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Essays On Maternal Employment Policies

by

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Submitted in partial fulfillment of the requirements for
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The undersigned hereby certify that they have read and recommend to the Department of Economics for acceptance a thesis entitled **"Essays on Maternal Employment Policies"** by **Ágnes Szabó-Morvai**.
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Hereby I testify that this thesis contains no material accepted for any other degree in any other institution and that it contains no material previously written and/or published by another person except where appropriate acknowledgement is made.

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DISCLOSURE OF CO-AUTHORS CONTRIBUTION

Title of paper: Subsidized Childcare Matters for Maternal Labor Supply. Evidence from Hungary

Co-author: Anna Lovasz

The nature of cooperation and the roles of the individual co-authors and approximate share of each co-author in the joint work: The paper was developed in cooperation with Anna. Both of us were extensively involved in working out the methodology, running the regressions and writing the text of the paper, with that Anna's contribution was more pronounced in writing and mine in calculations.

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Abstract

The thesis consists of one co-authored and two single-authored chapters on the effect of family policies on maternal labor supply. Each chapter consists of empirical investigations of the family policies, using Hungarian Labor Force Survey microdata. Chapter 1 examines the effect of childcare availability on the labor supply of mothers of 3-year-olds. We exploit a date-of-birth eligibility cutoff at the age of 3, where on one side of the cutoff childcare availability is high, whereas on the other it is low. By applying novel measurement strategy, we overcome some data issues, and show that the results are robust to various specifications. We find that a 10 percentage point increase in childcare coverage induces 1.8 percentage increase in maternal labor supply.

In Chapter 2 I use difference-in-differences method to estimate the causal effect of the maternal benefit (GYED) on maternal labor supply and employment probabilities. I find that in the first two years after giving birth, there is no significant effect, however, from the third year, the maternal leave affects maternal employment probability negatively.

Chapter 3 provides an analysis of the START Plusz hiring tax credit program. The program is available for mothers with children under 4, and I include mothers of 5-7 as a control group. The findings of the analysis show that before the economic crisis it had had a positive significant effect on some subgroups of the targeted population.

Chapter 1: Subsidized Childcare Matters for Maternal Labor Supply. Evidence from Hungary

(co-author: Anna Lovasz)

Chapter 1 contributes to the literature by estimating the effect of subsidized childcare availability on Hungarian mothers' labor supply based on a discontinuity in kindergarten eligibility rules. We identify the effect at a child age when the mothers'

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participation rate is still lower than that of mothers with older children, 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 a 10 percent increase in the fraction of children covered by subsidized childcare would increase maternal labor market participation by 13.5 percentage points, compared to a baseline 50% participation rate.

Chapter 2: Who Benefits from Child Benefits? The Labor Supply Effects of Maternal Cash Benefit

Chapter 2 contributes to the literature on the examination of the effect of restoring maternity cash benefit in 2000 on labor market participation and employment probability of mothers in Hungary. In the first two years of motherhood, no significant employment effects can be demonstrated. However, after the second year of motherhood, a negative employment effect is found for female with low level of education, although the large cash benefit is received only until the end of the second year. This can be explained with the wealth effect of the cash benefit: the accumulated monetary reserves allow these mothers to choose staying at home instead of undertaking a full-time job.

Chapter 3: Evaluating The Effect Of START Plusz Hiring Tax Credit Program On The Employment Probability Of Mothers With Kindergarten-Age Child

Chapter 3 contributes to the literature on the measurement of the effect of a hiring tax credit program on maternal labor supply. In Hungary, a hiring tax credit program, START Plusz was introduced in 2007 for mothers with a child younger than 4 in order to increase their employment probability. The policy setting allows for using

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similar mothers with children of age 5-7 as a control group. Though the program is practically open for all education groups, those with vocational and high school level educational attainment get involved in the program with higher probability compared to lower and higher educated mothers. This group is examined in detail, and I find a significant 10.2 percentage point employment effect for mothers with two or more children, however, the results of the program was washed away most probably by the effects of the global economic crisis by 2009.

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Chapter 1

Subsidized Childcare Matters for Maternal Labor Supply. Evidence from Hungary

Co-author: Anna Lovasz

1.1 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 the 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. However, the empirical results of the regarding literature is mixed[3].

We use the discontinuity in the eligibility rules of subsidized kindergarten in Hungary to identify the childcare effect on maternal labor supply. The eligibility of 3-year-olds depends on whether the child was born before or after the eligibility cutoff point, 1st

[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).

[3] The findings of the empirical research body range from zero effect to rather large positive effects of subsidized childcare on maternal labor supply and employment.

January. In the paper, we compare the labor market participation of mothers at the two sides of the cutoff point. By comparing mothers of children of the same age we can disentangle the effect of childcare from the effect of parental leave and preference changes that are related to child age. We provide an intent-to-treat analysis, as it is the increased childcare availability and not the enrollment itself which is of first order relevance to policy. Due to data constraints, the window around the cutoff is rather wide, which raises concerns about seasonality bias, as noted by Bound and Jaeger (1994). To address this problem, 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. The seasonally corrected results are similar to the baseline results.

The labor force participation rate of the treatment group is 57.9% and that of the control group is 49.7%. According to the administrative data, the fraction of children covered by childcare is 74.2% in case of the treatment group and it is only 10.2% in case of the control group. Taking the actual size of the childcare coverage increase into account, we find that if the fraction of children covered by subsidized childcare increased from 0 to 100% - i.e. if subsidized childcare became available to mothers who did not previously have access at all - their participation rate would increase by 13.5 percentage points, compared to a baseline 50% participation rate.

The results of the numerous previous estimates available from various countries are mixed for two reasons. First, the results are sensitive to the estimation methods used. The structural models have the advantage of being able to control for fertility and other types of selection biases, however, they usually utilize cross-section data and are based on strict behavioral and distributional assumptions. 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

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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).

The studies using policy changes for identification, require fewer assumptions and may eliminate the omitted variables bias, however, they are based on the crucial assumption that the policy change is exogenous. 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; Havnes and Mogstad, 2011). 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.

Cutoff-based estimates are rare in the literature; nevertheless, they have the potential to create truly exogenous variation in the availability of childcare. Cutoff-based methods need no stringent assumptions on exogeneity, yet they need a cutoff and large data sets. 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. For instance, Gelbach (2002), Fitzpatrick (2010) and Bauernschuster and Schlotter (2015) belong to this category. This local nature of the estimated affects may explain why the results are mixed: many studies identify the effect at a child age where the participation rate of mothers has almost entirely reached the participation rate of mothers with older children.

Our study fits into the narrow strand of cutoff-based estimations. Contrary to most of the previous cutoff-based studies, our analysis identifies the effect of childcare availability at age 3 of the child when the participation of mothers in our setting is low (47% as opposed to the 67% rate of mothers with older children). Ideally, we would carry out this analysis in the same spirit and similar fashion as is done by Fitzpatrick (2010) and Gelbach (2002). However, because of the limitations of the data, there are some issues to tackle, for instance seasonal bias and contamination of age-related effects. Nevertheless, our estimates indicate that the results are clearly robust to these issues.

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Additionally, our analysis bears specific policy relevance. This is the first paper to measure the effect of subsidized childcare in an institutional framework serves weakly the reconciliation of family and work obligations. One of the most essential elements of the institutions, the attitudes of the Hungarian population towards working mothers is rather traditional, opposing the labor market participation of mothers with a young child. This is confirmed by that 66.1% of the Hungarian respondents in the International Social Survey Programme questionnaire in 2002 conceived that a preschool child is likely to suffer if his or her mother works. This rate is 15 percentage points above the average, the 6th highest among the 35 participating countries[4]. Another vital institutional element, the possibility to reconcile between work and family is below the European average. 21.4% of the Hungarians stated in the European Survey on Working Conditions in 2010 that the working hours do not fit well in with the family or social commitments outside work, thus Hungary ranked the 24th among the 35 participating European countries.

Consequently, this study is informative to policymakers who should take such non-supportive factors into account, for instance in the EU when considering how childcare targets would affect maternal labor supply in Eastern and Southern European countries, or in the US when thinking about the effect of childcare availability on Southern immigrants.

1.2 Data

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

[4] Hungary did not participate in the 2012 survey, but for those countries participating in both surveys, the ranking changed very little, and the correlation between the ratios in the two survey years was 92%. This indicates that these attitudes do not change rapidly, and the 2002 data is still relevant. Countries from Southern America (Brazil, Chile, Mexico), and Southern and Eastern Europe (Portugal, Bulgaria) belonged to the most traditional countries in this survey.

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households are rotated in each quarter; 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,[5] and include mothers with 1 or more children.

The dataset includes detailed demographic and labor market data about each individual. Our labor supply measure is the binary variable of labor market participation, which is based on the ILO definition of participation. We include individual (age, schooling, occupation), family (number of children, husband's labor market status), and regional (settlement type, region, local unemployment) characteristics linked from the T-STAR regional dataset of the Hungarian Central Statistical Office as control variables (see Table 1. for the list of variables).

Finally, the database has two drawbacks that need to be highlighted. First, the exact date of child birth is not available, we only know the birth quarter. As a result, relatively wide window around the cutoff is needed in the estimation. Second, there is no data on actual enrollment to kindergarten. While our main analysis focuses on intent-to-treat effects of kindergarten availability, actual coverage rates help assessing the magnitudes of our results. For coverage rates we rely on administrative data aggregated to small regional units. In order to check for the plausibility of using the administrative data, we carried out additional analysis using data from the 2011 Hungarian census. We analyzed actual

[5] It is important to emphasize that we always examine the youngest child, as only mothers who do not 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 likely to be the same in the treatment and the control group.

enrollment rates and found that the administrative data is informative about the enrollment rates around the cutoff point.

1.3 Institutional Framework

Figure 1. illustrates the participation rate of Hungarian mothers, by 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 participation is due to several potential factors that change simultaneously with childcare enrollment around age 3 of the child: subsidized parental leave ends, and preferences regarding the separation of mothers from their children may change.

Kindergarten also becomes available at the age of 3. Subsidized nursery schools accept children between the ages of 5 months and 3 years, while kindergartens accept children from the age of 3 to 6 in the analyzed period. Up to the age of 5, it is not compulsory for the institutions to accept the child and it is not for the families to enroll her. The rate of children covered by kindergartens is significantly higher (74.2% on average) than that covered by nursery schools (10.2% on average). The kindergarten school year begins in September.

The cutoff rule for subsidized kindergartens is the following. Children turning 3 after December 31 may enroll only in the following September. Those born between September 1 and December 31 may enroll in the September before their third birthday. Most of the latter group enrolls by September 1, but some of them enroll later in that year. The compliance with this rule is high, as is seen on Figure 2 in the Appendix.

There are two further factors that change significantly around age 3. The first is flat-rate parental leave subsidy, which is received by each mother when the child is between the ages of 2 and 3, the period of interest in our analysis.[6] One parent in each family is entitled to it; the overwhelming majority (98.1%) is taken up by mothers. The amount of the parental leave subsidy is low (23.4% of the average female wage in 2008); nevertheless, it may still have an impact on the labor supply decision of mothers, especially for those with low expected wage.

Second, preferences regarding separation from the child may also change when the child is around the age of 3. A survey by Blaskó (2011) suggests that these preferences change sharply at age 3. The survey results show that the ratio of those believing that the child is old enough for the mother to return to work increases from 19.6% to 76% at the age of 3.

1.4 Methodology and results

The basic idea of the cutoff-based methodology, inspired by Angrist and Krueger (1991), is to use the birthdate of the child to sort the individuals into the treatment and the control groups. We compare mothers at the two sides of the cutoff with children of similar ages. 74.2% of those born before the cutoff date are covered by subsidized childcare, but this rate is only 10.2% for those on the other side of the cutoff.

The treatment variable is defined as follows:

$$T_i = \begin{cases} 1 \quad if & 1^{st} August \leq b_i \leq 31^{st}\ December \\ 0 \quad if & 1^{st}\ January \leq b_i \leq 31^{st} May \end{cases} \tag{1}$$

[6] 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.

where b_i is the date of the third birthday of the youngest child, and January 1 is the cutoff date. In order for the estimated treatment effect to be unbiased, we need the sorting into treatment to be random.

By the standard argument of regression-discontinuity design, 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 31 are very similar to mothers of children born on January 1. Unfortunately, due to small sample size and the imprecise data on birthdates, 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 possible sources of bias more carefully. First, as outlined previously, not only does childcare availability increase around age 3, but parental leave subsidy also ends, and the willingness to separate from the child may increase as well. These age-related changes can lead to significant differences between the groups, because the average age of children in the two groups differs significantly.[7]

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 selected mothers into the treatment group whose child was born between 1st August and 31st December and were interviewed between 1st January and 31st March. We constructed the control group similarly, with dates for child birth 1st January and 31st May, and dates for the interview 1st June and 31st August.

[7] 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 the 1st of 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. Preferences regarding separation may also change significantly during 5 months.

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.

The descriptive statistics for the treatment and the control group are presented in Table 1. This table serves as a preview of the results: it is apparent that most characteristics are similar in the two groups. On the other hand, they do differ significantly in terms of the participation rate and the rate of children covered by subsidized childcare rate. The similarity of the characteristics suggests that selection into the groups based on the date of birth is random.

Figure 3 provides a graphical illustration of the treatment effect. It shows that the participation rates of the treated and control mothers move together as children grow older, except for a period following age 3, when the treated mothers' participation rate is higher for a while. This corresponds exactly to the period when they become eligible to subsidized kindergarten while the control group does not, suggesting that childcare availability positively impacts mothers' labor supply.

In Table 1 and Figure 3 the raw differences between the two groups are exhibited. To check the robustness of our results and arrive at more precise estimates, we control for differences in various characteristics between the groups in the following regression:

$$L_{yri} = \beta T_i + \alpha_y + \gamma_r + X'_{yri}\pi_1 + S'_{yr}\pi_2 + \xi_{yri} \qquad (2)$$

The subscripts indicate yearly (y), regional (r), and individual (i) variation. L_{yri} is the participation dummy for individual i. The equation adjusts for a set of individual (X_{yri}) and regional covariates (S_{yr}), α_y represents year fixed effects, and γ_r region fixed effects.

The parameter β captures the effect of belonging to the treatment group on the probability of labor market participation. It can be interpreted as representing how much more active mothers are if they are eligible for kindergarten rather than nursery school, which has significantly lower coverage. Panel (a) of Table 2 shows the results.

Belonging to the treatment group increases the probability of labor market participation by 7.8-8.5 percentage points after the third birthday. The estimates are significant at the 1% level in all three specifications. Year and regional fixed effects are controlled for in each specification, while demographic and regional control variables are added gradually. 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.

In order to interpret the magnitude of these results, we take the national average childcare availability for the treated and the control group into account and calculate the Wald estimator.

$$W = \frac{(L^S|T=1)-(L^S|T=0)}{(C|T=1)-(C|T=0)} \qquad (3)$$

where C is childcare coverage, the fraction of children covered by subsidized childcare. Using the participation and coverage rates given in Table 1, $W = 0.128$. This means that increasing childcare coverage by 10 percentage points would cause a roughly 1.28 percentage point increase in female participation rate. To refine this result, we estimate the following two-sample two-stage least squares (2SLS) regression in order to take regional differences of coverage and participation rates into account. For that, we apply a data strategy similar to Angrist (1990) and supplement the database with administrative data on childcare coverage rates. The first stage is:

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

Where

$$C_{yri} \equiv p^n_{yr}(1 - T_{yri}) + p^k_{yr}T_{yri} \quad (5)$$

p^n_{yr} is nursery school coverage[8] and p^k_{yr} is kindergarten coverage in township r and year y. C_{yri} is the regionally aggregated childcare coverage[9] in township r and year y for the relevant treatment group. Equation (5) shows that each individual is assigned the relevant regional nursery school coverage if the individual belongs to the control group and the relevant regional kindergarten coverage if the individual belongs to the treatment group. Equation (4) 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:

$$L_{yri} = \beta_2 \widehat{C_{yri}} m_{yri} + \alpha_y + \gamma_r + X'_{yri}\pi_{21} + S'_{yr}\pi_{22} + \pi_{23}\widehat{C_{yri}} + \pi_{24}m_{yri} + \xi_{2yri} \quad (6)$$

Where $\widehat{C_{yri}}$ represent the fitted values of C_{yri} from the first stage regression. In this setup, the parameter β_1 in the first-stage reflects how much group membership determines childcare availability. The parameter β_2 in the second stage is the main parameter of

[8] Nursery (kindergarten) coverage rate is defined as the number of seats available in nursery (kindergarten) in each township, divided by the number of children of age 0-2.99 (3-5.99) 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.

[9] Using aggregated coverage may introduce a 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 actual chance of acceptance. 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 paper, this error should not differ among treatment and control groups, and should therefore not bias the IV results

interest: it shows the estimated effect of childcare availability on labor supply, net of any seasonal effects.

The 2SLS results, depicted in Table 2.b, indicate a similar effect as the Wald estimator. In the third specification with all controls included, the effect of increasing childcare coverage by 10 percentage points, is a 1.35 percentage point increase in participation probability. The first stage results (Eq. (4)) are reported in Table 3.

1.5 Robustness and long-term effects

In the setup presented in the previous section, 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) argue 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 the same seasonal effects, but not the treatment effect, allowing us to separate out seasonal factors.[10] 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:

$$m_{yri} = \begin{cases} 1 & if\ 3 \leq a_{yri} < 4 \\ 0 & if\ 4 \leq a_{yri} < 6 \end{cases} \tag{7}$$

where a_{yri} indicates the age of the youngest child. The following DID regression is run:

$$L_{yri} = \beta^{s} T_{yri} m_{yri} + \alpha_y + \gamma_r + X'_{yri}\pi_1 + S'_{yr}\pi_2 + \pi_3 T_{yri} + \pi_4 m_{yri} + \xi_{yri} \tag{8}$$

where estimated effect corrected for seasonality is β^{s}, the coefficient of the interaction term. The coefficient estimates are reported in Table 4.a.

The corresponding 2SLS equations that help expressing the size of the effect in relation to childcare coverage, are the following:

$$C_{yri} = \beta_1 T_{yri} m_{yri} + \alpha_y + \gamma_r + X'_{yri}\pi_{11} + S'_{yr}\pi_{12} + \pi_{13} T_{yri} + \pi_{14} m_{yri} + \xi_{1yri} \tag{9}$$

Where (5) holds and the second stage is:

$$L_{yri} = \beta_2 \widehat{C_{yri}} m_{yri} + \alpha_y + \gamma_r + X'_{yri}\pi_{21} + S'_{yr}\pi_{22} + \pi_{23} \widehat{C_{yri}} + \pi_{24} m_{yri} + \xi_{2yri} \tag{10}$$

The results are reported in Table 4.b.

The estimates decrease by 2.2 percentage points to around 0.06 in the reduced form and by 4 percentage points to 0.095 in the 2SLS specification after correcting for

[10] According to our calculations, the seasonal effects suffered by the different age groups are similar. The regarding tests are not reported in the paper.

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seasonality compared to the baseline estimates reported in Table 2. This suggests that some seasonal bias may indeed be present, as the magnitude of the effect is affected a little by the correction. The estimate is still significant, and highly robust to the inclusion of control variables. The results are robust to altering the comparison group to those with children of age 2.

The key assumption for the DID estimates is that the participation probability in treatment and control group would follow the same time trend in the absence of the treatment. This parallel trends assumption may be tested by running regressions with various placebo cutoffs before 1st January, the actual cutoff date. We use 1st November and 1st September as placebo cutoffs and find that the estimated effect is insignificant[11], thus the assumption is likely to hold. The reduced form results without seasonal correction are reported in Table 9.

As a 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 6 summarizes the results. They indicate that there is a significant effect at age 3, but there is no effect at other ages. These findings are in line with what we observe in Figure 3: there is no significant difference between the groups – i.e. no birthdate-related effects – apart from at age 3, due to the difference in kindergarten eligibility.

Next, we narrow the birthdate windows around the cutoff from 5 to 4 months[12] and 3 months.[13] The results are similar to our main results, and are shown in

[11] The results are omitted, but available upon request

[12] Treatment mothers: children born between September and December, control mothers: children born between January and April.

[13] Treatment mothers: children born between October and December, control mothers: children born between January and March.

Table 7. 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.[14] We run the same specifications based on this measure as well, shown in

Table 8. 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.

1.6 Conclusion

In this study, we provide a causal estimate of the effect of subsidized childcare availability on maternal labor supply. We analyze the case of mothers of 3-year-olds in Hungary, who are much more likely to be able to enroll to subsidized childcare if they turn 3 before the 1st of January. The applied estimation technique overcomes some estimation issues (endogeneity of childcare availability and contamination of child age-related changes), and the results are robust to corrections for these. 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 mothers with older children – thus there is still high

[14] 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.

potential for labor market reactivation – such a policy intervention can have a significant positive effect. The results show that a 10 percentage point increase in availability can increase mothers' activity rate by 1.35 percentage points.

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.

Our results suggest that subsidized childcare increases maternal labor supply, though in a lesser extent compared to countries with institutions that facilitate the reconciliation of family and work obligations. The estimates of Bauernschuster and Schlotter (2015) – using a very similar methodology on German data - suggest that a supporting environment results significantly larger labor supply effects[15].

The effectiveness of childcare expansion may be limited by several factors: characteristics of maternity and parental benefits, lack of flexible work forms, societal views, the inflexibility of childcare hours,[16] etc. Our results reflect that other factors have a large impact: when children are around the age of 3 there is a sharp increase in mothers' activity rates of about 31 percentage points, of which increased childcare availability explains 13.5 percentage points. Determining the effect of other factors is out of 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

[15] Bauernschuster and Schlotter (2015) find that access to subsidized childcare increases the maternal labor supply by 35 percentage points.

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

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which suggests that they are related to the institutional framework.[17] 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.

[17] 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.

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Chapter 2

Who Benefits from Child Benefits? The Labor Supply Effects of Maternal Cash Benefit

2.1 Introduction

Policies have been enacted across Europe[18], seeking to increase female labor force participation and birth rate. Some policies include providing a substantial cash benefit to new mothers for a few years after child birth so that income issues do not restrict family planning. However, low rate of child birth and labor market participation is even more serious a problem for most Southern- and Eastern-European countries, thus, it is of high importance to examine the potential causes in this region. This study aims to examine potential policy reasons for the low labor market participation of mothers in Hungary, one of the low-fertility-low-participation countries of the region. The policy mix (maternity leave, cash benefit, job protection etc.) has often contradictory effects on the target indicators, and generally only their composite effects are to be identified. In this paper, one single element of the policy mix is examined; the effect of parental cash benefit on female labor supply is identified through a policy change.

This study adds to the literature by being the first to examine the mid-term (1-5 years) effect of a parental leave cash benefit on labor supply in an institutional framework that does not facilitate reconciliation of family and work. The middle and long-term effects

[18] In Poland, Germany and Hungary for instance.

of family policies are rarely examined and the few studies available on the issue are carried out in countries with "family-friendly" labor markets (for instance Norway, Austria and Germany), which help parents reconcile work and family obligations.

Drange and Rege (2012) find that Norway's cash-for-care program served as an incentive to exit full-time employment until 2 years after birth. This employment effect lasted until age 4, past the two-year incentive period when mothers were no longer entitled for the benefit, but thereafter the employment effect perished, the mothers returned to employment. The explanation of the perishing is that mothers stayed attached to the labor market through part-time employment. In another article, Lalive and Zweimüller (2009) examine the parental leave reforms of Austria, which in 1990 increased the parental leave from one to two years, had a large negative effect on the labor market participation probability of mothers with a child of 2. Most mothers in the study started to work part-time immediately after giving birth, and even after ten years from the time of giving birth full-time employment was well below pre-birth employment rates. In a third paper, Schönberg and Ludsteck (2007) show for Germany's child cash benefit program that the opportunity for maternity leave extensions above the two years increased the spell of maternal non-employment. On the labor markets examined by these papers the governments have adopted policies such that mothers can reconcile family and workplace obligations. These countries enable females with young children to participate in the labor market through part-time employment (33%, 35% and 25% of females work part-time in these countries respectively). Moreover in Norway, subsidized childcare is available for a large proportion (47%) of children younger than 2, and 80% for the under 6-year-olds. A remarkable share of Austrian female employees (56.7%) reported in the Labor Force Survey (LFS) questionnaire in 2005 that they can take whole days off for family reasons. Moreover, 61.4% of Austrian women asserted their ability to vary the start or the end of the working day for family reasons. As a result, the mothers of young children in these countries are able to return to the labor market soon by utilizing flexible work arrangements, as the above mentioned articles demonstrate.

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On the contrary, in many countries of Southern and Eastern-Europe most of the available full-time jobs do not provide flexible work options for new mothers and part-time jobs are scarcely available. Mothers' work options are limited to either working full-time or not working at all. In some of these countries, the case is worsened by low coverage of institutional childcare below age 3[19]. Hungary belongs to this group of countries. A mere 8.7% of the 0-3 year-olds were placed in nursery schools in 2008. The case is much better for children of age 3-6, more than 85% of these children have access to daycare. Indeed, mothers' labor market participation is proven to be determined in a large part by government-subsidized daycare and part-time job availability. (Bredtmann, Kluve and Schaffner (2009), Gutierrez-Domenech (2003), Bick (2010), Del Boca (2002)) As a result, after birth, most mothers in Hungary have to entirely withdraw from the labor market at least until the child can be enrolled to institutional childcare. Even if child care is available from the government, it becomes a question of whether the mothers would choose to resume working full-time or stay home longer with the child. Those who plan to return to the labor market are urged to start the job search as soon as possible, as their professional knowledge deteriorates and their job network shrinks while at home, leading to their reemployment probability and expected wage decrease On the other hand, mothers may choose to withdraw from the labor market for a longer period, as they deem full-time work and rearing a young child (less than 5 years old) not reconcilable. They prefer that they can stay home when the child is ill, spend the time after kindergarten together, etc. In such an institutional framework, similar family policies may have different effects compared to countries with family-friendly labor markets. The introduction of a parental leave with cash benefit may facilitate work-life balance in two ways. It may help either by providing means for outsourcing some of the housework, hiring a nanny and take a full-time job, or just the

[19] This cumulative disadvantage is present in a few European countries, such as Bulgaria, Greece, Hungary, Malta, Poland, Romania and Slovakia.

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opposite, it may supply with financials to afford staying home longer. Sauer-Cubizolles et al. (1999) also emphasize the importance of family benefits in reconciling family and work.

The paper uses micro data of the Labor Force Survey (LFS) to assess the short and long-term labor market effects of the Hungarian parental leave, GYED[20] enacted in 2000. GYED is a cash benefit which may be received until the child turns 2. The beneficiary receives a monthly amount of 70% of the previous one[21] year's average wage, with a ceiling of approximately EUR 360. Apart from Köllő (2008), this is the first paper that evaluates labor market effects of GYED. Köllő (2008) utilizes the termination of GYED in 1995, and finds no significant labor market effects. This paper in turn utilizes the re-launching of GYED in 2000 and finds a significant negative effect on labor supply, which is in line with the findings of Scharle (2007) on the Hungarian labor market.

In 2011, the amount of family cash benefits (in which GYED takes up a significant amount) reached 2.2% of Hungarian GDP, which was the fifth largest spending of this type among OECD countries, according to the OECD Family Database. The fertility and labor market outcomes of this system are very poor though. Hungarian mothers with 0-3 year old kids have the lowest employment rate (15%), and those with 3-6-year-olds have the third lowest, 55% employment rate in the EU (Blaskó (2009)). Blaskó (2011) gives a detailed description on the participation preferences of Hungarian women after birth. More than 94% of the Hungarians presume that the mother should stay home at least until the child turns 2. Moreover, Bálint and Köllő (2008) show that an average Hungarian woman stays home for 4.7 years after giving birth. On the other hand, the Hungarian fertility rate is positively affected by the present system of cash benefits (see Gábos, Gál and Kézdi (2008) and Kapitány and Spéder (2009)), but is still very low compared to the EU average.

This study focuses on the labor supply effect of this system, the probability of participation and employment of mothers with young children on the labor market. A

[20] "Gyermekgondozási díj" is the Hungarian name of the child cash benefit program, abbreviated GYED.

[21] The exact calculation period depends on various factors.

difference-in-differences (D-I-D) analysis is done, where the treatment (eligible for GYED) and the control (non-eligible for GYED) groups are compared before and after the launch of GYED in 2000 to estimate the labor market effect (probability of labor market participation and employment) of GYED availability. First, a linear probability model is used to estimate the effect of GYED on labor market outcomes, and then hazard model estimations are used to refine the results. The regression results reveal that GYED has a significant negative effect on participation and employment probability after the entitlement for the cash benefit ceased. This causes remarkable delay in returning to the labor market.

There are numerous explanations on why temporary withdrawal should affect long-term labor market outcomes of mothers. First off, the period of non-employment while on cash benefits may decrease women's human capital. (Mincer and Polachek (1974)). Gutierrez-Domenech (2005) finds that the longer a mother stays away from employment after child birth, the lower her reemployment probability. Even if a mother is able to find employment, the probability of reemployment at the previous wage level is also reduced (Mincer and Polachek (1974)). Kunze (2002) examined human capital depreciation in Western-Germany for parental leave and other factors and found that career interruptions due to parental leave has larger wage penalty, compared to interruption due to unemployment or national service. Prolonged absence from the labor market may lead to human capital gains in domestic duties, which further induces women to stay home (Becker (1991)). The size of the career-relevant network also influences the chance of reemployment probability. The longer the mother stays home, her network wanes increasingly. (Rees (1966))

Most papers examining child cash benefits for new mothers focus on immediate labor market effects of maternity leave. The studies are consistent that maternity leave has a significant effect on female labor supply. Longer maternity leaves are proven to increase return rate to previous employer and time spent out of the labor market after the leave ends. (See for instance Baker and Milligan (2008), Brugiavini et al. (2012), Baum (2003) ,

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Berhemann and Riphahn (2011)), Spiess and Wrohlich (2008), Haan and Wrohlich (2007) Fehr and Ujhelyiova (2010).)

The remainder of the paper is organized as follows. In section 2, I give a brief overview the Hungarian child benefit system and its most important changes in 2000. Section 3 gives a detailed description about the data used. In the fourth part, the most important identification issues are discussed. Section 5 presents the estimations and their results. Finally, in Section 6 conclusions are drawn.

2.2 Hungarian child benefit system

The Hungarian child benefit system is rather generous, regarding both the amount and also the duration of the benefits. From a few weeks before birth until age 3 of the child, the parents are entitled for some kind of benefit, as Figure 4 illustrates.

In the period before the examined policy change (1997-1999), as it can be seen on the top of the figure, only Extended parental leave (GYES) and Maternity leave (TES, or TGYAS) were available for the parents[22]. As the bottom part of Figure 4 illustrates, in the period after the policy change (2000-2002), GYED also became available.

TGYAS provided a monthly sum to those eligible, which equaled 70% of the previous average monthly wage. The benefit may have been received for 6 months. Mothers were eligible who had worked for at least half a year in the two years before giving birth. The main eligibility rule was supplemented by some other minor eligibility conditions, for instance eligibility with the previous child, full-time student status, etc. To GYED, the same eligibility rules applied, only the sum of the benefit was capped, not to exceed the double of the minimum retirement pension. GYED was provided until the second birthday of the child. For those ineligible for TGYAS and GYED, extended parental leave (GYES) was available in both periods from the date of birth until the third birthday of the child. Also for

[22] Family allowance is not mentioned here, as it did not change in this period for either group in the analysis.

the eligible, after the second birthday of the child, when GYED is not available anymore, GYES is granted until the third birthday of the child, similar to the ineligible.

The right and left panels of Figure 4 show the control and the treatment group: the control group consists of those ineligible for GYED. The treatment group incorporates two kinds of people: would have been eligible in the before period, and who were eligible in the after period. The eligibility is not observed in the period before the policy change. Therefore, as described later, the treatment status is imputed for the whole observation period based on the eligibility data of the period after the policy change.

Figure 5 shows the average monthly amount of GYED and GYES through time. As these figures reveal, the number of GYED recipients has shrink since 1990, but their number stayed comparable to the GYES recipient group who were non-eligible for GYED.

The existence of the GYED ceiling results that the top wage earners have a lower wage replacement rate compared to those who are not affected by this maximum. In 2010, 36.8% of the GYED recipients were affected by the GYED ceiling. This means that 36.8% of the GYED recipients would have received a higher amount in absence of the maximum limit. As a result, they had a less than 70% wage replacement rate. The others remained under the limit, so they had exactly 70% replacement rate.

2.3 Dataset and key variables

The analysis is carried out on a combined database, consisting of the Hungarian Labor Force Survey (H-LFS) data, T-STAR geographical data and data on the time needed to access the nearest municipality from the settlement of living. The H-LFS is a rotating panel dataset constructed from quarterly waves, each wave consisting of 70-80 thousand observations. The sample is stratified and clustered geographically. The unit of observation is a household, approximately 1/6th of which are removed and replaced by another household in each wave with each household staying in the sample for six periods at most. Each and every families and family members are documented in the observed household, along with their job market statuses, search activities and demographics. Based on the anonym identifiers, it is possible to link observations over time, so the database can be used as a panel dataset. The observations are weighted in the sample in order to maintain a representative sample.

The sample consists of women who gave birth to a child in the past 4 years, and whose family status is “wife”, “companion” or “one parent with a child”. I excluded women from the sample who are loosely attached to the labor market and who have never entered the labor force. I did not include males, because a mere 1.3% of GYED recipients were males (or females other than the mother) in the whole observation period.

Through the whole article, the age of the youngest child is referred to as the age of the child. There are some cases when a new child is born before an older child becomes two. In these cases, GYED eligibility is prolonged. For tractability reasons, I omit such observations from the sample.

The key explanatory variables of the model are *Treatment*, *After* and their interaction, *D = Treatment*After*. *Treatment* equals 1 if the mother is eligible and 0 if not eligible for GYED, that is, belongs to the treatment or the control group. In the 2000-2002 period *Treatment* is observable for those mothers children less than 2 years. In the period between 1997-1999 there was no GYED benefit and so no *Treatment* data is available, I have only information about the working history. Also, eligibility cannot be observed for those mothers who have a kid older than two years. Thus, the data on working history is used to impute the eligibility for all individuals[23].

Based on the 2001-2005 data of mothers (for whom both eligibility data and employment history is available) with children less than 1.5 years of age, I determined the working history which best separates the eligible population from the non-eligible. Those last having worked 40 months or less before child birth are regarded as eligible. On Figure

[23] According to law, eligibility is determined by working history in a large part. There are some minor conditions of eligibility, but from the analysis of the post-policy data at hand, it is clear that working history is by far the most important among the rules. The dataset contains information about the date when the mother was last employed. This variable proves sufficient to impute the treatment status.

6, I have plotted the rate of those receiving GYED, relative to the whole group receiving GYED or not. I have also plotted a polynomial trend, which shows that the rule indeed separates the group with low and high probability of being eligible.

The eligibility rule predicts eligibility fairly well in the sample from 2001-2005, the treatment status is predicted correctly in 74% (69% + 5%) of the cases, as Table 11 indicates. There is no reason to think that the precision of the eligibility imputation would be worse in the 1997-1999 period.

Though the imputation process seems rather successful, the rate of misclassification is still 26%. This may introduce biases in the estimation process. An analysis about the possible size of the imputation bias is included in Appendix IV.

After is a dummy variable indicating whether the child was born in a period, when the whole 1.5 year of GYED was available. As GYED was reintroduced on January 1, 2000, all mothers belong to the *After* period, whose child was younger than half year on that day[24]. Consequently, *After* equals 1 if the child was born on July 1, 1999 or later. Those born before that, belong to the *Before* period. *Before* and *After* periods consist of three years of data each.

The treatment effect is measured by the coefficient of D, which equals 1 if Treatment=1 and After=1, and equals zero otherwise.

The rest of the explanatory variables are standard factors of participation decision (i.e. age, level of education, regional variables, local unemployment, etc.).

Table 12 provides descriptive statistics about the sample divided by treatment status and *Before* and *After* policy periods. The statistics were calculated using population weights.

[24] There is a transitional sample of the mothers, whose child was less than 2 years old in January 1, 2000, but older than half year. These mothers became eligible for GYED on January 1, but they did not receive it through the whole 1.5 year, only for a shorter period, until the child turn 2. It is up to a decision in which group to add them. In this paper, these mothers belong to the Before period, in order to have the After group potentially receive the whole 1.5 year of GYED.

Table 12 indicates that the composition of the treatment and the control groups are different in a few important aspects: education level, employment history and local employment prospects (local unemployment level). In order to compare the treatment and control groups with similar characteristics, I used propensity score matching and dropped those observations from both groups that proved to contrast the most with the other group. In this way, 25% of the observations from both the treatment and the control group were dropped. As a result, the similarity of the treatment and the control group increased. At the same time, of course, the representativeness of the sample is diminished, as those with the very best labor market opportunities and those with the worst are dropped from the sample. However, the results are valid for the average females who belong to the region of common support.

A striking fact appears in Table 13, that both reemployment probability and probability of returning to the labor market increases for the control group between the two periods, however the same probabilities increase by much lower or even decrease in case of the treatment group. This may seem counterintuitive, because one would expect that the policy changed the behavior of the treatment group. Nevertheless, the increase of employment probability between the two periods is the effect of a general economic upsurge of the 2000's, which would have affected both groups similarly, by increasing their employment and participation rates were it not there GYED to mitigate this effect in case of the treatment group. (see Figure 11)

2.4 Econometric design and results

2.4.1 Identification

If a randomized experiment could be done, it would unfold the true effect of the treatment on the treated group. Though such an experiment is impossible to carry out, the thought experiment helps reveal the most important identification issues.

In this experiment, there would be women thinking about giving birth in period 0. In period 1, the experimenter assigns them randomly between the control group and the treatment group. The individuals in the control group do not receive Benefit, while those in the treatment group do. Then their fertility outcomes and their consequent labor supply decisions are observed. Finally, the labor supply outcomes of the two groups are compared. The difference is the effect of the Benefit on female labor supply. Labor supply is assumed to be affected by the treatment through various channels, which are presented below.

First, treatment may affect fertility decisions. Some of the control group members may decide not to have a child, as income lost from being unemployed would be too high and a lack of cash benefit would lead to a decision to not have children. In other words, they have a high alternative cost of giving birth. They decide not to bear children and stay active in the labor market. Through this channel, treatment could decrease labor market participation. Let us call this channel "sample selection".

Second, after birth until the child turns two, the treatment group members receive a high sum of cash benefit. This increases the reservation wage of the treatment group members; thus, fewer of them return to the labor market in this period. They may start to look for a job later than those in the control group because they stay home longer on average. This affects their human capital and the reemployment probability. Let us call this channel "income effect".

Third, after the second birthday of the child the treatment group mothers no longer receive the Benefit. However, the Benefit may have a longer-lasting effect, through the wealth accumulated through the months of receiving the benefit, this is called the wealth effect. Even if the amount of GYED were not accumulated, it may have helped the recipient families to preserve their savings, so the wealth effect still applies. In contrast, the non-eligible families had to use up their own savings to make their living in the period when the mother is out of work. Those who have more savings left (eligible group) may decide to stay home a few more months to take care of the child. On the other hand, those stringent of money (ineligible group) need to return to the labor market. On average, treated individuals have more wealth accumulated, which allows them to decide to stay home. This channel is called wealth effect. Income and wealth effect are going to be examined with linear probability models.

2.4.2 Baseline estimates

First of all, a preliminary calculation is provided about the employment and participation effect of the policy change. I show that the simple differences of the raw employment and participation probabilities (of Table 13) are already demonstrative of the size and the direction of the effect.

$$\Delta = (P^{Treated,After} - P^{Treated,Before}) - (P^{Control,After} - P^{Control,Before}) \qquad (1)$$

The results of this rudimentary DID analysis, involving no control variables, are shown in Table 14.

In the following subsections, I will provide parametric model estimations which complement these preliminary estimates. First, linear probability models are estimated which enrich the preliminary results by including various controls, and also provide standard errors for the point estimates. Second, hazard models are presented to further refine the estimations and explore the dynamics of the return process.

2.4.3 Linear probability models

In this part, I use two-state Markov-chain models for the purposes of the analysis. The dataset is utilized as a panel, in which two consecutive periods are used to calculate the transition probabilities between labor market states. The timing of the model has two periods. In the first period (t=0) working status (starting state of the transition) and the child's age are observed, individuals are sorted into the treatment or the control group. In the second period (t=1) the new labor market status (the end state of the transition) is observed. For an individual who is present in the database for six waves for instance, there are four transition observations available, so she is present in the dataset four times. To account for these duplications, the errors are clustered by individual.

Certainly, it would be more conventional, simpler and rather natural to model the events with labor status probabilities. However, this strategy would not work because of a data problem. As neither treatment status, nor reemployment date is available for individuals with employment as the starting labor status in the sample, these observations are dropped. As a result, raw state frequencies are biased and the employment and participation probabilities would be seriously underestimated. However, transition probabilities are unaffected by this problem, because only transitions with inactivity as a starting state are to be included in the measurement. (See more on this in Appendix II.) Thus, it does not matter whether individuals with employment as starting state are dropped or not. Even so, as the stocks build up from flows, this method allows inferring to the magnitude of stocks.

The following linear probability models are estimated.

$$\mathrm{trans(empl)_i} = \beta_0 + \beta_1 * \mathrm{After_i} + \beta_2 * \mathrm{Treatment_i} + \beta_3 * \mathrm{D_i} + \delta' \mathrm{Controls_i} + \varepsilon_\mathrm{i} \quad (2)$$

and

$$\mathrm{trans(part)_i} = \beta_0 + \beta_1 * \mathrm{After_i} + \beta_2 * \mathrm{Treatment_i} + \beta_3 * \mathrm{D_i} + \delta' \mathrm{Controls_i} + \varepsilon_\mathrm{i} \quad (3)$$

where $trans(empl) = 0$ if the individual is non-employed in t=0 and in t=1, and $trans(empl) = 1$ if the individual is non-employed in t=0 and employed in t=1. Similarly, $trans(part) = 0$ if the individual does not participate in the labor market in t=0 and in t=1, and $trans(part) = 1$ if the individual is non-participating in t=0 and participating in t=1. Any other cases are dropped. The parameter of interest is β_3.

The regression results are reported in Table 5. The estimations are repeated for two child age categories. The estimates that incorporate the first two years of the child are meant to check for the income effect. The estimates referring to the period after the second birthday test whether there is any wealth effect. The estimation samples are divided to subsamples. The members of the high level education group have a high school graduation with profession or higher education level. Those with levels of education lower than that belong to the low level education group.

The estimates indicate that the quarterly transition to participation probability of the treatment group decreased by 1.6% on average as a result of GYED. This equals a 6.4% decrease in yearly transition probabilities. Taking into account that there are 5 years included in the study, this sums up to a 32% overall employment effect. Before the second birthday GYED does not have a significant effect on participation nor on employment either in the group of high or the low educated mothers. This result is not surprising, because the participation and employment rates of Hungarian mothers of children younger than two are rather low, about 0-10%. Thus GYED could scarcely have a significantly large negative impact on them.

On the contrary, the effect after the second birthday is significant and negative in case of the employment of mothers with low level of education. This supports the wealth effect hypothesis. The probability of quarterly transition from non-employment to employment after the second birthday decreases by 2.4% as a result of GYED. For the whole time span included in the analysis, until the 5th birthday, this equals a 28.8% increase. This estimation is likely to underestimate the effect, as the fact of censoring is not handled in the Markov model. In the next section, the dynamics of the labor market return are examined.

2.4.4 Hazard models for labor market participation

The dependent variable of the hazard model presented in this subsection is duration from the child's date of birth to the first labor market participation of the mother. The analysis time starts for each mother at the date of child birth. In the majority of the cases the mothers do not participate in the labor market after the date of childbirth. Then after a period, mothers start to participate in the labor force again, that is, they start job search or become employed. However, there are mothers who are not followed until their return to the labor market; thus, there is right censoring in the model.

The duration of non-participation in the labor market after giving birth is measured with a hazard model. Let T be the random variable of the duration, with $T \geq 0$. Let t be a realization of T. Let the participation hazard function show for a small $h \geq 0$ the probability that a given mother will return to the labor market over the period $[t, t + h]$, given that she did not participate until time t.

$$\lambda(t) = \lim_{h \to 0} \frac{\Pr(t \leq T \leq t + h | t \leq T)}{h} \tag{4}$$

and thus

$$f(t) = \lambda(t) e^{-\int_0^t \lambda(s) ds} \tag{5}$$

There are a few points to stress about model selection. First, it is important to review all the factors affecting the hazard of reentering. At the beginning of the spell, the hazard of reentering is very low, because only few women would like to go back to work with a less than one-year-old child in Hungary. Then the hazard starts to increase faster, as more and more women want to get back to work. As time elapses, reemployment becomes more difficult because women at home do not follow the trends of their profession, their knowledge becomes outdated or they fell out from the daily business routine. This effect is presumed to be stronger for a high-skilled workforce. This means that the duration dependence of the hazard ratio is likely to be negative.

$$\frac{\lambda(t)}{dt} < 0 \tag{6}$$

The accuracy of measurement of the duration length hinges on two factors, the accuracy of the date of birth and the time of reemployment. For some years, the precise birthdate of a child is not listed. In those years, I estimate the birthdate to be +/- 45 days on a uniform distribution from the quarter in which they were born. In later years, when the precise birthdate is listed, the exact date of birth is used[25]. For the years, where actual date of birth is available in the database, I could plot actual against imputed birth dates. The result is shown on Figure 7.

There are similar data issues with the reemployment dates. There is quarterly data available on the employment status of the mothers, so the measurement error also lies between 0-90 days. On the other hand, the distribution may have some mass points, because of the practice of choosing the first day of the month as starting date. If the distribution were truly uniform, the expected value of the measurement error of the spell length (time elapsed between birth and reemployment date) would be 36 days. This error is independent of other factors related to the hazard rate, and is relatively small compared to the average spell length in the sample.

2.4.5 Semi-parametric model

First, Cox proportional hazard model is estimated, in which

$$\lambda(x, t) = \lambda_0(t)e^{\beta' x} \tag{7}$$

is assumed. The advantage of this model is that $\lambda_0(t)$ is estimated non-parametrically, thus, no specific assumptions are needed. The only important assumption needed is the proportionality assumption. The estimated cumulative hazard curves of the treated and the control group are plotted on Figure 8.

The cumulative hazard curves are mostly parallel to each other, especially after the first year (0 on the horizontal axis), which is of special interest to this paper. This confirms

[25] In the dataset, there is yearly data on the age of the family members. However, utilizing that the quarterly reported age increases by one in the quarter of birth, I have information on which quarter the child was born. This means that the measurement error of the date of birth is of uniform distribution, and lies between 0-90 days, with an expected value of 45 days.

the assumption that these are scaled versions of each other, that is, the survival functions are proportional to each other.

The hazard sample consists of 6,685 subjects, of which 1,158 exits from non-participation is observed. The rest of the sample is censored; these individuals are still out of the labor force when they exit from the sample. The large number of censored observations is likely to introduce an expansion bias (bias away from zero (Rigobon and Stoker (2007))), because those with longer duration are more likely to be censored. At the extreme, those never returning to the labor market after giving birth will be censored for sure. The likelihood function of the estimation with censored observations is the following:

$$\ell_i = f(x_i, t_i; \theta)^{1(t_i<c_i)}[1 - F(x_i, t_i; \theta)^{1(t_i=c_i)}] \quad (8)$$

where t_i is the analysis time for individual i, and c_i is the date of censoring.

The parametric part of the model is the following:

$$\exp\{\beta' x_i\} = \exp\{\beta_0 + \beta_1 * After_i + \beta_2 * Treatment_i + \beta_3 * D_i + \delta' Controls_i + \varepsilon_i\} \quad (9)$$

The estimated baseline hazard functions and their 95% confidence intervals are shown on Figure 9 by treatment status and before and after periods.

After the 2000 policy change, reentering hazard following the second birthday increases for both the treatment and the control group. However, it is clear that the increase is larger in case of the control group.

First, the regression results from the Cox model are reported in Table 16 with the standard errors under the estimated parameters. The regression results confirm that the Benefit has a significant negative impact on reentering hazard. It decreases by 37% according to the third specification. As a robustness check, two types of parametric models, an exponential and a Weibull model were estimated. The results are similar to the Cox results and are omitted. The estimated value of the ceteris paribus effect of the Benefit is significant and negative, -37%. That is, the hazard of return to labor market decreases by 37% if someone becomes eligible for the Benefit ceteris paribus. However, this result is likely to be biased; the real effect is expected to be closer to zero. Thus, the estimation can be regarded as an upper bound (in absolute terms) to the effect.

2.5 Results

The results of the Markov and the survival models suggest a participation effect between 32% and 37% in negative terms. Taking into account the positive selection into motherhood, the real effect should be larger in absolute value, than the estimates. Thus, the 32% lower bound is valid for the effect, but we cannot tell the upper bound from the estimates at hand.

At first glance, these results seem surprising, because GYED is received in the first and second year of the motherhood. So, one would expect a sharp decrease in reemployment probability in these two years, and none or much smaller effect in the consecutive years. However, reemployment probability in the first two years of the motherhood is less than 2% across each group and each period, which indicates a very strong preference for staying home with a child younger than 2 years, regardless of the transfer received. Thus, launching GYED has narrow scope to further decrease reemployment probability in the first and second year after giving birth.

The effect of GYED in the third and fourth year can be explained by its effect on accumulated wealth. Having received a large monthly sum in the first two years of motherhood, makes it possible for the mother to afford one or two more years spent at home with the child.

The results are significant in case of the mothers with low levels of education, which may be explained by the size of the child benefit. It is possible that in case of low educated (and most probably low income) mothers, the sum of GYED is relatively high compared to the previous earnings, because it is not affected by the GYED ceiling. Thus, the child benefit induces a behavior change in case of these mothers. Furthermore, it is likely that those with high level of education, are able to adjust their labor supply timing to their preferences even in absence of GYED. As Blaskó (2009) suggests, it is not the exact timing of return to labor market that matters for the child wellbeing, rather that the mother can adjust the timing to her personal preferences. These results suggest that low income mothers benefit from the GYED by becoming able to adjust their labor supply to their own preferences.

The results raise questions about the optimal design of child benefits. There is a tradeoff between labor market efficiency and redistributive effects of child benefits. In case of the GYED, the redistributive effects are stronger, as the child benefit has a higher replacement rate in case of lower income mothers. On the other hand, and as a result of the design, these mothers suffer more in terms of labor market efficiency, as their return to the labor market is significantly delayed by the child benefit.

First, it suggests that those with high level of education, and most probably with higher income, are able to adjust their labor supply timing to their preferences even in absence of GYED. On the other hand, those with lower income should return to the labor market sooner than their ideal in absence of GYED. As Blaskó (2009) suggests, it is not the exact timing of return to labor market that matters for the child wellbeing, rather that the mother can adjust the timing to her personal preferences. Thus, these results suggest that children (and mothers) of low income families benefit from the GYED by becoming able to adjust their labor supply to their own preferences. Second, it may be that in case of the lower educated mothers, the wage replacement rate is higher compared to those with higher levels of education. (Recall that the amount is capped which affected more than 30% of the mothers in 2010.) Thus, the difference between the effects for high and low level educated mothers may be due to the size of the benefit: the larger the benefit, the larger the effect.

2.6 Identification issues and robustness

The first identification issue to deal with is sample selection, namely negative and positive selection into motherhood. The standard negative selection into motherhood

(Lundberg and Rose (2000)) works as follows: those mothers with lower productivity (less talent, less career-oriented) are more likely to bear a child as the alternative cost of the child is lower in their case. Career-oriented attitude is likely to be correlated with reemployment probability, so the sample selection is endogenous. Accordingly, unobserved heterogeneity of women causes endogenous sample selection. Mroz (1987) examines the exogeneity of fertility to labor supply decision, and confirms that including such an exogeneity assumption does not imply significant change in the results. Modena and Sabatini (2010) come to the same conclusion, that is, data does not support that higher opportunity cost of motherhood is responsible for lower fertility. On the contrary, Lundberg and Rose (2000) find evidence by visual inspection on negative selection into motherhood, but its magnitude and significance is unknown. Adda and Stevens (2011) detect negative selection into motherhood. Those with high ability represent 27.4% of their sample, and the total fertility rate of this group is 1.53 compared to 1.83 of the low ability group. This suggests a 17% fertility decrease on average. Similarly, Gayle and Miller (2006) find that the number of children is negatively related to the level of education, because the higher alternative cost of children for higher educated. There are some further papers that underpin endogenous fertility (e.g. Keane and Sauer (2009)). This type of sample selection may be present throughout the whole observation period (1997-2002).

After the reintroduction of GYED in 2000, there is positive selection. Those who had a job before child birth, which made them eligible for GYED (a higher sum of benefit than before), would decide to bear a child, as the alternative cost of child bearing decreased for them. Of course, the alternative cost of childbearing did not change for the ineligible. Laroque and Salainé (2005) show that financial incentives indeed increase fertility, a child benefit of 0.3% of the GDP is expected to raise total fertility by 0.3 percentage point. Gábos et al. (2008) demonstrate a similar effect on Hungarian data.

As a solution to this problem, many authors assume joint fertility and labor supply decision (e.g. Apps and Rees (2004), Laroque and Salainé (2005), Bick (2010)). The structure of the problem in this article does not allow for such a structural model. However, the direction of the bias can be derived, as follows.

In the period where the GYED did not exist, there is only negative selection present. In the post-policy period there is the possibility for negative and positive sample selection present at the same time. Assuming that the magnitude of the negative selection does not change in these years, it is fairly easy to show that there is upward bias resulting from the selection.

Let H_{ij}^{k} denote the hazard of participation, where

$$\mathrm{i} = \begin{cases} \mathrm{t} & \text{if Treatment Group} \\ \mathrm{c} & \text{if Control Group} \end{cases} \tag{10}$$

and

$$\mathrm{j} = \begin{cases} 0 & \text{if Before Policy Period} \\ 1 & \text{if After Policy Period} \end{cases} \tag{11}$$

and

$$\mathrm{k} = \left\{\begin{matrix} 0 & \text{if no sample selection} \\ - & \text{if only negative sample selection} \\ + & \text{if negative and positive selection} \end{matrix}\right\} \text{assumed} \tag{12}$$

Let TE^{k} denote treatment effect for three different cases, indicated by superscript $k = \{0, -, +\}$.

Let's start with the case when there is no sample selection. In this case, the treatment effect is:

$$\mathrm{TE}^{0} = (\mathrm{P}_{\mathrm{t1}}^{0} - \mathrm{P}_{\mathrm{c1}}^{0}) - (\mathrm{P}_{\mathrm{t0}}^{0} - \mathrm{P}_{\mathrm{c0}}^{0}) \tag{13}$$

This is the true treatment effect.

In the next step, look at the case when negative selection is taken into account. I assume that the magnitude of the selection bias (ε_{ij}) does not change between the two periods. However, selection bias may be different for treated and control groups, such that: $\varepsilon_{i0} = \varepsilon_{i1} = \varepsilon_i$. This assumption indicates that the change in GYED regulation did not affect productivity and expected wage. I do not assume anything about the size and sign of ε_c and ε_t. The participation hazards in this case are the following:

$$\mathrm{H}_{t1}^{-} = \mathrm{H}_{t1}^{0} - \varepsilon_t \tag{14}$$

$$\mathrm{H}_{t0}^{-} = \mathrm{H}_{t0}^{0} - \varepsilon_t \tag{15}$$

$$\mathrm{H}_{c1}^{-} = \mathrm{H}_{c1}^{0} - \varepsilon_c \tag{16}$$

$$\mathrm{H}_{c0}^{-} = \mathrm{H}_{c0}^{0} - \varepsilon_c \tag{17}$$

It can be shown easily that $TE^{-} = TE^{0}$:

$$\mathrm{TE}^{-} = (\mathrm{P}_{t1}^{-} - \mathrm{P}_{c1}^{-}) - (\mathrm{P}_{t0}^{-} - \mathrm{P}_{c0}^{-}) = ([\mathrm{P}_{t1}^{-} - \varepsilon_t] - [\mathrm{P}_{c1}^{-} - \varepsilon_c]) - ([\mathrm{P}_{t0}^{-} - \varepsilon_t] - [\mathrm{P}_{c0}^{-} - \varepsilon_c]) = \mathrm{TE}^{0} \tag{18}$$

In the third case, I assume that both negative and positive selection is present. In fact, this is what can be measured with the methodology used in this paper. I assume that the overall selection bias is additively separable: $S(p) = \varepsilon(p) + \mu(p)$ where μ denotes the bias caused by the positive selection. The positive selection is present only in the treatment group, in the second period, $i = t,\ j = 1$.

The participation probabilities are the following:

$$P_{t1}^{+} = P_{t1}^{-} + \mu_t \tag{19}$$

$$P_{t0}^{+} = P_{t0}^{-} \tag{20}$$

$$P_{c1}^{+} = P_{c1}^{-} \tag{21}$$

$$P_{c0}^{+} = P_{c0}^{-} \tag{22}$$

A further assumption is that $\mu_t > 0$. This assumption owes the idea that the higher the expected benefit – that is, the higher the productivity and the probability of participation – the stronger the positive selection will be. As a consequence, on average, individuals with higher participation probability will select into the treatment sample more frequently after the policy change, so the average participation probability will be higher.

It is straightforward to show that $TE^{+} > TE^{0}$:

$$TE^{+} = (P_{t1}^{+} - P_{c1}^{+}) - (P_{t0}^{+} - P_{c0}^{+}) = ([P_{t1}^{-} + \mu_t] - P_{c1}^{-}) - (P_{t0}^{-} - P_{c0}^{-}) = TE^{0} + \mu_t > TE^{0} \tag{23}$$

Thus, the overall selection process causes a positive bias in the regression results. If the Benefit has a negative effect on labor supply as expected, this bias means that a smaller negative effect is measured in the regressions.

2.7 Endogenous treatment

The present paper makes use of the policy change in 2000, when the Benefit was reintroduced after four years. This time variation allows assessing the labor supply effects of such a large amount of cash benefit. However, the treatment variable is clearly endogenous, as it is defined by previous working history, which affects employment probability and labor supply. Still, the time variation allows for identification, if DID approach is used. The most important assumption for identification in a DID setup is that

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the participation probability of the treatment and the control group follow a parallel trend in the period preceding the policy change. If it can be assumed that the participation rate of the groups move together, then DID identifies the treatment effect. The parallel trend assumption is commonly examined by testing the significance of a placebo treatment in the period before the policy change. The results for the test are presented in Table 17 for placebo treatment dates on 1st December 1996 and 1st December 1997. The coefficient is near zero and insignificant in each specifications, providing a strong evidence for parallel trends assumption.

To make sure that the D-I-D approach is valid, it is important that there is no other major policy change in the sample period that would affect the outcome variables differently across the treatment and control groups. In the period of the examination, a major policy change was implemented, which could potentially have an effect on the labor supply.

The mandatory minimum wage was nearly doubled in two steps in 2001 and 2002, which was quite a large change compared to the previous years (see Figure 12). Though there is no wage data available in LFS, it seems a plausible assumption that the wage level correlates with the level of education. The distribution of education is more or less the same in the treatment and the control group after matching (check Table 13), thus, the ratio of individuals affected by minimum wage laws are comparable in the two groups. As a result, the difference between the treatment and the control group should not come from the minimum wage change. Nevertheless, I test the minimum wage effect by directly including its yearly sum in the regressions, which does not change the estimation results (tables are omitted).

2.8 Conclusion

This paper provides evidence on the long run negative effect of maternal cash benefit on female labor supply. There are a few studies available measuring this effect in countries with an institutional background which supports reconciliation of family and work for young mothers. These studies measured negative significant effect on female labor supply in the middle or the long run. On the contrary, the author of this article does not know about any studies regarding countries where the labor market and childcare institutions do not facilitate such reconciliation. The hypothesis of the study is that mothers use the monetary resources received to reconcile family and work duties, either by staying home longer or returning to labor market and maybe outsourcing some of the housework or buying childcare services.

The estimations show that mothers indeed stay home longer, the cash benefit affects labor supply in the middle run (2-5 years after birth). Those mothers with low level of education, and probably low income, are more affected than those with higher level of education. This suggests an explanation that those with higher level of education are able to adjust their labor supply behavior to their and the family's needs, even in absence of the cash benefit. On the contrary, the benefit helps mothers with low level of education to delay their return to labor market and thus adjust labor supply to their preferences.

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Chapter 3

Evaluating The Effect Of START Plusz Hiring Tax Credit Program On The Employment Probability Of Mothers With Kindergarten-Age Child

3.1 Introduction

In the recent years many countries started to apply tax credit in order to increase the participation and employment rate of disadvantaged groups like the long-term unemployed, the disabled, the young or single mothers. After the US, earned income tax credit or similar schemes were introduced in Canada, the UK, Sweden and other countries. In 2007, Hungary extended its hiring tax credit program 'START' to mothers with a child under 4. The new program (called 'START Plusz') offered reductions from the social security tax obligations of employers who newly hired from the target group.

The hiring tax credit programs designed for mothers of a young child help those with potentially disadvantageous labor market position to become employed. There are two potential sources of the disadvantage in case of mothers. First, the more time mothers spend home with raising the child, the more their human capital deteriorates. (Mincer and Polachek (1974)) Their actual professional knowledge and skills depreciate, they fail to keep up with their profession, and their professional network shrinks as well. The other potential source of lower employment probability is the higher expected levels of absence due to the child's sickness in the first few years (Amilon and Wallette (2009)). Both factors are more prevalent for mothers with more children, respectively because of longer time at home needed for more children (except of course the case of twins), and because of the higher cumulative probability of having a sick child. This paper confirms that the wage subsidy has a higher effect on the employment probability in case of mothers with multiple children.

It is of outstanding importance to study factors influencing maternal employment in countries struggling with low fertility rates, as employment prospects of mothers after delivering a baby strongly influence fertility decisions (Baughman and Dickert-Conlin (2003); Milligan (2005); Brewer et al. (2008)). The more likely is the mother to get a job after the child rearing period, the more willing the family is to decide to have a child.

The START Plusz tax credit program is regarded as successful in government communication, but there is not much empirical evidence on its actual effects. Scharle et al. (2013) use administrative data for policy evaluation purposes. Unfortunately the administrative data includes no personal or family characteristics, moreover, it does not allow for defining a suitable control group. As a consequence, that data is not suitable to provide a measurement on the employment effect of the policy.

This paper aims to answer the question whether START Plusz tax credit program increases mothers' employment probability. I use a difference-in-differences approach using the years before and after the introduction of the program in July 2007. I show that the tax credit did not have a significant effect on the mothers' group as a whole, but it had a significant positive effect in one subgroup: mothers of multiple children, with vocational and higher educational attainment. I provide a robustness check using placebo treatment in years around 2007 and show that the results found are indeed the effect of the policy change in 2007. The effect however vanishes after two years, most probably as a result of the economic crisis.

The remainder of the paper is organized as follows. In Section 2, a simple theoretical framework is presented, and also an insight into the institutional background for START

Plusz Program in Hungary. This is followed by a description of the estimation methodology and the dataset used in Section 3. In Section 4 the estimation results are presented and Section 5 concludes.

3.2 Background and framework

3.2.1 Theoretical framework and related literature

This paper takes a slightly modified version of the simple labor supply and demand framework of Dickert-Conlin and Holtz-Eakin (2000) and Neumark (2011) as a basis for examining the potential effects of a tax credit offered to firms on new hires. Panel a. of Figure 13 shows the effect of a hiring tax credit which reduces the effective employment cost w to w(1-c). First, the labor demand curve shifts outward, because at each level of wage, the effective cost of the employer is lower. Thus, from the same human resources budget, more employees could be hired. As a result, the employment increases from E to E', and the wage paid to workers increases from w to w'.

According to this simple theoretical framework, the hiring tax credit increases employment rate. The incidence of tax credit may be on the employer or the employee[26], thus the effective net wage may (Figure 13.c) or may not (Figure 13.b) increase. It mainly depends on the actual rate of unemployment, the size of the tax credit and the elasticity of labor supply. However, this article does not examine the incidence rate of the policy; I rather concentrate on the employment effect.

Such a tax credit may introduce some negative effects as well. To begin with, the program subsidizes new hires. It is widely documented in the literature that hiring tax credit may boost job destruction rates by creating incentives for churning employees, so that employers could benefit from the subsidy on new hires. (see e.g. Mortensen and

[26] Leigh [2010] for instance, studies Earned Income Tax credit of the United States, and finds that if the generosity of EITC increases by 10 percent, the wage falls by 5 percent for school dropouts and by 2 percent for those with high school diploma.

Pissarides (2001)) As a result, as shown in Figure 14, the substitution of existing employees (rectangle A) to new hires (rectangle B) can be quite large. Another related problem is that the tax credit may be given to new hires which would have occurred anyway. Bartik (2001) approximates the ratio of needlessly subsidized new hires to 90%. This means that the overall employment rate is only slightly affected by the tax credit.

START Plusz was not intended to increase overall employment, only to increase employability of the disadvantaged groups. However, I take a note on the possible substitution effect between the group of the targeted and not targeted job seekers, which I will return to later on. It is essential to add at this point, that even if the number of new hires would not increase in the economy, the relative employment growth of the disadvantaged group would count as a success of the program.

The third problem is that in case of tax credits and wage subsidies targeting the disadvantaged a stigmatizing effect was found, and as a result, the participation rates of employers were strikingly low, which annulated the possible employment effect of the subsidy. (see e.g.: Burtless (1985), Deuchert and Kauer (2013)) In case of START Plusz Program for mothers, there is no such effect, because being eligible for the credit does not reveal any additional information about the employee to the employer.

As a further issue, administrative burden related to the subsidy may diminish its impact by decreasing net gains of the employers from participating in the program. The administrative cost related to START Plusz Program was very low; it was only a small fraction of the expected monetary gains from the program (Scharle (2013)).

There are numerous examples of tax credits and wage subsidies around the world among the developed and the developing countries. (For instance, Work Opportunities Tax Credit in the USA, SPAK in the Netherlands, Youth Wage Subsidy Pilot in South Africa etc.). Accordingly, there are many policy papers that evaluate the effects of such policies. About programs in the US, Hotz and Scholtz (2003) and Neumark (2011) provide an overview. De Giorgi (2005) reports a significant employment effect about 6 percentage points of New Deal for Young People program in the UK. The same program is studied by Blundell et al.

(2004), Van Reenen (2001) and Dorsett (2004), who finds that the wage subsidy decreases unemployment probability much more compared to training, work experience in the civil sphere or public work.

Some papers examine the effect of tax credit programs on female participation, for instance of Earned Income tax credit (EITC) in the US (e.g. Eissa and Liebman (1996), Meyer and Rosenbaum (2001)) and Working Families' Tax Credit (WFTC) in the UK (e.g. Gregg and Harkness (2003), Francesconi, van der Klaauw (2007), Blundell et al. (2005) and Blundell et al. (2013)). These articles are mainly concerned with single mothers and most of them find a significant positive effect.

The most relevant strand of literature to this paper focuses on female employment effects of tax credits. There are studies on the WFTC in the UK (e.g. Azmat (2014) measuring an ambiguous employment effect; Francesconi and van der Klaauw (2007) estimating a 5 percentage points employment increase for single mothers and Blundell et al. (2013) using a life-cycle model); on the Spanish tax credit (Azmat and González (2010) finding a positive effect of 2% and suggesting that the effects are more pronounced for less-educated women), the Swedish EITC (Edmark et al. (2012) finding no significant effects) and the EITC in France (Stancanelli (2008) detecting negative employment effect for married women, positive employment impact for cohabiting women and no significant effect for single women; Bloemen and Stancanelli (2007) finding no significant effect). The microsimulation results of Haan and Myck (2006) suggest that introducing a tax credit similar to that of the UK would increase employment rate of single individuals but decrease that of couples in Germany.

In many studies, (see e.g. Brewer and Browne (2006) for a review) females without a child are used as a control group for difference-in-differences estimates. However, this control group is suspected to violate key identifying assumptions because of the changes in relative covariates over time, as mentioned by Azmat (2014). In this study, I use the specific policy rule for identification, that only mothers of the youngest child up to 4 years are eligible for the tax credit in 2007. Thus, mothers with a kindergarten-aged (5-7-year-old) child can be used as a control group, which is much more similar to the treatment group, thus the difference between the employment rate of the two groups is safely assumed to be stable over time in absence of the treatment.

3.3 Institutional background and basic facts

The START Plusz Program was launched in 2007 with the aim to provide incentives to employers to hire disadvantaged people. The form of the subsidy was a hiring tax credit, which was available if the prospective employee applied for a START Plusz Card at latest the day before the employment started. In that case, the employer was exempt from paying a part of its social security tax obligations. In the first year of holding the Card, the amount of the credit was a 17 percentage point reduction of social security taxes (instead of 32%, the employer has to pay 15% and is exempt from lump-sum social security payment, EHO), up to the double of the minimum wage. In the second year, the reduction is lower, 7 percentage points (instead of 32%, the employer has to pay 25% and is exempt from lump-sum social security payment, EHO). As a result, the maximum tax credit amounts to about 17% of the total cost of employment in the first year and reduces to 7% of that in the second year. This is the treatment intensity of the START Plusz program[27].

The START Plusz Card was available from 1st July 2007, until 31st December 2011. Non-employed mothers with a child of age 1-3.99 were eligible for the Card, after the end of their family benefit (GYES[28], GYED[29], GYET[30]), or mothers who wanted to work besides receiving parental leave (GYES). If the mother becomes employed within the eligibility

[27] It is very low compared to National Supported Work (NSW) in the US for instance, which covered the whole wage cost of the target group (e.g. single mothers, drug users, high school dropouts) for 9-18 months, moreover, provided consultation possibilities for the participants.

[28] Extended parental leave (Gyermekgondozási segély)

[29] Parental leave (Gyermekgondozási díj)

[30] Extended parental leave (Gyermeknevelési támogatás)

period (before the child's fourth birthday), her employer in entitled for the subsidy for two years after issuance of the Card.

Long-term unemployed were eligible for the Card as well. In the administrative data, these groups cannot be easily separated, thus the available statistics and also the results of Scharle et al. (2013) refer to the combined group of mothers and long term unemployed. However, in the dataset I use for evaluation, the distinction is straightforward, thus in this paper, I focus on the employment effects of mothers and omit long-term unemployed from the analysis.

The Program was administered by the National Tax Agency (NAV). According to their data, until the end of 2010 9621 Cards were issued to females. It is unknown how many of these cardholders were long term unemployed and what fraction consisted of mothers with young child. Based on CSO[31] demographic data, the total number of cardholders was approximately 3.3% of the total population of mothers with a child aged 1-3.99 years. Thus, this is the upper bound for the rate of treated to eligible. The average age of the Card holders was 34 years (Scharle et al. (2013)). The educational attainment of the Card holders is reported in Table 19. According to the last column, those with vocational and high school, applied for the Card in a higher rate compared to those with elementary schooling and college degree. Thus, if the Card has any effect at all, in spite of its low budget and low number of treated, I expect to find that effect in this group.

Applying for the Card posed small administrative burden on the employer and the employee. The eligible mothers could apply rather easily, and the employers were not forced to further employ the Card holders (during or after the Card validity), if they did not like to. Therefore, only 82% of the female cardholders were employed at the same employer half a year after entering a job. As a result of the decreasing rate of the subsidy, there is a breakpoint at the end of the first year of the Card's validity (and also the end of the first year of employment). By the end of the first year, this rate shrank to 60%, and after two

[31] Central Statistics Office of Hungary. http://www.ksh.hu/nepszamlalas/tablak_demografia

years it was merely 35%. (Scharle (2013)) In contrast, the employment rate of those starting National Supported Work program of the United States was 65% by the end of the first year and 40% after the end of the subsidized period.

3.4 Methodology and data

As the subsidy period starts on the very day of registering for the Card, the rational application strategy is the following. The eligible mothers did not apply for the Card in advance. Rather, most probably, they applied for it only after receiving an employment offer, right before the first day of employment, thus maximizing the subsidized period available with the Card.

In Table 20, the timing of cardholders' employment is listed. It is apparent that most of them become employed indeed shortly after applying for the Card, which underlines the strategic application process. There are only a few cases (3% of the cardholders) when the cardholder does not get a job, and the reason probably is that the employer retrieved the employment offer after the employee registered for the Card, or the employee was simply not aware of the rules.

As a consequence of this strategic process, there are some important issues which cannot be examined directly, because the actual treatment is observed mostly only in cases when the individual received an employment offer. Thus, the question whether actually holding a Card reduced the time searching for a job or whether holding a Card increased employment probability, cannot be studied in this policy setup.

As a result, this study executes an intent-to-treat analysis, and use a DID method. Non-employed mothers with a child of age 3-4 are eligible for the Card after 1st July 2007. Thus these mothers belong to the treatment group in the after policy period (1st July 2007-1st July 2008) (*treatment-after* group).

Mothers with children aged 1-3 are also eligible for the subsidy. However, data shows that the vast majority of mothers intend to go back to work when the child reaches age 3. Also, focusing on the 3-year-olds eliminates other influencing factors such as maternity leave or childcare availability issues, which pose serious restrictions on maternal labor supply at younger ages of the child (see Lovasz and Szabo-Morvai (2013) for details).

Choosing the control group and the before period is not straightforward, as it is shown in Figure 15. On the figure the age of child is depicted against calendar time. The vertical line at year 2007 represents the border between before and after periods. Along with passing of time, trivially the individuals grow older, thus they move upwards along the 45^0 lines, each starting at point (*date of birth; 0*).

The control group and the before period have to be defined such that no individuals could move between the DID categories as the time goes by, introducing bias to the estimations. A natural candidate for the "before" group in the estimation would be the year right before the introduction of the Program (1st July 2006-1st July 2007). Also, it would be straightforward to define the mothers of older children attending to kindergarten (4-7-year-olds) as the control group. Nevertheless, as it is shown on Panel a of Figure 15, there are two subgroups (A and B) which may change DID group as time elapses. Group A (highlighted triangle) consists of mothers whose child is 3-4 years old before the Program is introduced, and they are still in this age category after the introduction. Panel b is the enlargement of the part of Panel a, which includes group A and B. It shows that a mother would be untreated in point a (because the Policy is not implemented yet), then become potentially treated (eligible) after the implementation (point b) and become untreated again (because of ineligibility) after her child turns 4 (point c).

It may happen that these mothers in group A postpone their job search or the employment contracting in order to become eligible for the subsidy. It may simply be the case that before the introduction of the subsidy, they are informally employed, and their contract is formalized after the subsidy is introduced. This would decrease the observed employment rate of the *treatment-before* group and increase it in the *treatment-after* group, and it would bias the estimates upwards.

On the other hand, group B consists of mothers who are eligible for the subsidy in a period, but then their child turns 4 and they lose eligibility. These mothers may bring forward their job search (if in absence of the subsidy they wanted to return to the labor market later) in order to become eligible for the subsidy. This would result in observing a lower employment rate in the *control-after* group and a higher employment rate in the *treatment-after* group, which would again bias the results upwards.

To eliminate these potential pitfalls, I define the next closest groups in which such cross-group movements are not present. In order to do that, the cohort born after 1st July 2003 should be dropped from the comparison groups, so the before period is 1st July 2005 - 1st July 2006, and the control group is mothers with a child of 5-7 as depicted on Panel c.

The Program required that the applicant should be non-employed at the time of the application, however, non-employment as a pre-requisite to eligibility was non-effective. The claimant had to be non-employed only on the very day of claiming the Card. Thus, using very simple administrative techniques (officially end employment on the first day, apply for the Card on the second day and reemploy her on the third day), practically both employed and non-employed mothers could go for the Card. Consequently, both groups are included in the analysis.

As a result, a difference-in-differences estimation is carried out. The resulting estimation model is a linear probability model with year and region fixed effects to account for specific features of local labor markets.

$$E_i = \beta * After * T + \pi_1 After + \pi_2 T + \alpha_y + \gamma_r + X_i' \Omega + S_{yr}' \Pi + \xi_{yri} \qquad (1)$$

E_i is the employment dummy variable which is 1 if the individual is employed, and 0 otherwise. The DID variables are $After$ (1 in the year after the policy implementation and 0 before) and T (1 for mothers with the youngest child of age 3-4, and 0 for those 5-7). These variables are included in the regression along with their interaction. The effect of the wage subsidy on the probability of employment is measured by $\hat{\beta}$, the coefficient of the interaction $After * T$. Also year fixed effects (α_y), region fixed effects (γ_r), a vector of individual and family characteristics (X_i) and a vector of region-specific variables (S_{yr}) are included in the regression.

In the analysis, I use the data of the Hungarian Labor Force Survey (H-LFS). The unit of observation is households and each person in a household is documented. A rich set of individual characteristics, employment status, educational attainment, age, marital status etc., and relevant information about the family members is also available. The LFS contains quarterly data, where an individual is rotated out of the sample after 6 quarters the latest. Mothers over 20 of age are included in the sample whose youngest child is either 3-3.99 or 5-6.99 years old. The most important descriptive statistics are shown in Table 25.

In order to identify the policy effect, I assume that the treatment and the control group followed a similar trend in time before the policy change. As can be seen on Figure 16, the assumption is likely to hold.

The dashed line indicates the starting date of the policy, and the shaded areas show the time periods included in the estimation as before and after periods. A clear employment rate increase can be seen on Panel a. at the time of the policy intervention for the treatment group. By the third quarter of 2009, 2 years after the introduction of the tax credit, (by the time when the effects of the economic crisis strongly hit the labor market, see Figure 17) the employment rate of the treatment group seems to have fallen back even below the previous employment levels, and have experienced a larger fall than the control group. These patterns are even more eye-catching on Panel b., which shows the case for mothers with high school education.

3.5 Results

The regression results presented in Table 21 show that the effect of the wage subsidy is practically zero if we examine the whole sample of eligible mothers (Panel I.). (For detailed regression results see Table 26 and

Table 27) Model 1 presents the baseline results with no controls included. As individual and family controls are included in Model 2, and also regional variables in Model 3, the effect remains insignificant.

As mentioned in the introduction, the subsample of mothers with more children are of special interest in this study, because they are likely to have spent longer time at home with children and thus their human capital is more deteriorated than mothers' of one child. Moreover, more children increase the probability of at least one child being sick, which further dampens their productivity through sick leave frequency. Panel II. presents the regression results on the sample restricted to mothers with more than one child. In the baseline model (Model 1), the effect of the subsidy is insignificant, but adding control variables (Model 2 and 3) the estimated effect becomes significant at the 1%. The results show that being eligible for the tax credit, the employment probability of mothers with multiple children increased by 4.3 percentage points. This is a 10% increase compared to the pre-policy period employment rate of this group. The program seems not to have any effect on mothers of one child. The explanation, beyond human capital and sick leave frequency arguments may concern further child bearing plans, which may induce mothers stay home even if their employment prospects were better.

While the treatment intensity and the treated to eligible ratio varies significantly between education groups, the results for these subgroups are presented in Panel III. The result is not unexpected, the Program has a large and significant effect on those with high school (with the highest treated to eligible ratio) and near zero on other groups. The coefficient estimates are significant at the 1% level in each model, and the result in Model 3 shows that the Program increases the employment probability for mothers with high school education and more than one child by 30.9 percentage points. On the other hand, the employment probability of those with lower or higher educational attainment are unaffected. (see Table 21)

In order to check for robustness of the functional form, I have run logit regressions on the same variables and it does not change the results significantly. The results of the logit regressions are not presented.

The next subsection discusses the robustness of these results.

3.5.1 Robustness check

A robustness check is carried out using pseudo-treatments in a few years around the true implementation year, 2007. As it is shown in Table 22, running the same regression for consecutive years, gives a significant positive result in the year of the Program, a significant negative result two years later, in 2009, and in any other year the pseudo-effect is insignificant. This underpins the result of the previous section, that the Program had a significant positive effect on the employment probability of eligible mothers with more kids. However, in 2009 the economic crisis hit the Hungarian labor market (as Figure 17 demonstrates), and this washed away the results of the Program.

3.5.2 Logistic regression

As a further robustness check, the linear regressions of the previous subsection are switched to logistic regressions.

$$ln\frac{E_i}{1-E_i} = \beta * After * T + \pi_1 After + \pi_2 T + \alpha_y + \gamma_r + X_i'\Omega + S_{yr}'\Pi + \xi_{yri} \qquad (2)$$

As reported in Table 23, the main results of the estimation do not change.

3.5.3 Substitution effect

The validity of the results presented in the previous section heavily rely on the assumption that the employment increase of the treatment group relative to the control group does not come from a mere substitution effect. To check this, the evolution of the control group employment rate is compared to other employment groups, such as mothers with a child of age 8-9 years, childless females and males. The following linear regression is measured:

$$E_i = \beta * After * S + \pi_1 After + \pi_2 S + \alpha_y + \gamma_r + X_i'\Omega + S_{yr}'\Pi + \xi_{yri} \quad (3)$$

Where $S = 1$ if the individual is a mother with the youngest child being 5-6 years old. $S = 0$ if the individual is a member of one of the three comparison groups. The coefficient estimates (and the standard errors) regarding the three comparison groups are presented in Table 24.

The results suggest that there is no substitution effect, the employment rate of the control group does not decrease compared to other labor market groups. On the contrary, there seems to be a slight increase relative to some of the groups.

3.6 Conclusion

In this paper I have examined the effect of START Plusz hiring credit program of Hungary on the employment probability of mothers. In this program, the treatment itself is not observed directly, thus an intent-to-treat analysis is carried out. Only previously mothers whose child is not yet 4 years old are eligible for the tax credit. The comparison group is mothers of 5-7-year-olds, who are not entitled for the subsidy. It is shown in Figure 16 that the employment probability of the two groups is stable over time. The estimates suggest that the Program has no significant effect on the target group as a whole and most of its examined subgroups. However, there is a significant positive effect of the program for the employment of mothers with two or more children. Having access to the Card increases the employment probability of this subgroup by 4.3 percentage points.

This analysis reveals the partial labor market effects of START Plusz hiring tax credit. I demonstrate that as a consequence of the low treatment intensity and the likely low rate of treated to eligible, the effect is negligible on most parts of the target group. The analysis does not calculate net social effects of the program. However, had the significant effects comprise of purely net new jobs, considering the size of the affected group, the net social effect of the program would still be negligible from the society's point of view.

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4.2 Appendix for Chapter 1

Table 1: Summary statistics of the estimation sample by group

	Child of age 3 (m=1)			Child of age 4-5 (m=0)		
	Treatment (a)	Control(b)	Diff/SD	Treatment (a)	Control(b)	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	

Source: Hungarian Labour Force Survey, 1998-2011.
Notes: (a) Children born between August 1 – December 31. Mothers observed through January 1 – March 31
(b) Children born between January 1 – May 31. Mothers observed through June 1 – August 31

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Table 2: Reduced form (Eq.(1)) and 2SLS (Eq.(2-4)) results without seasonal correction

	(a) Reduced form ($\vartheta = T$) Eq. (1)			(b) 2SLS ($\vartheta = \hat{C}$) Eq. (2-4)		
Specifications	1	2	3	1	2	3
ϑ	0.078**	0.085**	0.082**	0.129**	0.141**	0.135**
(Clustered, robust SE)	(0.022)	(0.023)	(0.022)	(0.032)	(0.034)	(0.034)
# of children		-0.117**	-0.117**		-0.118**	-0.118**
		(0.021)	(0.022)		(0.019)	(0.019)
Partner w/o job		0.003	0.007		0.004	0.008
		(0.063)	(0.062)		(0.056)	(0.056)
Partner w/ job		0.032	0.032		0.033	0.034
		(0.062)	(0.062)		(0.056)	(0.055)
Vocational school		0.191**	0.186**		0.191**	0.187**
		(0.035)	(0.035)		(0.031)	(0.031)
High school		0.250**	0.245**		0.251**	0.246**
		(0.035)	(0.035)		(0.031)	(0.031)
University		0.374**	0.367**		0.374**	0.367**
		(0.051)	(0.050)		(0.045)	(0.045)
Age		0.018	0.020		0.018	0.020
		(0.021)	(0.021)		(0.018)	(0.019)
Age squared		-0.000	-0.000		-0.000	-0.000
		(0.000)	(0.000)		(0.000)	(0.000)
Partner: University		0.089*	0.083		0.087*	0.081*
		(0.044)	(0.044)		(0.039)	(0.039)
Partner: High sc.		0.074	0.071		0.075	0.072
		(0.060)	(0.060)		(0.052)	(0.053)
Partner: Vocational		0.063	0.060		0.063*	0.061
		(0.036)	(0.036)		(0.032)	(0.032)
Partner's age		-0.004*	-0.004*		-0.004**	-0.004**
		(0.002)	(0.002)		(0.002)	(0.002)
Unemployment level			-2.006**			-2.027**
			(0.765)			(0.681)
Village			0.218**			0.219**

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			(0.064)			(0.057)
City			0.243**			0.241**
			(0.058)			(0.051)
Large city			0.250**			0.250**
			(0.072)			(0.064)
Constant	0.480**	0.168	0.074	0.129**	0.141**	0.135**
	(0.099)	(0.384)	(0.374)	(0.099)	(0.384)	(0.374)
R2	0.245	0.316	0.318	0.023	0.114	0.117
AIC	3789.217	3494.741	3491.096	3676.866	3404.436	3401.121
N	3244	3244	3244	3018	3018	3018
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. Clustered, robust standard errors are given in parentheses. Stars indicate significance as: * p<0.05; ** p<0.01.*

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Table 3: First stage results (Eq. (4)) of the 2SLS regression without seasonality correction

	Eq.(4)	
	Coef.	Robust SE
C	0.135	0.034
# of children	-0.118	0.019
Partner w/o job	0.008	0.056
Partner w/ job	0.034	0.055
Vocational school	0.187	0.031
High school	0.246	0.031
University	0.367	0.045
Age	0.020	0.019
Age squared	0.000	0.000
Partner: University	0.081	0.039
Partner: High school	0.072	0.053
Partner: Vocational	0.061	0.032
Partner's age	-0.004	0.002
Unemployment level	-20.027	0.681
Village	0.219	0.057
City	0.241	0.051
Large city	0.250	0.064
R2	0.1172	
N	3018	

Source: H-LFS and T-STAR datasets, years 1998-2011.
Note: The dependent variable is the participation dummy.

Table 4: Reduced form (Eq.(8)) and 2SLS (Eq.(9-10)) results with seasonality correction

	(a) Reduced form ($\vartheta = \mathrm{T}$) Eq.(8)			(b) 2SLS ($\vartheta = \hat{\mathrm{C}}$) Eq.(9-10)		
	(1)	(2)	(3)	(1)	(2)	(3)
$\vartheta * \mathrm{m}$	0.061*	0.060*	0.060*	0.096**	0.095*	0.095*
	(0.024)	(0.027)	(0.026)	(0.037)	(0.041)	(0.041)
ϑ	0.007	0.014	0.012	0.014	0.025	0.021
	(0.018)	(0.016)	(0.016)	(0.027)	(0.024)	(0.024)
m	-0.169**	-0.156**	-0.156**	-0.179**	-0.166**	-0.166**
	(0.024)	(0.023)	(0.023)	(0.027)	(0.027)	(0.027)
# of children		-0.123**	-0.122**		-0.125**	-0.124**
		(0.015)	(0.015)		(0.014)	(0.014)
Partner w/o job		-0.004	0.000		-0.006	-0.002
		(0.043)	(0.043)		(0.042)	(0.041)
Partner w/ job		0.038	0.039		0.037	0.039
		(0.043)	(0.043)		(0.042)	(0.041)
Vocational school		0.177**	0.175**		0.178**	0.176**
		(0.020)	(0.020)		(0.019)	(0.019)
High school		0.289**	0.287**		0.289**	0.286**
		(0.019)	(0.019)		(0.018)	(0.018)
University		0.415**	0.412**		0.414**	0.410**
		(0.037)	(0.037)		(0.035)	(0.035)
Age		-0.004	-0.004		-0.004	-0.004
		(0.012)	(0.012)		(0.012)	(0.012)
Age squared		0.000	0.000		0.000	0.000
		(0.000)	(0.000)		(0.000)	(0.000)
Partner: University		0.058*	0.055*		0.058*	0.054*
		(0.025)	(0.024)		(0.024)	(0.023)
Partner: High sc.		0.087*	0.085*		0.088*	0.085*
		(0.037)	(0.037)		(0.036)	(0.036)
Partner: Vocational		0.075**	0.073**		0.074**	0.072**
		(0.023)	(0.023)		(0.022)	(0.022)
Partner's age		-0.005**	-0.005**		-0.005**	-0.005**
		(0.001)	(0.001)		(0.001)	(0.001)
Unemployment level			-1.218**			-1.251**
			(0.470)			(0.450)
Village			0.100**			0.100**
			(0.031)			(0.030)
City			0.102**			0.104**
			(0.020)			(0.019)
Large city			0.118**			0.119**

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			(0.045)			(0.043)
Constant	0.627**	0.700**	0.690**			
	(0.052)	(0.217)	(0.218)			
R2	0.179	0.272	0.273	0.025	0.135	0.136
AIC	10632.499	9578.493	9572.289	10482.230	9449.600	9442.833
N	8980	8980	8980	8809	8809	8809
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.*

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

	(1) Linear Probability Model	(2) Cutoff-based	(3) Cutoff-based with seasonal correction
Coefficient estimate	0.347**	0.177**	0.095**
N	13527	1826	8811
Adj. R2	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 years old children. Stars indicate significance as: * p<0.05; ** p<0.01.*

Table 6: 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
R2	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

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 1 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.*

Table 7: 2SLS results with 3 and 4 month windows around the cutoff (Specification 3 with all explanatory variables included)

	Window: 4 months		Window: 3 months	
	2SLS w/o seasonal correction Eq. (2-4)	2SLS w/ seasonal correction Eq. (9-10)	2SLS w/o seasonal correction Eq. (2-4)	2SLS w/ seasonal correction Eq. (9-10)
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:	0.142*	0.059	0.247*	0.087

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University				
	(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)
R2	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 8: 2SLS results with employment as the dependent variable

	2SLS w/o seasonal correction Eq. (2-4)	2SLS w/ seasonal correction Eq. (9-10)
C	0.124**	0.021
	(0.036)	(0.023)
C*m		0.077*
		(0.039)
m		-0.170**
		(0.023)
# of children	-0.114**	-0.114**
	(0.017)	(0.013)
Partner w/o job	-0.015	-0.023
	(0.059)	(0.044)
Partner w/ job	0.056	0.049
	(0.060)	(0.044)
Vocational school	0.143**	0.177**
	(0.029)	(0.018)
High school	0.245**	0.293**
	(0.032)	(0.018)
University	0.413**	0.465**
	(0.041)	(0.030)
Age	0.011	0.003
	(0.022)	(0.012)
Age squared	-0.000	-0.000
	(0.000)	(0.000)
Partner: University	0.055	0.046
	(0.038)	(0.025)
Partner: High sc.	0.057	0.075**
	(0.043)	(0.027)
Partner: Vocational	0.043	0.062**
	(0.030)	(0.020)
Partner's age	-0.004*	-0.003**
	(0.002)	(0.001)
Unemployment level	-1.508**	-1.169**
	(0.580)	(0.382)
Village	0.277**	0.141**
	(0.050)	(0.029)
City	0.267**	0.143**
	(0.042)	(0.019)
Large city	0.236**	0.157**
	(0.059)	(0.042)
R2	0.126	0.146
AIC	3418.963	9957.799
N	3018	8809

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Year dummies	x	x
Individual controls	x	x
Regional controls	x	x

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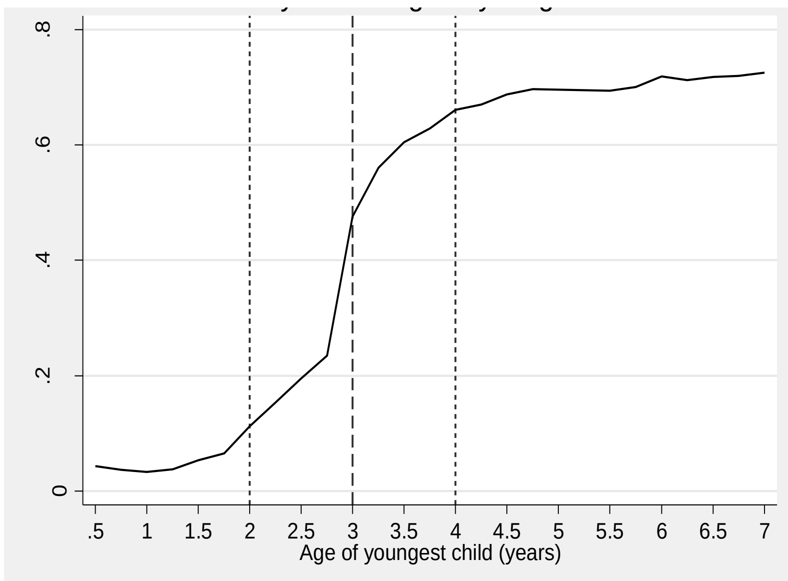
Table 9: Reduced form results for placebo cutoffs (Eq.(1))

	Cutoff date: November 1 Specifications			Cutoff date: September 1 Specifications		
	(1)	(2)	(3)	(1)	(2)	(3)
T	0.026	0.030	0.029	-0.019	-0.021	-0.021
	(0.019)	(0.020)	(0.020)	(0.025)	(0.025)	(0.025)
# of children		-0.134**	-0.131**		-0.118**	-0.116**
		(0.020)	(0.020)		(0.019)	(0.019)
Partner w/o job		-0.029	-0.023		-0.003	-0.006
		(0.061)	(0.060)		(0.088)	(0.088)
Partner w/ job		0.021	0.025		0.035	0.028
		(0.054)	(0.054)		(0.081)	(0.081)
Vocational school		0.206**	0.203**		0.145**	0.141**
		(0.036)	(0.036)		(0.035)	(0.035)
High school		0.273**	0.269**		0.219**	0.214**
		(0.032)	(0.031)		(0.046)	(0.046)
University		0.426**	0.417**		0.400**	0.393**
		(0.041)	(0.040)		(0.046)	(0.046)
Age		0.026	0.027		0.027	0.027
		(0.022)	(0.022)		(0.020)	(0.020)
Age squared		-0.000	-0.000		-0.000	-0.000
		(0.000)	(0.000)		(0.000)	(0.000)
Partner: University		0.051	0.043		0.011	0.010
		(0.048)	(0.047)		(0.048)	(0.048)
Partner: High sc.		0.067	0.059		0.073	0.072
		(0.051)	(0.051)		(0.069)	(0.069)
Partner: Vocationa..		0.062	0.058		0.071	0.070
		(0.032)	(0.032)		(0.040)	(0.041)
Partner's age		-0.003*	-0.003*		-0.005*	-0.004
		(0.001)	(0.001)		(0.002)	(0.002)
Unemployment level			-0.436			-1.762*
			(0.891)			(0.735)
Village			-0.037			-0.016
			(0.050)			(0.071)
City			-0.128**			0.004
			(0.031)			(0.053)
Large city			-0.032			0.028
			(0.065)			(0.064)

Constant	0.720**	0.252	0.333	0.581**	0.144	0.257
	(0.119)	(0.344)	(0.365)	(0.070)	(0.293)	(0.285)
R^2	0.236	0.32	0.323	0.247	0.32	0.322
AIC	3963.927	3594.151	3588.504	3742.727	3438.164	3435.935
N	3373	3373	3373	3229	3229	3229

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



Source: Hungarian Labour Force Survey, 1998-2011.

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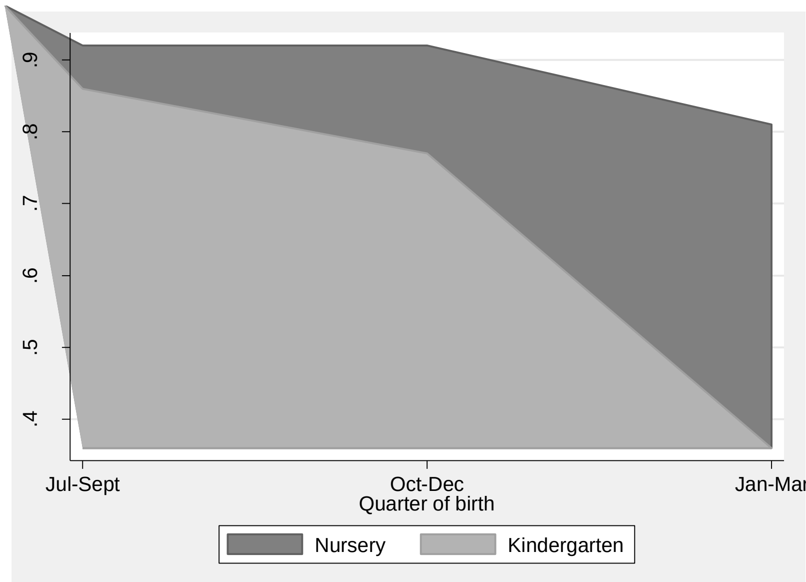
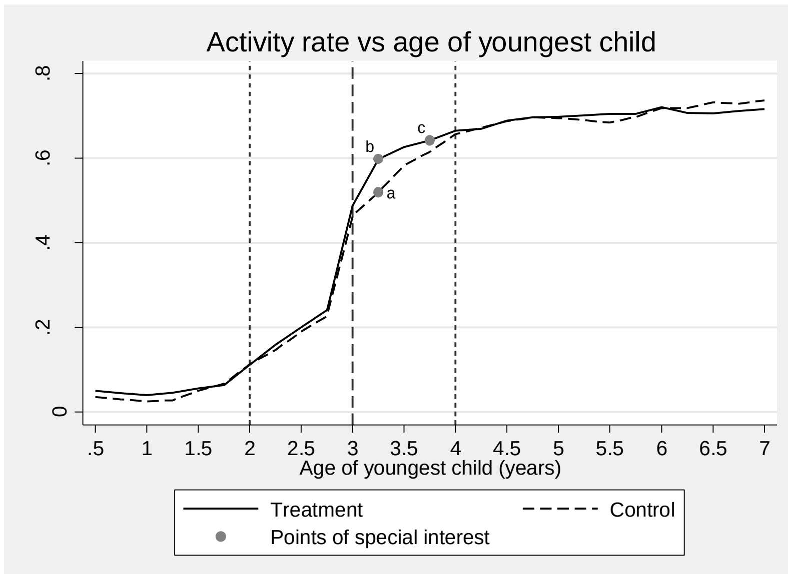
Figure 2: Enrollment rate around the third birthday of children born in a given quarter



Source: EU-SILC, 2006-2012

Figure 3: The participation rate of mothers in Hungary, by the age of their youngest child and by treatment status



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

4.2.1 Seasonal effects

In order to evaluate the assumption that the treatment and the comparison groups (mothers of 3-year-olds and 4-5-year-olds) are affected by the same seasonal effects, we calculate the difference in quarterly participation rate of the groups, and run a regression with it as the dependent variable and quarter dummies as explanatory variables:

$$\left(P_q^{g2} - P_q^{g1}\right) = \beta_1 + \beta_2 Q_2 + \beta_3 Q_3 + \beta_4 Q_4 + \varepsilon_q$$

Where P_q^{gi} is the participation rate of mothers of group i ($i = 1$ for mothers of 3-year-olds and $i = 2$ for mothers of 4-5-year-olds) in quarter q and Q_j is the dummy variable for the j-th quarter in the year.

Table 10 shows the results: none of the quarter (seasonal) dummies are significant in the regression, suggesting that there is no significant deviation in the seasonality of participation of the groups of mothers with 3 year olds and 4-5 year olds.

Table 10.: Testing the difference in the seasonality of participation of mothers with 3 year olds and mothers with 4-5 year olds

Participation rate difference	Spec1	Spec4
	(b/se)	(b/se)
2nd quarter	0.021	0.023
	(0.016)	(0.017)
3rd quarter	0.019	0.031
	(0.016)	(0.017)
4th quarter	0.010	0.016
	(0.016)	(0.017)
Constant	0.119**	0.101**
	(0.011)	(0.012)
R^2	0.038	0.060
AIC	-193.290	-186.682
N	58	58

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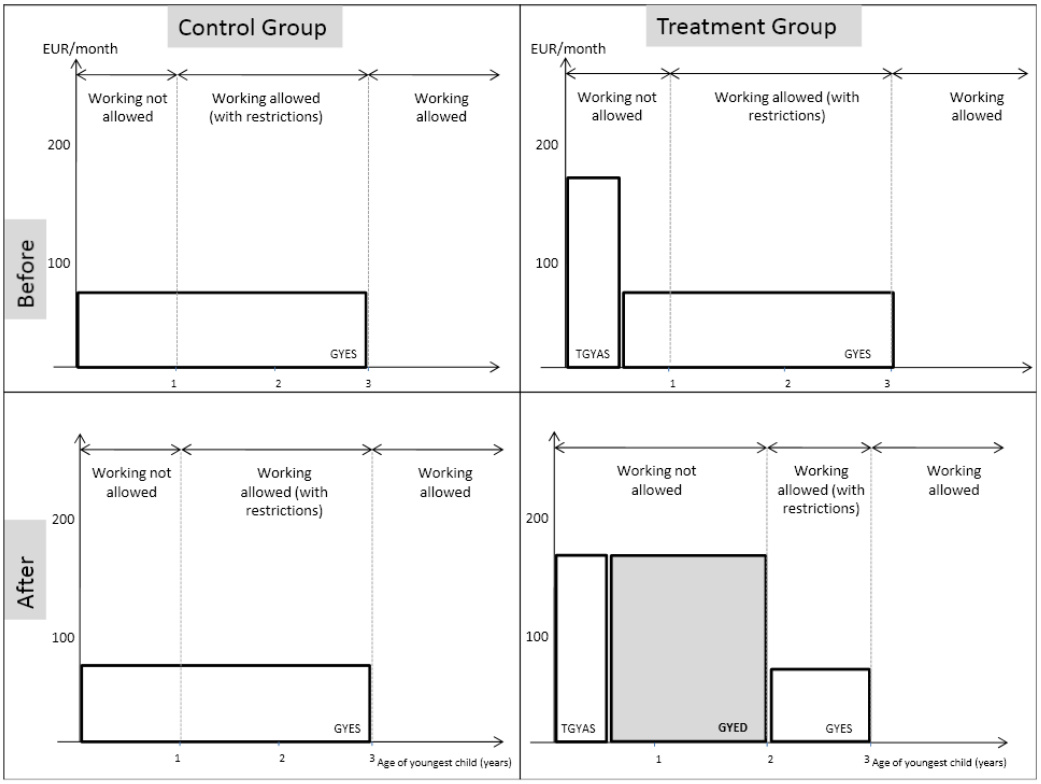
Figure 4: Child Cash Benefits in Hungary



Note: The top left panel shows the EUR/month amount of family benefit available before the policy change (1997-2000) in case of the control group, whose members are ineligible for TGYAS of GYED. The top right panel shows the case of the eligible in the same period. The bottom panels illustrate the case after the policy change (2000-2003), with GYED, the benefit analyzed in the paper highlighted with grey. On the top of each panel, work restrictions are indicated.

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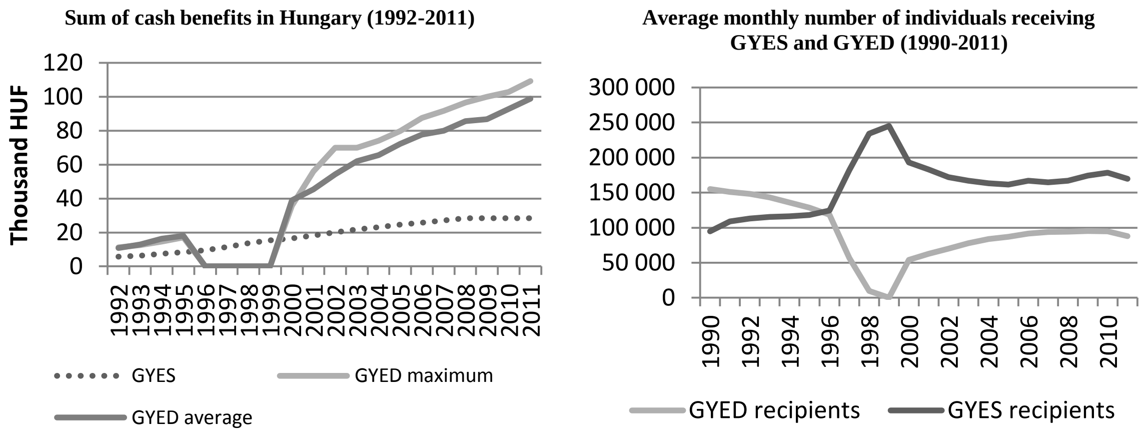
Figure 5: Evolution of GYED amount and number of recipients compared to GYES



Data source: Central Statistical Office, Hungary

Figure 6: Determining treatment status from time length out of work

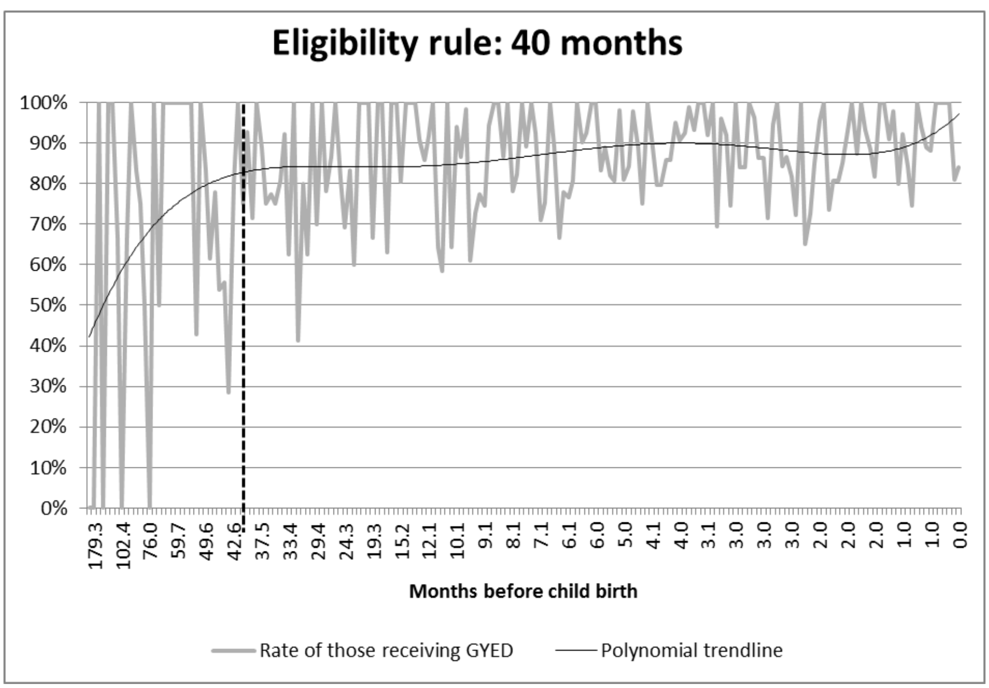


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Table 11: Percentage correctly predicted: treatment versus control status

		Data		
		Treatment	Control	# of observations
Imputation	Treatment	69%	12%	15 233
	Control	13%	5%	3 474
	# of observations	15 391	3 396	18 707

Table 12: Descriptive statistics by treatment status and period

Variable	Control, Before	Control, After	Treatment, Before	Treatment, After
Number of observations	7,900	7,666	16,452	14,386
Probability of returning to labor market (in 0-5 years)[32]	2.43	5.28	4.33	4.59
Probability of reemployment (in 0-5 years)	3.25	6.13	5.18	5.93
Probability of reemployment (in 2-5 years)	4.41	8.98	8.62	10.28
Age (years)	31.2	31.9	28.9	29.7
Level of education (%): less than 8 years of primary school	3.35	2.36	1.03	0.65
primary school	29.49	22.59	20.54	18.40
vocational school	27.37	28.65	32.00	32.41
high school graduation w/o profession	8.42	9.86	10.84	9.71
high school graduation w/ profession	19.11	20.10	23.43	25.02
college	8.67	10.79	9.03	10.20
university	3.58	5.65	3.12	3.61
# of children (%)	1.56	1.51	1.26	1.27
Partner (%): none	9.16	9.61	8.62	9.32
partner w/o job	16.86	12.95	11.88	9.58
partner w/ job	73.98	77.44	79.51	81.10
Local unemployment level (%)	4.0	3.7	3.8	3.5
Previous employment, within 8 years (%): state-owned	26.23	23.98	33.58	27.80
privately owned	25.13	38.11	40.43	52.60
other	18.75	13.95	25.46	19.22
none	29.89	23.96	0.53	0.39
Size/type of settlement of living (%): Budapest	25.73	27.44	29.34	29.27
large city	42.09	36.43	35.95	34.67
small city	17.34	17.95	19.07	20.78
village	14.84	18.19	15.63	15.28
Region of living (%): Region1: Közép-Magyarország	26.03	30.77	27.28	28.48
Region2: Közép-Dunántúl	10.36	10.51	12.37	10.91
Region3: Nyugat-Dunántúl	11.00	8.71	9.59	10.27
Region4: Dél-Dunántúl	8.63	11.18	9.50	9.67
Region5: Észak-Magyarország	12.98	10.20	12.06	11.80
Region6: Észak-Alföld	18.03	15.24	14.35	14.99
Region7: Dél-Alföld	12.97	13.40	14.84	13.87

[32] Based on quarterly transition data. The average probability of transition from non-participation to participation in a quarter, among mothers with a kid aged 0-5.

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Table 13: Descriptive statistics by treatment status and period, after matching

Variable	Control, Before	Control, After	Treatment, Before	Treatment, After
Number of observations	6,849	5,037	11,822	11,842
Probability of returning to labor market (in 0-5 years)[33]	2.97	5.23	5.89	5.87
Probability of reemployment (in 0-5 years)	2.23	4.45	5.00	4.61
Probability of reemployment (in 2-5 years)	4.17	9.25	9.03	10.81
Age (years)	31.1	30.8	30.4	29.7
Level of education (%): less than 8 years of primary school	2.42	1.77	0.89	0.37
primary school	26.51	19.16	18.51	15.75
vocational school	26.39	31.39	25.79	30.35
high school graduation w/o profession	8.99	11.71	11.70	10.37
high school graduation w/ profession	20.58	22.92	24.23	27.25
college	9.39	8.45	13.58	11.19
university	5.74	4.60	5.29	4.72
# of children (%)	1.48	1.41	1.31	1.22
Partner (%): none	9.29	9.56	8.23	9.29
partner w/o job	15.75	12.11	11.24	9.29
partner w/ job	74.96	78.33	80.53	81.42
Local unemployment level (%)	4.0	3.7	3.6	3.4
Previous employment, within 8 years (%): state-owned	27.31	23.90	39.08	28.77
privately owned	26.60	40.03	34.48	51.11
other	19.76	14.92	25.91	19.84
none	26.32	21.14	0.52	0.28
Size/type of settlement of living (%): Budapest	25.23	32.13	27.68	29.26
large city	41.40	36.54	34.45	34.04
small city	17.99	16.98	19.10	20.38
village	15.37	14.35	18.76	16.31
Region of living (%): Region1: Közép-Magyarország	27.12	26.97	30.28	29.07
Region2: Közép-Dunántúl	10.41	11.63	11.73	10.33
Region3: Nyugat-Dunántúl	11.10	7.71	9.64	10.72
Region4: Dél-Dunántúl	8.48	11.50	8.95	9.48
Region5: Észak-Magyarország	12.52	11.86	11.22	11.88
Region6: Észak-Alföld	17.77	15.78	14.23	14.74
Region7: Dél-Alföld	12.61	14.55	13.95	13.78

[33] Based on quarterly transition data. The average probability of transition from non-participation to participation in a quarter, among mothers with a kid aged 0-5.

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Table 14: Results of a simple DID analysis

	Δ
Probability of returning to labor market (in 0-5 years)	-2.59
Probability of reemployment (in 0-5 years)	-2.13
Probability of reemployment (in 2-5 years)	-2.91

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Table 15: Linear Probability Model for transition

Age of youngest child	0-5 years old		0-2 years old						2-5 years old					
Level of education	High & Low		High & Low		High		Low		High & Low		High		Low	
Dependent variable	Empl.	Part.	Empl.	Part.	Empl.	Part.	Empl.	Part.	Empl.	Part.	Empl.	Part.	Empl.	Part.
After	-0.047***	-0.057***	0.000	-0.002	0.005	0.006	-0.000	-0.005	-0.028*	-0.043**	0.005	-0.009	-0.044**	-0.056***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.03)	(0.03)	(0.01)	(0.02)
Treatment	0.009	0.012*	-0.000	-0.001	0.006	0.008	-0.002	-0.004	0.004	0.010	-0.002	0.026	0.011	0.010
	(0.00)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)
D	-0.017**	-0.016**	-0.005	-0.003	-0.011	-0.012	-0.003	0.001	-0.023*	-0.017	-0.031	-0.044	-0.024*	-0.013
	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)
Individual controls	x	x	x	x	x	x	x	x	x	x	x	x	x	x
Region FE	x	x	x	x	x	x	x	x	x	x	x	x	x	x
Year FE	x	x	x	x	x	x	x	x	x	x	x	x	x	x
N	42252	40344	22890	22805	9789	9761	13101	13044	19362	17539	6327	5822	13035	11717
AIC	-8860	-4030	-3220	-3060	-10200	-9540	-25300	-24200	7198.5	9156.7	3952.9	4193.6	2438.2	4480.5

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Figure 7: Data accuracy of birth dates

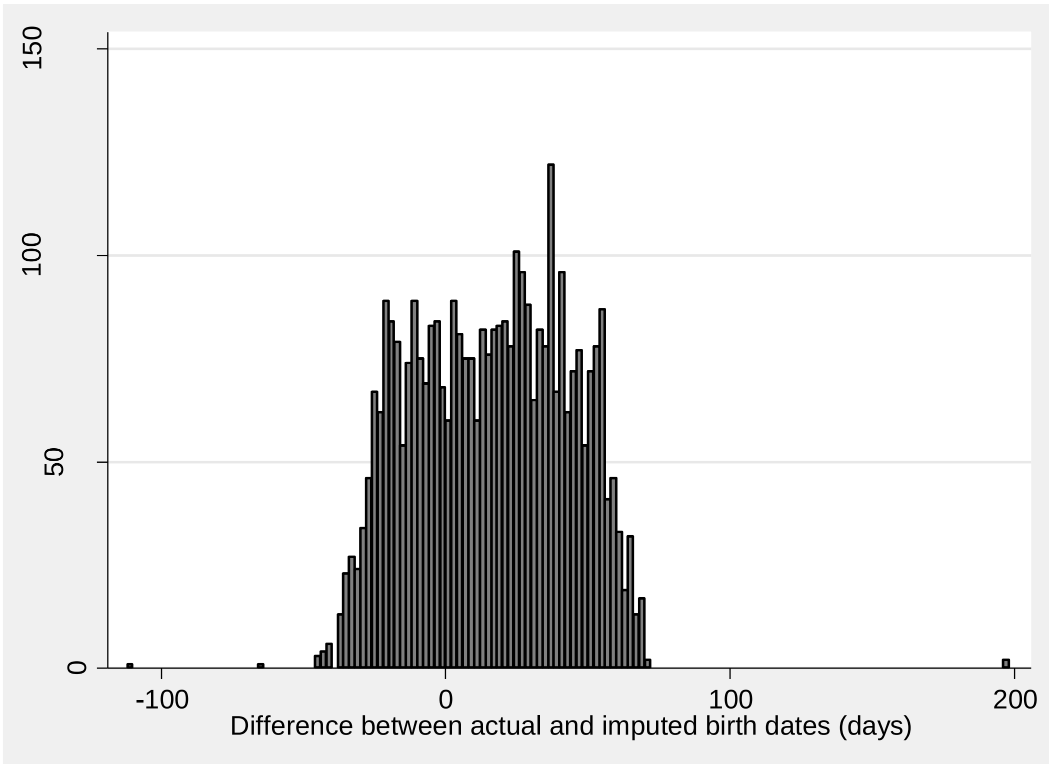

Figure 8: Proportionality assumption

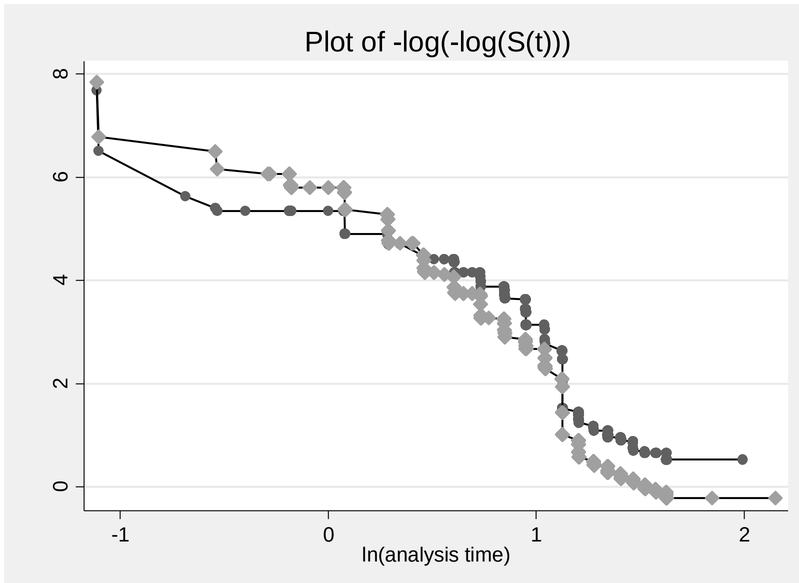

Figure 9: Hazard curves

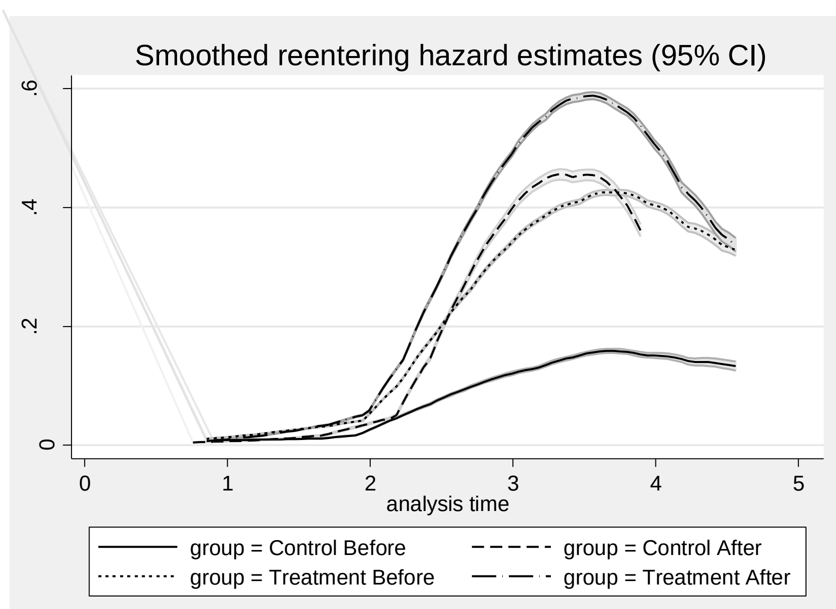

Table 16: Cox proportional hazard model results (exponentiated coefficients)

Variable	(1)	(2)	(3)
After	1.408	1.378	1.475*
	(0.26)	(0.25)	(0.27)
Treatment	5.726***	5.708***	5.975***
	(0.76)	(0.76)	(0.80)
D	0.619**	0.651**	0.630**
	(0.09)	(0.10)	(0.10)
1997	2.019	2.467	
	(1.05)	(1.28)	
1998	1.325	1.565	1.639
	(0.41)	(0.48)	(0.51)
1999	0.910	1.052	1.115
	(0.23)	(0.27)	(0.29)
2000	0.815	0.930	0.954
	(0.20)	(0.23)	(0.24)
2001	0.824	0.928	0.946
	(0.20)	(0.22)	(0.23)
2002	0.987	1.089	1.083
	(0.22)	(0.24)	(0.24)
2003	1.087	1.169	1.166
	(0.23)	(0.24)	(0.25)
2004	1.226	1.332	1.315
	(0.25)	(0.27)	(0.27)
2005	1.594*	1.759**	1.756**
	(0.33)	(0.36)	(0.37)
2006	1.394	1.493	1.507
	(0.29)	(0.31)	(0.32)
2007	1.090	1.147	1.159
	(0.23)	(0.24)	(0.24)
2008	1.220	1.241	1.272
	(0.25)	(0.26)	(0.27)
2009	1.046	1.071	1.143
	(0.22)	(0.23)	(0.24)
2010	1.261	1.286	1.352
	(0.26)	(0.27)	(0.28)
# of 0-6 kids		0.909	0.921
		(0.06)	(0.06)

Partner w/o job	1.118	1.163
	(0.21)	(0.22)
Partner w/ job	0.938	0.928
	(0.09)	(0.09)
Educ: vocational	1.237**	1.239**
	(0.10)	(0.10)
Educ: high school	1.490***	1.517***
	(0.12)	(0.12)
Educ: university	1.740***	1.785***
	(0.18)	(0.18)
Age	1.108*	1.110*
	(0.05)	(0.05)
Age squared	0.998*	0.998*
	(0.00)	(0.00)
Partner: University	1.273*	1.293*
	(0.15)	(0.15)
Partner: High sc.	1.306**	1.312**
	(0.13)	(0.13)
Partner: Vocationa..	1.175	1.160
	(0.10)	(0.10)
Partner's age	0.999	0.999
	(0.00)	(0.00)
Reg. unemp. level		0.051**
		(0.05)
Live in village		0.891
		(0.10)
Live in city		0.897
		(0.10)
Live in large city		0.884
		(0.11)
Kozep-Dunantul		1.585***
		(0.15)
Nyugat-Dunantul		1.556***
		(0.15)
Del-Dunantul		1.508***
		(0.17)
Eszak-Magyarorszag		1.628***

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			(0.18)
Eszak-Alfold			1.890***
			(0.20)
Del-Alfold			1.563***
			(0.15)
Quarter2			0.848*
			(0.06)
Quarter3			0.747***
			(0.05)
Quarter4			0.915
			(0.06)
N	54437	54435	53919
AIC	32700	32600	32500

Table 17: Testing parallel trend assumption

	Placebo treatment date					
	1st December 1997			1st December 1996		
	(1)	(2)	(3)	(1)	(2)	(3)
D	0.008	0.004	0.004	0.006	0.005	0.011
(s.e.)	(0.009)	(0.008)	(0.009)	(0.016)	(0.016)	(0.016)
R^2	0.037	0.04	0.052	0.043	0.045	0.061
	-	-	-		-	-
AIC	1886.63	2622.63	2741.28	56.16	163.738	218.517
N	12118	11751	11751	4800	4696	4696
Individual & family controls		x	x		x	x
Regional controls			x			x

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4.3.1 Childcare benefit system and parental leave in Hungary

Year	GYED	GYES
1992	W	W, PT
1993	W	W, PT
1994	W	W, PT
1995	W	W, PT
1996	-	M, PT
1997	-	M, PT
1998	-	M, PT
1999	-	U, FTH
2000	W	U, FTH
2001	W	U, FTH
2002	W	U, FTH
2003	W	U, FTH
2004	W	U, FTH
2005	W	U, FTH
2006	W	U, FT

Source: Köllő (2008)
(U: Universal; W: Tied to previous working history; M: Means-tested; PT: part time employment allowed; FTH: full-time employment allowed at home 1.5 year after birth; FT: full-time employment allowed 1 year after birth)

GYES

GYES is a childcare aid which is a relatively small amount benefit, but is available for any Hungarian citizen with a child up to 3, irrespective of previous work history.

Between 1996 and 1998 eligibility for this benefit depended on family income. The amount of GYES was fixed at appr. EUR 100 per month per family - independently of number of children - in 1996, and this amount was increased in each year by a rate comparable to the inflation rate.

Until the child is 1.5, the mother should not be working, or else she loses eligibility for GYES. Between 1996 and 1998 the mother was allowed to have a part-time job after the child turned 1.5, and keep eligibility for GYES. From 1999 the mother was allowed to undertake a full-time job while working at home and keep her eligibility after the child has reached age of 1.5. GYES cannot be received together with GYED or TGYAS.

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GYED

This type of benefit did not exist in the 1996-99 period, it was launched in 2000. GYED is a childcare benefit of relatively high amount, which is tied to the previous work history of the mother. She is eligible for the benefit if she has worked at least for 180 days in the past 2 years. She is also eligible if she received GYED in the previous period. GYED amounts to the 70% of the average of past 2 years' salary, with a ceiling of twice the old-age pension minimum. This benefit may be received from the date of child birth until the child becomes 2, and the mother should not be working throughout the whole period. This child benefit remained unchanged until 2009.

TGYAS

The amount and the eligibility criteria of this benefit are mostly the same as those of GYED, with two exceptions. TGYAS can be received during the parental leave, which is as long as 24 weeks, of which at least 4 weeks should fall before the child birth, and the remainder may be claimed after birth. Also, there is no ceiling for the amount of TGYAS given, which is advantageous for those having received high wage before.

Only one of GYED, GYES and TGYAS could be received at the same time.

Family allowance

This benefit is a relatively small amount, but - under general circumstances - all households are eligible which have children under 18 - or under 23 and still be studying. The amount of family allowance is appr. EUR 50 per child, increasing with the number of children. The amount may be higher in case of seriously handicapped children, disadvantaged families or single parents. Eligibility and the amount does not depend on previous income, or work history. This benefit may be claimed together with other childcare benefits, like GYES, GYED or TGYAS.

4.3.2 On data availability

Information on the treatment status (whether she had worked before giving birth) is available only for those women

- whose first observation is before giving birth
- whose first observation is after giving birth and the observed labor status is non-employed.

The reason is that the date of previous employment is asked only in case the individual is not employed at the time of the interview.

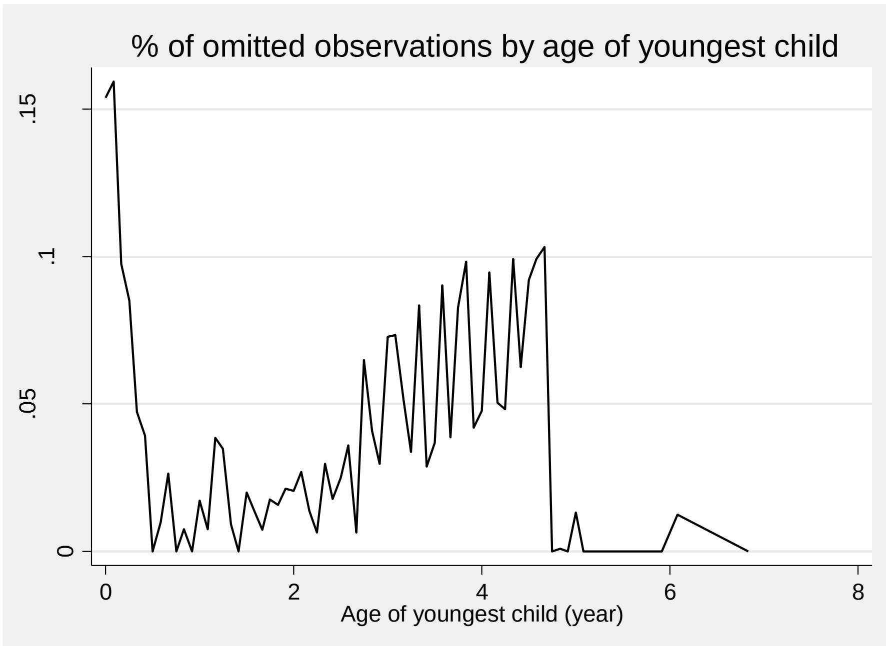
Information on the starting date of the analysis time is available for each individual, as the age of the child is available.

The reemployment date is

- available for those whose first observed labor status is non-employed and the last is employed.
- right censored for those whose first observed labor status is non-employed and the last is also non-employed.
- unavailable for those whose first observed labor status is employed. These observations should be omitted.

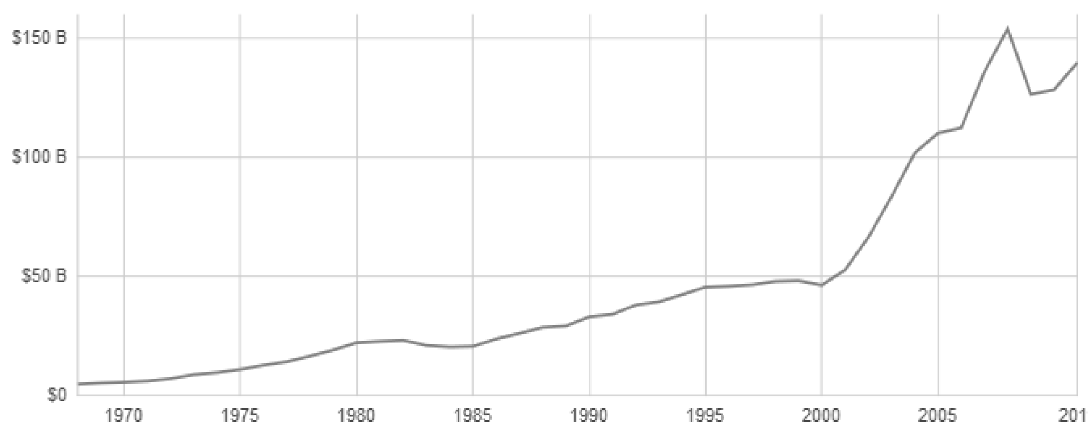
To sum up, nor treatment status, neither reemployment date is available for those observations, for which the first observed labor status is employed. These observations are omitted. The ratio of these omitted observations stays around 5% of the sample, and barely ever exceeds 10%. (It is about 15% just after birth, because of the birth date measurement error.) However, it should be noted that omitting these observations may bias the results.

Figure 10: Omitted observations



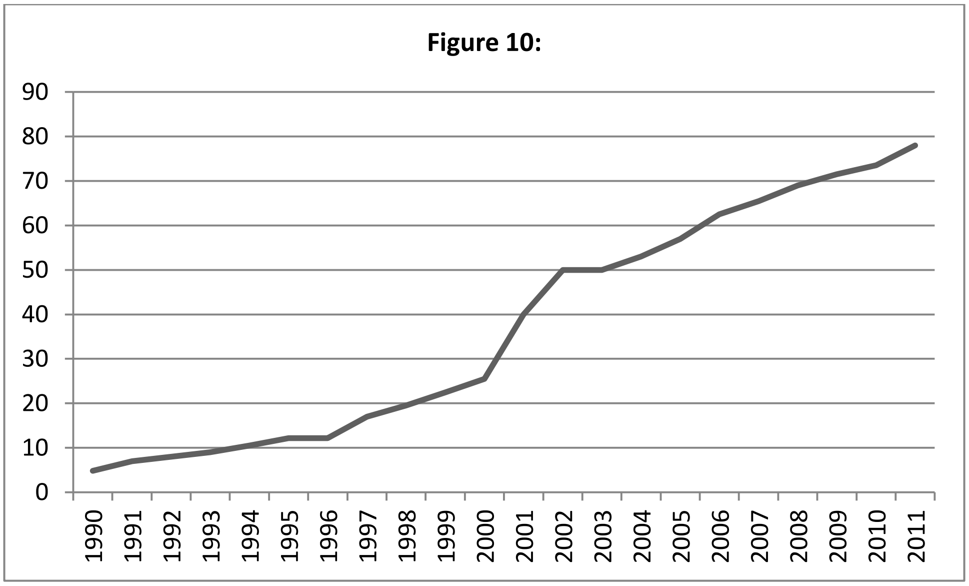
4.3.3 Additional figures

Figure 11: Hungarian GDP



Source: WorldBank GDP data

Figure 12: Minimum wage (thousand HUF)



Source: CSO

4.3.4 Imputation bias

In order to gain a clear picture about the bias caused by the imputation of treatment status, statistics are provided about the predicted and actual treatment groups. The sample of mothers with children younger than 1.5 years, observed after the policy change is used for this analysis. The difference of the means are tested with a t-test, which allows variances to differ in the groups.

In many aspects, the predicted and the actual groups are not statistically significantly different. However, there are some dimensions where the imputation introduces bias. As a result of the imputation, there are significantly lower educated mothers included in the control group than the actual, and significantly higher educated mothers are included in the treatment group. Similarly, the average participation and employment rates are underestimated in case of the control group. Nevertheless, if these biases are present in both the before and the after periods, the size or the direction of the overall bias cannot be predicted either.

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18. Table: Actual and predicted treatment and control group characteristics

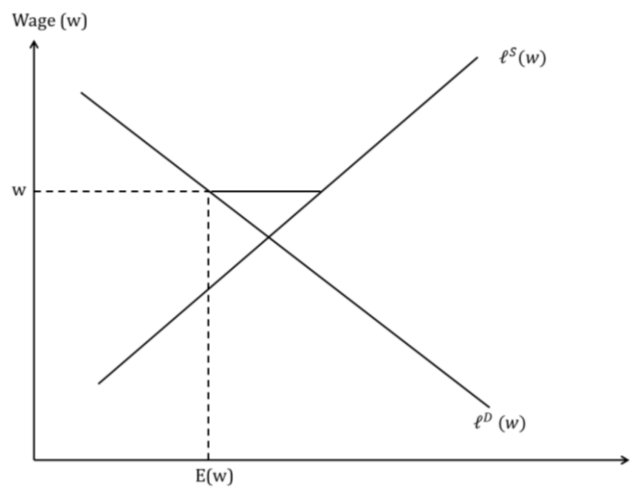
	Treatment group			Control group		
	Predicted	Actual	P-value of t-test for difference	Predicted	Actual	P-value of t-test for difference
# of kids	1.259	1.274	0.378	1.446	1.429	0.437
University degree (%)	0.238	0.206	0.021	0.077	0.111	0.001
High school (%)	0.475	0.386	0.000	0.196	0.283	0.000
Vocational school (%)	0.241	0.276	0.019	0.379	0.345	0.031
Partner's age (years)	29.713	30.180	0.147	30.120	29.660	0.203
Local unemployment rate (%)	0.031	0.036	0.000	0.051	0.046	0.001
Age (years)	29.340	29.240	0.489	29.400	29.530	0.472
Child age (years)	1.209	1.209	0.903	1.210	1.211	0.773
Village (%)	0.323	0.315	0.596	0.280	0.288	0.621
City (%)	0.358	0.398	0.015	0.555	0.514	0.016
Large city (%)	0.184	0.176	0.508	0.100	0.107	0.481
Participation rate (%)	0.011	0.009	0.562	0.041	0.064	0.003
Employment rate (%)	0.009	0.006	0.311	0.040	0.064	0.002

4.4 Appendix for Chapter 3

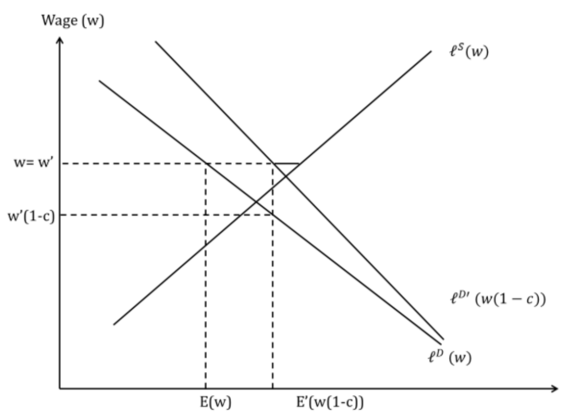
Figure 13: Wage subsidy offered to firms: a simple framework

Labor market with initial unemployment,

without tax credit



b. Labor market with low tax credit



c. Labor market with high tax credit

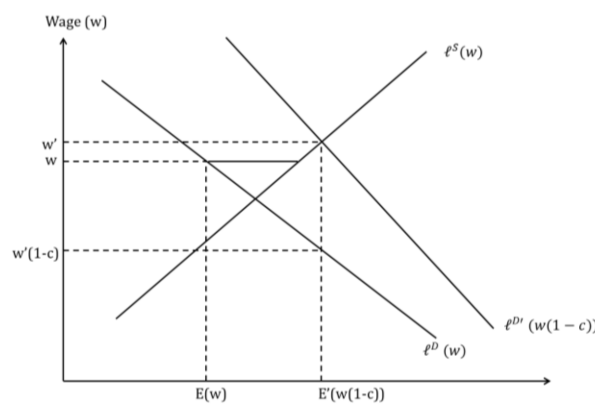


Figure 14: Subsidy costs

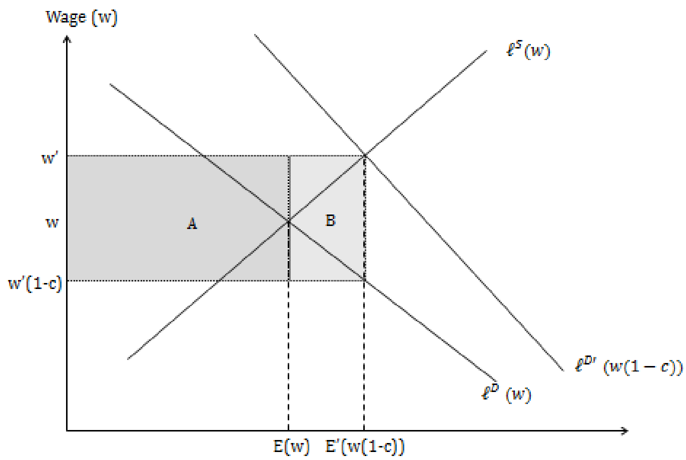


Table 19: Educational attainment of female START Plusz Card holders under age 50

	Number of female cardholders (treated)	Share of cards by level of education	Share of eligible population	Ratio of treated to eligible
Total	9527	100%	100%	3.3%
Elementary	924	11.7%	28.1%	1.1%
Vocational & High school	5806	73.4%	57.9%	3.5%
College and higher	1181	14.9%	13.98%	2.9%

Source of data: Scharle (2013) and Hungarian Labor Force Survey data

Table 20: START Plusz cardholders' employment timing after registering for the Card

	Number of people	%
Within 30 days	11 901	81.69
Between 30 and 90 days	961	6.60
After 90 days	1 264	8.68
Non-employed	443	3.04
Total	14 569	100.00

Source of data: Scharle et al. (2013)

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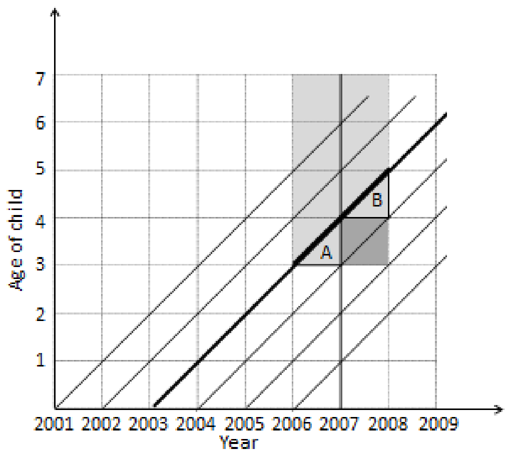
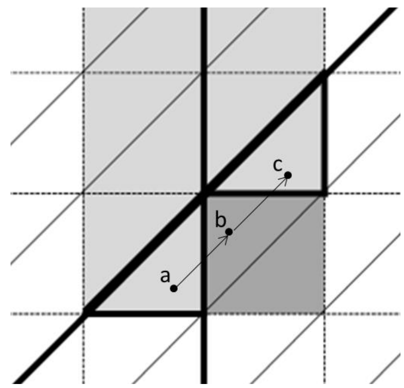
Figure 15: Defining DID groups

a. Movement between categories



b. Movement of one individual through time



c. No movement between categories

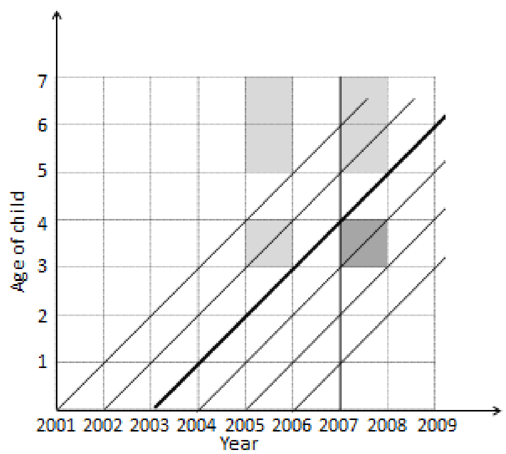


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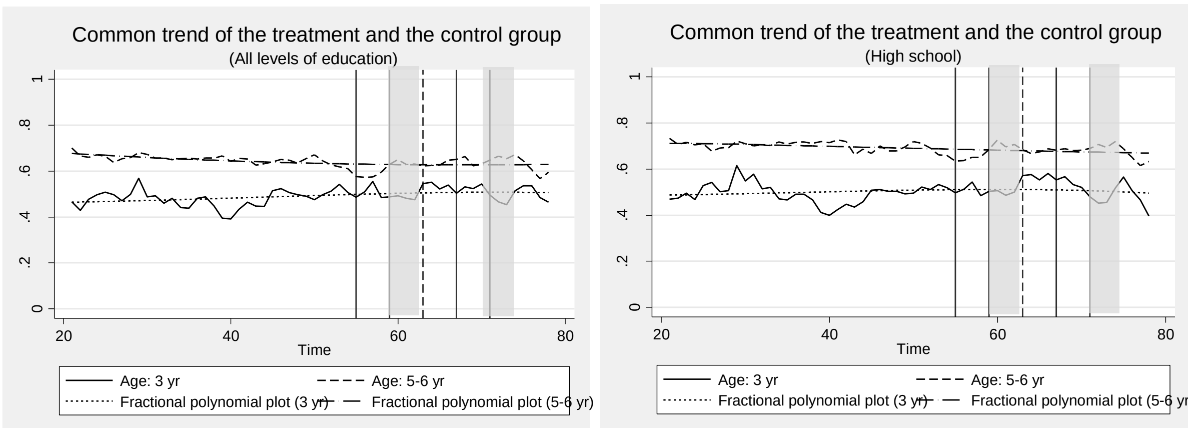
Figure 16: Employment rate trends for the treatment and the control group

a. All levels of education

b. Mothers with High school



Source of the data: H-LFS

Table 21: Linear regression coefficient estimates for $\boldsymbol{\beta}$

(robust, clustered standard errors in parenthesis)

	(1)	(2)	(3)
I.) Full sample			
	-0.030	0.003	-0.003
	(0.032)	(0.027)	(0.027)
II.) Multiple children			
	0.021	0.041**	0.043**
	(0.028)	(0.011)	(0.010)
III.) Multiple children, by education			
Elementary	-0.077	-0.085	-0.083
	(0.087)	(0.086)	(0.068)
Vocational	-0.052	-0.116	-0.041
	(0.168)	(0.146)	(0.125)
High school	0.304*	0.298*	0.309*
	(0.109)	(0.100)	(0.100)
College or higher	-0.073	-0.085	-0.045
	(0.120)	(0.127)	(0.087)
Individual controls		x	x
Regional controls			x

*Note: I estimate a linear model with year and region fixed effects. The dependent variable is employment dummy, taking the value 1 if employed and 0 otherwise. The standard errors are robust and clustered by regions. Significance levels: * 5%; ** 1%*

Table 22: Robustness check: pseudo-treatment for each year, mothers with more than one child

	Placebo treatment year						
	2004	2005	2006	2007	2008	2009	2010
Coefficient	0.015	0.026	-0.058	0.087**	-0.011	-0.213**	0.003
Standard error	(0.050)	(0.050)	(0.046)	(0.022)	(0.055)	(0.037)	(0.106)
R^2	0.215	0.215	0.239	0.210	0.258	0.217	0.269
N	1675	2133	1841	1881.000	1611.000	1475.000	1291.000

*Note: I estimate a linear model with year and region fixed effects. The dependent variable is employment dummy, taking the value 1 if employed and 0 otherwise. The standard errors are robust and clustered by regions. Significance levels: * 5%; ** 1%*

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Table 23: Logit regression coefficient estimates for $\boldsymbol{\beta}$

(robust, clustered standard errors in parenthesis)

	Model 1	Model 2	Model 3
I.) Full sample			
	-0.122	0.006	-0.034
	(0.138)	(0.138)	(0.142)
II.) Multiple children			
	0.290	0.447**	0.486**
	(0.183)	(0.112)	(0.113)
III.) Multiple children, by education			
Elementary	-0.303	-0.535	-0.807
	(0.469)	(0.499)	(0.423)
Vocational	0.036	-0.339	-0.262
	(0.646)	(0.600)	(0.646)
High school	1.336**	1.345**	1.488**
	(0.409)	(0.418)	(0.439)
College or higher	-0.613	-0.726	-0.638
	(0.669)	(0.653)	(0.559)
Individual controls		x	x
Regional controls			x

*Note: I estimate a logit model with year and region fixed effects. The dependent variable is employment dummy, taking the value 1 if employed and 0 otherwise. The standard errors are robust and clustered by regions. Significance levels: * 5%; ** 1%*

Table 24: Substitution effects

Mothers of 8-9-year-olds	Childless females	Males
0.031	0.038**	0.040*
(0.015)	(0.01)	(0.012)

Table 25: Descriptive statistics

Time period	July 2004-July 2007		July 2007-July 2010	
Age of the youngest child (treatment status)	5-7 year (control)	3 year (treatment)	5-7 year (control)	3 year (treatment)
Observations	9 651	5 491	8 466	4 443
Employment rate in the group	61.3%	50.4%	64.4%	51.5%
Number of children	1.1	1.4	1.1	1.4
Age of the youngest child	5.9	3.5	5.9	3.4
Mother's age	34.3	32.6	35.4	33.7
Level of education: Primary school	22.1%	17.4%	18.9%	18.5%
Level of education: Vocational school	28.0%	26.2%	28.3%	24.6%
Level of education: High school	34.6%	36.7%	36.0%	32.7%
Level of education: University	15.3%	19.7%	16.9%	24.3%
No partner	13.8%	9.6%	14.0%	11.3%
Non-employed partner	12.7%	11.2%	14.2%	13.0%
Employed partner	73.5%	79.2%	71.8%	75.7%
Village	30.2%	28.5%	32.2%	29.2%
City	39.3%	37.5%	36.1%	35.4%
Large city	17.4%	19.5%	16.0%	19.7%
Budapest	13.1%	14.5%	15.7%	15.7%
Region1	26.0%	28.3%	29.9%	31.8%
Region2	10.7%	12.2%	10.4%	9.4%
Region3	9.4%	8.9%	9.7%	9.1%
Region4	9.7%	10.0%	9.8%	10.4%
Region5	13.4%	11.4%	12.1%	11.1%
Region6	16.4%	15.9%	15.6%	15.1%
Region7	14.5%	13.3%	12.6%	13.1%
Local unemployment rate	4.4%	4.1%	5.5%	5.4%
Nursery coverage	9.9%	10.5%	11.2%	11.5%
Kindergarten coverage	114.2%	113.9%	113.5%	112.6%
Population of the settlement	249	274	295	299

Table 26: Regression results: entire sample

	(1) b/se	(2) b/se	(3) b/se
After	0.047	0.033	0.046*
	(0.024)	(0.017)	(0.017)
Treatment	-0.063	-0.076	-0.078
	(0.027)	(0.032)	(0.032)
After*Treatment	-0.017	-0.016	-0.016
	(0.013)	(0.008)	(0.007)
2005		-0.030	-0.024
		(0.019)	(0.018)
2007		0.248**	0.224**
		(0.022)	(0.023)
# of children		0.391**	0.353**
		(0.030)	(0.024)
Partner w/o job		0.527**	0.481**
		(0.053)	(0.042)
Partner w/ job		0.007	0.006
		(0.014)	(0.013)
Vocational school		-0.000	-0.000
		(0.000)	(0.000)
High school		0.000	0.000
		(.)	(.)
University		0.000	0.000
		(.)	(.)
Age		0.000	0.000
		(.)	(.)
Age squared		0.000	0.000
		(.)	(.)
Partner: University			-1.789**
			(0.289)
Partner: High sc.			0.074**
			(0.013)
Partner: Vocational			0.061
			(0.029)
Partner's age			0.090*
			(0.029)
Unemployment level			0.302*
			(0.112)
Village			0.042
			(0.043)
City	0.584**	0.325	0.304
	(0.013)	(0.250)	(0.244)
Large city	0.047	0.033	0.046*

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	(0.024)	(0.017)	(0.017)
Nursery availability	-0.063	-0.076	-0.078
	(0.027)	(0.032)	(0.032)
Kindergarten availability	-0.030	0.003	-0.003
	(0.032)	(0.027)	(0.027)
Constant	-0.017	-0.016	-0.016
	(0.013)	(0.008)	(0.007)
R^2	0.029	0.172	0.188
AIC	12565.828	11135.023	10962.630
N	8972	8972	8968
Individual controls		*	*
Regional controls			*

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Table 27: Regression results: mothers with more than one child

	(1) b/se	(2) b/se	(3) b/se
After	0.038	0.020	0.026
	(0.052)	(0.019)	(0.017)
Treatment	-0.059	-0.100*	-0.106*
	(0.028)	(0.033)	(0.035)
After*Treatment	0.051	0.077*	0.087**
	(0.045)	(0.024)	(0.022)
2005	-0.025	-0.022	-0.014
	(0.033)	(0.026)	(0.024)
2007	-0.080	-0.067	-0.072
	(0.045)	(0.040)	(0.038)
# of children		-0.073	-0.063
		(0.089)	(0.087)
Partner w/o job		-0.123	-0.097
		(0.057)	(0.055)
Partner w/ job		-0.013	-0.013
		(0.047)	(0.046)
Vocational school		0.120	0.103
		(0.050)	(0.048)
High school		0.340**	0.300**
		(0.040)	(0.045)
University		0.508**	0.456**
		(0.049)	(0.041)
Age		-0.037	-0.034
		(0.036)	(0.037)
Age squared		0.000	0.000
		(0.001)	(0.001)
Partner: University		0.000	0.000
		(.)	(.)
Partner: High sc.		0.000	0.000
		(.)	(.)
Partner: Vocational		0.000	0.000
		(.)	(.)
Partner's age		0.000	0.000
		(.)	(.)
Unemployment level			-0.888*
			(0.256)
Village			0.051
			(0.034)
City			-0.050
			(0.048)
Large city			0.076

			(0.050)
Nursery availability			0.062
			(0.222)
Kindergarten availability			0.022
			(0.069)
Constant	0.503**	1.134	1.085
	(0.026)	(0.696)	(0.772)
R^2	0.046	0.194	0.210
AIC	2647.743	2333.164	2292.177
N	1883	1883	1881
Individual controls		*	*
Regional controls			*

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Table 28: Robustness check: 2004-2007

	2004			2005			2006			2007		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se
After	-0.015	-0.006	0.010	-0.024	-0.007	-0.009	-0.022	0.025	0.023	0.038	0.020	0.026
	(0.047)	(0.056)	(0.055)	(0.078)	(0.050)	(0.051)	(0.077)	(0.088)	(0.087)	(0.052)	(0.019)	(0.017)
Treatment	-0.079	-0.103	-0.108	-0.114*	-0.112*	-0.107*	-0.055	-0.059	-0.072	-0.059	-0.100*	-0.106*
	(0.067)	(0.054)	(0.052)	(0.040)	(0.041)	(0.041)	(0.044)	(0.042)	(0.040)	(0.028)	(0.033)	(0.035)
After*Treatment	0.018	0.019	0.015	0.058	0.036	0.026	-0.032	-0.070	-0.058	0.051	0.077*	0.087**
	(0.054)	(0.053)	(0.050)	(0.060)	(0.045)	(0.050)	(0.058)	(0.050)	(0.046)	(0.045)	(0.024)	(0.022)
R^2	0.050	0.207	0.215	0.040	0.203	0.215	0.060	0.225	0.239	0.046	0.194	0.210
AIC	2324.112	2023.697	2004.270	3002.829	2606.261	2572.891	2532.847	2178.264	2138.544	2647.743	2333.164	2292.177
N	1677	1677	1675	2135	2135	2133	1846	1846	1841	1883	1883	1881
Year FE	x	x	x	x	x	x	x	x	x	x	x	x
Region FE	x	x	x	x	x	x	x	x	x	x	x	x
Individual controls		x	x		x	x		x	x		x	x
Regional controls			x			x			x			x

Note: Tables of robustness check are split for tractability reasons. The robustness checks for all years are executed in the same way. The column for year 2007 is repeated in both tables, for comparability.

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Table 29: Robustness check: 2007-2010

	2007			2008			2009			2010		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se
After	0.038	0.020	0.026	0.071	0.015	0.048	0.008	0.011	0.025	-0.020	-0.042	-0.043
	(0.052)	(0.019)	(0.017)	(0.094)	(0.077)	(0.078)	(0.041)	(0.042)	(0.048)	(0.094)	(0.080)	(0.083)
Treatment	-0.059	-0.100*	-0.106*	-0.077*	-0.133**	-0.134**	-0.008	-0.026	-0.031	-0.155*	-0.166*	-0.159
	(0.028)	(0.033)	(0.035)	(0.027)	(0.029)	(0.028)	(0.030)	(0.029)	(0.034)	(0.061)	(0.062)	(0.065)
After*Treatment	0.051	0.077*	0.087**	-0.078	-0.012	-0.011	-0.169**	-0.209**	-0.213**	0.052	0.003	0.003
	(0.045)	(0.024)	(0.022)	(0.056)	(0.054)	(0.055)	(0.038)	(0.033)	(0.037)	(0.115)	(0.103)	(0.106)
R^2	0.046	0.194	0.210	0.058	0.248	0.258	0.047	0.183	0.217	0.066	0.261	0.269
AIC	2647.743	2333.164	2292.177	2239.319	1875.897	1849.722	2067.212	1843.430	1780.461	1789.757	1490.309	1476.022
N	1883	1883	1881	1616	1616	1611	1475	1475	1475	1291	1291	1291
Year FE	x	x	x	x	x	x	x	x	x	x	x	x
Region FE	x	x	x	x	x	x	x	x	x	x	x	x
Individual controls		x	x		x	x		x	x		x	x
Regional controls			x			x			x			x

Table 30: Robustness check, full estimation results of Model 3

	2004 (3) b/se	2005 (3) b/se	2006 (3) b/se	2007 (3) b/se	2008 (3) b/se	2009 (3) b/se	2010 (3) b/se
After	0.010	-0.009	0.023	0.026	0.048	0.025	-0.043
	(0.055)	(0.051)	(0.087)	(0.017)	(0.078)	(0.048)	(0.083)
Treatment	-0.108	-0.107*	-0.072	-0.106*	-0.134**	-0.031	-0.159
	(0.052)	(0.041)	(0.040)	(0.035)	(0.028)	(0.034)	(0.065)
After*Treatment	0.015	0.026	-0.058	0.087**	-0.011	-0.213**	0.003
	(0.050)	(0.050)	(0.046)	(0.022)	(0.055)	(0.037)	(0.106)
Year 1	-0.035	-0.002	-0.036	-0.014	-0.011	-0.078	0.005
	(0.035)	(0.011)	(0.036)	(0.024)	(0.031)	(0.048)	(0.047)
Year 2	-0.040	-0.013	-0.007	-0.072	0.003	-0.017	0.024
	(0.031)	(0.027)	(0.031)	(0.038)	(0.051)	(0.019)	(0.016)
# of children	-0.205**	-0.216**	-0.189*	-0.063	-0.092	0.008	-0.051
	(0.034)	(0.053)	(0.070)	(0.087)	(0.061)	(0.070)	(0.040)
Partner w/o job	0.021	-0.163**	0.075	-0.097	-0.017	0.023	0.044
	(0.061)	(0.041)	(0.061)	(0.055)	(0.048)	(0.057)	(0.041)
Partner w/ job	0.017	-0.138	0.038	-0.013	-0.023	0.085*	0.063
	(0.068)	(0.059)	(0.061)	(0.046)	(0.051)	(0.027)	(0.043)
Vocational school	0.184*	0.112**	0.273**	0.103	0.257**	0.181*	0.193*
	(0.063)	(0.018)	(0.063)	(0.048)	(0.032)	(0.073)	(0.066)
High school	0.289**	0.278**	0.300**	0.300**	0.402**	0.348**	0.455**
	(0.059)	(0.053)	(0.054)	(0.045)	(0.039)	(0.066)	(0.078)
University	0.546**	0.486**	0.574**	0.456**	0.616**	0.472**	0.579**
	(0.077)	(0.044)	(0.085)	(0.041)	(0.053)	(0.059)	(0.081)
Age	0.023	-0.018	-0.012	-0.034	-0.042	-0.017	-0.026
	(0.021)	(0.022)	(0.044)	(0.037)	(0.047)	(0.030)	(0.029)
Age squared	-0.000	0.000	0.000	0.000	0.001	0.000	0.000
	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)
Unemployment level	-0.949	-1.015*	-1.747*	-0.888*	-1.752**	-0.730*	-1.499**
	(0.474)	(0.338)	(0.578)	(0.256)	(0.312)	(0.280)	(0.387)
Village	0.047	0.052	0.045	0.051	0.102**	0.288**	-0.036
	(0.051)	(0.025)	(0.048)	(0.034)	(0.023)	(0.058)	(0.042)
City	-0.007	-0.033	0.001	-0.050	0.096	0.225*	-0.024
	(0.068)	(0.063)	(0.075)	(0.048)	(0.048)	(0.070)	(0.049)
Large city	0.101	0.034	0.123	0.076	0.084	0.365**	-0.026
	(0.051)	(0.031)	(0.074)	(0.050)	(0.062)	(0.056)	(0.047)
Nursery availability	-0.286	0.069	-0.453	0.062	-0.032	0.268	-0.307

	(0.277)	(0.232)	(0.243)	(0.222)	(0.159)	(0.381)	(0.304)
Kindergarten availability	0.082	0.093	-0.038	0.022	-0.024	-0.061	0.155
	(0.106)	(0.077)	(0.101)	(0.069)	(0.062)	(0.078)	(0.092)
Constant	0.403	1.181*	0.907	1.085	1.169	0.340	0.898
	(0.418)	(0.467)	(0.852)	(0.772)	(0.894)	(0.460)	(0.514)
R^2	0.215	0.215	0.239	0.210	0.258	0.217	0.269
AIC	2004.270	2572.891	2138.544	2292.177	1849.722	1780.461	1476.022
N	1675	2133	1841	1881	1611	1475	1291

Figure 17: Employment rate (Hungary, whole population)