is 6, which equals 1.5 years. The sample is representative of Hungary; sample weights based on the data of the Hungarian Central Statistical Office (CSO) are used. One wave consists of about 70-80 thousand observations, however, only a fraction of these are used.

Our restricted sample includes mothers with or without a partner. We exclude fathers from the analysis, because in Hungary it is quite rare that fathers stay at home with the child and mothers go back to work. As it can be seen from the data, between 1996 and 2011, a mere 1.9% of those receiving parental leave payments were males. We define variables indicating treatment and control groups (T) and the corresponding comparison group of mothers of 4-5 year olds, and limit our estimation sample to these groups, observed in the periods indicated in the sampling design.

In the H-LFS dataset, detailed demographic and labor market data are included about each individual, and supplementary questionnaires give more details on certain topics for each year. We use information on the individual's labor market activity as our labor supply measure and include as controls individual and family characteristics (shown in Appendix Table A1.). Individuals are classified as active if they have completed at least one hour of paid work in the previous week, or if they are available for work and actively seeking for a job (ILO definition). We use this dummy variable as our dependent variable in the estimation.[17] This means that we are not considering changes in hours of employment, however, as noted earlier, part-time work is rare in Hungary, so choices are made mostly between working and non-working.

The individual level LFS data is linked with T-STAR township level regional data on childcare availability, as well as other regional characteristics, via township codes. The focus of our analysis, childcare availability, is constructed based on the number of nursery and kindergarten spots in the township, and the number of children of the given age groups (0-3 for nursery school, 3-6 for kindergarten) in the population. We aggregate the coverage of formal childcare institutions in order to take agglomeration effects and commuting into account by merging townships based on previous data (Kertesi et al. [2012]), defining the regions used in our estimation. The region level childcare coverage measure is available from 1997 to 2011, and can

[17] We also use an employment dummy as the dependent variable as a further check. The results show similar overall trends as those presented here.

be linked to the LFS data for each of those years. We include regional descriptive variables of the population and economy, as well as year dummies in the regressions.

Summary statistics of the variables used in the estimations are given in Appendix Table A1. The table gives the means of the individual, family, and regional controls used in the estimation (as well as occupational data, for the sake of comparison). The third column in each panel (for mothers of 3 year olds, and mothers of 4-5 year olds) gives the difference in the treatment and control group's means, divided by the standard deviation of the control group. This measures the difference between groups in terms of number of standard deviations. The most significant difference between the groups can be seen in the mean participation rates: it is 59.6% for the treatment, and 51.5% for the control group (a difference of 0.16 standard deviations). The difference in activity rates shrinks to 0.17 percentage points for 4-5 year olds.

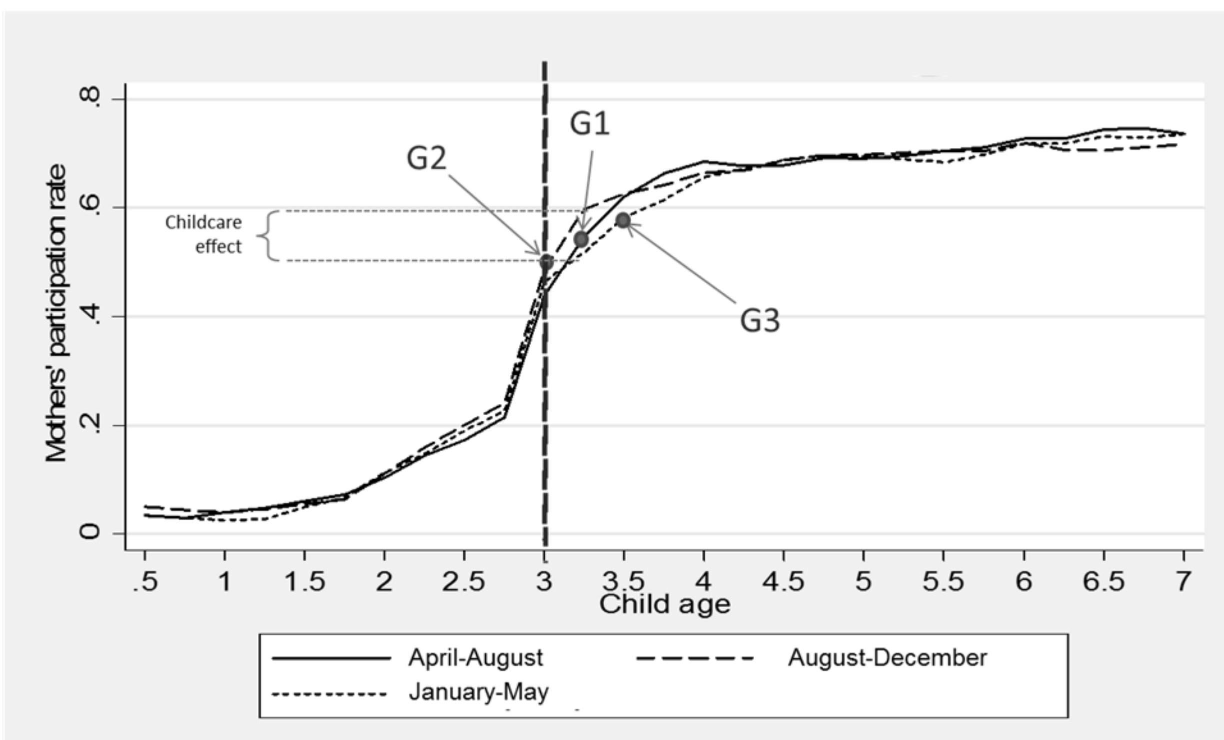
Most individual and regional characteristics that are used as explanatory variables are very similar in the treatment and control groups of both age groups. This is important for the validity of the RD setup, in that the birth date of the children can be considered random, and the compared groups are similar on average apart from treatment status. The biggest differences among mothers of 3 year olds can be seen in the type of settlement and nursery school coverage. The treatment group is 3.9 percentage points more likely to live in a city than a town, equivalent to 0.1 standard deviations. Nursery coverage is correspondingly 1 percentage point higher for the treatment group (0.1 SDs). Although these differences are not huge, they do suggest some seasonality may exist in the characteristics of the two groups, and the DID seasonality correction is important. Differencing can capture seasonal differences, as the comparison groups of mothers with 4-5 year old children show a similar pattern in terms of type of living place.

V. RESULTS

V.1. MOTHERS' MEAN PARTICIPATION RATES OVER CHILDREN'S AGE

We begin our discussion of the estimation results by presenting a graphical analysis of the effect of treatment. Figure 5 shows the activity rates of mothers of Group 1, 2 and 3 over a longer time span, for child ages 0.5-7. Mothers are sorted into three groups based on the birth month of their child as described earlier, and their average participation rate is calculated at each age (in quarters) of their child. The points in time when they are expected to become eligible to enroll into kindergarten are indicated by the circles marked G1, G2, and G3.

FIGURE 5: PARTICIPATION RATES OF MOTHERS BY BIRTH DATE OF THEIR CHILD



Notes: H-LFS, 1997-2010. Each line represents averages for mothers with children born in the months indicated. Points G1, G2, and G3 mark expected time when children of the respective group become eligible to enroll in kindergarten.

All three groups show a gradual increase in the labor market participation, the rate of which increases after age 2 of the child, especially sharply right before age 3, and levels off after age 4 around 0.75. The gradual increase over time is in line with gradually changing separation preferences. The sharp increase after age 3 may reflect the effect of the end of parental leave, as well as a more sudden change in preferences regarding separation. The groups do not differ in these: parental leave and preferences are only dependent on the child's age and independent of group membership (birth date). The three lines therefore move together in general, which is in line with only age-dependent changes.

There is, however, a difference between the groups' participation rates after age 3 of the children. Any differences between the groups are calendar date-related. In our case, these are due to the difference in the kindergarten eligibility cutoff: mothers in the three groups wait different lengths of time to gain access to kindergarten, which affects their participation rates. The figure shows that the participation rate of Group 2, with children born August-December[18] increases first, suggesting that they are in an advantageous position in terms of being able to enroll their children at the earliest age on average. The pre-September Group 1 is next, while the post-January Group 3 lags behind the other two.

These results suggest that the effective cutoff date is January 1st rather than September 1st. This is in line with continuous enrollment of children of Group 2 mothers, born in the months immediately after September. It also implies that those in Group 3, with children born after

[18] If Group 2 is defined as September-January or as a four month window of September-December, the graph shows very similar trends.

January 1st, have to wait until next September. The figure suggests that childcare availability has a positive effect on mothers' labor supply: the sooner after their 3rd birthday the children are eligible for kindergarten (able to use subsidized childcare), the sooner their mothers return to the labor market. The order in which the three groups show an increase in the mothers' participation rates corresponds to their average waiting time for kindergarten eligibility. As marked in the figure, the vertical distance between the lines of Group 2 and Group 3 reflect the childcare effect, estimated for the January cutoff, one quarter after the children turn 3. Of course, seasonal effects may play a role here as well, and are not yet controlled for. We turn our attention to the RD estimation results to see more precisely what the magnitude of the childcare effect is.

V.2. RD REGRESSION RESULTS

The reduced form regression results based on the modified RD are shown in Table 1 for all three cutoffs. The table contains only the coefficient estimates of interest: the dummy variable indicating treatment group membership (*T=1* if treated), seasonality comparison group membership (m=1 if child is 3-3.6, m=0 if child is 4-5), and their interaction.[19] The first three columns of the results give baseline estimates without the seasonality correction, for specifications with no controls (1), individual controls (2), and individual and regional controls (3). The last three columns show the same three specifications with the seasonality correction, when the comparison groups of mothers are included. Coefficient estimates measuring the effect of subsidized childcare availability are given in bold: for the baseline regressions, these are the

[19] Full estimation results can be seen in Appendix Table A2.

coefficients of *T*, while for the seasonality-corrected regressions, they are the coefficients of the interaction term *T*m*.

Of the three cutoffs, the results for January 1^{st} show the most significant effect, ranging from 0.078 to 0.085 in the baseline specification. The September 1^{st} cutoff, which would show the most significant impact if only children born prior to September 1^{st} can enroll in kindergarten this school year, gives insignificant estimates. The largest estimated impact of the January 1^{st} cutoff confirms what Figure 5 implied: that in reality, children born after September 1^{st}, but before January (Group 2) are able to enroll this school year, there are enough kindergarten places left to accept them. Those born after January (Group 3), however, have to wait until the next September, as no places are available when they turn 3. The interval cutoff estimates for September 1^{st}-January 1^{st}, are between the two point cutoffs, positive, but smaller in magnitude and less significant estimates than the January 1^{st} cutoff results, between 0.042-0.051.

The seasonality-corrected results show a similar pattern, with slightly lower estimates: the results for the January 1^{st} cutoff are all significant and around 0.06. This suggests that there is some seasonality bias in the uncorrected estimates, so we consider the DID specification (the final column) to be our best estimate. The coefficients of *m*, which signals membership in the group with children aged 3-3.6, are significant and negative, reflecting the difference in labor market activity on average compared to mothers with older, 4-5 year-old children. The coefficient estimates of *T*, which capture seasonality that is common to all mothers, are not significant. It is also worth noting that the stability of our main coefficient estimate, that of the interaction variable *T*m* (and T in the baseline regressions), over the different specifications of controls provides a robustness check. Since the treatment and the control group should not differ significantly in terms of individual and regional characteristics on average, adding additional controls should not affect the estimates significantly, which is confirmed by a comparison of the various columns.

TABLE 1: MODIFIED RD, REDUCED FORM REGRESSION RESULTS, VARIOUS CUTOFFS

Reduced form regressions						
Specification	1	2	3	1	2	3
Cutoff: January 1						
	Baseline			Seasonality-corrected		
T*m				**0.061***	**0.060***	**0.060***
				(0.024)	**(0.027)**	**(0.026)**
T	**0.078****	**0.085****	**0.082****	0.007	0.014	0.012
	(0.02)	**(0.02)**	**(0.02)**	(0.018)	(0.016)	(0.016)
m				-0.169**	-0.156**	-0.156**
				(0.024)	(0.023)	(0.023)
N	3244	3244	3244	8982	8982	8982
Cutoff: September 1						
	Baseline			Seasonality-corrected		
T*m				**-0.023**	**-0.029**	**-0.027**
				(0.031)	**(0.032)**	**(0.032)**
T	**-0.019**	**-0.021**	**-0.021**	0.001	0.006	0.004
	(0.03)	**(0.02)**	**(0.02)**	(0.022)	(0.022)	(0.021)
m				-0.102**	-0.081**	-0.082**
				(0.016)	(0.017)	(0.017)
N	3229	3229	3229	8871	8871	8871
Cutoff: September 1-December 31						
	Baseline			Seasonality-corrected		
T*m				**0.045***	**0.041**	**0.041**
				(0.022)	**(0.021)**	**(0.021)**
T	**0.042**	**0.051***	**0.050***	-0.005	0.006	0.005
	(0.022)	**(0.022)**	**(0.022)**	(0.016)	(0.016)	(0.016)
m				-0.165**	-0.150**	-0.150**
				(0.024)	(0.024)	(0.024)
N	3181	3181	3181	8830	8830	8830
Year dummies	x	x	x	x	x	x
Individual controls		x	x		x	x
Regional controls			x			x

*Notes: Estimation based on H-LFS and T-STAR datasets, years 1997-2010. The dependent variable is the participation dummy. The table gives coefficient estimates of the dummy variable indicating treatment group membership (T=1 if treated), seasonality comparison group membership (m=0 if child is 4-5), and their interaction. Year dummies are included in all regressions. Standard errors are given in parentheses. Stars indicate significance as: * p<0.05; ** p<0.01.*

The preferred January cutoff reduced form estimate can be interpreted as suggesting that if Group 3 had the same access to childcare as Group 2 – i.e. if nursery coverage would be as high as kindergarten coverage – their participation rate would increase by 6 percentage points. To gain a better understanding of the magnitude of the impact of childcare availability, we turn our attention to the 2SLS results, shown in Table 2, in the same format, for the January cutoff.[20] The baseline results show the coefficient estimate of childcare coverage ranging from 0.13-0.14. The seasonality-corrected estimates are again lower, and very similar in all three specifications. In the preferred specification, with the full set of controls, it is 0.095 and significant at the 5 percent level. This result suggests that if childcare coverage increased from 0 to 100%, i.e. if subsidized childcare became available to mothers of children around age 3 who did not previously have access, their activity rate would increase by 9.5 percentage points. As the baseline participation rate of the control group is 51.5% (see Table A1 in the Appendix), this equals 18 percent. In terms of Hungary, this means that if the average nursery school coverage (13%) for mothers whose children are around 3 years old but not eligible for kindergarten increased to the level of kindergarten coverage (78%), their activity rate would increase by roughly 12%.

[20] Full results are given in Appendix Table A2. First stage results are given in Appendix Table A5. We also estimate the model with 3 and 4 month windows (Table A3). They show a very similar pattern (and estimate magnitudes), but with lower significance due to the smaller sample sizes.

TABLE 2: MODIFIED RD, 2SLS REGRESSION RESULTS, JANUARY CUTOFF

2SLS						
Specification	1	2	3	1	2	3
Cutoff: January 1						
	Baseline			Seasonality-corrected		
C	**0.129****	**0.141****	**0.135****	0.014	0.025	0.021
	(0.032)	**(0.034)**	**(0.034)**	(0.027)	(0.024)	(0.024)
C*m				**0.096****	**0.095***	**0.095***
				(0.037)	**(0.041)**	**(0.041)**
m				-0.179**	-0.166**	-0.166**
				(0.027)	(0.027)	(0.027)
N	3018	3018	3018	8811	8811	8811
Year dummies	x	x	x	x	x	x
Individual controls		x	x		x	x
Regional controls			x			x

*Notes: Estimation based on H-LFS and T-STAR datasets, years 1997-2010. The dependent variable is the participation dummy. The table gives coefficient estimates of regional childcare coverage relevant to the given group (kindergarten if treated, nursery if not), the dummy indicating seasonality comparison group membership (m=0 if child is 4-5), and their interaction. Year dummies are included in all regressions. Standard errors are given in parentheses. Stars indicate significance as: * p<0.05; ** p<0.01.*

As a check that the results are robust and meaningful, we carry out the reduced form estimation presented in this section for each age group from 1 to 7 years, using the January 1st cutoff. Table 3 summarizes the results.[21] They indicate that there is a significant effect at age 3 of the child, but there is no effect at other ages. These findings are in line with what we observe in Figure 5: there is no significant difference between the groups – i.e. no calendar date-related effects – apart from at age 3, due to kindergarten enrollment. The figure and Table 3 both suggest a small difference after age 1, which may correspond to differences in probabilities of enrollment into nursery school, but the estimated effect is insignificant.

[21] Full results are given in Appendix Table A4.

TABLE 3: REDUCED FORM RESULTS FOR EACH CHILD AGE IN YEARS, JANUARY CUTOFF

	Child age						
	Year 1	Year 2	Year 3	Year 4	Year 5	Year 6	Year 7
T	0.021	0.009	0.082**	-0.010	0.009	-0.009	0.008
	(0.012)	(0.015)	(0.022)	(0.028)	(0.021)	(0.024)	(0.020)
N	3796	3688	3244	2883	2853	2666	2603

*Notes: The table shows the result of a reduced-form estimate with January 1st cutoff. Estimation based on H-LFS and T-STAR datasets, years 1997-2010, sample of mothers. The dependent variable is the activity dummy. The table gives coefficient estimates of regional childcare coverage relevant to the given group (kindergarten if treated, nursery if not), the dummy indicating seasonality comparison group membership (m=0 if child is 4-5), and their interaction. Year dummies are included in all regressions. Standard errors are given in parentheses. Stars indicate significance as: * p<0.05; ** p<0.01.*

Contrary to the previous RD estimate for US mothers with 4-year-olds (Fitzpatrick 2010), our results suggest that subsidized childcare availability has a significant impact on mothers' labor supply. The estimates are in line with some earlier findings from the U.S. (Cascio 2009, Gelbach 2002). These comparisons bring up an important issue: the external validity of all estimates of childcare impact is limited by certain factors. The methodology used in our paper is inherently suited to measure local effects around the cutoff point (age 3), thus the extendibility to other child ages is limited. Furthermore, whether childcare availability has an impact on mothers' labor supply, and how large the impact is, depends on whether at the given age of the child, in the given country, lack of childcare is a binding constraint on mothers or not.[22]

[22] For example, the most likely reason why we find a significant impact while the other study using RD (Fitzpatrick 2010) does not may be explained by such differences. We estimate the effect at age 3 of children in Hungary, when the participation rate of mothers is still low, and parental leave is just ending, so lack of childcare availability constrains mothers who are still inactive at that point. The study by Fitzpatrick focuses on mothers of older children in the US, by which time most mothers who plan to work at all have already returned to the labor market, making other childcare arrangements, and parental leave ended a long time ago.

These issues regarding the external validity of estimates suggest that a comparison of the magnitude to previous international estimates would not be very meaningful. In order to interpret the importance of the modified RD methodology used, we therefore compare our main results to other estimates using the same database but various methods. Table 4 summarizes estimates of the impact of childcare availability on mothers' labor supply based on a linear probability model (LPM) and a model that includes regional fixed effects (FE). These estimates are obtained using methods similar to those in previous international studies based on structural models (see notes below). The table also shows estimates based on standard RD, and our modified RD method. The difference between these is (a) the sample, since in the standard RD setup treatment and control groups are observed at the same date (the quarter after the cutoff date), while in the modified RD setup both groups are observed in the quarter after their child turns 3, and (b) the seasonality correction used in the modified RD setup.

TABLE 4: SUMMARY OF ESTIMATED CHILDCARE EFFECT USING VARIOUS METHODS

	(1) Linear Probability Model	(2) Regional Fixed Effects Model	(3) Standard RD	(4) Extended RD
Coefficient estimate	**0.322****	**0.324****	**0.3181****	**0.095****
Years included	2001-2011	2001-2011	1997-2011	1997-2011
Number of observations	38221	38221	3184	8811
Adj. R2	0.415	0.443	0.142	0.136
Year dummies	x	x	x	x
Individual controls	x	x	x	x
Maternity leave	x	x		
Regional controls	x	x	x	x

*Notes: Estimation based on H-LFS and T-STAR datasets, sample of mothers. Standard errors are given in parentheses. Stars indicate significance as: * p<0.05; ** p<0.01. Columns 1 and 2: the estimation is carried out on pooled individual level data of mothers of 2.5-3.5 year olds, for the years 1997-2010. The dependent variable is the activity dummy. The explanatory variable of interest is the local area coverage rate relevant to the child of the given age. Controls included are the same as those in the RD specifications. Additionally, the fixed effects model accounts for regional fixed effects.*

The structural estimates in columns 1 and 2 are much higher than our preferred modified RD estimate, and strongly significant: around 0.32 in the LPM and in the FE specification. These identify the childcare effect from regional and time variation in the coverage rates, and are based on large samples due to fewer sample restrictions. The individual controls also include a dummy variable indicating whether the mother is on maternity leave. Despite the relatively detailed controls compared to previous studies using OLS and FE, these estimates are prone to endogeneity bias from unobserved regional characteristics. This highlights a key strength of the RD design: that it uses random selection into treatment and control groups for identification, thereby entirely eliminating endogeneity bias.

The standard RD estimate is also much higher than the modified RD estimate, at 0.31. Since it cannot separate the effect of childcare from other factors that are age-dependent and change simultaneously, it captures the sum the childcare effect, the effect of the end of parental leave, and changes in preferences regarding separation from the child. Based on this, the institutional characteristics, and evidence on preferences, separating out other age-related effects is important. As we noted earlier, due to the timing of childcare and parental leave policies in other EU-wide, this bias will also affect standard RD estimates in other countries. The strength of the modified RD method is that it makes it possible to separate these effects.

An important policy implication of our study stands out when we take the overall pattern into account. Figure 5 shows that there is a sharp increase in mothers' participation rates around age 3 of their child of about 31 percentage points, which is confirmed by the standard RD estimate. Of this increase, increased childcare availability explains about 9.5 percentage points, about one third. We cannot determine the exact role of the other factors, however, the end of parental leave is unlikely to explain the rest, as the monetary amount received in the last year before the child turns 3 is comparable to the childcare subsidy. Therefore, preferences regarding separation probably play a key role, which may themselves be affected by the institutional framework. The final message is therefore that a combined policy approach is necessary for achieving an increase in the labor supply of mothers, which takes the signals given into consideration as well.

VI. CONCLUSION

In this study, we develop a modified version of RD, which combines a new sampling design and DID to measure the effect of childcare availability on mothers' labor supply in a case when other, child age related factors change discontinuously at the cutoff. The method allows us to separate calendar-related and age-related effects. Our results suggest that eligibility for subsidized childcare increases the participation rate of mothers with children around age 3 by about 9.5 percentage points (18 percent). The estimate is lower than those seen in some previous studies using structural model-based OLS and panel methods, but gives strong new evidence that childcare availability has a significant impact on mothers' labor supply. The results highlight the importance of eliminating endogeneity biases due to unobserved individual and regional characteristics, as well as separating the childcare effect from other factors that are age-related.

We measure the impact at a point in time when the participation rate of mothers is still low, many of them had not yet returned to the labor market even if they plan to do so, and they are still likely to be constrained by lack of childcare availability. The external validity of the estimates is limited by cross-country differences and differences related to child age, making it difficult to compare their magnitude to previous international results or extend them to younger children. However, our study provides strong new evidence that a significant impact exists if childcare is not available at the child age when mothers are considering returning to the labor market.

Our estimates of the impact of childcare availability, and the overall changes seen around age 3 of children in Hungary, point to an interesting puzzle. From the standard RD results and the evolution of participation rates, we see that there is a 31 percentage point increase in the participation rate of mothers when their child turns 3. This is partly due to the increase in subsidized childcare availability, which explains almost a third, while the rest is due to the end of parental leave, changes in preferences regarding separation, and other unobserved factors. The timing suggests that changes in preferences are related to the institutional framework, which can have an influence through several possible channels.[23] Additionally, the effectiveness of childcare expansion in increasing mothers' labor supply may be limited by other factors, such as the lack of availability of part time work, and the inflexibility of childcare hours.[24] To sum up, policymakers need to take both possible complementarities with other factors, as well as the signaling effect of the institutional framework into consideration while designing effective policies for increasing maternal labor supply. Further research is needed to determine the role of these factors and what policy steps can influence them.

[23] The length of parental leave and starting age of kindergarten may be perceived as a signal by mothers, suggesting that age 3 is the appropriate time for separating from the child and returning to work. It is possible that, lacking clear views on the matter, mothers simply use the age suggested by the institutional framework as a rule of thumb. Employers may assume that after age 3, childcare duties of mothers are less of a constraint and be more willing to employ them, which, in turn, may influence mothers' expectations and participation.

[24] In Hungary, state-owned institutions provide childcare from 6 a.m. to 4 p.m. The ratio of part time jobs is low, about 4.4% of overall employment (H-LFS). Del Boca (2002) also points out that policies need to combine the aims of more flexible work schedule choices and greater child care availability.

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APPENDIX

TABLE A1: SUMMARY STATISTICS OF THE ESTIMATION SAMPLE, BY GROUP, JANUARY CUTOFF

Summary statistics	m=1: child aged 3			m=0: child age 4-5		
	Treatment	Control	Diff/SD	Treatment	Control	Diff/SD
Mother						
Activity rate (1997-2011) (%)	59.60%	51.50%	0.161	68.32%	68.15%	0.004
Number of children	1.3	1.3	-0.022	1.1	1.1	-0.04
Age of youngest child	3.3	3.3	-0.03	4.8	4.8	-0.043
Age (years)	31.1	31.1	0.001	32.4	32.5	-0.004
Education (%)						
Primary	23.60%	22.10%	0.037	23.20%	23.10%	0
Vocational school	26.90%	27.20%	-0.006	28.00%	25.30%	0.063
High school	31.90%	33.30%	-0.03	34.40%	35.00%	-0.013
University	17.60%	17.50%	0.004	14.50%	16.60%	-0.057
Occupation (%)						
Leader, executive	19.90%	20.60%	-0.016	20.20%	18.20%	0.053
Higher educ. requiring	1.80%	1.90%	-0.006	2.10%	2.60%	-0.031
GED requiring	11.40%	12.10%	-0.022	10.00%	12.00%	-0.061
Clerical, customer service	15.40%	14.70%	0.02	15.20%	14.40%	0.022
Service, commerce	9.50%	9.30%	0.005	9.70%	10.70%	-0.033
Agricultural	17.00%	20.10%	-0.077	18.50%	18.20%	0.008
Construction, industry	1.20%	0.80%	0.05	2.00%	1.70%	0.019
Operation, assembly	8.80%	7.30%	0.056	7.60%	6.90%	0.028
Unskilled	8.20%	8.10%	0.004	7.80%	7.40%	0.012
Armed forces	6.70%	5.00%	0.077	7.00%	7.80%	-0.033
Husband or partner						
Age (years)	30	29.8	0.017	30.8	30.8	-0.002
Employment status (%)						
No partner	13.30%	13.20%	0.004	14.10%	12.70%	0.042
Partner without job	13.30%	13.20%	0.004	14.10%	12.70%	0.042
Partner with job	76.00%	75.60%	0.007	73.20%	75.00%	-0.042
Education (%)						
Primary	16.60%	16.00%	0.017	15.80%	16.80%	-0.025
Vocational school	38.20%	38.20%	0	38.50%	37.90%	0.012
High school	20.70%	21.40%	-0.017	21.80%	22.30%	-0.012
University	13.40%	13.00%	0.012	11.00%	10.50%	0.014

Occupation (%)						
Leader, exec.	17.80%	17.80%	0.002	20.60%	17.70%	0.076
Higher educ. Requiring	6.30%	5.90%	0.015	5.60%	5.60%	-0.001
GED requiring	7.60%	7.70%	-0.006	5.80%	5.60%	0.007
Clerical, customer serv.	7.20%	7.10%	0.003	6.60%	7.10%	-0.019
Service, commerce	0.30%	0.70%	-0.052	0.60%	0.50%	0.021
Agricultural	11.00%	12.00%	-0.032	11.00%	10.40%	0.02
Construction, industry	3.50%	3.80%	-0.017	4.40%	4.00%	0.021
Operation, assembly	25.00%	24.70%	0.005	25.50%	27.20%	-0.038
Unskilled	14.90%	13.70%	0.032	14.30%	14.30%	0
Armed forces	6.60%	6.40%	0.004	5.50%	7.50%	-0.075
Environment						
Type of settlement (%)						
Village	27.50%	28.60%	-0.025	28.80%	26.80%	0.045
Town	35.70%	40.70%	-0.103	39.50%	42.60%	-0.063
City	21.00%	17.10%	0.104	19.10%	17.60%	0.039
Region (%)						
Central Hungary	28.10%	28.30%	-0.005	26.40%	25.50%	0.022
Central Transdanubia	10.60%	10.70%	-0.003	10.90%	11.10%	-0.008
Western Transdanubia	9.30%	9.40%	-0.003	9.30%	9.60%	-0.007
Southern Transdanubia	9.70%	9.40%	0.008	10.20%	10.60%	-0.013
Northern Hungary	14.10%	11.20%	0.092	12.90%	12.80%	0.003
Northern Plains	15.00%	16.80%	-0.049	16.80%	16.60%	0.006
Southern Plains	13.20%	14.20%	-0.027	13.50%	13.90%	-0.012
Unemployment rate (%)	4.40%	4.40%	0.006	4.60%	4.60%	-0.017
Nursery coverage (%)	11.20%	10.20%	0.106	10.50%	10.00%	0.053
Kindergarten coverage (%)	105.10%	105.00%	0.005	103.50%	102.80%	0.022
Average population	310147	260321	0.085	248879	252224	-0.006
Number of obs.	1732	1577		2975	2868	

TABLE A2: MODIFIED RD, FULL REDUCED FORM REGRESSION RESULTS, ALL THREE CUTOFFS

	Cutoff: January 1						Cutoff: September 1						Cutoff: September 1 - December 31					
Dependent variable: L	Baseline			Seasonality-corrected			Baseline			Seasonality-corrected			Baseline			Seasonality-corrected		
Specification	1	2	3	1	2	3	1	2	3	1	2	3	1	2	3	1	2	3
T*m				**0.061***	**0.060***	**0.060***				**-0.023**	**-0.029**	**-0.027**				**0.045***	**0.041**	**0.041**
				(0.024)	**(0.027)**	**(0.026)**				**(0.031)**	**(0.032)**	**(0.032)**				**(0.022)**	**(0.021)**	**(0.021)**
T	**0.078****	**0.085****	**0.082****	0.007	0.014	0.012	**-0.019**	**-0.021**	**-0.021**	0.001	0.006	0.004	**0.042**	**0.051***	**0.050***	-0.005	0.006	0.005
	(0.022)	**(0.023)**	**(0.022)**	(0.018)	(0.016)	(0.016)	**(0.03)**	**(0.02)**	**(0.02)**	(0.022)	(0.022)	(0.021)	**(0.022)**	**(0.022)**	**(0.022)**	(0.016)	(0.016)	(0.016)
m				-0.169**	-0.156**	-0.156**				-0.102**	-0.081**	-0.082**				-0.165**	-0.150**	-0.150**
				(0.024)	(0.023)	(0.023)				(0.016)	(0.017)	(0.017)				(0.024)	(0.024)	(0.024)
# of children		-0.117**	-0.117**		-0.123**	-0.122**		-0.118**	-0.116**		-0.125**	-0.124**		-0.150**	-0.148**		-0.134**	-0.132**
		(0.021)	(0.022)		(0.015)	(0.015)		(0.02)	(0.02)		(0.014)	(0.014)		(0.023)	(0.023)		(0.014)	(0.014)
Partner w/o job		0.003	0.007		-0.004	0.000		-0.003	-0.006		0.009	0.012		0.031	0.033		-0.034	-0.031
		(0.063)	(0.062)		(0.043)	(0.043)		(0.09)	(0.09)		(0.060)	(0.060)		(0.106)	(0.107)		(0.043)	(0.042)
Partner w/ job		0.032	0.032		0.038	0.039		0.035	0.028		0.056	0.056		0.079	0.080		0.011	0.011
		(0.062)	(0.062)		(0.043)	(0.043)		(0.08)	(0.08)		(0.060)	(0.060)		(0.091)	(0.093)		(0.041)	(0.041)
Vocational school		0.191**	0.186**		0.177**	0.175**		0.145**	0.141**		0.187**	0.185**		0.141**	0.139**		0.138**	0.135**
		(0.035)	(0.035)		(0.020)	(0.020)		(0.03)	(0.04)		(0.019)	(0.019)		(0.032)	(0.032)		(0.020)	(0.020)
High school		0.250**	0.245**		0.289**	0.287**		0.219**	0.214**		0.276**	0.273**		0.213**	0.209**		0.239**	0.236**
		(0.035)	(0.035)		(0.019)	(0.019)		(0.05)	(0.05)		(0.027)	(0.026)		(0.038)	(0.038)		(0.024)	(0.023)
University		0.374**	0.367**		0.415**	0.412**		0.400**	0.393**		0.404**	0.399**		0.386**	0.380**		0.383**	0.378**
		(0.051)	(0.050)		(0.037)	(0.037)		(0.05)	(0.05)		(0.032)	(0.032)		(0.043)	(0.043)		(0.033)	(0.032)
Age		0.018	0.020		-0.004	-0.004		0.027	0.027		0.003	0.003		0.027	0.028		-0.009	-0.009
		(0.021)	(0.021)		(0.012)	(0.012)		(0.02)	(0.02)		(0.016)	(0.016)		(0.024)	(0.024)		(0.014)	(0.013)
Age squared		-0.000	-0.000		0.000	0.000		-0.000	-0.000		-0.000	-0.000		-0.000	-0.000		0.000	0.000
		(0.000)	(0.000)		(0.000)	(0.000)		(0.00)	(0.00)		(0.000)	(0.000)		(0.000)	(0.000)		(0.000)	(0.000)
Partner: University		0.089*	0.083		0.058*	0.055*		0.011	0.010		0.022	0.019		-0.006	-0.009		0.029	0.024
		(0.044)	(0.044)		(0.025)	(0.024)		(0.05)	(0.05)		(0.026)	(0.026)		(0.044)	(0.044)		(0.026)	(0.025)
Partner: High sc.		0.074	0.071		0.087*	0.085*		0.073	0.072		0.073*	0.070*		0.057	0.053		0.084**	0.080**
		(0.060)	(0.060)		(0.037)	(0.037)		(0.07)	(0.07)		(0.032)	(0.032)		(0.054)	(0.054)		(0.025)	(0.025)
Partner: Vocationa..		0.063	0.060		0.075**	0.073**		0.071	0.070		0.045*	0.043*		0.060	0.058		0.066**	0.065**
		(0.036)	(0.036)		(0.023)	(0.023)		(0.04)	(0.04)		(0.020)	(0.020)		(0.034)	(0.034)		(0.019)	(0.019)
Partner's age		-0.004*	-0.004*		-0.005**	-0.005**		-0.005*	-0.004		-0.004**	-0.004**		-0.005*	-0.005*		-0.004**	-0.004**
		(0.002)	(0.002)		(0.001)	(0.001)		(0.00)	(0.00)		(0.001)	(0.001)		(0.002)	(0.003)		(0.001)	(0.001)
Unemployment level			-2.006**			-1.218**			-1.762*			-1.480**			-1.045			-1.195*
			(0.765)			(0.470)			(0.73)			(0.462)			(0.679)			(0.486)
Village			0.218**			0.100**			-0.016			0.170**			0.341**			0.054
			(0.064)			(0.031)			(0.07)			(0.032)			(0.074)			(0.032)
City			0.243**			0.102**			0.004			0.170**			0.305**			0.028
			(0.058)			(0.020)			(0.05)			(0.016)			(0.060)			(0.020)
Large city			0.250**			0.118**			0.028			0.198**			0.327**			0.088*
			(0.072)			(0.045)			(0.06)			(0.035)			(0.088)			(0.040)
_cons	0.480**	0.168	0.074	0.627**	0.700**	0.690**	0.581**	0.144	0.257	0.716**	0.640*	0.585*	0.508**	0.156	-0.067	0.734**	0.939**	0.971**
	(0.099)	(0.384)	(0.374)	(0.052)	(0.217)	(0.218)	(0.07)	(0.29)	(0.28)	(0.030)	(0.256)	(0.259)	(0.067)	(0.368)	(0.386)	(0.036)	(0.213)	(0.217)
r2	0.245	0.316	0.318	0.179	0.272	0.273	0.247	0.32	0.322	0.178	0.262	0.264	0.243	0.314	0.315	0.193	0.27	0.272
aic	3789.217	3494.741	3491.096	10633.75	9579.45	9573.245	3742.727	3438.164	3435.935	10493.57	9558.548	9544.196	3743.841	3452.497	3453.368	10330.8	9466.56	9451.333
N	3244	3244	3244	8982	8982	8982	3229	3229	3229	8871	8871	8871	3181	3181	3181	8830	8830	8830
Year dummies	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x
Individual controls		x	x		x	x		x	x		x	x		x	x		x	x
Regional controls			x			x			x			x			x			x

TABLE A3: MODIFIED RD, FULL 2SLS REGRESSION RESULTS, JANUARY CUTOFF, WITH 3, 4, AND 5 MONTH BIRTH DATE WINDOWS

	Cutoff: January 1																	
	Window: 5 months						Window: 4 months						Window: 3 months					
Dependent variable: L	Baseline			Seasonality-corrected			Baseline			Seasonality-corrected			Baseline			Seasonality-corrected		
Specification	1	2	3	1	2	3	1	2	3	1	2	3	1	2	3	1	2	3
C	0.129**	0.141**	0.135**	0.014	0.025	0.021	0.119**	0.154**	0.149**	0.001	0.009	0.006	0.126*	0.148*	0.147*	-0.037	-0.017	-0.021
	(0.032)	(0.034)	(0.034)	(0.027)	(0.024)	(0.024)	(0.043)	(0.042)	(0.042)	(0.035)	(0.033)	(0.033)	(0.053)	(0.058)	(0.058)	(0.049)	(0.046)	(0.046)
C*m				0.096**	0.095*	0.095*				0.094	0.107*	0.106				0.117	0.108	0.110
				(0.037)	(0.041)	(0.041)				(0.049)	(0.054)	(0.054)				(0.070)	(0.078)	(0.078)
m				-0.179**	-0.166**	-0.166**				-0.180**	-0.174**	-0.174**				-0.182**	-0.173**	-0.174**
				(0.027)	(0.027)	(0.027)				(0.025)	(0.026)	(0.026)				(0.038)	(0.039)	(0.040)
# of children		-0.118**	-0.118**		-0.125**	-0.124**		-0.100**	-0.101**		-0.120**	-0.119**		-0.041	-0.042		-0.098**	-0.099**
		(0.019)	(0.019)		(0.014)	(0.014)		(0.024)	(0.023)		(0.017)	(0.017)		(0.038)	(0.038)		(0.027)	(0.026)
Partner w/o job		0.004	0.008		-0.006	-0.002		0.035	0.037		-0.010	-0.005		0.302*	0.299*		0.012	0.018
		(0.056)	(0.056)		(0.042)	(0.041)		(0.087)	(0.085)		(0.061)	(0.060)		(0.144)	(0.139)		(0.072)	(0.071)
Partner w/ job		0.033	0.034		0.037	0.039		0.034	0.032		0.048	0.051		0.275*	0.271*		0.059	0.061
		(0.056)	(0.055)		(0.042)	(0.041)		(0.081)	(0.080)		(0.057)	(0.056)		(0.137)	(0.134)		(0.073)	(0.073)
Vocational school		0.191**	0.187**		0.178**	0.176**		0.154**	0.149**		0.153**	0.151**		0.154*	0.151*		0.169**	0.167**
		(0.031)	(0.031)		(0.019)	(0.019)		(0.042)	(0.042)		(0.027)	(0.027)		(0.063)	(0.063)		(0.037)	(0.036)
High school		0.251**	0.246**		0.289**	0.286**		0.204**	0.198**		0.275**	0.273**		0.134	0.131		0.241**	0.239**
		(0.031)	(0.031)		(0.018)	(0.018)		(0.056)	(0.057)		(0.027)	(0.027)		(0.084)	(0.085)		(0.046)	(0.046)
University		0.374**	0.367**		0.414**	0.410**		0.321**	0.314**		0.385**	0.383**		0.187	0.187		0.314**	0.312**
		(0.045)	(0.045)		(0.035)	(0.035)		(0.084)	(0.084)		(0.051)	(0.050)		(0.120)	(0.122)		(0.067)	(0.067)
Age		0.018	0.020		-0.004	-0.004		0.030	0.031		0.005	0.005		0.047	0.048		0.014	0.015
		(0.018)	(0.019)		(0.012)	(0.012)		(0.023)	(0.023)		(0.015)	(0.015)		(0.043)	(0.042)		(0.021)	(0.021)
Age squared		-0.000	-0.000		0.000	0.000		-0.000	-0.000		-0.000	-0.000		-0.001	-0.001		-0.000	-0.000
		(0.000)	(0.000)		(0.000)	(0.000)		(0.000)	(0.000)		(0.000)	(0.000)		(0.001)	(0.001)		(0.000)	(0.000)
Partner: University		0.087*	0.081*		0.058*	0.054*		0.145*	0.142*		0.061	0.059		0.245*	0.247*		0.089	0.087
		(0.039)	(0.039)		(0.024)	(0.023)		(0.066)	(0.066)		(0.032)	(0.031)		(0.122)	(0.122)		(0.050)	(0.050)
Partner: High sc.		0.075	0.072		0.087*	0.085*		0.136	0.136		0.096	0.095		0.164	0.165		0.085	0.084
		(0.052)	(0.053)		(0.036)	(0.036)		(0.089)	(0.090)		(0.053)	(0.052)		(0.131)	(0.131)		(0.059)	(0.059)
Partner: Vocationa..		0.063*	0.061		0.074**	0.072**		0.116*	0.116*		0.084**	0.083**		0.152	0.152		0.110*	0.108*
		(0.032)	(0.032)		(0.022)	(0.022)		(0.046)	(0.046)		(0.030)	(0.030)		(0.084)	(0.084)		(0.047)	(0.046)
Partner's age		-0.004**	-0.004**		-0.005**	-0.005**		-0.006**	-0.006**		-0.005**	-0.005**		-0.010**	-0.010**		-0.004	-0.004
		(0.002)	(0.002)		(0.001)	(0.001)		(0.002)	(0.002)		(0.001)	(0.001)		(0.004)	(0.004)		(0.002)	(0.002)
Unemployment level			-2.027**			-1.251**			-1.832			-1.180*			-1.467			-1.492
			(0.681)			(0.449)			(1.132)			(0.579)			(1.914)			(0.859)
Village			0.219**			0.100**			-0.169			0.134**			0.018			-0.173
			(0.057)			(0.030)			(0.127)			(0.042)			(0.110)			(0.105)
City			0.241**			0.104**			-0.136			0.151**			0.055			-0.148
			(0.051)			(0.019)			(0.138)			(0.031)			(0.095)			(0.103)
Large city			0.250**			0.119**			-0.134			0.146**						-0.156
			(0.064)			(0.043)			(0.150)			(0.056)						(0.116)
r2	0.023	0.114	0.117	0.025	0.135	0.136	0.029	0.113	0.115	0.032	0.141	0.142	0.044	0.116	0.117	0.037	0.119	0.121
aic	3676.866	3404.436	3401.121	10483.5	9450.574	9443.807	2229.482	2084.113	2085.522	6606.072	5951.522	5950.73	870.473	833.298	838.14	2852.428	2638.208	2639.585
N	3018	3018	3018	8811	8811	8811	1871	1871	1871	5696	5696	5696	782	782	782	2660	2660	2660
Year dummies	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x
Individual controls		x	x		x	x		x	x		x	x		x	x		x	x
Regional controls			x			x			x			x			x			x

TABLE A4: REDUCED FORM RESULTS FOR EACH CHILD AGE, JANUARY CUTOFF

Dependent variable: L	Year1	Year2	Year3	Year4	Year5	Year6	Year7
	b/se	b/se	b/se	b/se	b/se	b/se	b/se
T	0.021	0.009	0.082**	-0.01	0.009	-0.009	0.008
	-0.012	-0.015	-0.022	-0.028	-0.021	-0.024	-0.02
# of children	-0.021**	-0.048**	-0.117**	-0.120**	-0.171**	-0.210*	
	-0.01	-0.01	-0.02	-0.03	-0.05	-0.1	
Partner w/o job	-0.02	-0.068	0.007	0.032	-0.044	-0.212**	-0.166
	-0.02	-0.06	-0.06	-0.12	-0.08	-0.08	-0.13
Partner w/ job	-0.03	-0.081	0.032	0.077	-0.018	-0.129	-0.107
	-0.02	-0.06	-0.06	-0.11	-0.07	-0.07	-0.13
Vocational school	-0.009	0.003	0.186**	0.133**	0.203**	0.187**	0.200**
	-0.01	-0.02	-0.03	-0.03	-0.04	-0.04	-0.04
High school	0.01	0.075*	0.245**	0.298**	0.322**	0.287**	0.278**
	-0.01	-0.03	-0.03	-0.04	-0.03	-0.04	-0.04
University	0.035*	0.148**	0.367**	0.430**	0.440**	0.394**	0.371**
	-0.01	-0.05	-0.05	-0.04	-0.05	-0.05	-0.05
Age	0.009	0.024	0.02	-0.005	-0.039	-0.017	-0.013
	-0.01	-0.02	-0.02	-0.02	-0.02	-0.03	-0.04
Age squared	0	0	0	0	0	0	0
	0	0	0	0	0	0	0
Partner: University	0.027	0.021	0.083	0.03	0.074	0.009	0.077
	-0.02	-0.04	-0.04	-0.05	-0.04	-0.05	-0.05
Partner: High sc.	0.02	0.034	0.071	0.121	0.104**	0.046	0.113**
	-0.01	-0.03	-0.06	-0.06	-0.04	-0.04	-0.04
Partner: Vocational	0.009	0.028	0.06	0.093*	0.094**	0.063	0.086*
	-0.01	-0.02	-0.04	-0.04	-0.04	-0.04	-0.04
Partner's age	0	0.002	-0.004*	-0.006*	-0.003	0.002	0
	0	0	0	0	0	0	0
Unemployment level	0.341	0.207	-2.006**	-0.092	-2.795**	-1.679*	-1.04
	-0.21	-0.54	-0.76	-1.03	-0.81	-0.84	-1.18
Village	-0.092**	-0.001	0.218**	0.226**	0.008	-0.258**	0.146
	-0.02	-0.05	-0.06	-0.07	-0.06	-0.09	-0.08
City	-0.073**	-0.036	0.243**	0.197**	0.041	-0.249**	0.132*
	-0.01	-0.03	-0.06	-0.05	-0.03	-0.09	-0.07
Large city	-0.118**	0.025	0.250**	0.237**	0.021	-0.202*	0.207**
	-0.02	-0.04	-0.07	-0.08	-0.06	-0.09	-0.08
_cons	-0.132	-0.231	0.074	0.574	1.452**	1.457**	0.972
	-0.14	-0.26	-0.37	-0.38	-0.4	-0.48	-0.66
R^2	0.177	0.213	0.318	0.369	0.403	0.366	0.406
AIC	-2579	2055.402	3491.096	2579.223	2258.491	2197.612	1831.307
N	3796	3688	3244	2883	2853	2666	2603

TABLE A5: FIRST STAGE RESULTS OF THE 2SLS REGRESSION, JANUARY CUTOFF

Dependent variable: C	Coef.	Robust SE	t	P>t	[95% Conf	Interval]
T	0.596	0.015	40.43	0	0.567	0.624
# of children	-0.006	0.004	-1.28	0.202	-0.015	0.003
Partner w/o job	0.010	0.017	0.57	0.567	-0.023	0.042
Partner w/ job	0.003	0.012	0.25	0.804	-0.021	0.027
Vocational school	-0.001	0.006	-0.22	0.829	-0.012	0.010
High school	-0.010	0.006	-1.87	0.062	-0.021	0.001
University	-0.004	0.007	-0.57	0.566	-0.017	0.010
Age	-0.004	0.004	-1.02	0.306	-0.011	0.003
Age squared	0.000	0.000	1	0.319	0.000	0.000
Partner: University	0.014	0.007	1.96	0.051	0.000	0.028
Partner: High sc.	0.007	0.007	1.05	0.295	-0.006	0.020
Partner: Vocational	0.005	0.007	0.68	0.495	-0.009	0.018
Partner's age	0.000	0.000	-0.77	0.444	-0.001	0.001
Unemployment level	0.054	0.178	0.31	0.76	-0.296	0.404
village	0.019	0.013	1.43	0.153	-0.007	0.044
city	0.031	0.012	2.52	0.012	0.007	0.055
large_city	0.019	0.011	1.75	0.081	-0.002	0.040
N	3018	3018	3018	N	3018	3018