Does Subsidized Childcare Matter for Maternal Labor Supply? A Credible Cutoff-Based Estimate at a Policy-Relevant Point

Anna Lovász*

Ágnes Szabó-Morvai**

April 12, 2015

Abstract: We use IV estimation based on a kindergarten eligibility cutoff to provide a high internal validity causal estimate of the effect of subsidized childcare availability on mothers' labor supply. Contrary to prior cutoff-based studies, we estimate at a child age when the mothers' activity rate is still well below that of women, thus lack of childcare is potentially a binding constraint, and policy intervention may be effective. Our methodology ensures that similar individuals are compared, and possible seasonal effects are corrected for using difference in differences. The results show that access to subsidized childcare increases maternal participation by 19 percent.

Keywords: Subsidized Childcare, Maternal Labor Supply, Instrumental Variables

JEL codes: H24, J13, J22

This research was funded by OTKA grants KJS-K-101665/2011 and KJS-K-101862/2011. The authors would like to thank Andrea Canidio, John S. Earle, Péter Elek, Gábor Kézdi, János Köllő, Miklós Koren, Botond Kőszegi, Shelly Lundberg, Sergey Lychagin, Nándor Német, Ágota Scharle, Dick Startz, Ádám Szeidl, Álmos Telegdy and Balázs Váradi for their valuable advice. Participants of seminars at Central European University, Eötvös Lóránd University, HÉTFA Research Institute, the Centre for Economic and Regional Studies of the Hungarian Academy of Sciences, University of Washington, the Szirák Conference on Labor Market Research, the Visegrad 4 Workshop on Childcare organized by the Budapest Institute, and the EALE 2014 Conference (Ljubljana) provided us with many useful comments. The authors are grateful to the Data Bank at the Centre for Economic and Regional Studies.

*Institute of Economics, Centre for Economic and Regional Studies, Hungarian Academy of Sciences, and Department of Economics, Eötvös Lóránd University, lovasz.anna@krtk.mta.hu

**Central European University, and HÉTFA Research Institute, szabomorvaiagnes@hetfa.hu

I. INTRODUCTION

Encouraging higher labor market participation of women, especially mothers of young children, is an important policy goal in most countries.[1] The possible range of policy tools is varied, but recent consensus among policymakers is that the expansion of subsidized childcare is an important component.[2] To find the most effective mix of policies and forecast the benefits of investment in childcare expansion, it is important to estimate the impact of childcare on mothers' labor supply precisely. We provide a credible causal estimate at a child age when mothers' activity rate is still well below that of women, and lack of affordable childcare may be a binding constraint of mothers' return to the labor market. We utilize a kindergarten eligibility cutoff point based on the date of birth, which ensures random selection into treatment, and follow the instrumental variables (IV) logic of Angrist and Krueger (1991). As pointed out in Bound and Jaeger (1994), this method may suffer from seasonal biases if the window around the cutoff is too wide. In our case, concurrent child age-related changes near the cutoff may exacerbate the bias. We address these estimation issues and find strong evidence of a significant positive effect.

Figure 1.a. illustrates the activity rate of the focus of our analysis, Hungarian mothers, over the age of their youngest child. It shows a low rate prior to age 3 (when kindergarten enrollment begins), followed by a sharp increase, levelling off at age 4. This steep rise in activity is due to several factors that change simultaneously with childcare availability around age 3 of the child:

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

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

parental leave ends, and preferences regarding the separation of mothers from their children change. Due to data constraints, we cannot define treatment and control groups with narrow windows around the cutoff. As a result, if the groups are observed at a single point in time, it is not possible to ensure their similarity in terms of these other age-related aspects. We therefore select the estimation sample so that we observe both groups of mothers when their children are the same age instead (using longitudinal data). This ensures that the groups do not differ in terms of other age-related factors, only in childcare availability. Seasonal bias may arise because of differences in the children's birth dates and in the observation dates of mothers' labor status. To correct for this, a difference in differences (DID) model is estimated, based on groups of mothers of 4-5-year-olds who are subject to the same seasonal effects, but no childcare effect.

Figure 1: The activity rate of mothers in Hungary, by the age of their youngest child

Figure 1.a: All mothers

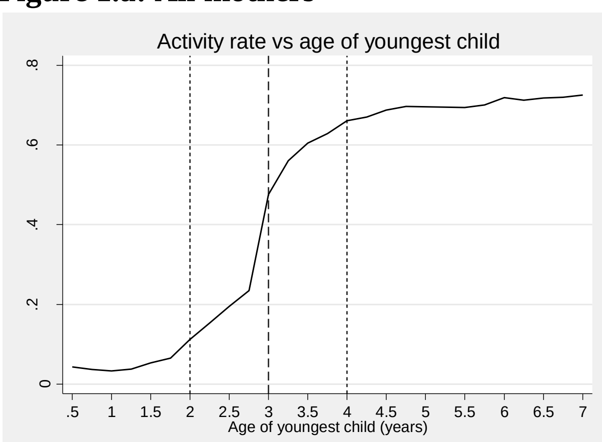
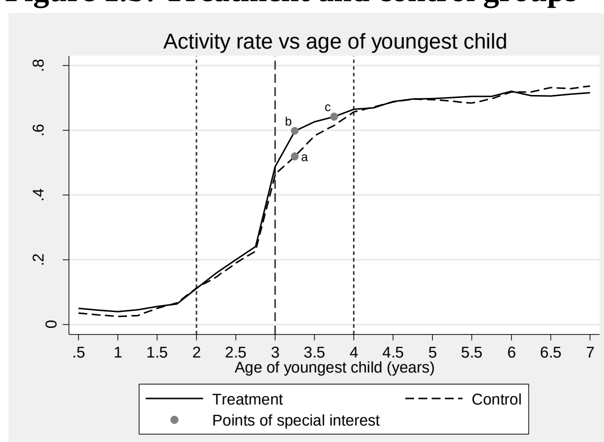


Figure 1.b: Treatment and control groups



Source: Hungarian Labour Force Survey, 1998-2011.
Note: Treatment group refers to mothers of children born 1st August- 31st December. Control group refers to mothers of children born 1st January-31st May.

The results point to an estimated childcare effect of 0.095, significant at the 5 percent level. 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 19 percent (based on their baseline activity rate of around 50%). Figure 1.b. gives strong preliminary evidence of this significant effect: it shows that the activity rates of the treatment and control groups move together as children grow older, except for a period following age 3, when the treatment group's is higher for a while. This corresponds exactly to the period when the group gains access to subsidized kindergarten while the control group does not, suggesting that childcare availability positively impacts mothers' labor supply. Other age-related factors affect both groups similarly, and should therefore not lead to the observed deviation in the activity rates.

Our results illustrate that estimates of the childcare effect are highly dependent on (a) whether the estimation method relies on exogenous change in childcare availability for identification, (b) at which stage of mothers' post-birth labor market reactivation the estimation is made, and (c) whether concurrent age-related changes are accounted for in cutoff-based estimates. Of the numerous previous estimates available from various countries, the most common are those based on structural models that use time or regional variation for identification. These have the advantage of being able to control for fertility and other types of selection biases, however, they are based on strict behavioral and distributional assumptions, and are likely to suffer from endogeneity bias.[3] Previous evidence from these studies is ambiguous.[4] The second strand of the

[3] Unobserved characteristics in the error term - mainly individual and regional - make childcare availability endogenous in the labor supply equation (e.g. migration between settlements, or the economic development of settlements), and most of these introduce an upward bias. For instance, in better developed regions, labor market opportunities are better, and demand for childcare institutions is higher, with more resources available for increasing capacities.

[4] Several support the existence of a negative effect of childcare costs on participation or employment (Lokshin 2004, Borra 2010, Kimmel 1992, Connelly 1992, Haan and Wrohlich 2011, Del Boca 2002), while others find little or no significant effect (Chevalier and Viitanen 2002, Chone, Le Blanc, and Robert-Bobee. 2003, Ribar 1995). The evidence from these studies varies not only because of differences in methodology and data, but also the age of the children analyzed, and cross-country differences in institutional and hard-to-observe preferential factors (Blau 2003).

literature, seeking a source of exogenous change for identification, uses difference in differences methods based on various policy changes. These require fewer assumptions and may eliminate the omitted variables bias, however, they are based on the crucial assumption that the policy change is exogenous, which may not be valid.[5] These also give ambiguous results.[6]

In the third group of studies, enrollment eligibility cutoffs based on birthdates provide a promising opportunity for estimating a credible causal effect, in that they create truly exogenous variation in childcare availability. The internal validity of these estimates is high, however, this comes at the cost of limited external validity, since they measure a local treatment effect. To our knowledge, so far only a few studies from the US used enrollment cutoffs to estimate the effect of childcare on mothers' labor supply. Gelbach (2002) analyzed Census data on 5-year-olds, using quarter of birth as an instrument for kindergarten enrollment, finding a significant positive effect of 6-24%. However, as shown later in our case as well, using such a wide window around the cutoff and assuming that the child's age is unrelated to the mother's labor supply leads to an upward bias in the estimate (Fitzpatrick 2010, p.58.). The method we propose avoids such bias by separating the childcare effect from age-related effects.

Using more detailed restricted-access Census data, Fitzpatrick (2010) carried out a standard regression discontinuity (RD) analysis, and found that at age 4 of children, subsidized pre-kindergarten eligibility has no significant impact on maternal labor supply. The difference

[5] Policy decisions about subsidized childcare supply may be endogenous as well if they depend on local childcare demand and related political pressures.

[6] Some policy change-based studies find a significant positive impact (Baker, Gruber, and Milligan 2008, Lefebvre and Merrigan 2008, Hardoy and Schone 2013), while others find none (Cascio 2009, Lundin et al. 2008). Baker et al. (2008) note that the estimated elasticities from policy change based studies (Berger and Black 1992, Gelbach 2002, Herbst 2008, Cascio 2009) are at the lower end of the range of estimates based on structural models Blau (2003).

between this result and that of Gelbach (2002) is likely due to the elimination of other child age-related effects (more narrow windows around the cutoff). On the other hand, the difference from our results reflects the importance of the point (child age) the childcare effect is estimated at. From a policy point of view, only points at which a considerable fraction of mothers are still outside the labor market, and therefore policies have potential impact, are relevant. By age 4 of children, mothers in the US have almost reached the overall rate of female employment (~70% vs. ~72%),[7] while Hungarian mothers of 3-year-olds are still quite far from the overall female activity rate (~47% vs. ~67%)[8] at the cutoff. Consequently, it is not surprising that no effect is found in the US case, while our estimates show a significant childcare impact.

By using an eligibility cutoff, eliminating bias from other age-related changes, and estimating at a point where an increase in childcare availability still has the potential to re-activate a significant portion of mothers, we provide policy-relevant new evidence of the childcare effect. At the same time, given the above considerations, our results are in line with the most relevant preceding literature in which cutoff-based estimates are given (Gelbach 2002, Fitzpatrick 2010). Evaluated with the Hungarian institutional context in mind, they suggest that childcare expansion has a significant impact despite potentially limiting factors, such as the lack of flexible work opportunities,[9] long maternity leave, cultural expectations, and their interactions.[10] However, childcare expansion only explains one third of the overall increase seen in mothers' activity rates

[7] Fitzpatrick (2010, p.11), and US Bureau of Labor Statistics (25-54-year-old females).
[8] Hungarian Labour Force Survey, and Eurostat (25-54-year-old females).
[9] The availability of part-time and telecommuting work is very limited in Hungary (and the CEE region), while childcare facilities have rigid hours of operation.
[10] The overall length of maternity and parental leave in Hungary is 3 years, which mothers may interpret as a signal of when they should return to work, referred to as an anchor effect by Kluve and Schmitz (2014). Societal views appear to be in line with this idea (Blaskó 2011).

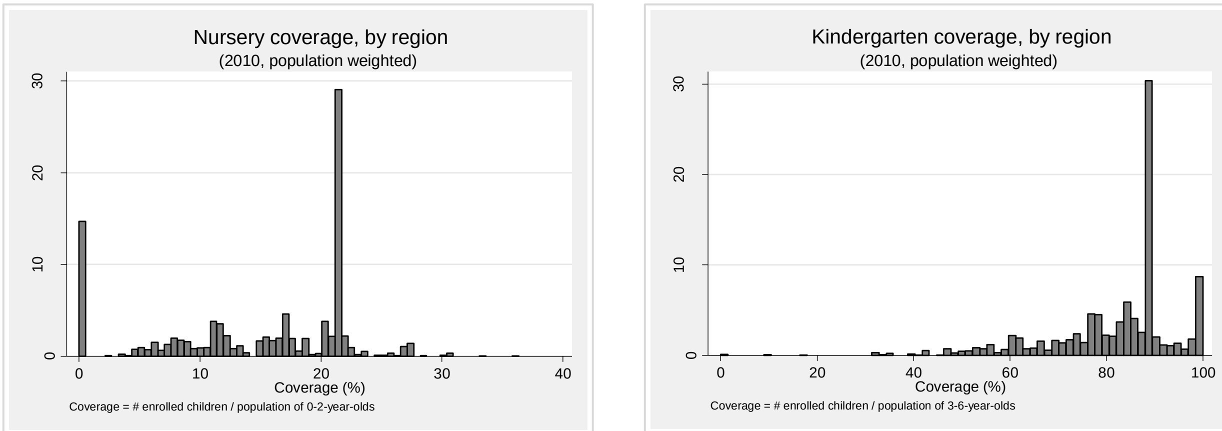
around age 3 of children (Figure 1), highlighting the importance of considering various policies - as well as their interactions and the signals they may send - in a comprehensive manner.

II. INSTITUTIONAL FRAMEWORK

In Hungary, private childcare is relatively expensive and unaffordable for many people, so subsidized state-run institutions provide the primary form of childcare.[11] Subsidized nursery schools accept children of age 5 months to 3 years, while kindergartens accept children from age 3 to 6 in the analyzed period. We disregard differences in the type of service these institutions provide in terms of child development, as we are only considering their role in terms of freeing up mothers' time. The coverage rate[12] of kindergartens is significantly higher (74.2% on average) than that of nursery schools (10.2% on average). Figure 2 illustrates the township-level distributions of state-run nursery and kindergarten coverage rates, illustrating the low availability of nurseries, the significantly higher availability of kindergartens, and their high regional variability. Those who are eligible for kindergarten face and perceive a significantly higher supply of subsidized childcare than those who are only eligible for nursery school.

[11] The data allows us to control for some other childcare options as well: we include a proxy the presence of a grandparent in the household among the controls. As expected, since mothers should not differ in this respect based on their child's birthdate, our estimates are unchanged.
[12] Coverage is defined as the number of children enrolled/children of the given age in the population. The calculation of the coverage rate used in the analysis is described in detail in the Data section.

Figure 2: Distribution of nursery and kindergarten coverage rates by township



Source: T-STAR Hungarian township level dataset, 2010.

Note: Coverage rate is defined as the number children enrolled in nursery/kindergarten in each township, divided by the number of children of relevant age (0-2.99 for nursery, 3-5.99 for kindergarten) in each township. Townships are merged based on data on commuting to childcare facilities (based on Kertesi et al. 2012), there are 530 of these.

The kindergarten school year begins in September. The main eligibility rule for subsidized kindergartens is that children who turn 3 prior to September 1st may enroll starting September 1st, and those born after may enroll as long as there are open spots available. In practice, spots are not usually filled by those born prior to September 1st, which means that children born between September 1 and December 31 are able to enroll in January. However, as free spots are filled by then, those born after December 31st are only able to enroll next September. The cutoff date relevant to our estimation is therefore the 1st of January.[13] Our analysis is based on two groups defined around this cutoff: treatment mothers, whose children are born prior to January 1st, are mostly able to enroll their children in kindergarten in January; and control mothers, whose children are born soon after January 1st, are only able to enroll in kindergarten next September. Although kindergartens consider further characteristics in admission decisions - such as income

[13] The January 1st cutoff date is also confirmed by data on individual-level childcare usage from the EU-SILC dataset (see Appendix Figure A1): the largest gap in enrollment rates occurs between those born before and after January 1st.

status, the presence of siblings, or the mothers' labor market status - these do not differ on average between the two groups, and do not affect our analysis.

There are some other factors that are relevant to mothers' labor supply that change around age 3 of the child. The first of these is flat-rate parental leave, which is received by each mother when the child is 2-3 years old, the period of interest in our analysis.[14] One parent in each family is entitled to it, however, the overwhelming majority (98.1%) is taken by mothers.[15] The amount of the parental leave is low (it was 23.4% of the average female wage in 2008), however, it may still have an impact on the labor supply decision of mothers with low expected wage or employment probability. Furthermore, the length of parental leave may be taken as an institutional signal (referred to as the anchor effect, see Kluve and Schmitz (2014)) regarding the "proper", socially accepted time for separation from the child, affecting mothers' preferences (discussed next).

In addition to the institutional factors, we also have to consider the role of preferences regarding separation from the child, which also change around age 3 of the child. Parents generally become gradually less attached as the child grows older, however, in the case of Hungary, there is reason to think that these preferences change sharply at age 3. A survey by Blaskó (2011) suggests that the majority (56.4%) of people believe age 3 is the earliest acceptable time for a mother to leave the child and return to work, while 19.6% responded age 2, and 19.7% gave a later age than 3. It is very likely that there is a correlation between the institutional setting and societal preferences

[14] Flat-rate parental leave is universal: it can be received by anyone, with high or low previous income, whether they were insured previously or not. The sum of this benefit equals the old-age pension minimum. Parental leave also provides basic health insurance and social security payments.
[15] According to Hungarian Labor Force Survey (H-LFS) data, between 1996 and 2011, a mere 1.9% of those receiving parental leave payments were males.

regarding the 3rd birthday is an important deadline (Hasková, Győri, and Szikra (2012)). Whether this change in preferences 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, due to employers assuming that mothers will be more reliable (absent less due to child illness, etc.) after this age, or other factors, it means that preference change should be accounted for in the estimation methodology.

III. DATA AND METHODOLOGY

III.1. DATASET

The primary source of the data used in the analysis is the Hungarian Labor Force Survey (H-LFS). It is a rotating panel dataset, which consists of individual-level data of all members of the household, which is the unit of observation. Approximately 17% of the households are rotated in each quarter, thus the maximum length of observation time is 1.5 years. The sample is representative of Hungary; sample weights based on the data of the Hungarian Central Statistical Office (CSO) are used. Our estimation sample includes mothers with or without a partner, for the years 1998-2011. Throughout the analysis we refer to the age of the youngest child in the family as child age,[16] and include mothers with 1 or more children. We exclude fathers from the

[16] It is important to emphasize that we always examine the youngest child, as only mothers who don't have an even younger child are likely to be affected by subsidized childcare availability for their 3-year-old. It may occur that expectant mothers are also included in the sample, if the birth occurred after the last observation in LFS. These mothers most probably do not plan to return to the labor market, irrespective of childcare availability. However, this does not bias the results, as the probability of their inclusion is the same in the treatment and the control group.

analysis, since, as described in the previous section, in Hungary it is rare that the father stays at home with the child and the mother goes back to work.

In the H-LFS dataset, detailed demographic and labor market data are included about each individual. We use information on the individual's labor market participation as our labor supply measure and include individual (e.g.: age, schooling, occupation), family (number of children, husband's labor market status), and regional[17] (settlement type, region, local unemployment) characteristics linked from the T-STAR regional dataset as controls (the full set of variables included can be seen in Appendix Table A1.). We use a dummy variable for participation as our dependent variable in the estimation.[18] Individuals are classified as participating in the labor market 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).

The focus variable of our analysis is childcare availability; however, it is not straightforward how this should be measured: as actual usage at the individual level, or availability at the regional level. The effect of childcare supply on labor supply can be illustrated with the following equation:

$$\frac{\partial L^S}{\partial C^S} = \frac{\partial L^S}{\partial C^D} \cdot \frac{\partial C^D}{\partial C^S} \qquad (1)$$

[17] The individual level LFS data is linked with T-STAR township level regional data on regional characteristics via township codes.

[18] We also use employment as an alternative dependent variable, as discussed in the results section. We do not consider changes in hours of employment, as part-time work is rare in Hungary, so choices are mostly made on the extensive margin.

The supply of subsidized childcare (C^S) is the number of available subsidized places (in the relevant township and year). Our expectation is that the more places are available, the more mothers decide to apply for childcare: $\frac{\partial C^D}{\partial C^S} > 0$. At the same time, we expect that the more mothers decide to enroll, the more of them will decide to return to the labor market: $\frac{\partial L^S}{\partial C^D} > 0$. Some articles (e.g. Del Boca (2002)) model these as simultaneous decisions. Since both parts of the process are equally important from a policy point of view, we carry out an intent-to-treat analysis and focus on the full effect of regional childcare availability.

Childcare availability (termed childcare coverage) variables are defined as the number of children enrolled in nursery/kindergarten in each township, divided by the number of children in the relevant age group (0-2.99 for nursery school/3-5.99 for kindergarten) in the population of the township.[19] As seen in Figure 2, kindergarten coverage is significantly higher (74.2%) than nursery coverage (10.2%).[20] A single coverage variable used in the regressions is created from the nursery and kindergarten measures based on individual eligibility: for mothers with children eligible for kindergarten, it is coded as the kindergarten coverage rate, and for those whose children are not, it is coded as the nursery coverage rate in their township. This variable measures

[19] Townships are defined taking commuting to childcare institutions into account, based on data collected in previous work by Kertesi et al (2012). Enrollment statistics are reported by the childcare institutions and collected by the Central Statistics Office, enrollment and population data come from the T-STAR regional dataset. Coverage variables for each year are merged with the LFS data based on township codes. The distributions of the coverage variables are shown in Figure 2 for the year 2010.

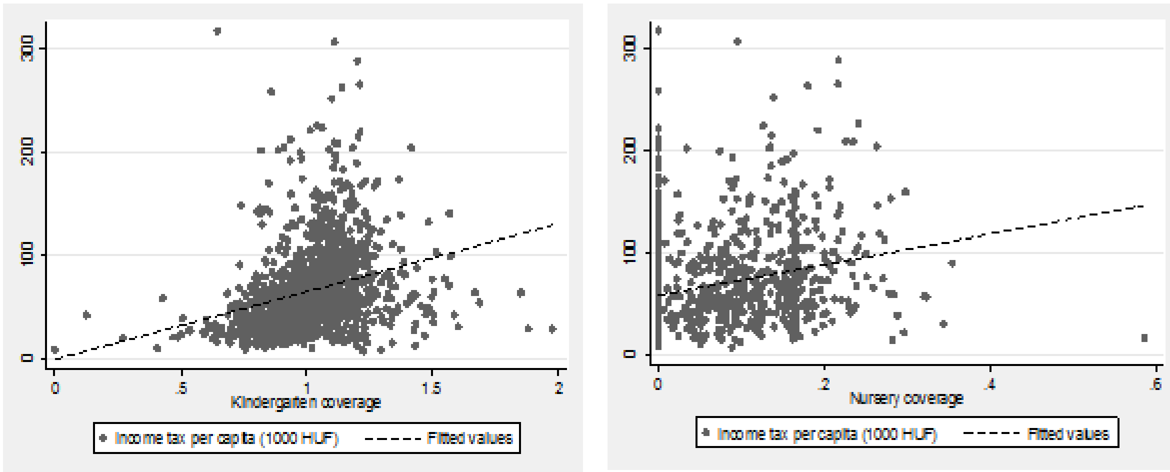
[20] It is possible that the difference between nursery and kindergarten coverage is lower if we only consider children closer to the cutoff age of 3: children just under 3 are more likely to get into nurseries than younger ones, and children just over 3 may be less likely to get into kindergarten than older ones. This would mean that our measure of the difference in coverage is biased upwards, and therefore, we underestimate the impact of childcare availability on labor supply. The T-STAR dataset does not contain statistics by more narrow age groups, but statistics based on individual level usage of childcare from the 2011 Census show similar rates for more narrow age groups, therefore, this bias should not be significant.

the size of subsidized childcare supply perceived by a mother (the individual probability of access to subsidized childcare) in a given township and year.

III.2. The instrumental variable

To estimate the effect of childcare availability on the labor supply of mothers, one could simply run an OLS regression with participation as the dependent variable, and the childcare measure and controls as the explanatory variables. However, there is strong reason to suspect that this estimate would be biased, due to the endogeneity of childcare availability: it is likely to be correlated with various observed and unobserved factors. For instance, consider economic development, as seen in Figure 3: the scatterplots demonstrate the positive relationship between income tax per capita (a proxy for GDP per capita) and childcare coverage rates. The reason for this relationship may be that governments with ample resources can afford to maintain more subsidized childcare places. At the same time, in a more developed township, labor demand is stronger, which may affect mothers' labor supply positively through their expectations. As a result, residents facing stronger labor demand may require more subsidized childcare places to be maintained.

Figure 3: Correlation between childcare availability and economic development



Source: T-STAR dataset, 2010.
Note: Coverage rate is defined as the number children enrolled in nursery/kindergarten in each township, divided by the number of children of relevant age (0-2.99 for nursery, 3-5.99 for kindergarten) in each township. Townships are merged based on data on commuting to childcare facilities (based on Kertesi et al. 2012), there are 530 of these.

The basic idea of our cutoff-based methodology is to use the birth date of the child as an instrumental variable for childcare availability to correct for such bias. The instrumental variable is defined as follows:

$$T_i = \begin{cases} 1 \quad \text{if} \qquad 1^{\text{st}}\text{August} \leq \mathrm{b_i} \leq 31^{\text{st}}\ \text{December} \\ 0 \quad \text{if} \qquad 1^{\text{st}}\ \text{January} \leq \mathrm{b_i} \leq 31^{\text{st}}\text{May} \end{cases} \qquad (2)$$

where b_i is the date of the third birthday of the mother's youngest child, and 1^{st} January is the cutoff date. In our case, mothers with children born between August 1^{st} and December 31^{st} are in the treatment group, for whom T=1, and those with children born between January 1^{st} and May 31^{st} are in the control group, for whom T=0. This instrument is strongly correlated with childcare availability, since treatment group mothers are mostly able to enroll their children in kindergarten (74% coverage), while control group mothers are only eligible for nursery school, which has significantly lower coverage (10%). At the same time, T should not be correlated with other individual and regional characteristics that influence the mothers' labor supply, if the birthdate of children – at least around the cutoff date - is random.

III.3. SAMPLING DESIGN AND SEASONALITY CORRECTION

It is easy to see that the selection of mothers into the groups can be regarded as random if the bandwidth around the cutoff is narrow enough: mothers of children born on December 31st are very similar to those born on January 1st. In our case, due to small sample size and the imprecise data on birth dates, we have to define groups as those with children born 5 months before and after the cutoff date. The wider windows around the cutoff mean that we need to consider certain age-related factors more carefully: as outlined previously, around age 3 of the child not only does childcare availability increases, parental leave also ends, and the willingness to separate from the child grows rapidly. These changes can lead to significant differences between the groups if they are compared in a cross-section of data, because the average age of children in the two groups differs significantly.[21]

In order to separate out these other effects from the childcare effect, we define the estimation sample so that we include mothers in the treatment and control groups with equal average child age. We utilize the longitudinal nature of the data, and observe the groups at different dates,

---

[21] With 5 month windows, child age differs by an average of 5 months between the two groups at any single point in time, so the effects of these differences may be significant. For example, by 1st June, parental leave had ended an average of 7.5 months ago for treatment group mothers, and only 2.5 months ago for control group mothers. Depending on how the end of parental leave affects mothers' activity, this may mean that the two groups differ significantly due to this difference as well. Preferences regarding separation may also change significantly during 5 months.

namely, we observe each group in the quarter after their child turned 3.[22] This sampling design ensures that the effect of parental leave and separation preferences will be the same on average in the two groups. The only difference left between them is therefore the difference in childcare availability.

In this setup, the treatment and the control groups differ notably in terms of both their dates of birth and of observation, which may introduce seasonal bias of various forms. First, Bound and Jaeger (1996) claim that quarter of birth may be associated with various individual characteristics. They cite Kestenbaum (1987), who find that parents with higher incomes tend to have spring babies. Second, child development may differ by season of birth, which may influence the mother's willingness to separate from the child. For instance, Currie and Schwandt (2013) show that even after controlling for maternal characteristics, health status and weight at birth depend on the season of birth. The third possible bias is related to the different dates of observation: labor demand varies seasonally as well, which affects the actual and expected probability of employment, and thereby, the labor supply of mothers.

In order to ensure that we measure the effect of childcare availability but not that of these seasonal factors, we expand the sample with reasonably close labor market substitutes, mothers of children aged 4-5 years (separated into two groups based on the same cutoff date), and run a difference in differences (DID) regression. 4-5 year old children already have access to kindergarten, irrespective of their birth date, so these comparison groups should be affected by

[22] This means that for the treatment group, mothers whose youngest child was born August-December are included in the sample in the quarter when the child is between 3-3.5 years old. For the control group, mothers whose youngest child was born January-May are included in the sample in the quarter when the child is between 3-3.5 years old.

the same seasonal effects, but not the treatment effect, allowing us to separate out seasonal factors.[23] Any difference between the two groups of mothers with 4-5 year olds should be the result of the seasonal factors mentioned above. We construct a variable indicating the original and the comparison sample:

$$m_i = \begin{cases} 1 & if\ 3 \leq a_i < 4 \\ 0 & if\ 4 \leq a_i < 6 \end{cases} \quad (3)$$

where a_i indicates the age of the youngest child. This means that we have four groups of mothers in our analysis, as summarized in Table 1.

Table 1: Definition of groups in the IV+DID analysis

	Treatment Born August-December	Control Born January-May
3-year-old child	T=1, m=1	T=0, m=1
4-5-year-old child	T=1, m=0	T=0, m=0

III.4. DESCRIPTIVE STATISTICS

The most important descriptive statistics are presented in Table 2 for all four groups of mothers. The full summary statistics table is given in Appendix Table A1. This table already demonstrates the main message of this paper: as it can be seen, the means of most variables are very similar in the treatment and the control groups. They do not differ in most characteristics, except for the dependent variable (activity rate) and the variable of interest (childcare coverage). This supports

[23] According to some tests not reported here, the seasonal effects suffered by the different age groups are similar.

that, indeed, selection into the groups, i.e. the birthdate of children, can be regarded random, and the compared groups are similar on average apart from treatment status. The largest difference among the groups lies in the type of living place. The treatment group is 3.8 percentage points more likely to live in a city than the control group. Although this difference is not huge, it suggests that the DID seasonality correction may be important. Differencing is likely to capture seasonal differences, as the comparison groups of mothers with 4-5-year-old children show a similar pattern.

Table 2: Summary statistics of the estimation sample by group

	Child of age 3 (m=1)			Child of age 4-5 (m=0)		
	Treatment	Control	P-value of t-test	Treatment	Control	P-value of t-test
Activity rate (%)	57.88	49.65	0.0000	65.66	65.01	0.6041
Childcare coverage (%)	74.2	10.2	0.0000	74.2	74.2	
Number of children	1.28	1.31	0.1235	1.12	1.14	0.0908
Age of youngest child (years)	3.32	3.32	0.1965	4.81	4.82	0.5480
Age (years)	30.37	30.45	0.6339	31.83	31.90	0.6278
Primary school (%)	27.1	26.6	0.7884	27.0	26.9	0.9891
Vocational school (%)	28.3	28.5	0.8577	28.9	26.7	0.0617
High school (%)	29.8	31.3	0.3506	31.8	32.6	0.5043
University (%)	14.7	13.3	0.2594	12.1	13.5	0.1124
Partner’s age (years)	29.68	29.87	0.6625	31.02	30.79	0.5058
Village (%)	29.8	30.3	0.7865	30.7	27.7	0.0155
City (%)	47.9	51.7	0.0299	50.6	54.3	0.0050
Large city (%)	14.4	11.5	0.0134	12.7	11.4	0.1370
Unemployment rate (%)	5.1	5.1	0.9029	5.1	5.2%	0.5600
Number of observations	1,667	1,577		2,869	2,867	

Source: Hungarian Labour Force Survey, 1998-2011.
Note: Child age refers to the age of the youngest child. Treatment group includes mothers whose children are born August-December, control group those with children born January-May. Welch’s (independent samples) t-test is calculated to test for the equality of each variable’s mean between the treatment and the control groups, the p-values of the two-tailed test are presented.

III.5. REGRESSION SPECIFICATION AND ESTIMATION ISSUES

In our preferred specification, which includes the seasonality correction (termed IV+DID), we run a 2SLS regression where the first stage is:[24]

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

Where

$$C_{yri} \equiv p_{yr}^n (1 - T_i) + p_{yr}^k T_i \,.$$

The subscripts indicate yearly (y), regional (r), and individual (i) variation: p_{yr}^n is nursery school coverage and p_{yr}^k is kindergarten coverage in township $r,$ and year y. C_{yri} is the regionally aggregated childcare coverage in township r and year y for the relevant group. We can think of C_{yri} as the probability that individual i has access to subsidized childcare. The equation further adjusts for a set of individual (X_i) and regional covariates (S_{yr}), α_y represents year fixed effects, and γ_r region fixed effects. The second stage regression is given by:

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

Where $\widehat{C_{yri}}$ represent the fitted values of C_{yri} from the first stage regression. In this setup, the parameter β_1 (the coefficient of T*m) reflects the first-stage effect of treatment on C, i.e. how much group membership determines childcare availability. The parameter β_2 in the second stage is the main parameter of interest: it shows the estimated effect of childcare availability on labor

[24] Note that because of the difference-in-differences setup, there are two endogenous regressors (C and C*m), thus, technically, we run two first stage regressions. For their results, see Appendix Table A2.

supply, net of any seasonal effects.[25] The parameter π_{24} (the coefficient of m), reflects the overall change in the activity rate of mothers between age 3 and age 4-5 of their youngest child.

The corresponding reduced form equation is:

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

The parameter β_R (the coefficient of the interaction term T*m), captures the reduced form effect of T_i on L_i, free of the seasonal effects. It can be interpreted as representing how much more active mothers are if they belong to the treatment rather than the control group, i.e., if they are eligible for kindergarten rather than nursery school, which has significantly lower coverage. The coefficient of T, π_{R3}, captures any differences due to being born before or after the cutoff that are common to mothers of 3 year olds and 4-5 year olds. Seasonal differences are reflected in this coefficient. The coefficient of m, π_{R4}, shows the difference in the activity rate of others of 4-5 year olds compared to 3 year olds that is the same for all birthdate groups, which we expect to be positive and significant since activity increases with child age (as seen in Figure 1).

There are some important concerns regarding the consistency of the estimates as related to the methodology applied in the analysis. First, regarding the measurement error of the childcare availability variable: the actual probability of access to subsidized childcare differs from the coverage measure used, due to specific acceptance rules of the institutions and the individual's characteristics. For instance, disadvantaged mothers may have a higher chance of acceptance.

[25] For purposes of comparison, we also run simple IV regressions (denoted IV) with no seasonality correction. The sample is then limited to the treatment and control groups of mothers of 3 year olds (observed in the quarter after the 3rd birthday, as in the IV+DID case), thus there is no m or interaction term on the right hand side. In this case, the coefficient of the coverage variable C is our focus.

This means that the childcare availability variable is measured with error, and a simple OLS regression would provide biased coefficient estimates. However, as discussed in the background section, this error should not differ among treatment and control groups, and should therefore not bias the IV results

Second, we analyze the assumptions of the instrumental variables method. T_i is a valid instrument, because it is uncorrelated with local economic development and demographic variables that may affect labor supply, as discussed above (see Table 2). On the other hand, it is a strong instrument, that is, it is strongly correlated with childcare availability due to eligibility differences before and after the cutoff: first stage results (Appendix Table A2) also confirm the strength of the instrument.[26]

Third, we discuss assumptions of the DID method. For DID estimates to be consistent, it is crucial that $Cov(\xi_{Ryri}, T_i) = 0$, $Cov(\xi_{Ryri}, m_i) = 0$ and $Cov(\xi_{Ryri}, T_i m_i) = 0$. Based on our discussion above and the group statistics (Table 2), the first and second conditions can safely be assumed to be valid. It is a common solution to check for the third condition - which is also referred to as the parallel-trend assumption - by doing placebo treatment regressions. We ran such regressions with various placebo cutoffs, and found that the estimated effect is insignificant (the results are omitted, but available upon request), thus the assumption is likely to hold.

[26] The eigenvalue of G_T, a test statistic for weak identification proposed by Cragg and Donald (1993) is 18422.59, which indicates a strong instrument, according to the critical values of Stock and Yogo (2005).

IV. RESULTS

IV.1. MAIN RESULTS

We begin by presenting our main 2SLS estimation results in Table 3 (with first stage results in Appendix Table A2). In the first three columns, the second stage results are estimated without the seasonality correction (denoted IV). In the next three columns, representing our preferred specification as given by equations (4) and (5), they are estimated with the seasonality correction (denoted IV+DID). Year and regional fixed effects are controlled for in every specification, while demographic and regional control variables are added gradually.

The childcare availability coefficient estimate when seasonal bias is not controlled for (the coefficient of the coverage variable C) is around 0.13-0.14, and significant in all three specifications. The estimate does not change significantly as additional controls are added, which again suggests that the control and treatment groups do not differ significantly in terms of their characteristics. The estimates (given by the coefficient of the C*m interaction term) decrease to around 0.095 with the seasonality correction, suggesting that some seasonal bias may indeed be present. The estimate is still significant, and highly robust to the specification of control variables. As expected, the coefficient of m is highly significant and negative (around -0.17), in line with the general increase in maternal activity between age 3 and age 4-5 of children.

The estimate of the impact of childcare availability suggests that if childcare coverage increased from 0 to 100% - i.e. if subsidized childcare became available to mothers of children around age 3 who did not previously have access - their activity rate would increase by 9.5 percentage points. As the baseline activity rate of the control group is around 50% (Table 2), this equals an increase of about 19 percent. This result enables us to forecast the expected impact of childcare expansion:

for example, the government recently changed eligibility rules so that all children can enroll in kindergarten immediately after they turn 3. This should lead to roughly the same increase in mothers' activity as we see between the two groups here, around 19 percent (9.5 percentage points).

Table 3: Main 2SLS Estimation Results

	IV			IV + DID		
Specifications	1	2	3	1	2	3
C*m				0.096**	0.095*	0.095*
				(0.037)	(0.041)	(0.041)
C	0.129**	0.141**	0.135**	0.014	0.025	0.021
	(0.032)	(0.034)	(0.034)	(0.027)	(0.024)	(0.024)
m				-0.179**	-0.166**	-0.166**
				(0.027)	(0.027)	(0.027)
N	3018	3018	3018	8811	8811	8811
Year dummies	x	x	x	x	x	x
Individual controls		x	x		x	x
Regional controls			x			x

Source: H-LFS and T-STAR datasets, years 1998-2011.
Note: The dependent variable is the participation dummy. The table gives coefficient estimates of township-level childcare coverage relevant to the given group (kindergarten if treated, nursery if not), the dummy indicating seasonality comparison group membership (m=0 if the child is 4-5), and their interaction. Year and region dummies are included in all regressions. Standard errors are given in parentheses. Stars indicate significance as: * $p<0.05$; ** $p<0.01$.

Reduced form estimates based on equation (6) (Appendix Table A3) also show a clear significant positive effect. The coefficient estimate of the interaction term T*m suggests that if the control group had the same access to childcare as the treatment group - if nursery coverage would be as high as kindergarten coverage - their activity rate would increase by 6 percentage points. The coefficient estimate of T is not significant, showing no evidence of strong seasonal differences that are common among mothers of 3 and 4-5 year olds. The coefficient of m is significant and around -0.16, reflecting a similar lower overall activity rate of mothers of 3 year olds compared to the comparison group of mothers of 4-5 year olds as seen in the second stage.

IV.2. Robustness and long term effects

As a first check that the results are robust and meaningful, we carry out the reduced form estimation for each child age group from 1 to 7 years, using the January 1st cutoff. Table 4 summarizes the results.[27] They indicate that there is a significant effect at age 3 of the child, but there is no effect at other ages. These findings are in line with what we observe in Figure 1.b: there is no significant difference between the groups – i.e. no birth date-related effects – apart from at age 3, due to the difference in kindergarten eligibility. The figure and table both show a small difference after age 1, which may correspond to differences in probabilities of enrollment into nursery school, but the estimated effect is insignificant. Based on this result and Figure 1.b, the difference between the groups does not last long, thus the difference in childcare availability does not seem to have long term effects on labor supply. Once the difference in eligibility ends, the activity rate of the control group catches up to that of the treatment group rapidly.

Table 4: Reduced form results at each child age

	Child age						
	Year 1	Year 2	Year 3	Year 4	Year 5	Year 6	Year 7
T*m	0.021	0.009	0.082**	-0.01	0.009	-0.009	0.008
	-0.012	-0.015	-0.022	-0.028	-0.021	-0.024	-0.02
N	3796	3688	3244	2883	2853	2666	2603

Source: H-LFS and T-STAR datasets, 1998-2011.

Note: The table shows the coefficient estimates of reduced-form regressions with control and treatment groups based on a January 1st cutoff: T=1 if birthdate is August-December, T=0 if it is January-May. IV based on T as the instrument is combined with DID, based on a comparison group of mothers of 4-5 year olds: m=1 if child age=3. The dependent variable is the participation dummy. Year dummies are included in all regressions. Standard errors are given in parentheses. Stars indicate significance as: * p<0.05; ** p<0.01.

[27] Full results are given in Appendix Table A4, these specifications are without the seasonality correction.

Next, we narrow the birth date windows around the cutoff from 5 to 4 months[28] and 3 months.[29] The results are similar to our main results, and are shown in Appendix Table A5. The estimates are of similar pattern and magnitude as those presented here for 5 month groups, however, as the sample size decreases, their significance decreases gradually. Results based on 4 month windows are around 0.1, near the border of significance, those based on 3 month windows are around 0.11 and just below significance due to slightly larger standard errors.

Finally, we test whether the childcare effect is still significant if we use employment as the dependent variable instead of participation.[30] We run the same 2SLS specifications based on this measure as well, shown in Appendix Table A6. The results also show a significant positive impact that is robust to the specification of controls: the coefficient estimate of C*m (childcare coverage) is around 0.08 with seasonality correction included. This suggests that the impact on employment is very similar to what we measure using participation, therefore our results can be directly compared to previous studies based on employment as the labor supply measure.

IV.3. COMPARISON WITH OTHER ESTIMATES

In the last part of the this section, we evaluate our estimate of the childcare effect relative to those based on other methods used in previous literature to highlight the most important econometric implications. The methodology used in our paper is inherently suited to measure local effects around the cutoff point, thus the extendibility to other ages of children is limited. At the same

[28] Treatment mothers: children born September-December, control mothers: children born January-April.
[29] Treatment mothers: children born October-December, control mothers: children born January-March.
[30] Most previous studies measure the effect on employment, however, since we aim to measure labor supply cleared from the effect of labor demand, our preferred dependent variable is labor market participation.

time, the childcare effect is very likely also dependent on various country-specific factors like benefit system and preferences. Thus a comparison of the magnitude to previous international estimates would have serious limitations. In order to illustrate the importance of the corrections made in our methodology, we therefore compare our main result to our own estimates which replicate various methods used in previous studies. The difference between these estimates is due to the different methodologies, as we use the same sample from H-LFS data, and the control variables included are the same in all specifications. Table 5 summarizes the results of this exercise.

Table 5: Summary of estimated childcare effect based on various methods

	(1) Linear Probability Model (OLS)	(2) IV	(3) IV+DID
Coefficient estimate	0.347**	0.177**	0.095**
N	13527	1826	8811
Adj. R^2	0.126	0.113	0.136
Year dummies	x	x	x
Individual controls	x	x	x
Regional controls	x	x	x

Source: H-LFS and T-STAR datasets, 1998-2011.
Note: Standard errors are given in parentheses. The dependent variable is the participation dummy. The explanatory variable of interest (C) is the local childcare coverage rate. Controls are the same in all specifications. Column 1: OLS carried out on pooled individual level data of mothers of 2.5-3.5 year olds. Column 2: IV estimation with T as the instrument carried out on a cross-section of data. Column 3: IV with T as the instrument is carried out on a sample where both groups are observed in the quarter after the child's 3rd birthday, and combined with DID based on comparison group of mothers of 4-5 year old children. Stars indicate significance as: * p<0.05; ** p<0.01.

The first column gives OLS estimates similar to the structural model-based estimates seen in previous international literature. The second column shows an IV estimate, with T as the instrument, based on a cross-section data, similar to the specification in Gelbach (2002). The last column shows our preferred specification of the IV+DID estimate estimated on longitudinal data. The difference between columns 2 and 3 is in (a) the sample, since in the simple IV setup treatment and control groups are observed at the same date (the quarter after the cutoff date), while in our IV+DID setup both groups are observed at the same child age (the quarter after their child turns 3), and (b) the seasonality correction used in the IV+DID case.

The OLS estimate is around 0.35 and strongly significant, much higher than our preferred IV+DID estimate. This specification identifies the childcare effect from regional and time variation in the coverage rate. Despite the relatively detailed controls, this estimate is prone to endogeneity bias from unobserved individual and regional characteristics. This highlights a key strength of the cutoff-based design: it uses the randomness of selection into treatment and control groups for identification, thereby the estimate is based on exogenous variation in childcare availability.

The simple IV estimate is significant and roughly half in magnitude of the OLS estimate, but still higher than the IV+DID estimate, at around 0.18. This is based on an instrument – group membership by child birth date – that is defined based on relatively wide (5-month) windows around the cutoff. Due to the average difference in child age when the groups are observed (at the same date, in a cross-section of data), the groups differ significantly in other factors besides treatment. The estimate therefore captures the sum of the childcare effect, and further factors: the end of parental leave, and the change in preferences regarding separation from the child. Furthermore, differences in season of birth may also bias the estimate. This comparison shows that even in the case of eligibility cutoff-based estimates, accounting for other age-related differences and seasonal bias is crucial. This can be ensured if one is able to define narrow windows around the cutoff, as in the RD method used in Fitzpatrick (2010). However, if data constraints do not allow for this, the sampling design and IV+DID method can also ensure that other age-related effects - as well as seasonal bias - are filtered out.

The problem with simultaneous age-related effects can also be illustrated on Figure 1.b. When the groups are observed at the same date, the average child age between the groups differs by 5 months. In this case, the distance is estimated between the activity rate of the treatment group at around age 3.75 (point c on Figure 1.b), and the control group at age 3.25 (point a). This means that some of the increase in the activity rate that happens in both groups – due to other child age-related factors – would be (incorrectly) included in the estimated childcare effect. The sampling design used in the IV+DID method, on the other hand, means estimating the vertical distance between the two groups at age 3.25 (points a and b), which should only be due to the difference in kindergarten access. Of course, compared to this graphical analysis, the IV+DID framework also corrects for seasonal bias, so it gives stronger evidence.

The data does not allow us to replicate the other most relevant previous cutoff-based estimate of Fitzpatrick (2010), however, we consider our estimate to be similarly credible due to the corrections applied. The fact that she finds no significant impact on labor supply in the US at age 4, while we do in Hungary at age 3, is likely due to difference in the point of estimation. Hungarian mothers are included in the estimation at a point when many of them are still inactive, but would like to return to the labor market, and our estimate shows that lack of childcare opportunities poses a binding constraint on their reactivation. Mothers in the US, however, have mostly already returned to the labor market if they wanted to, solving their childcare needs in other ways. We conclude from this analysis that the difference in the sample and methodology applied explains the deviation of the two previous cutoff-based US results.

V. POLICY IMPLICATIONS AND CONCLUSION

In this study, we provide a credible causal estimate of the effect of subsidized childcare availability on mothers' labor supply. We analyze the case of mothers of 3-year-olds in Hungary, who are much more likely to be able to enroll in state-run kindergartens if they turn 3 before January 1st. We overcome several estimation issues (endogeneity of childcare availability, wide window around the cutoff due to data constraints, concurrent child age-related changes, seasonal bias) using various techniques (IV, sampling design, DID). Fitzpatrick (2010) used cutoff-based analysis (RD) regarding US mothers of 4-year-olds and found that childcare availability increased enrollment but not labor supply. Our results suggest that if childcare opportunities are expanded at a child age when mothers' labor market activity is still relatively low compared to that of women overall – thus there is still high potential for reactivation – such a policy intervention can have a significant positive effect. An increase in availability from 0 to 100% - which can occur in places with no previous childcare institutions – can increase mothers' activity rate by 19%.

Our estimate focuses on intent-to-treat analysis, which allows us to make relevant predictions regarding the expected impact of investments in the expansion of subsidized childcare: we study the effect of childcare availability, not that of usage. Future research is needed in two main directions to further aid policymakers and pinpoint what conditions ensure the success of childcare expansion in terms of affecting labor supply. First, cross-country comparisons where the childcare effect can be measured at various points (child ages) can shed further light on when childcare availability is a binding constraint, and therefore, when expansion has the potential to lead to significant increase in the inflow of mothers to the active labor force.

Second, more information is needed on the interaction of childcare availability and other elements of the institutional framework and societal preferences. The effectiveness of childcare expansion may be limited by many factors: characteristics of maternity and parental benefits, lack of flexible work forms, societal views, the inflexibility of childcare hours,[31] etc. Our results reflect that other factors have a large impact: Figure 1 shows that there is a sharp increase in mothers' activity rates around age 3 of children of about 31 percentage points, of which increased childcare availability explains 9.5 percentage points, about one third. Determining the effect of other factors is outside the scope of this study, however, the end of parental leave is unlikely to explain the rest, since the monetary amount received in the last year before the child turns 3 is relatively small. Changes in preferences regarding separation probably also play a key role, the timing of which suggests that they are related to the institutional framework.[32] Studies based on both cross-country analysis of these characteristics, as well as unique econometric opportunities can shed light on the best comprehensive policy approach under various circumstances.

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

[32] This 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, suggesting that age 3 is the appropriate time for separating from the child and returning to work. It is possible that, lacking clear views on the matter, mothers simply use the age suggested by the institutional framework as a rule of thumb. Employers may assume that after age 3, childcare duties of mothers are less of a constraint and be more willing to employ them, which, in turn, may influence mothers' labor market expectations and activity.

REFERENCES

Angrist, Joshua D., and Alan B. Krueger. 1991. Does Compulsory School Attendance Affect Schooling and Earnings? Quarterly Journal of Economics 106, no.4: 979–1014.

Apps, Patricia and Rees, Ray. 2001. Fertility, female labor supply, and public policy. IZA Discussion Paper 409. Institute for the Study of Labor, Bonn.

Baker, Michael, Gruber, Jonathan, and Kevin Milligan. 2008. Universal Child Care, Maternal Labor Supply, and Family Well-Being. Journal of Political Economy 116, no.4: 709-745.

Berger, M. C., and D.A. Black. 1992. Child Care Subsidies, Quality of Care, and the Labor Supply of Low-Income, Single Mothers. Review of Economics and Statistics 74, no.4: 635-42.

Blaskó, Zsuzsa. 2011. [Three years at home with the child – but not at any cost - Public opinion regarding mothers' employment.] Három évig a gyermek mellett – de nem minden áron. A közvélemény a kisgyermekes anyák munkába állásáról. Demográfia 54, no.1: 23–44.

Blau, David. 2003. Child Care Subsidy Programs. In: Moffitt, Robert A. (ed.). Means-Tested Transfer Programs in the United States. University of Chicago Press. National Bureau of Economic Research: 443 – 516.

Bloom, David, Canning, David, Fink, Günther, and Finlay, Jocelyn E. 2009. Fertility, Female Labor Force Participation, and the Demographic Dividend. Journal of Economic Growth 14, no. 2: 79-101.

Bound, John, and David A. Jaeger. 1996. On the Validity of Season of Birth As An Instrument in Wage Equations: A Comment on Angrist and Krueger's "Does Compulsory School Attendance Affect Schooling and Earnings?" NBER Working Paper 5835. National Bureau of Economic Research, Chicago.

Bound, John, and David A. Jaeger. 1994. Evidence on the Validity of Cross-Sectional and Longitudinal Labor Market Data. Journal of Labor Economics, University of Chicago Press, vol. 12(3), pages 345-68, July.

Borra, Cristina. 2010. Childcare costs and Spanish mothers' labour force participation. Hacienda Publica Espanola / Revista de Economia Publica 194, no. 3: 9-40.

Cascio, Elizabeth U. 2009. Maternal Labor Supply and the Introduction of Kindergartens into American Public Schools. Journal of Human Resources 44, no.1: 140–170.

Chevalier, A. and Viitanen T. K. 2002. The causality between female labour force participation and the availability of childcare. Applied Economics Letters 2002/9: 915-918.

Chone, P., Le Blanc, D., Robert-Bobee, I. 2003. Female Labor Supply and Child Care in France. CESifo Working Paper No. 1059.

Connelly, Rachel. 1992. The Effect of Child Care Costs on Married Women's Labor Force Participation. The Review of Economics and Statistics 74, no. 1: 83-90.

Cragg, John G. and Stephen G. Donald. 1993. Testing Identifiability and Specification in Instrumental Variable Models. Econometric Theory 9: 222-240.

Currie, Janet and Hannes Schwandt. 2013. Within-mother analysis of seasonal patterns in health at birth. Proceedings of the National Academy of Sciences of the United States of America (PNAS) 110, no. 30: 12265-12270.

Del Boca, Daniela. 2002. The Effect of Child Care and Part Time Opportunities on Participation and Fertility Decisions in Italy. Journal of Population Economics 15: 549–73.

European Union (EU). 2002. Presidency Conclusions. European Council. Barcelona. 15 and 16 March. http://europa.eu/rapid/pressReleasesAction.do?reference=MEMO/08/592.

Fitzpatrick, Maria Donovan. 2010. Preschoolers Enrolled and Mothers at Work? The Effects of Universal Prekindergarten. Journal of Labor Economics 28, no. 1: 51-85.

Gelbach, Jonah B. 2002. Public Schooling for Young Children and Maternal Labor Supply. American Economic Review 92, no. 1: 307–322.

Haan, Peter, and Katharina Wrohlich. 2011. Can child care policy encourage employment and fertility? Evidence from a structural model. Labour Economics 18: 498–512.

Hardoy, Ines, and Schone, Pal. 2013. Enticing Even Higher Female Labor Supply – The Impact of Cheaper Day Care. Review of Economics of the Household, doi: 10.1007/s11150-013-9215-8.

Hasková, Hana, Győry, Adrienn, and Dorottya Szikra. 2012. How did we get the 'magic 3'? The timing of parental leave and child care services in the Visegrád-countries. Unpublished manuscript, Budapest Institute, Budapest.

Herbst, Chris M. 2010. The labor supply effects of child care costs and wages in the presence of subsidies and the earned income tax credit. Review of Economics of the Household 8: 199-230.

Kertesi, Gábor, Kézdi, Gábor, Molnár, Tímea, Szabó-Morvai, Ágnes. 2012. School Catchment Areas in Hungary. Unpublished manuscript, Institute of Economics, Centre for Economic and Regional Studies of the Hungarian Academy of Sciences Budapest.

Kestenbaum, B. 1987. Seasonality of Birth: Two Findings from the Decennial Census. Social Biology 34: 244-248.

Kimmel, Jean. 1992. Child Care and the Employment Behavior of Single and Married Mothers. Upjohn Institute Working Paper No. 93-14.

Kluve, Jochen, and Sebastian Schmitz. 2014. Parental Benefits and Mothers' Labor Market Outcomes in the Medium Run. ZBW - Deutsche Zentralbibliothek für Wirtschaftswissenschaften, Leibniz-Informationszentrum Wirtschaft.

Krusell, Per, Ohanian, Lee E., Ríos-Rull, José-Víctor, and Giovanni L. Violante. 2000. Capital-Skill Complementarity and Inequality: A Macroeconomic Analysis. Econometrica 68, no. 5: 1029-1053.

Lefebvre, Pierre, and Merrigan, Philip. 2008. Child-Care Policy and the Labor Supply of Mothers with Young Children: A Natural Experiment from Canada. Journal of Labor Economics 26, no. 3: 519-48.

Lokshin, Michael. 2004. Household Childcare Choices and Women's Work Behavior in Russia. The Journal of Human Resources 39, no.4: 1094-1115.

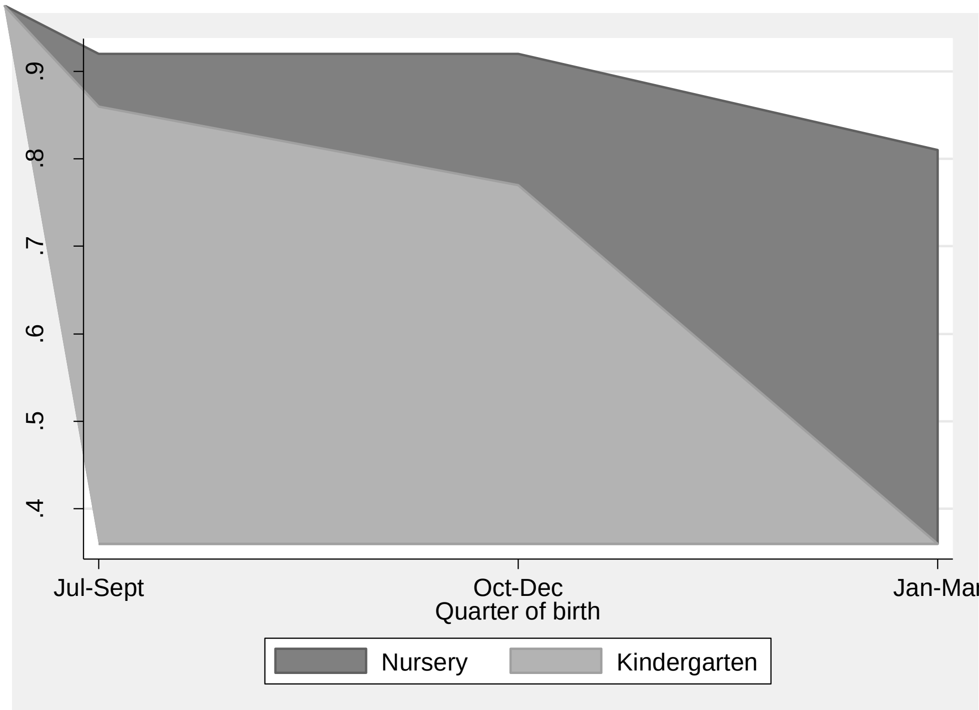
Lundin, Daniela, Mörk, Eva, and Björn Öckert. 2008. How far can reduced childcare prices push female labour supply? Labour Economics 15: 647–659.

Ribar, David C. 1995. A Structural Model of Child Care and the Labor Supply of Married Women. Journal of Labor Economics 13, no. 3: 558-597.

Stock, James H. and Motohiro Yogo (2005), "Testing for Weak Instruments in Linear IV Regression." Ch. 5 in J.H. Stock and D.W.K. Andrews (eds), Identification and Inference for Econometric Models: Essays in Honor of Thomas J. Rothenberg, Cambridge University Press

APPENDIX

Figure A1: Institutional framework: January cutoff



Source: EU-SILC, 2006-2012

Table A1: Full Summary statistics of the estimation sample by group

	Child of age 3 (m=1)			Child of age 4-5 (m=0)		
	Treatment	Control	Diff/SD	Treatment	Control	Diff/SD
Mother						
Activity rate (1997-2011) (%)	59.60	51.50	0.161	68.32	68.15	0.004
Childcare coverage (%)	74.2	10.2		74.2	74.2	
Number of children	1.3	1.3	-0.022	1.1	1.1	-0.04
Age of youngest child	3.3	3.3	-0.03	4.8	4.8	-0.043
Age (years)	31.1	31.1	0.001	32.4	32.5	-0.004
Education (%):						
Primary	23.60	22.10	0.037	23.20	23.10	0
Vocational school	26.90	27.20	-0.006	28.00	25.30	0.063
High school	31.90	33.30	-0.03	34.40	35.00	-0.013
University	17.60	17.50	0.004	14.50	16.60	-0.057
Occupation (%):						
Leader, executive	19.90	20.60	-0.016	20.20	18.20	0.053
Higher educ. requiring	1.80	1.90	-0.006	2.10	2.60	-0.031
GED requiring	11.40	12.10	-0.022	10.00	12.00	-0.061
Clerical, customer service	15.40	14.70	0.02	15.20	14.40	0.022
Service, commerce	9.50	9.30	0.005	9.70	10.70	-0.033
Agricultural	17.00	20.10	-0.077	18.50	18.20	0.008
Construction, industry	1.20	0.80	0.05	2.00	1.70	0.019
Operation, assembly	8.80	7.30	0.056	7.60	6.90	0.028
Unskilled	8.20	8.10	0.004	7.80	7.40	0.012
Armed forces	6.70	5.00	0.077	7.00	7.80	-0.033
Husband or partner						
Age (years)	30	29.8	0.017	30.8	30.8	-0.002
Employment status (%):						
No partner	13.30	13.20	0.004	14.10	12.70	0.042
Partner without job	13.30	13.20	0.004	14.10	12.70	0.042
Partner with job	76.00	75.60	0.007	73.20	75.00	-0.042
Education (%):						
Primary	16.60	16.00	0.017	15.80	16.80	-0.025
Vocational school	38.20	38.20	0	38.50	37.90	0.012
High school	20.70	21.40	-0.017	21.80	22.30	-0.012
University	13.40	13.00	0.012	11.00	10.50	0.014
Occupation (%):						
Leader, exec.	17.80	17.80	0.002	20.60	17.70	0.076

Higher educ. requiring	6.30	5.90	0.015	5.60	5.60	-0.001
GED requiring	7.60	7.70	-0.006	5.80	5.60	0.007
Clerical, customer serv.	7.20	7.10	0.003	6.60	7.10	-0.019
Service, commerce	0.30	0.70	-0.052	0.60	0.50	0.021
Agricultural	11.00	12.00	-0.032	11.00	10.40	0.02
Construction, industry	3.50	3.80	-0.017	4.40	4.00	0.021
Operation, assembly	25.00	24.70	0.005	25.50	27.20	-0.038
Unskilled	14.90	13.70	0.032	14.30	14.30	0
Armed forces	6.60	6.40	0.004	5.50	7.50	-0.075
Environment						
Type of settlement (%):						
Village	27.50	28.60	-0.025	28.80	26.80	0.045
Town	35.70	40.70	-0.103	39.50	42.60	-0.063
City	21.00	17.10	0.104	19.10	17.60	0.039
Region (%):						
Central Hungary	28.10	28.30	-0.005	26.40	25.50	0.022
Central Transdanubia	10.60	10.70	-0.003	10.90	11.10	-0.008
Western Transdanubia	9.30	9.40	-0.003	9.30	9.60	-0.007
Southern Transdanubia	9.70	9.40	0.008	10.20	10.60	-0.013
Northern Hungary	14.10	11.20	0.092	12.90	12.80	0.003
Northern Plains	15.00	16.80	-0.049	16.80	16.60	0.006
Southern Plains	13.20	14.20	-0.027	13.50	13.90	-0.012
Unemployment rate (%)	4.40	4.40	0.006	4.60	4.60	-0.017
Nursery coverage (%)	11.20	10.20	0.106	10.50	10.00	0.053
Kindergarten coverage (%)	105.10	105.00	0.005	103.50	102.80	0.022
Average population	310147	260321	0.085	248879	252224	-0.006
Number of obs.	1,732	1,577		2,975	2,868	

Table A2: First stage results of the 2SLS regression

	C		C*m	
	Coef.	Robust SE	Coef.	Robust SE
T*m	-0.004	0.005	0.618	0.014
T	0.608	0.013	-0.006	0.001
m	-0.002	0.003	0.106	0.018
# of children	0.000	0.004	-0.004	0.003
Partner w/o job	0.016	0.011	0.009	0.008
Partner w/ job	0.013	0.010	0.005	0.007
Vocational school	0.002	0.004	0.000	0.003
High school	-0.004	0.005	-0.003	0.003
University	-0.002	0.008	0.001	0.003
Age	-0.003	0.003	0.001	0.002
Age squared	0.000	0.000	0.000	0.000
Partner: University	-0.005	0.006	-0.001	0.004
Partner: High sc.	-0.005	0.005	-0.004	0.003
Partner: Vocationa..	0.001	0.004	0.000	0.003
Partner's age	0.000	0.000	0.000	0.000
Unemployment level	0.118	0.132	-0.051	0.071
Village	0.030	0.008	-0.026	0.006
City	0.033	0.005	-0.018	0.005
Large city	0.029	0.009	-0.022	0.005

Table A3: Reduced form results with seasonality correction

	Specifications		
	1	2	3
T*m	0.061*	0.060*	0.060*
	(0.024)	(0.027)	(0.026)
T	0.007	0.014	0.012
	(0.018)	(0.016)	(0.016)
m	-0.169**	-0.156**	-0.156**
	(0.024)	(0.023)	(0.023)
# of children		-0.123**	-0.122**
		(0.015)	(0.015)
Partner w/o job		-0.004	0.000
		(0.043)	(0.043)
Partner w/ job		0.038	0.039
		(0.043)	(0.043)
Vocational school		0.177**	0.175**
		(0.020)	(0.020)
High school		0.289**	0.287**
		(0.019)	(0.019)
University		0.415**	0.412**
		(0.037)	(0.037)
Age		-0.004	-0.004
		(0.012)	(0.012)
Age squared		0.000	0.000
		(0.000)	(0.000)
Partner: University		0.058*	0.055*
		(0.025)	(0.024)
Partner: High sc.		0.087*	0.085*
		(0.037)	(0.037)
Partner: Vocational		0.075**	0.073**
		(0.023)	(0.023)
Partner's age		-0.005**	-0.005**
		(0.001)	(0.001)
Unemployment level			-1.218**
			(0.470)
Village			0.100**
			(0.031)
City			0.102**
			(0.020)
Large city			0.118**
			(0.045)
Constant	0.627**	0.700**	0.690**
	(0.052)	(0.217)	(0.218)
R^2	0.179	0.272	0.273
AIC	10632.499	9578.493	9572.289
N	8980	8980	8980
Year dummies	x	x	x
Individual controls		x	x
Regional controls			x

Table A4: Reduced form results at each child age

	Child age						
	Year1	Year2	Year3	Year4	Year5	Year6	Year7
T	0.021	0.009	0.082**	-0.01	0.009	-0.009	0.008
	-0.012	-0.015	-0.022	-0.028	-0.021	-0.024	-0.02
# of children	-0.021**	-0.048**	-0.117**	-0.120**	-0.171**	-0.210*	
	-0.008	-0.01	-0.022	-0.028	-0.047	-0.096	
Partner w/o job	-0.02	-0.068	0.007	0.032	-0.044	-0.212**	-0.166
	-0.022	-0.058	-0.062	-0.121	-0.079	-0.082	-0.127
Partner w/ job	-0.03	-0.081	0.032	0.077	-0.018	-0.129	-0.107
	-0.022	-0.061	-0.062	-0.107	-0.074	-0.068	-0.127
Vocational school	-0.009	0.003	0.186**	0.133**	0.203**	0.187**	0.200**
	-0.009	-0.021	-0.035	-0.034	-0.038	-0.04	-0.04
High school	0.01	0.075*	0.245**	0.298**	0.322**	0.287**	0.278**
	-0.009	-0.029	-0.035	-0.036	-0.029	-0.039	-0.042
University	0.035*	0.148**	0.367**	0.430**	0.440**	0.394**	0.371**
	-0.015	-0.045	-0.05	-0.045	-0.045	-0.048	-0.05
Age	0.009	0.024	0.02	-0.005	-0.039	-0.017	-0.013
	-0.009	-0.016	-0.021	-0.024	-0.024	-0.028	-0.035
Partner: University	0.027	0.021	0.083	0.03	0.074	0.009	0.077
	-0.021	-0.042	-0.044	-0.045	-0.038	-0.05	-0.046
Partner: High sc.	0.02	0.034	0.071	0.121	0.104**	0.046	0.113**
	-0.011	-0.027	-0.06	-0.062	-0.036	-0.04	-0.041
Partner: Vocational	0.009	0.028	0.06	0.093*	0.094**	0.063	0.086*
	-0.007	-0.019	-0.036	-0.042	-0.035	-0.038	-0.038
Partner's age	0	0.002	-0.004*	-0.006*	-0.003	0.002	0
	-0.001	-0.002	-0.002	-0.003	-0.002	-0.002	-0.003
Unemployment level	0.341	0.207	-2.006**	-0.092	-2.795**	-1.679*	-1.04
	-0.21	-0.538	-0.765	-1.032	-0.808	-0.84	-1.185
Village	-0.092**	-0.001	0.218**	0.226**	0.008	-0.258**	0.146
	-0.018	-0.049	-0.064	-0.066	-0.057	-0.095	-0.084
City	-0.073**	-0.036	0.243**	0.197**	0.041	-0.249**	0.132*
	-0.011	-0.031	-0.058	-0.051	-0.035	-0.086	-0.066
Large city	-0.118**	0.025	0.250**	0.237**	0.021	-0.202*	0.207**
	-0.024	-0.043	-0.072	-0.076	-0.062	-0.089	-0.076
Constant	-0.132	-0.231	0.074	0.574	1.452**	1.457**	0.972
	-0.142	-0.26	-0.374	-0.383	-0.404	-0.481	-0.656
R^2	0.177	0.213	0.318	0.369	0.403	0.366	0.406
AIC	-2579	2055.402	3491.096	2579.223	2258.491	2197.612	1831.307
N	3796	3688	3244	2883	2853	2666	2603
Year dummies	x	x	x	x	x	x	x
Individual controls	x	x	x	x	x	x	x
Regional controls	x	x	x	x	x	x	x

Table A5: 2SLS results with 3 and 4 month windows around the cutoff

	Window: 4 months		Window: 3 months	
	IV	IV + DID	IV	IV + DID
Explanatory variables	3	3	3	3
C	0.149**	0.006	0.147*	-0.021
	(0.042)	(0.033)	(0.058)	(0.046)
C*m		0.106		0.110
		(0.054)		(0.078)
m		-0.174**		-0.174**
		(0.026)		(0.040)
# of children	-0.101**	-0.119**	-0.042	-0.099**
	(0.023)	(0.017)	(0.038)	(0.026)
Partner w/o job	0.037	-0.005	0.299*	0.018
	(0.085)	(0.060)	(0.139)	(0.071)
Partner w/ job	0.032	0.051	0.271*	0.061
	(0.080)	(0.056)	(0.134)	(0.073)
Vocational school	0.149**	0.151**	0.151*	0.167**
	(0.042)	(0.027)	(0.063)	(0.036)
High school	0.198**	0.273**	0.131	0.239**
	(0.057)	(0.027)	(0.085)	(0.046)
University	0.314**	0.383**	0.187	0.312**
	(0.084)	(0.050)	(0.122)	(0.067)
Age	0.031	0.005	0.048	0.015
	(0.023)	(0.015)	(0.042)	(0.021)
Age squared	-0.000	-0.000	-0.001	-0.000
	(0.000)	(0.000)	(0.001)	(0.000)
Partner: University	0.142*	0.059	0.247*	0.087
	(0.066)	(0.031)	(0.122)	(0.050)
Partner: High sc.	0.136	0.095	0.165	0.084
	(0.090)	(0.052)	(0.131)	(0.059)
Partner: Vocational	0.116*	0.083**	0.152	0.108*
	(0.046)	(0.030)	(0.084)	(0.046)
Partner's age	-0.006**	-0.005**	-0.010**	-0.004
	(0.002)	(0.001)	(0.004)	(0.002)
Unemployment level	-1.832	-1.180*	-1.467	-1.492
	(1.132)	(0.579)	(1.914)	(0.859)
Village	-0.169	0.134**	0.018	-0.173
	(0.127)	(0.042)	(0.110)	(0.105)
City	-0.136	0.151**	0.055	-0.148
	(0.138)	(0.031)	(0.095)	(0.103)
Large city	-0.134	0.146**		-0.156
	(0.150)	(0.056)		(0.116)
R^2	0.115	0.142	0.117	0.121
AIC	2,085.522	5,950.73	838.14	2,639.585
N	1,871	5,696	782	2,660
Year dummies	x	x	x	x
Individual controls	x	x	x	x
Regional controls	x	x	x	x

Table A6: 2SLS results with employment as the dependent variable

Explanatory variable	Specifications 1	2	3
C	0.009	0.025	0.021
	(0.027)	(0.023)	(0.023)
C*m	0.079*	0.077*	0.077*
	(0.036)	(0.039)	(0.039)
m	-0.184**	-0.170**	-0.170**
	(0.023)	(0.023)	(0.023)
# of children		-0.115**	-0.114**
		(0.013)	(0.013)
Partner w/o job		-0.027	-0.023
		(0.044)	(0.044)
Partner w/ job		0.047	0.049
		(0.044)	(0.044)
Vocational school		0.180**	0.177**
		(0.018)	(0.018)
High school		0.296**	0.293**
		(0.018)	(0.018)
University		0.469**	0.465**
		(0.030)	(0.030)
Age		0.002	0.003
		(0.012)	(0.012)
Age squared		-0.000	-0.000
		(0.000)	(0.000)
Partner: University		0.050*	0.046
		(0.025)	(0.025)
Partner: High sc.		0.078**	0.075**
		(0.027)	(0.027)
Partner: Vocational		0.064**	0.062**
		(0.020)	(0.020)
Partner's age		-0.003**	-0.003**
		(0.001)	(0.001)
Unemployment level			-1.169**
			(0.382)
Village			0.141**
			(0.029)
City			0.143**
			(0.019)
Large city			0.157**
			(0.042)
R^2	0.026	0.145	0.146
AIC	11,087.172	9,962.787	9,957.799
N	8,809	8,809	8,809
Year dummies	x	x	x
Individual controls		x	x
Regional controls			x