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Does Childcare Matter for Maternal Labor Supply? Pushing the limits of the Regression Discontinuity Framework

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Abstract

We use an extension of the RD approach based on a kindergarten enrollment cutoff date and a new resampling design to estimate the causal impact of subsidized childcare availability on Hungarian mothers' labor market participation around the 3rd birthday of the child. Besides standard fuzzy RD, which holds calendar time constant, we apply an alternative version where child's age is held constant, which enables us to (a) separate the childcare effect from other, age-specific effects, and (b) consider the effect of not only point, but interval cutoffs for eligibility. We combine RD with a difference-in-differences approach using a comparison group of mothers with children aged 4-5 to control for seasonal effects (parent selection, child development, within-year labor market fluctuations). Our estimates indicate that a mother with a 3 year old is 15% more likely to be active if her child is eligible for subsidized kindergarten, corresponding to previous estimates of labor supply elasticity of 0.3-0.75. This suggests that increased subsidized childcare availability and parental leave alone cannot explain the sharp increase in the rate of maternal participation seen around children's 3rd birthday, highlighting the importance of other factors such as separation preferences and flexible work forms.

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

JEL classification: J13, J22

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Befolyásolja a gyermekellátás az anyák munkakínálatát? A diszkontinuitás regressziós modell korlátainak kiterjesztése

Lovász Anna - Szabó-Morvai Ágnes

Összefoglaló

Az RD modell – újfajta mintavétel révén történő - kiterjesztésével azt vizsgáljuk, hogy az mekkora a gyermekellátás hatása a magyarországi anyák munkapiaci aktivitására a gyermekek 3. születésnapja körül. A standard "fuzzy" RD használata mellett egy alternatív verziót is alkalmazunk, ami révén (a) szét tudjuk választani a gyermekellátás hatását egyéb, gyermekkor függő hatásoktól (GYES vége, preferenciák változása), és (b) nem csak pont, de intervallum bekerülési küszöböt is vizsgálunk. A szezonalitási hatásokat (szülői szelekció, gyermekfejlődés, munkapiaci fluktuációk) úgy kezeljük, hogy az RD modellt egy difference-in-differences módszerrel kombináljuk, ahol a 4-5 éves gyerekkel rendelkező anyákat kontroll csoportként használjuk. Az eredményeink alapján egy 3 éves gyermek anyja 15%-al nagyobb valószínűséggel aktív, ha állami (ártámogatott) gyermekellátásban részesülhet, ami korábbi eredményekhez hasonló, kb. 0,3-0,75-ös munkakínálati rugalmasságnak felel meg. Ennek megfelelően elmondhatjuk, hogy az aktivitás 3 éves kornál látott hirtelen növekedését nem magyarázza sem a gyermekellátásra való jogosultság növekedése, sem a GYES vége, rámutatva, hogy egyéb tényezők, mint a kötődési preferenciák és a rugalmas munkaformák elérhetősége szerepe is jelentős.

Tárgyszavak: Gyermekellátás, anyák munkakínálata, diszkontinuitás regressziós modell

JEL kódok: J13, J22

I. INTRODUCTION

Encouraging higher participation of women in the labor market is an important policy goal in most countries.[1] In particular, the employment of mothers of young children has remained low despite the general increase seen in overall female employment, and they remain an important untapped workforce. The employment prospects of these women play a key role in two processes of outstanding importance. First, fertility rates, which are crucial in aging populations, depend crucially on the labor market opportunities of mothers. Second, if mothers stay at home for long time periods after giving birth, employers bear higher potential costs hiring them, and all women of childbearing age may be affected by statistical discrimination. There are many factors that may affect a woman's ability and willingness to work after having a child, such as parental leave, tax/child benefits, childcare availability and costs, flexible employment opportunities, preferences regarding separation from the child, societal attitudes, labor market opportunities and discrimination. The possible range of policy tools is correspondingly varied, but recent consensus among policymakers is that expanding subsidized childcare availability is a key step.[2]

To find the most effective mix of policies - and to forecast the benefits of investment in subsidized childcare facilities - it is important to estimate the impact of childcare (as well as other factors) on mothers' labor supply precisely. Yet the available empirical evidence so far is highly ambiguous, and dependent on methodology and data constraints. Our paper proposes an empirical method (*regression discontinuity with a resampling design*) for estimating the causal effect of childcare availability on mothers' labor supply that is very close to an experimental design. The discontinuity in childcare availability (at the enrollment cutoff date) allows us to untangle the effect from that of unobserved regional and individual level characteristics, while the resampling design separates the childcare effect from other child age-related factors (parental leave, preferences) that is discontinuous at the cutoff. This allows us to draw important conclusions regarding the role of childcare availability and other factors, as well as related policy implications.

[1] It is key to sustainable growth, satisfying long term labor demand, lowering budget deficits, and achieving gender equality (Bloom et al. (2009)). In aging populations, it is a crucial for demographic policy to ease constraints related to childbearing (Apps and Rees (2001)). Economies have increasing skill demand, and women are an important potential resource (Krusell et al. (2000)).

[2] In the US and Canada, several states introduced universal subsidized pre-kindergarten (Fitzpatrick (2010), Baker et al. (2008), Lefebvre and Merrigan (2008)). EU policymakers declare increasing childcare availability an important goal (Barcelona Summit, EU (2002)).

Previous estimates of the effect of childcare opportunities on mothers' labor supply use three main types of empirical methodology: estimates based on structural models, those based on policy change, and a recent RD estimate based on discontinuity at the enrollment cutoff date.[3] Each of these has its advantages and drawbacks. Studies built on structural models generally use regional or time variation in childcare prices to identify the impact on labor supply. The advantage of these studies is that they control for fertility and other selection biases.[4] However, there are several drawbacks as well. Such models are based on several behavioral and distributional assumptions, which can be quite restrictive. Unobserved characteristics in the error term - mainly individual and regional - may 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. These are generally not controlled for in the studies based on structural models due to data limitations.[5] The evidence from these studies varies not only because of differences in methodology and data, but also differences in the age of the children analyzed, and cross-country differences in hard-to-observe preferential[6] and institutional factors. Little is known about the source and impact of these. Several structural studies support the existence of a negative effect of childcare costs on participation or employment: Lokshin in Russia (2004), Borra (2010) in Spain, Kimmel (1992), Conelly (1992), and Conelly and Kimmel (2001) in the US, Haan and Wrohlich (2011) in Germany, Del Boca (2002) in Italy. On the other hand, some studies find little or no significant effect: Chevalier and Viitanen (2002) in the UK, Chone et al. (2003) in France, Ribar (1995) in the US.

More recent research noted that these empirical issues make it difficult to provide causal estimates of the impact of childcare based on regional and time variation, and looked to other sources of variation for identification. Several studies make use of a policy change and use difference in differences methods. The advantage of using such changes is that much fewer assumptions are needed for estimation, and they should eliminate omitted variables bias as long

3 Most studies are from Europe and North America, studies on developing countries are rare (Lokshin (2004)).

4 Fertility may be endogenous to post-birth labor market opportunities. A common solution to this is modeling labor supply and fertility simultaneously in a structural model, based on several behavioral and distributional assumptions (Ribar (1995), Powell (2002)). Some studies use correction methods for other types of selection (participation and, given participation, formal child-care utilization), but inappropriate exclusion restrictions may lead to biases (Lefebvre and Merrigan (2008)).

5 Reviewing US evidence, Blau (2003) concludes that the variability of empirical results stems from differences in model specification and econometric methodology. Del Boca (2002) uses an individual fixed-effects model of fertility and employment decisions for Italy, which alleviates bias that is constant over time.

6 Regarding separation preferences, see for instance the International Social Survey Programme. (http://zacat.gesis.org/webview/index.jsp?object=http://zacat.gesis.org/obj/fStudy/ZA3880%20). The data reflect a wide variation among countries: the ratio of those preferring to stay home with children under school age ranges from 13.6% (Israel) to 64.3% (New Zealand).

as the policy changes were exogenous. However, policy decisions about subsidized childcare supply may be endogenous as well if they depend on local childcare demand, which is likely the case. As a consequence, estimations based on a policy change may suffer from bias, because the exogeneity of the treatment is not necessarily ensured. Moreover, these methods do not control for fertility selection as structural model-based estimates do. Some policy change-based studies find a significant positive impact of childcare expansion (or childcare subsidy 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)).[7] 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 an important condition is that no other factors change discontinuously at the same cutoff point. This may limit the applicability of the RD method in measuring the childcare effect even if an enrollment cutoff date exists, if there are other child age-related factors that change discontinuously at the cutoff. RD also does not control for fertility selection as structural model-based methods do. Fitzpatrick (2010) uses an RD framework based on new policies in three US states that recently introduced universal prekindergarten programs (for 4-year-olds) with birth date based eligibility cutoffs. The study uses US Census information to estimate the difference in the labor supply of mothers whose children were born shortly before and after the cutoff. The results suggest that although universal childcare availability increased preschool enrollment by 14 percent, it had negligible effect on the labor supply of most women.

Throughout this article we stay in the realm of regression discontinuity framework, but use an extension of RD in order to address a violation of the requirement of standard RD that no other factor may be discontinuous at the cutoff point. The source of this violation is that in Hungary children who turn 3 before September 1st are eligible for subsidized kindergarten. However, there are two other major sources of discontinuity around age 3: parental leave ends,

[7] Baker et al. (2008) use the introduction of subsidized universal childcare in Quebec, and find strong evidence of a significant increase in maternal labor supply. Lefebvre and Merrigan (2008) compare multiple pre- and post-treatment periods for Quebec mothers, and mothers in other provinces, and find a large significant impact. Cascio (2009) uses the staggered timing and age targeting of new subsidies for kindergartens in the US, and detects no significant response. Lundin et al. (2008) use data from Sweden, and changes in prices due to reforms, finding an effect close to zero.

and, as Blaskó (2011) shows, preferences regarding separation from the child change sharply as well. This means that using standard RD, which holds observation calendar time constant, would not allow us to differentiate these factors from each other. To tackle this problem, we utilize a new resampling design, which holds the age of child at observation constant. That is to say, we estimate the difference in the activity of the treatment and control groups not at the same calendar date, but in the quarter after the child turned 3, so children in the two groups are the same age on average. As a result, discontinuities related to child age affect the treatment and the control groups similarly, and do not bias the estimation of the treatment effect.

Contrary to the standard RD setup, the resampling design makes it possible for us to separate birth date and age effects. However, it is important to note that in this setup, the groups differ in the season in which they are observed and that their child was born. This means that estimates may be affected by selection bias if the composition of parents differs by season of birth of children, or if labor market opportunities (and therefore, expectations) differ by season. We control for this by using a comparison group of mothers with 4-5 years old children and difference in differences. An additional benefit of our RD resampling design is that it allows us to define the cutoff more broadly, and explore the possibility of an eligibility cutoff that is not a single point in time, but a time interval. Empirical evidence suggests that in reality, there is a less-strictly enforced cutoff date,[8] so we explore the possibility of an eligibility cutoff that is not a single point in time, but a time interval (September 1 – January 1st). This is not possible in the standard RD setup, because it is important that the cutoff be one point in time so treatment and control groups are similar enough, ensuring that differences stemming from age differences and birth date differences are negligible. The resampling design allows the cutoff to be a time interval, while treatment and control groups remain similar along the age dimension.

Our results point to a significant effect of childcare availability on mothers' participation. The reduced form results show that women whose children are born in the months before the kindergarten eligibility cutoff are significantly (6 percentage points) more likely to be active in the labor market than those whose children are born immediately after the cutoff. 2SLS estimates suggest that increasing subsidized childcare availability for a child around 3 years old by 10 % increases the probability that a mother is active by 1.5%. This means that the participation rate of mothers with children aged 2-3 would increase by about 13.5% if nursery school coverage increased to the level of kindergarten coverage. Taking into account the average

[8] We interviewed directors of kindergartens about actual enrollment practices. We found that (a) they vary a lot, and (b) kindergartens handle rules elastically in order to maximize parental satisfaction and kindergarten cost efficiency. The results of the interviews are in line with our estimates, which suggest that the 1st September cutoff date does not effectively divide the treatment and control groups.

female wage rate and the price of non-subsidized childcare, this translates to a labor supply elasticity of 0.3-0.75, which is in line with previous labor supply elasticity estimates for EU countries (e.g. Bargain et al. (2011), Bargain and Peichl (2013)). Since the amount of parental leave is rather low in amount (5/8th of the childcare subsidy) in the final year, based on this elasticity estimate, subsidized childcare availability and parental leave explain only one half of the large increase in mothers' activity seen at the 3rd birthday. The policy implication of our paper is therefore that childcare expansion (and, in the case of Hungary, shorter parental leave) alone will not achieve the goals set for the labor market activity of mothers. Other exogenous factors, such as preferences and societal attitudes may have an important role, in line with the observed cross-country differences in labor supply of mothers of children a young ages.

II. BACKGROUND AND FRAMEWORK

Before giving the details of the methodology and estimation results, we begin our discussion by describing important details of the institutional setting in Hungary that are relevant to our analysis. We focus on the childcare system, as well as two other major sources of change around age 3 of the child that affect our estimation: parental leave and preferences regarding separation from the child.[9] We then present a simple theoretical framework, and discuss the effect of these factors on labor supply and on our estimation methodology.

II.1. BACKGROUND ON DETERMINANTS OF LABOR SUPPLY IN HUNGARY

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.[10] The childcare services of these institutions are free of charge, but parents pay for the meals, and make minor material contributions (toilet paper, etc.), which amount to approximately EUR 20 a month. There are also nursery schools owned and maintained by churches, foundations, or private

[9] Some previous studies deal with various aspects of the Hungarian system: the fertility effects of the benefit system (e.g. Gábos et al. (2009)), the effects of mothers' employment on child development, and related attitudes (e.g. Blaskó (2005, 2008, 2011)), and labor market effects of child benefits (e.g. Lakatos (1996), Frey (2002), Nagy and Pongrácz (2009), Köllő and Bálint (2008), Szabó-Morvai (2013)). The labor market effect of the childcare system is barely analyzed: Blaskó et al. (2009), offer a review of this topic, and an approximation of the excess demand for formal childcare.

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

owners. Some of them offer their services for free; some of them charge a fee (approx. EUR 100-150 monthly). Nursery school coverage and usage are relatively low: a mere 11% of children under age 3 were attending nursery school in 2009 (EU-SILC), and approximately 9% of the settlements had a nursery school. This is rather low relative to the EU average, as can be seen in Figure 1, however, other CEE countries – with the exception of Slovenia – show a similar trend. The admissions process into nursery schools is highly competitive, and admittance is based on general rules as well as subjective factors.[11] For children above 3, state-owned kindergarten becomes available (depending on their birth date, as described below), which costs about the same as nursery school. Kindergarten coverage is significantly higher, around 80% in 2009 (EU-SILC), and relatively high compared to other countries (Figure 1). This means that when a child turns 3 and becomes eligible for kindergarten, the mother's childcare opportunities expand significantly: the expected cost of childcare decreases, as their child becomes very likely to be accepted into subsidized childcare.

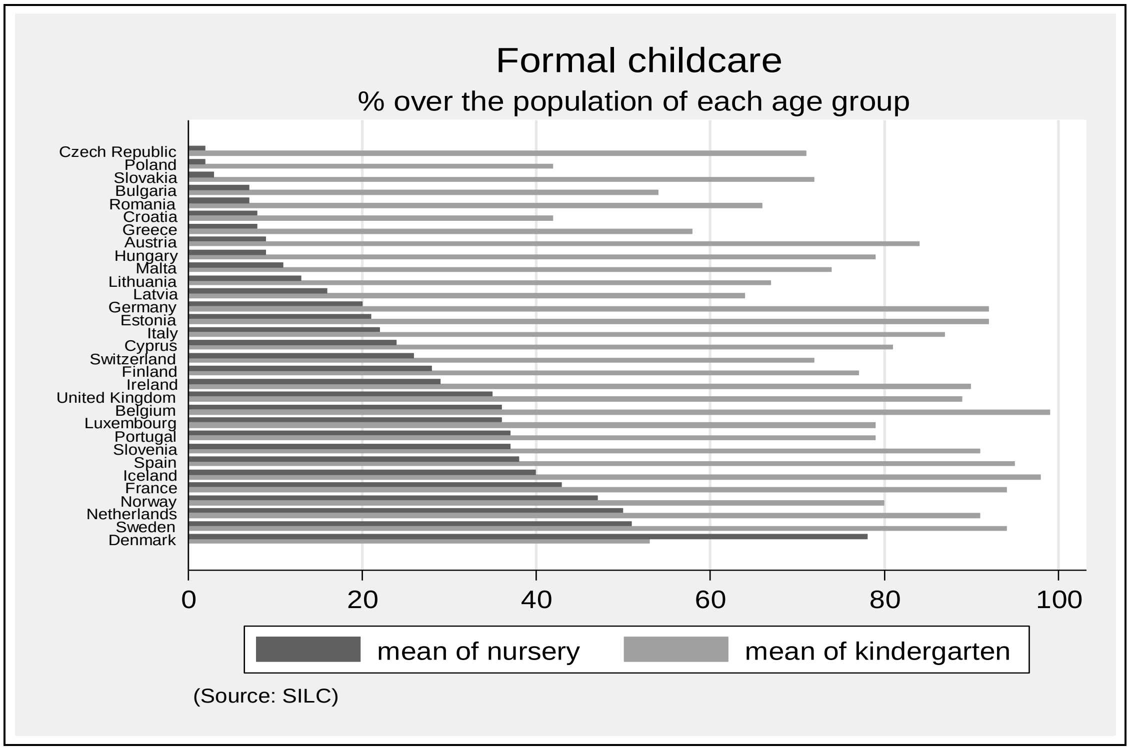
Figure 2 shows our own calculations of nursery and kindergarten coverage rates based on T-STAR Hungarian regional data, at the level of our empirical analysis,[12] in 2010. Coverage is calculated as the number of childcare places divided by the number of children of the given age group in the region. These statistics highlight the lack of childcare availability (which we use as a synonym of coverage throughout the paper) before age 3 of the child: 13% of the Hungarian population lives in a region with 0% nursery school coverage, which means that nursery school is not available in their township or be reasonably commuted to. The bulk of the population lives in regions where 10-25% of children under 3 years can access nursery school places, and there is no region in Hungary where more than 35% of the children can go to nursery school. Average coverage is around 9%. Kindergarten coverage is much more favorable: most regions have around 100% coverage, that is, most children should have a place in kindergarten.

[11] The school year starts in September, but parents have to apply well in advance. Acceptance rules may differ by institution. Children generally have priority if they live permanently in the given township, if they have older siblings already enrolled, if both of their parents work or study full-time, if they have a single parent, or are at risk (e.g. disadvantaged social situation). Besides general rules, subjective factors (e.g. acquaintance, sympathy) also affect the acceptance decision.

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

Figure 1

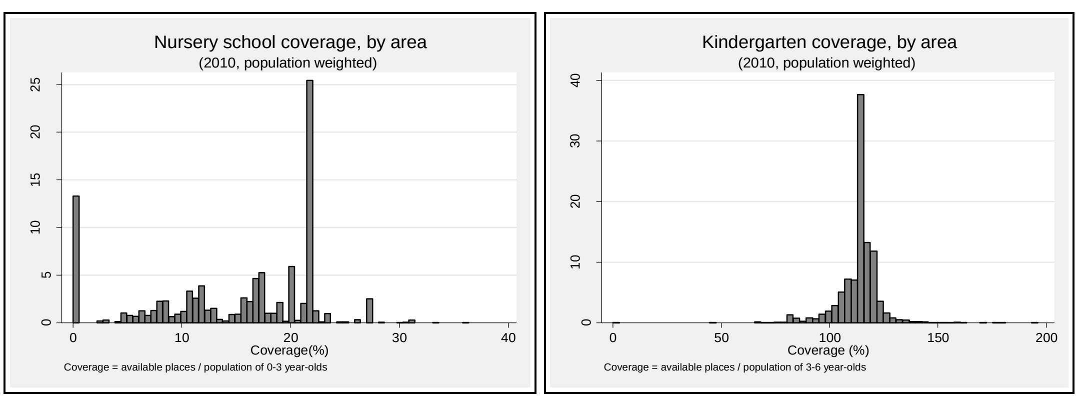
Percentage of the population of the relevant age groups attending formal childcare institutions, EU countries, 2009



Source: SILC[13]

Figure 2

Distribution of nursery and kindergarten coverage rates by region, 2010



Notes: Based on T-STAR Hungarian regional data. Coverage rate: the number of childcare places within each region, divided by the number of children of relevant age in each region. Region refers to townships merged based on commuting data.

13 http://epp.eurostat.ec.europa.eu/portal/page/portal/income_social_inclusion_living_conditions/data/database

The kindergarten school year begins in September. Officially, children who turn 3 (have a birth date) prior to September 1st are accepted into state-run kindergartens in the given school year, while children born after September 1st are accepted next year. However, kindergartens may accept every child over age 3 as space becomes available throughout the year. According to childcare professionals, children most often do enroll in next September, when older children leave kindergarten for primary school and spaces open up. At the same time, some children are allowed to enroll in September even if the child has not turned 3 yet, and some institutions have an additional enrollment wave in January. This means that the September 1st cutoff, in fact, may not be strictly enforced, which affects our estimation. To determine the actual cutoff, we explore several alternative specifications of the cutoff (September 1, January 1, and September 1-January 1) and compare the results. This allows us to draw some interesting conclusions regarding how the actual admittance process is carried out, without limiting our analysis to the official September 1 rule.

From 1993, this system of formal childcare was amended with the possibility of opening family daycare service centers, though these are not very common. In 1999, there were altogether 28 such institutions in Hungary, their number increased to 70 by 2003 (Rajkort (2006)). Family daycare centers are generally privately owned and provide paid childcare for about EUR 100-150 monthly, similar to private-owned formal institutions.[14] In addition to formal childcare arrangements, it is also possible to ensure childcare informally. Informal childcare may be the primary childcare possibility for some families, or it may complement formal arrangements. Informal care may be provided by for instance an au-pair, a helping family member (a grandmother), or a neighbor. The au-pair is expensive (approx. EUR 700 for a month), the others are usually for free. Although our focus is on the effect of the provision of low-cost formal childcare, we control for the availability of family daycare services and the presence of potential informal childcare providers in the household using the estimation method: the randomness of children's birth dates ensures that these should be equally available, on average, for both groups. The September 1st enrollment cutoff date does not apply to private-owned kindergartens (or grannies), so our estimates actually capture the effect of differences in subsidized state childcare availability between the groups.

[14] Calculated with 8 hours a day, 20 days a month, for this and all other price estimates given.

Parental leave

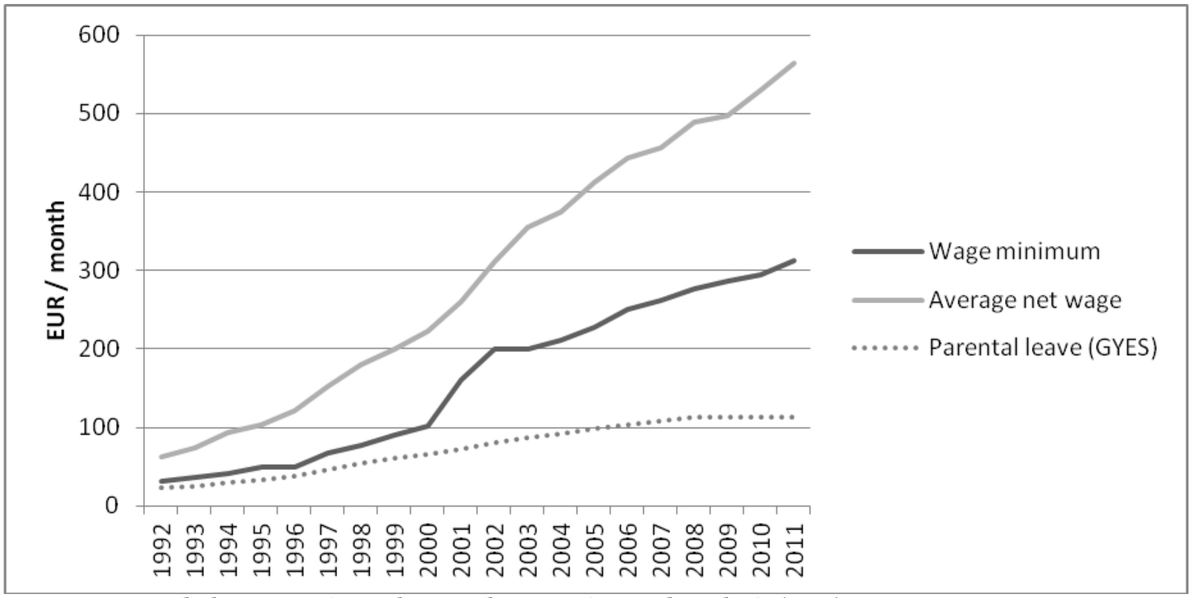
In order to understand the labor supply decision of mothers with young children, it is important to also review the parental leave system, which also impacts mothers' participation when children are young (Szabo-Morvai (2013)). For our purposes, flat-rate parental leave is of special interest among the available benefits, because it is given to families when the youngest child is under 3 years old, and it is terminated afterwards.[15] Flat-rate parental leave is universal in Hungary, and can be received by anyone, whether they were previously insured or not. One parent in each family is entitled to it, though statistics show that the overwhelming majority (98.1%) is taken by mothers, not fathers according to the H-LFS data. For this parent, full-time employment is restricted to telecommuting only, and part time employment is allowed. The sum of this benefit in the final year equals the old-age pension minimum, which was around EUR 105 in 2010. 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, effecting mothers' preferences (discussed next).[16] To sum up, since parental leave ends at age 3, it is highly relevant to our estimation, which is based on the discontinuous change in subsidized childcare availability at that time. Therefore this is an important issue that we address both in the theoretical framework, and in the empirical method based on the resampling design.

[15] Some receive this flat-rate payment between ages 2-3 of the child, while some receive it for the full 0-3 years. Other types of benefits (e.g. family allowance) are not relevant to our estimation, as there is no significant change in them around age 3 of the child.

[16] Such institutional signaling of the childcare system is discussed by Hasková et al. (2012)

Figure 3

Average wage, wage minimum, and parental leave in Hungary (1992-2010)



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

Other Factors: separation preferences

In addition to the two main institutional factors (subsidized childcare availability and parental leave), in the case of Hungary, we also have to consider the role of preferences regarding separation from the child. 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 will 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 an additional problem, which is that these preferences may also change discontinuously at age 3 of the child. A 2009 survey by Blaskó (2011) suggests that 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/individual preferences in the 3rd birthday being set as 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

To set up our model, we first have to clarify that we view childcare availability and childcare cost as the two sides of a medal. On the one hand, there is always some kind of childcare available, if one is willing to pay for it. On the other hand, if we restrict our interest to state-subsidized childcare, we can say that – knowing the expected price of each type of childcare – a given increase in subsidized childcare availability decreases the expected childcare price by a certain amount. Thus, subsidized childcare availability and expected childcare cost can be converted between each other. We utilize both sides of the medal in our study. We have accurate data on availability, thus we use it in our estimations. However, when it comes to the individual's labor supply decision, it is expected childcare price which is taken into account, and we interpret our estimation results in that light as well, converting them to elasticities. As a result, we introduce a theoretical model which is based on the cost of childcare.

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, that is, practically anyone can hire a nanny, if they can afford it. However, mothers face a significantly lower cost of childcare if subsidized institutional childcare is available to them.[17]

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

[17] Heckman (1974) emphasized the role of unpaid care, included in the model as a zero-cost childcare form.

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.[18]

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

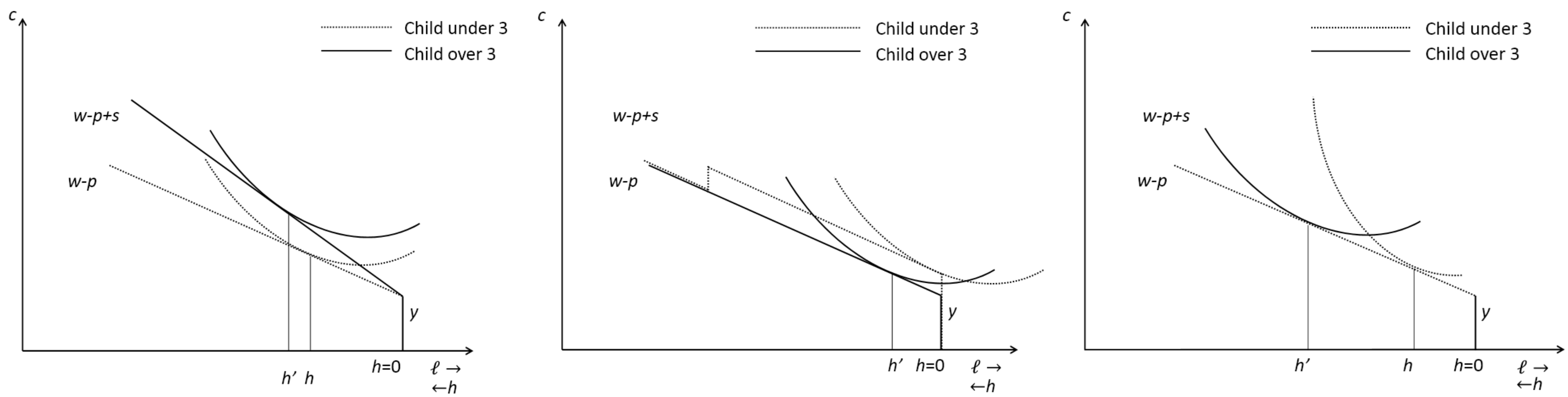
However, the other changes described above take place around age 3 of the child, which may confound the issue in the case of Hungary. First, one is that mothers are eligible for the flat-rate parental leave up to the child's 3rd birthday. Figure 4.b. depicts the labor supply of the mother when she is 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.

[18] 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: $h + \ell = 1$.

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 an extension of RD, which we call RD with a resampling design, in order to tailor the estimation to a case where some of the classical 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 very similar in age, birth date, and observation date, as well as every other characteristic except that only one group received the treatment. Another key condition for identification (among some others) is that nothing other than the treatment changes discontinuously around the cutoff point. These two requirements can be relaxed by using resampling design. The resampling method differs from the classic RD in that here we use *repeated cross section samples*, one for the treatment and one for the control group, such that the average child age is held constant for the treatment and the control group. This allows for the separation of the calendar date-specific treatment effect from other factors that are age-dependent. In this setup, the cutoff separating the treatment and the control groups may be a point or a time interval. In case of an interval cutoff, the two groups will still be similar in age due to the resampling design, but differ in birth date and observation date.

These latter differences may introduce seasonal bias of some 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. We check this assumption by noting that in our sample, the level of education of the mothers in the treatment and the control group does not differ significantly in Hungary. However, there may be other types of selection present, as we will see in our descriptive statistics in the next section. 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, especially in certain industries. Labor demand, in turn, determines the actual and expected probability of employment, which strongly affects labor supply. The usual solution of including season fixed effects cannot be applied here, as seasonal effects are highly correlated with the instrument T of the 2SLS regressions. Instead, in order to cope with possible seasonal biases, we include a comparison group which is very similar to the original sample, but does not differ by child's birth date in terms of treatment status. We use the comparison group to execute a difference-in-differences estimation: the intra-year variations of the variables and the sample are captured by the comparison group, so the observed differences among two groups of mothers of 3 year olds can be ascribed to the treatment.

To sum up, we introduce resampling, an extension of RD, which combines RD with the DID method. By using resampling, two requirements of RD may be relaxed: (1) no other thing change discontinuously at the cutoff, and (2) there is a point cutoff. However, it has two additional requirements compared to standard RD: (1) it requires repeated cross section or panel data, and (2) a comparison group is needed in which either everyone or no one is treated. In the Results section we compare the results from the standard RD and RD with a resampling design to draw conclusions regarding the role of childcare in maternal labor supply, as well as the role of parental leave and other factors. Additionally, the different cutoff specifications (point and interval) provide information on the actual enrollment practices regarding cutoff dates in Hungary.

STANDARD RD

Regression discontinuity design is based on the following discontinuity:

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

$D_i = 1$ indicates that the child uses subsidized childcare. b_i is the month of the third birthday of the child[19], and b_0 is the month which includes the cutoff date. As discussed in the background section, the actual effective cutoff date is questionable in Hungary, so we use September 1st and January 1st as two alternative cutoffs. π_r^n is nursery school coverage, π_r^k is kindergarten coverage in region r. We define the instrumental variable as follows:

$$T_{yri} = \begin{cases} 0 \ if \ \ b_i < b_0 \\ 1 \ if \ \ b_0 \leq b_i \end{cases}$$

T_{yri} is the instrumental variable of a fuzzy RD design, as the probability of childcare usage changes with T_{yri} from 9% to 99% on average, depending on the region. The treatment group includes mothers whose child turned 3 in the 5 months prior to the cutoff, who are eligible for

[19] We use a natural numbering 1-12 for birthmonths. In case of a January cutoff, $b_i = 0$ for the preceiding December, $b_i = -1$ for November, etc.

kindergarten enrollment. The control group includes mothers whose child turned 3 in the 5 months after the cutoff, thus, as a general rule, are not eligible for state-subsidized kindergarten enrollment until significantly later. The mothers in both groups are observed on average 1.5 months after the cutoff. In general, we include 5 months of birth dates for each group, but we decrease the timeframe as a robustness check.[20]

We run individual level two stage least squares regressions of the following form:

$$C_{yri} = \beta_1 T_{yri} + \alpha_y + \gamma_r + X'_{yri}\pi_{11} + S'_{yr}\pi_{12} + \xi_{1yri} \quad (1)$$
$$L_{yri} = \beta_2 C_{yri} + \alpha_y + \gamma_r + X'_{yri}\pi_{21} + S'_{yr}\pi_{22} + \xi_{2yri} \quad (2)$$

The corresponding reduced form equation is:

$$L_{yri} = \beta_R T_{yri} + \alpha_y + \gamma_r + X'_{yri}\pi_{R1} + S'_{yr}\pi_{R2} + \xi_{Ryri} \quad (3)$$

Where equation (1) shows the first stage regression, (2) shows the second stage regression, and equation (3) represents the reduced form regression. The parameter β_1 reflects the first-stage effect of T_{yri} on C_{yri}, the regional childcare coverage faced by individual i. β_R on the other hand, captures the reduced-form effect of T_{yri} on L_{yri}, the individual labor supply, which can be zero or one. The effects are measured adjusting for a set of individual (X_{yri}) and regional covariates (S_{yr}). Child care coverage (C_{yri}), labor supply (L_{yri}), and individual covariates (X_{yri}) vary by year (y), region (r), and individual (i), while α_y represents year fixed effects, and γ_r region fixed effects.

In the case of Hungary, estimation results using standard RD capture the effect of not only increased childcare availability, but other factors described in the background section as well: the end of parental leave when the child turns age 3, and changes in preferences regarding separation from the child around the 3rd birthday. Children in the treatment group are 3.5 years old on average, whereas children in the control group are 3 months younger. Thus, for treatment group, more time has passed since the end of the parental leave, which may induce mothers to increase their labor supply. On the other hand, these children are older, and the mothers might be more willing to separate from them and go back to work.

RESAMPLING DESIGN

In order to separate the effect of childcare availability from the other effects, we use a resampling design. As childcare availability depends on birth date, while the other factors (parental leave and separation preferences) are related to child age, this method enables us to do so. We define

[20] We carry out the estimation with 3 and 4 month groups as well. The results show a similar pattern, though the significance of the estimates is lower due to the smaller sample sizes.

the treatment and the control groups similarly to the classic RD case, but we observe each mother the same length of time (4 months) on average after their child's 3rd birthday:

$$T_{yri} = \begin{cases} 0 & if \quad b_i < b_0 \quad and \quad 3 < a_i \leq 3.6 \\ 1 & if \quad b_0 + l \leq b_i \quad and \quad 3 < a_i \leq 3.6 \end{cases}$$

where a_i is the age of the child, and l is the length of the interval cutoff, in months. In case of a point cutoff, $l = 0$, while an interval cutoff means that $l > 0$.

As in the previous setup, T_{yri} is strongly correlated with childcare costs, 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 quarter does not correlate with any other factor that is relevant for labor market decisions. T_{yri} is not correlated with any observed or unobserved individual or regional level characteristics, with the exception of possible seasonality effects noted earlier (addressed below). 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. 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 order to address the previously noted seasonal bias problem, we expand both treatment and control groups with reasonably close labor market substitutes, namely, mothers of children aged 4-5 years, and estimate a DID regression. For this comparison group, childcare availability no longer differs for the treatment and the control group (by birth date). Thus, any difference between them should be the result of the factors mentioned above: selection among the groups or within-year variations in the labor market (such as exogenous random shocks or seasonal effects). 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 $M^{4-5} = 1$. The 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:

$$T_{yri} = \begin{cases} 0 & if \quad b_i < b_0 \quad and \quad 4 < a_i \leq 5.6 \\ 1 & if \quad b_0 + l \leq b_i \quad and \quad 4 < a_i \leq 5.6 \end{cases}$$

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

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

We then run the 2SLS regression on the extended sample, and include m_{yri} and the interaction of m_{yri} and T_{yri} as additional controls:

$$C_{yri} = \beta_1 T_{yri} m_{yri} + \alpha_y + \gamma_r + X'_{yri}\pi_{11} + S'_{yr}\pi_{12} + \pi_{13}T_{yri} + \pi_{14}m_{yri} + \xi_{1yri} \quad (4)$$

$$L_{yri} = \beta_2 C_{yri} m_{yri} + \alpha_y + \gamma_r + X'_{yri}\pi_{21} + S'_{yr}\pi_{22} + \pi_{23}C_{yri} + \pi_{24}m_{yri} + \xi_{2yri} \quad (5)$$

$$L_{yri} = \beta_R T_{yri} m_{yri} + \alpha_y + \gamma_r + X'_{yri}\pi_{R1} + S'_{yr}\pi_{R2} + \pi_{R3}T_{yri} + \pi_{R4}m_{yri} + \xi_{Ryri} \quad (6)$$

In this setup, the parameter β_2 shows the effect of C_{yri} on L_{yri}, net of any seasonal effects, while β_R is the reduced form effect of T_{yri} on L_{yri}, free of the within-year effects.

The ability to compare groups based on an interval cutoff allows us to examine the treatment effect for 3 different groups. Group 1, where the kids were born in April-August, can enroll soon after their 3rd birthday in September. The enrollment date of Group 2, where the kids were born in September-January, is unclear. If the law (allowing the child to enroll in September only if she has turned 3) is strictly enforced, and there is no continuous enrollment throughout the school year, they can enroll only in next September, shortly before they turn 4. But if the law is not enforced, they can enroll before their 3rd birthday, or if possible, immediately after their 3rd birthday. If they can enroll in January in an additional wave, their average waiting time for enrollment will be similar to that of Group 1. Group 3, where the child was born in January-May, most likely has to wait to enroll until next September, that is, 4-9 months after their 3rd birthday. A comparison of the results with different cutoffs can reveal whether Group 2 is in the most advantageous position, suggesting that the law is not strictly enforced, or there is continuous enrollment up to January. In that is the case, the opportunities of the first group are similar, or slightly worse, while the third group is clearly in the most disadvantageous position. Thus, we would expect the largest and most significant effect in case of the January 1st cutoff (where groups 2 and 3 are compared), a moderate effect in case of the September 1st – January 1st interval cutoff (where groups 1 and 3 are compared), and a slightly negative or insignificant effect in case of the September 1st cutoff (where groups 1 and 2 are compared).

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 about all members of the households selected in the sampling process. If a household consists of more than one family, then all of them are included, with different family identification numbers. Approximately 17% of the households are rotated in each quarter; the maximum number of periods for observation is 6, which equals one and a half year. 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 can be used for our purposes. 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 H-LFS dataset, between 1996 and 2011, a mere 1.9% of those receiving parental leave payments were males. We define variables indicating treatment and control groups (z) and the corresponding comparison group of mothers of 4-5 year olds, and limit our estimation sample to these groups, observed in the time periods indicated in the resampling design, as described in the methodology.

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. In our analysis, we use information on the individual's labor market activity as our labor supply measure, and include as controls individual characteristics (education, occupation, age, etc.), and family characteristics (marital status, partner's work status, number of children, etc). 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.[21] This means that we are not considering changes in hours of employment, because, 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, linked via township codes. The focus of our analysis, childcare availability, is constructed from the T-STAR database based on the number of

[21] We also run our estimation with an employment dummy as the dependent variable as a further check. The results show similar overall trends as those presented here.

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

Summary statistics of the variables used in the estimations are given in Appendix Table A1, shown for the January 1st cutoff. The table gives the means of the control individual, family, and regional variables 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 found in the participation rate of mothers of 3-year olds: it is 59.6% for the treatment, and 51.5% for the control group. This means the difference is about 0.16 standard deviations. The difference in activity rates shrinks to 0.17 percentage points for 4-5 year olds. The two subsamples of mothers with 3-year-olds and 4-5-year-olds are similar in most aspects, except for trivial differences stemming from the construction of the subsamples: the average age of the parents and the children differs slightly, and mothers with 4-5-year-old children have more children on average.

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 the treatment. Based on the number of standard deviations measure, 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, which is a 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. Therefore the DID seasonality correction may be important in our estimation. The differencing should 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

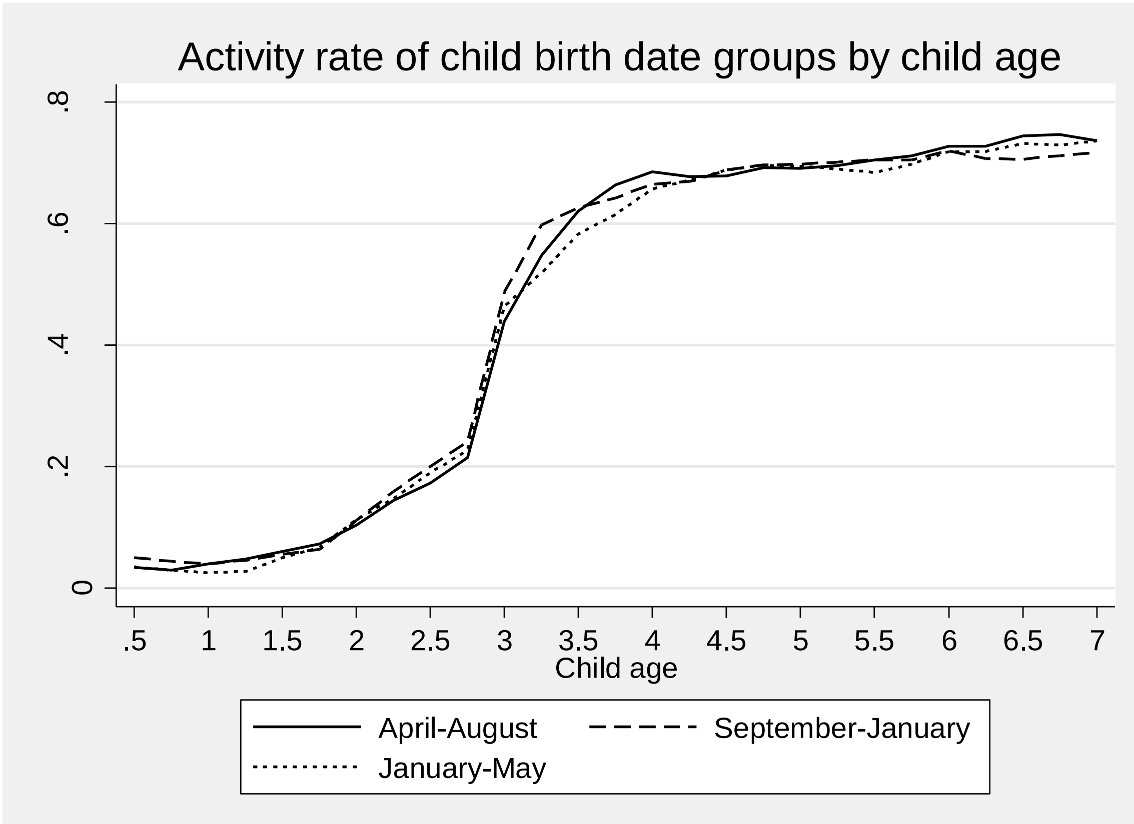
V.1. GRAPHICAL ANALYSIS: MOTHERS' ACTIVITY RATES OVER CHILDREN'S AGE

We begin our description of the estimation results by presenting a graphical analysis of the effect of treatment. Using the repeated cross-section (in our case, panel) dataset, we graph the activity rates of mothers of the three groups compared in case of the various cutoffs over a longer time span. Figure 5 presents the activity rates of the groups for child ages 0.5-7. Mothers are grouped based on the birth date of their child, and their average activity rate is calculated at each age (in quarters) of their child. All three groups show a gradual increase in the labor market activity, the rate of which increases after age 2 of the child, especially sharply after 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 any more sudden changes in preferences regarding separation that may occur at age 3, as discussed earlier. 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). Any difference between the groups is due to the difference in childcare availability due to the kindergarten eligibility cutoff: mothers in the various groups wait different lengths of time to gain access to kindergarten on average. Of course, seasonal effects may play a role here as well, and are not controlled for.

The graph shows that while the three lines move together in general, there is a difference after age 3 of children. The activity of the group with children born September-January increases the most (or earliest), suggesting that they indeed are in an advantageous position, and are able to enroll their children at the earliest age on average. The pre-September group is next, while the January-May group lags behind the other two. These results suggest that the effective cutoff date is January 1st rather than September 1st., leading to the largest treatment effect. We now turn our attention to the RD estimation results to see more precisely what the magnitude of this effect is, and to control for possible bias from seasonal effects.

Figure 5

Activity rates of mothers by birth date of their child



V.2. RD REGRESSION RESULTS

The estimation results are presented for three designs: standard RD, resampling with a point cutoff, and resampling with an interval cutoff. The comparison of these results allows us to draw some conclusions about the relative importance of not only childcare availability, but also the other factors that lead to changes in mothers' labor supply around age 3 of their children.

As a starting point, Table 1 presents estimation results based on the standard RD design[22], though this method cannot produce unbiased estimates of the effect of childcare availability in our case, as discussed. In the standard RD setup, the two groups differ not only in childcare availability, but also child age related factors, specifically, parental leave and attachment preferences. The treatment group is observed after the child has turned 3, so after the end of parental leave. The control group is observed before or as the child turns 3, therefore most of these mothers still receive parental leave, and most probably are more attached to the child on

[22] For the he full set of results see Appendix Table A2.

average. The standard RD estimates therefore capture the combined effect of all of these factors. Results are presented with January 1st and September 1st cutoff dates, and for three specifications of control variables. The upper panel of reduced form estimates shows a large and significant positive effect of around 0.25 in the January cutoff, and 0.24 in the September cutoff case. The 2SLS regressions give coefficient estimates of around 0.32 and 0.31 for the childcare coverage variable, however, this is biased, as both coverage, and the instrument *T*, are correlated with the other two factors.[23] Therefore, we cannot draw conclusions regarding the magnitude of the childcare effect alone, but we can say that there is a very significant change that takes place when children turn 3 *and* become eligible for kindergarten.

[23] These estimates are very close to what we find if we run similarly specified OLS or regional fixed effects regressions on the full sample of mothers of 2-4 year olds in the LFS dataset, which range from 0.3-0.4. This suggests that standard RD results are in fact driven by the other factors that change discontinuously at age 3.

Table 1

Standard RD regression results

Specification	1	2	3	1	2	3
Reduced form						
Cutoff:	January 1			September 1		
T	**0.253****	**0.252****	**0.247****	**0.236****	**0.236****	**0.242****
	(0.025)	**(0.022)**	**(0.023)**	**(0.024)**	**(0.023)**	**(0.023)**
N	3444	3444	3379	3370	3370	3255
Adj. R2	0.284	0.336	0.333	0.262	0.318	0.324
2SLS						
Cutoff:	January 1			September 1		
T	**0.323****	**0.323****	**0.318****	**0.304****	**0.303****	**0.314****
	(0.026)	**(0.023)**	**(0.024)**	**(0.026)**	**(0.025)**	**(0.026)**
N	3248	3248	3184	3196	3196	3076
Adj. R2	0.075	0.143	0.142	0.068	0.137	0.137
Year dummies	x	x	x	x	X	x
No controls	x			x		
Individual controls		x			X	
Individual and regional controls			x			x

Notes: Estimation based on H-LFS and T-STAR datasets, years 1997-2010. The dependent variable is the activity dummy. The table gives coefficient estimates of the dummy variable indicating treatment group membership (z=1 if treated). Year dummies are included in all regressions. Standard errors are given in parentheses. Stars indicate significance as: * p<0.05; ** p<0.01; *** p<0.001.

More appropriate for our task of measuring the childcare effect, the reduced form regression results based on RD with a resampling design (point and interval cutoffs) are shown in Table 2. 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.[24] 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 mothers are included. Coefficient estimates measuring the effect of subsidized childcare availability are given in bold: for the baseline regressions, these are the coefficients of *T*, while for the seasonality-corrected regressions, they are the coefficients of the interaction term *T*m*.

[24] Full estimation results can be seen in Appendix Table A3.

Of the three cutoffs, the RD results for January 1st show the most significant effect, ranging from 0.082 to 0.095 in the baseline specification. The September 1st cutoff, which should theoretically show the most significant if regulations are strictly enforced, shows no significant effect. The interval cutoff estimate, for September 1st-January 1st, is between the two point cutoffs, with somewhat smaller and less significant estimates than the January 1st cutoff results. The seasonality-corrected results show a similar pattern with slightly lower estimates: the results for January 1st range between 0.06-0.064. 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. 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 groups should not differ significantly in terms of individual and regional characteristics on average.

The larger estimated impact of the January 1st cutoff suggests that in reality, children born up to December are either allowed to enroll in January, immediately after their 3rd birthday, or even in September, prior to their birthday. Those born between September and December spend the shortest time, on average, waiting for enrollment eligibility, even less than those born prior to September 1st. Children born after December 31st, however, have a significantly lower probability of being enrolled in kindergarten soon after their 3rd birthday, and likely have to wait until the next September. Therefore, for mothers, having a child born in the months before January means higher childcare availability, which leads to a higher probability that they will be active in the labor market than those with children born after January 1st. Based on the most stringent estimate, being eligible for kindergarten increases a mother's probability of being active by 6 percentage points.

Table 2

RD with resampling design and interval cutoff, reduced form regression results

Reduced form regressions						
Specification	1	2	3	1	2	3
Cutoff: January 1						
	Baseline			Seasonality-corrected		
T*m				**0.064****	**0.064***	**0.060***
				(0.02)	**(0.03)**	**(0.03)**
T	**0.086*****	**0.095*****	**0.082*****	0.012	0.019	0.012
	(0.02)	**(0.02)**	**(0.02)**	(0.02)	(0.02)	(0.02)
m				-0.170***	-0.156***	-0.156***
				(0.02)	(0.02)	(0.02)
N	3309	3309	3244	9152	9152	8982
Cutoff: September 1						
	Baseline			Seasonality-corrected		
T*m				**-0.026**	**-0.035**	**-0.027**
				(0.03)	**(0.03)**	**(0.03)**
T	**-0.028**	**-0.030**	**-0.021**	-0.001	0.006	0.004
	(0.03)	**(0.02)**	**(0.02)**	(0.02)	(0.02)	(0.02)
m				-0.101***	-0.082***	-0.082***
				(0.02)	(0.02)	(0.02)
N	3344	3344	3229	9183	9183	8871
Cutoff: September 1-January 1						
	Baseline			Seasonality-corrected		
T*m				**0.039**	**0.035**	**0.041**
				(0.02)	**(0.02)**	**(0.02)**
T	**0.030**	**0.040***	**0.050***	-0.009	0.004	0.005
	(0.02)	**(0.02)**	**(0.02)**	(0.02)	(0.02)	(0.02)
m				-0.164***	-0.151***	-0.150***
				(0.02)	(0.02)	(0.02)
N	3296	3296	3181	9142	9142	8830
Year dummies	X	x	x	x	x	x
No controls	X			x		
Individual controls		x			x	
Individual and regional controls			x			x

Notes: Estimation based on H-LFS and T-STAR datasets, years 1997-2010. The dependent variable is the activity dummy. The table gives coefficient estimates of the dummy variable indicating treatment group membership (z=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; *** p<0.001.

To gain a better understanding of the magnitude of the impact, and to compare it to labor supply elasticity estimates, we turn our attention to the 2SLS results, shown in Table 3 in the same format.[25] Again, the January 1st cutoff shows the greatest impact, while September 1st shows no significant impact, and the interval cutoff is between the two, but closer to the January 1st estimates. For January 1st, baseline results show the coefficient estimate of childcare coverage ranging from 0.11-0.12. The seasonality-corrected estimates are around 0.8, with the full set of controls it is 0.076 and significant. This suggests that seasonal effects do have an impact and need to be taken into consideration. This result suggests that if childcare coverage increased from 0 to 100%, i.e. if subsidized childcare became available to mothers who did not previously have access, their activity rate would increase by 7.6 percentage points, or about 15 percent. In terms of Hungary, this means that if the average nursery school coverage for mothers of 2-3 year olds (10%) increased to the level of kindergarten coverage (around 99%), their activity rate would increase by roughly 13%.

Contrary to the previous RD estimate for US mothers with 4 year olds, our results suggest that subsidized childcare availability does have a significant impact on mothers' labor supply. The estimated impact is in line with earlier findings from the US: Cascio (2009) found an increase of 12%, and Gelbach (2002) found an impact of 7% for mothers of 5 year olds being eligible for kindergarten. However, the estimated impact is relatively low, in line with recent findings that female labor supply elasticity has declined (Blau and Kahn (2007), Heim (2007)). Converting our estimate shows that it is in line with labor supply elasticities estimated for EU countries (Bargain et al. (2011), Bargain and Peichl (2013)). Bargain et al. (2011) estimate elasticity between 0.2-0.6. In Hungary, a 40 000 HUF increase in the net wage (the subsidy received with state–run childcare), translates to a 20-50% wage increase (for net wages between 80.000 – 200.000 HUF). This increases labor supply by 15%, which is an elasticity of 0.3-0.75.

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

Table 3

RD with resampling design and interval cutoff, 2SLS regression results

2SLS regressions						
Specification	1	2	3	1	2	3
Cutoff: January 1						
	Baseline			Seasonality-corrected		
C*m				**0.080****	**0.081***	**0.076***
				(0.03)	**(0.03)**	**(0.03)**
C	**0.110*****	**0.121*****	**0.106*****	0.015	0.024	0.015
	(0.03)	**(0.03)**	**(0.03)**	(0.02)	(0.02)	(0.02)
m				-0.179***	-0.166***	-0.165***
				(0.03)	(0.03)	(0.03)
N	3116	3116	3054	9087	9087	8914
Cutoff: September 1						
	Baseline			Seasonality-corrected		
C*m				**-0.033**	**-0.043**	**-0.035**
				(0.04)	**(0.04)**	**(0.04)**
C	**-0.036**	**-0.038**	**-0.027**	-0.001	0.008	0.005
	(0.03)	**(0.03)**	**(0.03)**	(0.03)	(0.03)	(0.03)
m				-0.098***	-0.077***	-0.078***
				(0.02)	(0.02)	(0.02)
N	3152	3152	3037	9112	9112	8797
Cutoff: September 1-December 31						
	Baseline			Seasonality-corrected		
C*m				**0.049***	**0.043**	**0.052***
				(0.02)	**(0.02)**	**(0.03)**
C	**0.039**	**0.051***	**0.064***	-0.011	0.005	0.006
	(0.02)	**(0.02)**	**(0.03)**	(0.02)	(0.02)	(0.02)
m				-0.169***	-0.156***	-0.156***
				(0.03)	(0.02)	(0.02)
N	3120	3120	3000	9064	9064	8748
Year dummies	x	x	x	x	x	X
No controls	x			x		
Individual controls		x			x	
Individual and regional controls			x			X

Notes: Estimation based on H-LFS and T-STAR datasets, years 1997-2010. 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; *** p<0.001.

An important implication of our study can be seen when we take the standard RD results into account as well. We saw that there is a large increase in mothers' activity rates around age 3 of their child, which is due to the combination of the effect of increased childcare availability, the end of parental leave, and other exogenous factors that change at that time, for example, separation preferences. Based on the estimated impact of monetary constraints, the end of parental leave does not, in itself, or together with the childcare effect, explain this change. The standard RD estimates show an increase in mothers' activity of about 31 percentage points. Of this, we estimate that childcare availability leads to an increase of 7.6 percentage points. Based on the fact that the childcare subsidy is about 40 000 HUF, while the parental leave payment prior to age 3 is only 25 000 HUF (105 Euro in 2010), the effect of the latter should be no more than 7.6 percentage points as well. This suggests that at least 15.8 of the overall 31 percentage point change seen in mothers' activity at age 3 of children, i.e. roughly one half, remains unexplained. This may be due to individual preferences, or some other exogenous factors, but we can see that they change significantly around age 3 of children, suggesting some link to the institutional framework.

As a final 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 of age, using the January 1st cutoff. Table 4 summarizes the results,[26] which indicate that there is a significant effect at age 3 of the child, but there is no effect at other ages, in line with our model.

Table 4

Robustness check: resampling for each age group

	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**
	(0.01)	**(0.01)**	**(0.02)**	**(0.03)**	**(0.02)**	**(0.02)**	**(0.02)**
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. 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; *** p<0.001.

[26] Full results are given in Appendix Table A5.

VI. CONCLUSION

In this study, we develop an extension of the RD framework, which uses a resampling 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 as well at the cutoff. The method allows us to separate calendar-related and age-related effects, as well as to explore not only point, but interval cutoffs in the RD framework. Our results suggest that eligibility for subsidized childcare increases mothers' activity by about 7.6 percentage points (15 percent). Based on the size of the subsidy, this corresponds to previous labor supply elasticity estimates of 0.3-0.75 (Bargain et al. (2011), Bargain and Peichl (2013)). This explains about a quarter of the 31 percentage point increase in mothers' participation that we see when children turn 3.

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 services in terms of hours offered.[27] This means that mothers are constrained in their time spent working, and suffer a disadvantage competing with coworkers, providing a basis for employer statistical discrimination. As there may be a complementarity between the availability of childcare and flexible jobs, increasing the availability of inflexible state childcare alone will not improve mothers' labor market opportunities as much as it would in combination with additional flexible work opportunities and childcare services.[28]

The comparison of our results from the three RD designs point to an interesting puzzle. From the standard RD results, we see that there is a 31 percentage point increase in the activity of mothers when their child turns 3. This is partly due to the increase in subsidized childcare availability (7.6 percentage points). Parental leave should have (maximum) similar impact based on monetary incentives alone (parental leave is about 5/8th of the childcare subsidy). This means that changes in these two factors explain half of the sudden increase in activity at age 3 of the child, while the other half, 15.8 percentage points, remains unexplained. Further research is needed to determine what other factors play a role, and what policy steps can influence them.

[27] In Hungary, state-owned institutions provide childcare on workdays, usually from 6 a.m. to 4 p.m. The ratio of part time jobs is low, about 4.4% of overall employment (H-LFS), which poses a real constraint.
[28] Del Boca (2002) states that policies need to combine the aims of more flexible work schedule choices and greater child care availability.

The timing suggests that these factors are related to the institutional framework, which can have an influence through several possible channels. The length of parental leave and starting age of kindergarten may be perceived as a signal by mothers (and society), suggesting that age 3 is the appropriate time for separating from the child and returning to work. It is also possible that, lacking clear views on the matter, mothers simply use the age suggested by the institutional framework as a rule of thumb for when they should return to work. At the same time, employers may assume that after age 3, childcare duties of mothers are less of a constraint (children get sick less, need less attention), and be more willing to employ them. This, in turn, may influence mothers' expectations and activity. To sum up, we do not know what the underlying mechanisms are, but our results suggest that individual (societal) preferences have an important role. Institutions and policies influence mothers beyond monetary incentives. Policymakers need to take both possible complementarities with other factors, as well as the signaling effect of the institutional framework into consideration to successfully increase maternal labor supply.

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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) (%)	59.6%	51.5%	0.161	68.32%	68.15%	0.004
Number of children	1.3	1.3	-0.022	1.1	1.1	-0.040
Age of youngest child	3.3	3.3	-0.030	4.8	4.8	-0.043
Age (years)	31.1	31.1	0.001	32.4	32.5	-0.004
Education (%)						
Primary	23.6%	22.1%	0.037	23.2%	23.1%	0.000
Vocational school	26.9%	27.2%	-0.006	28.0%	25.3%	0.063
High school	31.9%	33.3%	-0.030	34.4%	35.0%	-0.013
University	17.6%	17.5%	0.004	14.5%	16.6%	-0.057
Occupation (%)						
Leader, executive	19.9%	20.6%	-0.016	20.2%	18.2%	0.053
Higher educ. requiring	1.8%	1.9%	-0.006	2.1%	2.6%	-0.031
GED requiring	11.4%	12.1%	-0.022	10.0%	12.0%	-0.061
Clerical, customer service	15.4%	14.7%	0.020	15.2%	14.4%	0.022
Service, commerce	9.5%	9.3%	0.005	9.7%	10.7%	-0.033
Agricultural	17.0%	20.1%	-0.077	18.5%	18.2%	0.008
Construction, industry	1.2%	0.8%	0.050	2.0%	1.7%	0.019
Operation, assembly	8.8%	7.3%	0.056	7.6%	6.9%	0.028
Unskilled	8.2%	8.1%	0.004	7.8%	7.4%	0.012
Armed forces	6.7%	5.0%	0.077	7.0%	7.8%	-0.033
Husband or partner						
Age (years)	30.0	29.8	0.017	30.8	30.8	-0.002
Employment status (%)						
No partner	13.3%	13.2%	0.004	14.1%	12.7%	0.042
Partner without job	13.3%	13.2%	0.004	14.1%	12.7%	0.042
Partner with job	76.0%	75.6%	0.007	73.2%	75.0%	-0.042
Education (%)						
Primary	16.6%	16.0%	0.017	15.8%	16.8%	-0.025
Vocational school	38.2%	38.2%	0.000	38.5%	37.9%	0.012
High school	20.7%	21.4%	-0.017	21.8%	22.3%	-0.012

University	13.4%	13.0%	0.012	11.0%	10.5%	0.014
Occupation (%)						
Leader, exec.	17.8%	17.8%	0.002	20.6%	17.7%	0.076
Higher educ. Requiring	6.3%	5.9%	0.015	5.6%	5.6%	-0.001
GED requiring	7.6%	7.7%	-0.006	5.8%	5.6%	0.007
Clerical, customer serv.	7.2%	7.1%	0.003	6.6%	7.1%	-0.019
Service, commerce	0.3%	0.7%	-0.052	0.6%	0.5%	0.021
Agricultural	11.0%	12.0%	-0.032	11.0%	10.4%	0.020
Construction, industry	3.5%	3.8%	-0.017	4.4%	4.0%	0.021
Operation, assembly	25.0%	24.7%	0.005	25.5%	27.2%	-0.038
Unskilled	14.9%	13.7%	0.032	14.3%	14.3%	0.000
Armed forces	6.6%	6.4%	0.004	5.5%	7.5%	-0.075
Environment						
Type of settlement (%)						
Village	27.5%	28.6%	-0.025	28.8%	26.8%	0.045
Town	35.7%	40.7%	-0.103	39.5%	42.6%	-0.063
City	21.0%	17.1%	0.104	19.1%	17.6%	0.039
Region (%)						
Central Hungary	28.1%	28.3%	-0.005	26.4%	25.5%	0.022
Central Transdanubia	10.6%	10.7%	-0.003	10.9%	11.1%	-0.008
Western Transdanubia	9.3%	9.4%	-0.003	9.3%	9.6%	-0.007
Southern Transdanubia	9.7%	9.4%	0.008	10.2%	10.6%	-0.013
Northern Hungary	14.1%	11.2%	0.092	12.9%	12.8%	0.003
Northern Plains	15.0%	16.8%	-0.049	16.8%	16.6%	0.006
Southern Plains	13.2%	14.2%	-0.027	13.5%	13.9%	-0.012
Unemployment rate (%)	4.4%	4.4%	0.006	4.6%	4.6%	-0.017
Nursery coverage (%)	11.2%	10.2%	0.106	10.5%	10.0%	0.053
Kindergarten coverage (%)	105.1%	105.0%	0.005	103.5%	102.8%	0.022
Average population	310147	260321	0.085	248879	252224	-0.006
Number of obs.	1732	1577		2975	2868	

Table A2

Standard RD design, regression results

September cutoff								January cutoff							
	Reduced form				2SLS				Reduced form				2SLS		
Variable	1	2	3	Variable	1	2	3	Variable	1	2	3	Variable	1	2	3
T	**0.2365**	**0.236**	**0.2416**	**C**	**0.3039**	**0.303**	**0.3139**	**T**	**0.2531**	**0.2528**	**0.2469**	**C**	**0.3229**	**0.3226**	**0.3181**
	0.0244	**0.0237**	**0.0233**		**0.0259**	**0.0252**	**0.0257**		**0.0252**	**0.0224**	**0.023**		**0.0256**	**0.0232**	**0.0239**
# of children		-0.082	-0.073	# of children		-0.0809	-0.0715	# of children		-0.0928	-0.0866	# of children		-0.0927	-0.087
		0.0204	0.0214			0.0178	0.0186			0.0185	0.0197			0.0171	0.0183
Partner w/o job		-0.013	-0.041	Partner w/o job		-0.0162	-0.044	Partner w/o job		-0.0167	-0.0005	Partner w/o job		-0.02	-0.004
		0.0799	0.0734			0.071	0.065			0.0711	0.0684			0.0635	0.0609
Partner w/ job		0.0576	0.0248	Partner w/ job		0.0536	0.021	Partner w/ job		0.0023	0.0088	Partner w/ job		-0.0044	0.0011
		0.088	0.0804			0.0776	0.0708			0.0641	0.0635			0.0579	0.0572
Vocational school		0.0898	0.0928	Vocational school		0.0833	0.0858	Vocational school		0.1428	0.1523	Vocational school		0.1408	0.1507
		0.036	0.0313			0.0323	0.028			0.035	0.0353			0.0311	0.0311
High school		0.1866	0.1786	High school		0.1843	0.1765	High school		0.2036	0.2068	High school		0.2024	0.2062
		0.0461	0.0374			0.0402	0.0324			0.0403	0.0422			0.0348	0.0362
University		0.3727	0.3534	University		0.3738	0.3551	University		0.3587	0.3545	University		0.3626	0.3593
		0.0528	0.05			0.0477	0.045			0.0492	0.0545			0.0422	0.0466
Age		0.0048	-6E-04	Age		0.0066	0.0015	Age		0.0141	0.02	Age		0.0147	0.0205
		0.0287	0.0301			0.0252	0.0263			0.0191	0.0204			0.0171	0.018
Age squared		0	0.0001	Age squared		-0.0001	0	Age squared		-0.0002	-0.0003	Age squared		-0.0002	-3E-04
		0.0005	0.0005			0.0004	0.0004			0.0003	0.0003			0.0003	0.0003
Partner: University		-0.018	-0.011	Partner: University		-0.0256	-0.0186	Partner: University		0.0165	0.0206	Partner: University		0.0147	0.0195
		0.0545	0.0524			0.0489	0.0469			0.0469	0.043			0.0412	0.0377
Partner: High sc.		0.0099	0.0285	Partner: High sc.		0.0064	0.0246	Partner: High sc.		0.0287	0.0242	Partner: High sc.		0.0321	0.028
		0.0461	0.0463			0.0411	0.0413			0.0422	0.0481			0.0372	0.0422
Partner: Vocationa..		0.0433	0.0543	Partner: Vocationa..		0.0433	0.0544	Partner: Vocationa..		0.0483	0.0452	Partner: Vocationa..		0.0499	0.0474
		0.0371	0.036			0.0328	0.0319			0.0343	0.0365			0.0307	0.0326
Partner's age		-0.003	-0.003	Partner's age		-0.0032	-0.0029	Partner's age		-0.0028	-0.0031	Partner's age		-0.0028	-0.003
		0.0021	0.002			0.0019	0.0018			0.0016	0.0018			0.0015	0.0016
Unemployment level			-1.563	Unemployment level			-1.4188	Unemployment level			-1.5839	Unemployment level			-1.336
			0.5469				0.4886				0.7003				0.6385
Village			-0.143	Village			-0.15	Village			0.1126	Village			0.1006
			0.0502				0.0444				0.0675				0.0602
City			-0.143	City			-0.1516	City			0.1573	City			0.1432
			0.0334				0.0288				0.0592				0.0528
Large city			-0.12	Large city			-0.121	Large city			0.2186	Large city			0.2054
			0.0644				0.0576				0.072				0.0631
Constant	0.3929	0.2407	0.5347					Constant	0.2896	0.0467	-0.0669				
	0.0836	0.4315	0.463						0.0999	0.3198	0.3332				
N	3370	3370	3255					N	3444	3444	3379				
adjusted				N	3196	3196	3076	adjusted				N	3248	3248	3184
r2	0.2627	0.3182	0.3248	adjusted				r2	0.2847	0.3366	0.3332	adjusted			
aic	3833.173	3593.7	3442.7	r2	0.0678	0.137	0.1374	aic	3855.11	3619.85	3567.49	r2	0.0755	0.1426	0.1423

Table A3

RD with resampling design, full reduced form regression results

	Cutoff: January 1						Cutoff: September 1						Cutoff: September 1-December 31					
Specification	1	2	3	1	2	3	1	2	3	1	2	3	1	2	3	1	2	3
Year dummies	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x
	Baseline			Seasonality-corrected			Baseline			Seasonality-corrected			Baseline			Seasonality-corrected		
T*m				**0.064****	**0.064***	**0.060***				**-0.026**	**-0.035**	**-0.027**				**0.039**	**0.035**	**0.041**
				(0.02)	**(0.03)**	**(0.03)**				**(0.03)**	**(0.03)**	**(0.03)**				**(0.02)**	**(0.02)**	**(0.02)**
T	**0.086*****	**0.095*****	**0.082*****	0.012	0.019	0.012	**-0.028**	**-0.030**	**-0.021**	-0.001	0.006	0.004	**0.030**	**0.040***	**0.050***	-0.009	0.004	0.005
	(0.02)	**(0.02)**	**(0.02)**	(0.02)	(0.02)	(0.02)	**(0.03)**	**(0.02)**	**(0.02)**	(0.02)	(0.02)	(0.02)	**(0.02)**	**(0.02)**	**(0.02)**	(0.02)	(0.02)	(0.02)
m				-0.170***	-0.156***	-0.156***				-0.101***	-0.082***	-0.082***				-0.164***	-0.151***	-0.150***
				(0.02)	(0.02)	(0.02)				(0.02)	(0.02)	(0.02)				(0.02)	(0.02)	(0.02)
# of children		-0.124***	-0.117***		-0.127***	-0.122***		-0.123***	-0.116***		-0.123***	-0.124***		-0.151***	-0.148***		-0.131***	-0.132***
		(0.02)	(0.02)		(0.02)	(0.02)		(0.02)	(0.02)		(0.01)	(0.01)		(0.02)	(0.02)		(0.01)	(0.01)
Partner w/o job		-0.007	0.007		-0.005	0.000		0.018	-0.006		0.021	0.012		0.050	0.033		-0.020	-0.031
		(0.06)	(0.06)		(0.04)	(0.04)		(0.09)	(0.09)		(0.07)	(0.06)		(0.11)	(0.11)		(0.04)	(0.04)
Partner w/ job		0.033	0.032		0.038	0.039		0.062	0.028		0.067	0.056		0.099	0.080		0.022	0.011
		(0.06)	(0.06)		(0.04)	(0.04)		(0.08)	(0.08)		(0.07)	(0.06)		(0.10)	(0.09)		(0.04)	(0.04)
Vocational school		0.168***	0.186***		0.168***	0.175***		0.134***	0.141***		0.187***	0.185***		0.129***	0.139***		0.136***	0.135***
		(0.04)	(0.03)		(0.02)	(0.02)		(0.04)	(0.04)		(0.02)	(0.02)		(0.03)	(0.03)		(0.02)	(0.02)
High school		0.233***	0.245***		0.281***	0.287***		0.220***	0.214***		0.276***	0.273***		0.213***	0.209***		0.240***	0.236***
		(0.03)	(0.03)		(0.02)	(0.02)		(0.06)	(0.05)		(0.03)	(0.03)		(0.05)	(0.04)		(0.03)	(0.02)
University		0.365***	0.367***		0.412***	0.412***		0.415***	0.393***		0.410***	0.399***		0.399***	0.380***		0.388***	0.378***
		(0.05)	(0.05)		(0.03)	(0.04)		(0.05)	(0.05)		(0.03)	(0.03)		(0.05)	(0.04)		(0.03)	(0.03)
Age		0.014	0.020		-0.008	-0.004		0.030	0.027		-0.002	0.003		0.025	0.028		-0.013	-0.009
		(0.02)	(0.02)		(0.01)	(0.01)		(0.02)	(0.02)		(0.01)	(0.02)		(0.02)	(0.02)		(0.01)	(0.01)
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
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)
Partner: University		0.078*	0.083		0.052*	0.055*		-0.003	0.010		0.021	0.019		-0.013	-0.009		0.031	0.024
		(0.04)	(0.04)		(0.02)	(0.02)		(0.05)	(0.05)		(0.03)	(0.03)		(0.04)	(0.04)		(0.03)	(0.03)
Partner: High sc.		0.077	0.071		0.085**	0.085*		0.046	0.072		0.068*	0.070*		0.043	0.053		0.083**	0.080**
		(0.05)	(0.06)		(0.03)	(0.04)		(0.07)	(0.07)		(0.03)	(0.03)		(0.06)	(0.05)		(0.03)	(0.03)
Partner: Voc. Sc.		0.066*	0.060		0.074**	0.073**		0.055	0.070		0.041	0.043*		0.048	0.058		0.062**	0.065***
		(0.03)	(0.04)		(0.02)	(0.02)		(0.04)	(0.04)		(0.02)	(0.02)		(0.04)	(0.03)		(0.02)	(0.02)
Partner's age		-0.004*	-0.004*		-0.004***	-0.005***		-0.005	-0.004		-0.004**	-0.004**		-0.005*	-0.005*		-0.004**	-0.004**
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)
Unemp. level			-2.006**			-1.218**			-1.762*			-1.480**			-1.045			-1.195*
			(0.76)			(0.47)			(0.73)			(0.46)			(0.68)			(0.49)
Village			0.218***			0.100**			-0.016			0.170***			0.341***			0.054
			(0.06)			(0.03)			(0.07)			(0.03)			(0.07)			(0.03)
City			0.243***			0.102***			0.004			0.170***			0.305***			0.028
			(0.06)			(0.02)			(0.05)			(0.02)			(0.06)			(0.02)
Large city			0.250***			0.118**			0.028			0.198***			0.327***			0.088*
			(0.07)			(0.05)			(0.06)			(0.04)			(0.09)			(0.04)
_cons	0.467***	0.237	0.074	0.621***	0.778***	0.690**	0.594***	0.114	0.257	0.719***	0.711**	0.585*	0.521***	0.190	-0.067	0.740***	1.003***	0.971***
	(0.10)	(0.40)	(0.37)	(0.05)	(0.22)	(0.22)	(0.07)	(0.25)	(0.28)	(0.03)	(0.23)	(0.26)	(0.07)	(0.36)	(0.39)	(0.04)	(0.21)	(0.22)
Year dummies	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x
r2	0.248	0.317	0.318	0.179	0.27	0.273	0.238	0.314	0.322	0.174	0.259	0.264	0.234	0.307	0.315	0.189	0.267	0.272
aic	3846.88	3551.975	3491.096	10812.093	9758.754	9573.245	3921.809	3595.612	3435.935	10917.77	9946.386	9544.196	3920.509	3611.168	3453.368	10756.71	9855.974	9451.333
N	3309	3309	3244	9152	9152	8982	3344	3344	3229	9183	9183	8871	3296	3296	3181	9142	9142	8830

Table A4

RD with resampling design, full 2SLS regression results

	Cutoff: January 1						Cutoff: September 1						Cutoff: September 1-December 31					
Specification	1	2	3	1	2	3	1	2	3	1	2	3	1	2	3	1	2	3
Year dummies	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x
	Baseline			Seasonality-corrected			Baseline			Seasonality-corrected			Baseline			Seasonality-corrected		
C	**0.110*** **	**0.121*** **	**0.106*** **	0.015	0.024	0.015	**-0.036**	**-0.038**	**-0.027**	-0.001	0.008	0.005	**0.039**	**0.051***	**0.064***	-0.011	0.005	0.006
	(0.03)	**(0.03)**	**(0.03)**	(0.02)	(0.02)	(0.02)	**(0.03)**	**(0.03)**	**(0.03)**	(0.03)	(0.03)	(0.03)	**(0.02)**	**(0.02)**	**(0.03)**	(0.02)	(0.02)	(0.02)
C*m				**0.080** **	**0.081***	**0.076***				**-0.033**	**-0.043**	**-0.035**				**0.049***	**0.043**	**0.052***
				(0.03)	**(0.03)**	**(0.03)**				**(0.04)**	**(0.04)**	**(0.04)**				**(0.02)**	**(0.02)**	**(0.03)**
m				-0.179***	-0.166***	-0.165***				-0.098***	-0.077***	-0.078***				-0.169***	-0.156***	-0.156***
				(0.03)	(0.03)	(0.03)				(0.02)	(0.02)	(0.02)				(0.03)	(0.02)	(0.02)
# of children		-0.123***	-0.116***		-0.126***	-0.122***		-0.123***	-0.116***		-0.123***	-0.124***		-0.151***	-0.148***		-0.131***	-0.132***
		(0.02)	(0.02)		(0.01)	(0.01)		(0.02)	(0.02)		(0.01)	(0.01)		(0.02)	(0.02)		(0.01)	(0.01)
Partner w/o job		-0.008	0.006		-0.006	-0.000		0.018	-0.006		0.021	0.012		0.051	0.035		-0.020	-0.031
		(0.06)	(0.05)		(0.04)	(0.04)		(0.08)	(0.08)		(0.06)	(0.06)		(0.10)	(0.09)		(0.04)	(0.04)
Partner w/ job		0.031	0.031		0.037	0.039		0.062	0.028		0.067	0.056		0.100	0.081		0.022	0.011
		(0.05)	(0.05)		(0.04)	(0.04)		(0.07)	(0.07)		(0.06)	(0.06)		(0.09)	(0.08)		(0.04)	(0.04)
Vocational school		0.167***	0.186***		0.168***	0.174***		0.135***	0.142***		0.187***	0.185***		0.128***	0.138***		0.136***	0.135***
		(0.03)	(0.03)		(0.02)	(0.02)		(0.03)	(0.03)		(0.02)	(0.02)		(0.03)	(0.03)		(0.02)	(0.02)
High school		0.232***	0.245***		0.281***	0.287***		0.221***	0.214***		0.276***	0.273***		0.213***	0.210***		0.240***	0.236***
		(0.03)	(0.03)		(0.02)	(0.02)		(0.05)	(0.04)		(0.03)	(0.03)		(0.04)	(0.03)		(0.03)	(0.02)
University		0.365***	0.368***		0.412***	0.411***		0.415***	0.393***		0.410***	0.399***		0.399***	0.380***		0.388***	0.378***
		(0.04)	(0.04)		(0.03)	(0.04)		(0.04)	(0.04)		(0.03)	(0.03)		(0.04)	(0.04)		(0.03)	(0.03)
Age		0.015	0.021		-0.009	-0.004		0.030*	0.027		-0.002	0.003		0.026	0.029		-0.013	-0.009
		(0.02)	(0.02)		(0.01)	(0.01)		(0.01)	(0.02)		(0.01)	(0.01)		(0.02)	(0.02)		(0.01)	(0.01)
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
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)
Partner: University		0.077*	0.082*		0.052*	0.055*		-0.002	0.010		0.022	0.019		-0.014	-0.010		0.031	0.023
		(0.03)	(0.04)		(0.02)	(0.02)		(0.04)	(0.04)		(0.03)	(0.02)		(0.04)	(0.04)		(0.02)	(0.02)
Partner: High sc.		0.077	0.071		0.086**	0.085*		0.045	0.072		0.068*	0.070*		0.043	0.053		0.083***	0.080***
		(0.05)	(0.05)		(0.03)	(0.04)		(0.06)	(0.06)		(0.03)	(0.03)		(0.05)	(0.05)		(0.02)	(0.02)
Partner: Vocationa..		0.065*	0.060		0.074***	0.073***		0.055	0.070		0.041*	0.043*		0.048	0.057		0.062***	0.065***
		(0.03)	(0.03)		(0.02)	(0.02)		(0.04)	(0.04)		(0.02)	(0.02)		(0.03)	(0.03)		(0.02)	(0.02)
Partner's age		-0.004**	-0.004**		-0.004***	-0.005***		-0.005*	-0.004*		-0.004**	-0.004**		-0.005*	-0.005*		-0.004***	-0.003***
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)
Unemployment level			-1.918**			-1.191**			-1.776**			-1.485***			-0.986			-1.179*
			(0.68)			(0.45)			(0.65)			(0.44)			(0.60)			(0.46)
Village			0.214***			0.101***			-0.014			0.169***			0.343***			0.055
			(0.06)			(0.03)			(0.06)			(0.03)			(0.07)			(0.03)
City			0.236***			0.103***			0.006			0.170***			0.306***			0.029
			(0.05)			(0.02)			(0.05)			(0.02)			(0.05)			(0.02)
Large city			0.246***			0.119**			0.029			0.198***			0.330***			0.089*
			(0.06)			(0.04)			(0.06)			(0.03)			(0.08)			(0.04)
Year dummies	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x
r2	0.023	0.112	0.114	0.025	0.133	0.136	0.013	0.111	0.111	0.017	0.118	0.12	0.01	0.105	0.106	0.027	0.121	0.123
aic	3785.051	3511.442	3449.452	10793.06	9750.067	9562.318	3856.854	3551.134	3394.993	10895.52	9933.523	9529.204	3844.547	3553.291	3395.93	10721.53	9829.471	9423.95
N	3116	3116	3054	9087	9087	8914	3152	3152	3037	9112	9112	8797	3120	3120	3000	9064	9064	8748

Table A5

Robustness check: Robustness check: resampling for each age group

	Child age						
	Year1	Year2	Year3	Year4	Year5	Year6	Year7
T	**0.021**	**0.009**	**0.082*** **	**-0.010**	**0.009**	**-0.009**	**0.008**
	(0.01)	**(0.01)**	**(0.02)**	**(0.03)**	**(0.02)**	**(0.02)**	**(0.02)**
# of children	-0.021**	-0.048***	-0.117***	-0.120***	-0.171***	-0.210*	-0.190*
	(0.01)	(0.01)	(0.02)	(0.03)	(0.05)	(0.10)	(0.09)
Partner w/o job	-0.020	-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	-0.030	-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 schoo	-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	0.010	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	0.020	-0.005	-0.039	-0.017	-0.013
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.04)
Age squared	-0.000	-0.000	-0.000	0.000	0.000	0.000	0.000
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Partner: Universi	0.027	0.021	0.083	0.030	0.074	0.009	0.077
	(0.02)	(0.04)	(0.04)	(0.05)	(0.04)	(0.05)	(0.05)
Partner: High sc.	0.020	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: Vocatio	0.009	0.028	0.060	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	-0.000	0.002	-0.004*	-0.006*	-0.003	0.002	0.000
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Unemployment l	0.341	0.207	-2.006**	-0.092	-2.795***	-1.679*	-1.040
	(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)
Year dummies	x	x	x	x	x	x	x
r2	0.177	0.213	0.318	0.369	0.403	0.366	0.406
aic	-2578.996	2055.402	3491.096	2579.223	2258.491	2197.612	1831.307
N	3796.000	3688.000	3244.000	2883.000	2853.000	2666.000	2603.000