Childcare subsidies and labour supply - Evidence from a large Dutch reform

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Abstract

After the introduction of the Law on Childcare in 2005, childcare subsidies in the Netherlands became much more generous. Public spending on childcare increased from 1 to 3 billion euro over the period 2004–2009. Using a differences-in-differences strategy we find that, despite the substantial budgetary outlay, this reform had only a modest impact on employment. Furthermore, the rather small effects we find are likely confounded by a coincident increase in the EITC for parents with young children of 0.6 billion euro, which presumably also served to increase the labour supply of the group. The joint reform increased the maternal employment rate by 2.3 percentage points (3.0%) and maternal hours worked by 1.1 hours per week (6.2%). The results further suggest that the reform slightly reduced hours worked by fathers.

JEL codes: C21, H40, J13, J22
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1 Introduction

Many countries seek to increase the labour force participation of mothers with young children. Policymakers often point to Scandinavia, where public spending on childcare is high and participation rates of mothers are high as well. Indeed, several countries and regions have adopted part of the Scandinavian model by providing generous childcare subsidies to parents with young children (*e.g.* the Netherlands, Quebec) or are in the process of doing so (*e.g.* Germany).

In this paper we study the causal effect of childcare subsidies on labour supply by means of a large, recent reform in the Netherlands. After the introduction of the Law on Childcare in 2005, childcare subsidies in the Netherlands became much more generous. The average effective parental fee for formal childcare was cut in half, and subsidies were extended to so-called guestparent care (small-scale care at the home of the 'guestparent' or at the home of the children). As a result, public spending on childcare skyrocketed, from 1 billion euro in 2004 to 3 billion euro (0.5% of GDP) in 2009. Over the same period, the government also increased targeted earned income tax credits (EITCs) for the same parents. Budgetary outlays of these EITCs increased from 0.7 billion euro in 2004 to 1.3 billion euro in 2009. Since both policies target the same treatment group, the modest labour supply effects we find are the combined treatment effects of the childcare and the EITC reform.

We estimate the effect of the joint reform using data from the Labour Force Survey of Statistics Netherlands for the period 1995-2009, employing a differences-in-differences (DD) strategy. We estimate the effect on the participation rate and hours worked per week. The treatment group consists of parents 20 to 50 years of age with a youngest child up to 12 years of age. As a control group we use parents 20 to 50 years of age with a youngest child 12 to 17 years of age. This control group is chosen because the trends in participation and average hours worked per week of the treatment and control group are very similar before the reform, and placebo treatment dummies are insignificant. Unfortunately, we do not have linked individual data on labour supply and the use of childcare. Hence, we estimate an intention-to-treat effect.

Our main findings are as follows. First, we find that the reform increased the participation rate of women in the treatment group by 2.3 percentage points (3.0%). Second, the reform increased the average number of hours worked per week by women in the treatment group by 1.1 hours per week (6.2%), and reduced the hours worked per week by men in the treatment group by 0.3 hours per week (0.8%). Third, the policy seems to have been rather costly in terms of additional government spending per additional person and per additional fulltime equivalent employed. Spending on childcare subsidies and EITCs for parents with young children increased by 2.6 billion euro, whereas the treatment effect on the number of persons and fulltime equivalents employed was just 30 thousand additional persons and 30 thousand additional fulltime equivalents, respectively. This suggests an additional public spending of 87 thousand euro per additional person employed. Given that modal wage income in 2009 was around 32,500 euro, and the average taxes paid on this modal wage income were less than 10 thousand euro,[1] the additional costs for the

[1]Own calculations using Microtax of CPB Netherlands Bureau for Economic Policy Analysis.

government seem to have been much larger than the additional receipts, even if we allow for some additional savings on social assistance benefits (of approximately 14,000 euro per person) for single parents that started to work.[2] Why was the reform so costly? A substantial share of the higher subsidies was paid to parents that already used formal childcare at the lower pre-reform subsidy. In addition, the higher subsidy also caused a large shift from informal to formal childcare. Indeed, a back-of-the-envelope calculation suggests only a 0.19 (0.23) percentage point increase in the maternal employment rate per percentage point increase in the enrollment rate of children in daycare (out-of-school care).

There is an extensive literature that considers the relationship between parental labour supply and the cost of childcare using structural models and cross-sectional data. An in-depth overview is given in Blau and Currie (2006), who report estimated (childcare) price elasticities of female labour force participation ranging from 0.06 to -3.60. They argue that only a small part of this variation is due to differences in the composition of the sample or different data sources. Most of the variation seems to be due to identification problems related to the endogeneity of the explanatory variables.[3] To solve this problem, exogenous variation in the cost of childcare is needed. Therefore, the focus has shifted to quasi-experimental methods that use policy changes or discontinuities in policies as exogenous variation in childcare prices for parents. As a result, there is a small but growing body of quasi-experimental literature that studies the impact of changes or differences in childcare costs on labour supply.

In Section 6 we give a detailed overview of estimated treatment effects and study characteristics of related studies using natural experiments. A number of papers find rather small labour supply effects: Lundin et al. (2008) for Sweden, Havnes and Mogstad (2011a) for Norway and Fitzpatrick (2010) for the US. However, there are also a number of papers that find substantial labour supply effects, overall or for subgroups, in particular Baker et al. (2008) and Lefebvre and Merrigan (2008) for a reform in Quebec. When we compare our findings to related studies, our estimated treatment effects take an intermediate position. One potential explanation for why we find smaller effects than e.g. Baker et al. (2008) and Lefebvre and Merrigan (2008) is that we consider data from a recent period, where the pre-reform participation rate is already relatively high. However, some authors (e.g. Goux and Maurin, 2010; Havnes and Mogstad, 2011a) also point to potential pitfalls in the analysis of the reform in Quebec, where the treatment effect may in part have been driven by differential trends and/or other reforms. One potential explanation for why we find larger effects than the studies by Lundin et al. (2008) and Havnes and Mogstad (2011a) is that both workers and non-workers are eligible for childcare subsidies in Norway and Sweden, whereas only working single parents and two-earner couples are eligible for childcare subsidies in the Netherlands. This can also explain why we find larger effects than the US studies that consider differences in enrollment in pre-school, which is also

[2]In our data set we do not have information on how participation in formal childcare affects childrens' outcomes, nor do we have information on the impact on the well-being of parents, as in Baker et al. (2008). A full cost-benefit analysis of the reform we consider would have to take these effects into account, along with distributional effects of the reform.

[3]For example, unobserved characteristics are likely to influence both the costs of childcare (which depend on income) and the labour supply decision.

universal and not targeted solely at working parents.

We make a number of contributions to the literature. First, we study a very recent reform in a highly developed OECD country. This makes our results particularly relevant for other highly developed OECD countries that are considering to expand their formal childcare programs, since the initial maternal employment rate and public spending on childcare are arguably quite similar to many of these countries.[4] Indeed, as shown in Section 6, the effect of expanding subsidized childcare on maternal employment rates is lower in countries with a high initial maternal employment rate. Furthermore, being one of the few studies to use the Labour Force Survey, we can also determine the effect on hours worked, next to the effect on the participation rate. We find that the effect on hours worked by women is twice as large as the effect on the participation rate of women in percentage terms. Also, we study a reform that expands subsidies for both daycare and out-of-school care. To the best of our knowledge we are the first quasi-experimental study to look at the effect of out-of-school care on parental labour supply. Finally, our study is also unique in that we have ten years of pre-reform data and five years of post-reform data. This enables us to do placebo tests in a number of pre-reform periods, and to study both the short- and medium-run effects.

The outline of the paper is as follows. Section 2 describes the main aspects of the reform we exploit in the empirical analysis. Section 3 discusses our empirical methodology. In Section 4 we present our dataset and some descriptive statistics. Section 5 gives the estimation results for participation and hours worked. In Section 6 we compare our findings and study characteristics with related quasi-experimental studies. Section 7 concludes. An online appendix contains supplementary material.

2 The reform

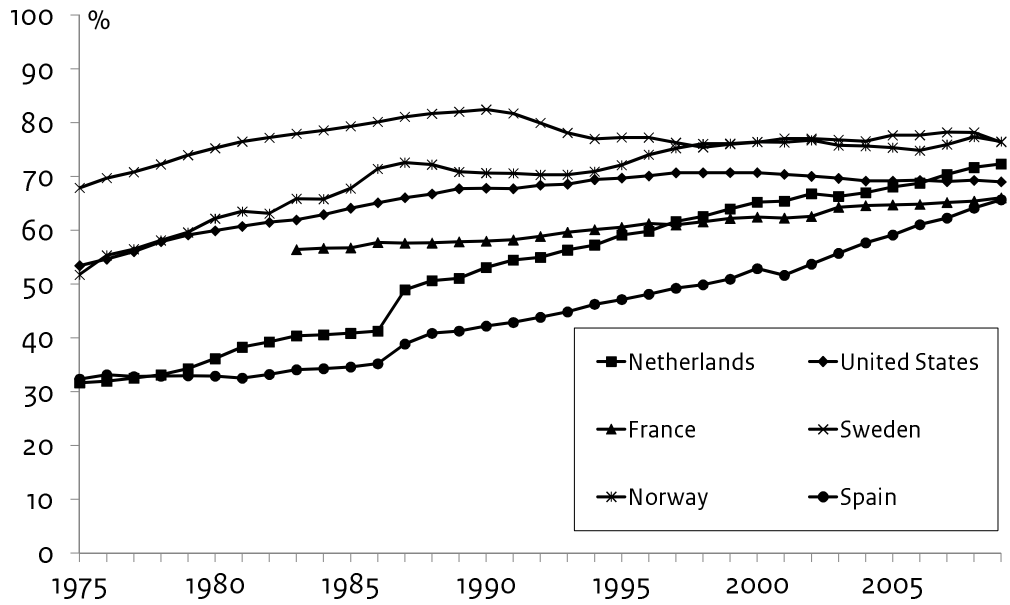
In the beginning of the 1970s, the employment rate of women (15–64 years of age) in the Netherlands, close to 30%, was rather low by international standards; see Figure 1. But following the economic crisis in the early 1980s, the employment rate of women in the Netherlands started to rise.[5] The strong rise in the participation rate of Dutch women continued all the way up to the reforms in 2005–2009, which we consider below. Indeed, by 2004 the participation rate of women in the Netherlands was among the highest in the OECD, close to 70%, falling just short of the participation rates in Norway, Sweden and the US.

Whereas the participation rate of women in the Netherlands showed a strong rise since the mid 1980s, a sizeable gap remained in terms of hours worked per week by employed women; see Figure 2. Furthermore, the gap with other OECD countries has been rather stable over time. In 2004, employed women in the Netherlands worked on average approximately 24 hours per week, while their counterparts in other OECD countries worked 5 to 10 hours per week more. Indeed, in 2004, the share of women working

[4]See e.g. OECD (2007, Table 3.2, Chart 6.1).

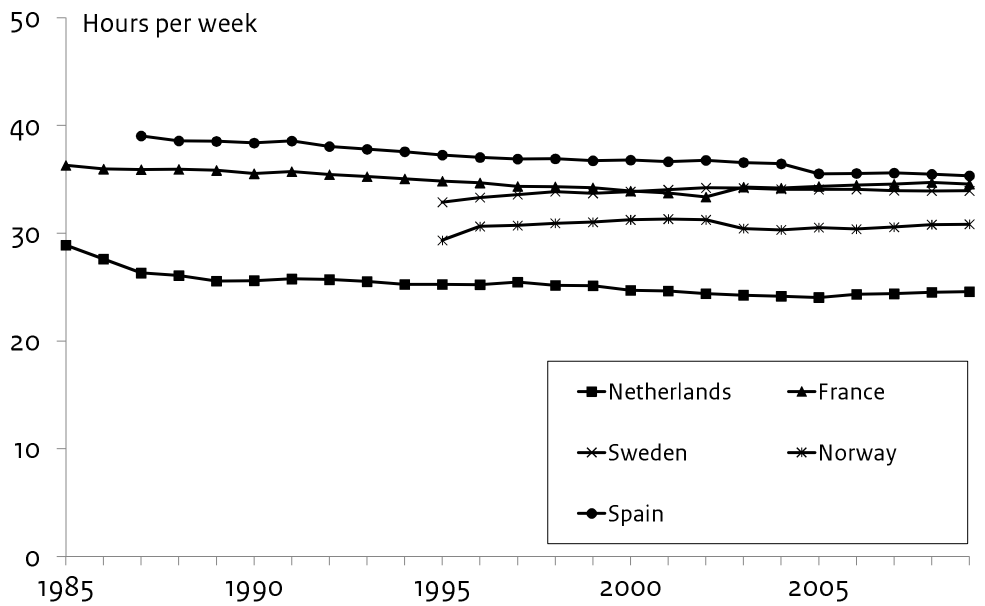
[5]For a detailed analysis of trends in female labour force participation in the Netherlands, see Euwals et al. (2011). Over the past decades, the rise in participation by mothers of young children was particularly strong.

Figure 1: Participation rate by women 15–64 years of age: 1975–2009



Source: OECD (2013).

Figure 2: Hours worked per week by employed women 15–64 years of age: 1985–2009



Source: OECD (2013).

part-time in the Netherlands was 60%, by far the largest share in the OECD (OECD, 2013).

To further promote the labour force participation of Dutch women, in persons but also in hours per week, the Dutch government implemented a series of reforms in the period 2005–2009. The two main goals of the reforms were: i) to make it easier for parents to combine work and care, and ii) to promote good quality care. With i) the government also planned to stimulate the labour force participation of parents (Ministry of Social Affairs and Employment, 2012, p.2). The most important changes took place in 2006 and 2007 when childcare subsidies were increased such that on average the parental fee decreased by 50%. Below, following a brief introduction into the pre-reform childcare market in the Netherlands, we give a historical account of the policy changes over the period 2005–2009, and indicate their relative importance for our analysis.

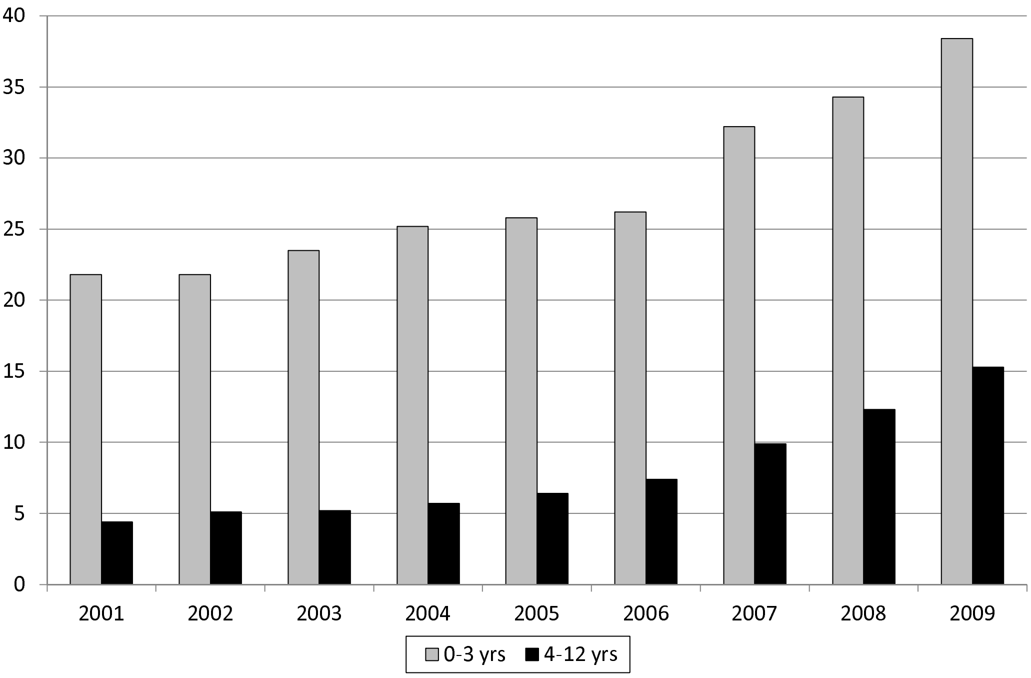
Children in the Netherlands go to primary school when they turn 4, and most children are 12 years old when they go to secondary school. Before the age of 4, children can go to centre-based daycare, so-called playgroups and informal care. Before the introduction of the Law on Childcare (*Wet kinderopvang*) in 2005, centre-based daycare was subsidized at different rates.[6] The majority (76%) of places was subsidized directly by employers and local governments.[7] These places had lower effective parental fees than so-called 'unsubsidized' places (24%), the costs of which were however partly tax deductible for parents. To qualify for the subsidies and tax deduction, both parents for two-parent households and one parent for single-parent households need to work. The total enrollment rate of children 0–3 years of age in centre-based care was 25% in 2004. Next to centre-based care a large number of children also go to playgroups (*peuterspeelzalen*). This is part-time care for less than 4 hours per day, mostly used by families in which one of the parents does not work. The enrollment rate of children 0–3 in playgroups was also close to 25%. Children that are in primary school can go to centre-based out-of-school care and informal care. Similar to daycare, before the introduction of the Law on Childcare, subsidized and unsubsidized centre-based out-of-school care places co-existed, where the costs of unsubsidized places were partly tax deductible for parents. The pre-reform enrollment rate of 4–12 year olds in centre-based care was below 6% in 2004.

The introduction of the Law on Childcare in 2005 unified the subsidies for centre-based care. From 2005 onwards, all centre-based places qualified for the same subsidy from the government, and subsidies were no longer transferred directly to childcare institutions but to parents using formal childcare. This increased the subsidy for parents with children going to an unsubsidized place before 2005 (before the reform they were eligible for a tax deduction that was typically lower than the subsidy after the reform). With the introduction of the Law on Childcare so-called guestparent care also became eligible for subsidies, becoming part of formal childcare. Guestparent care is small-scale care at the home of the guestparent or at the home of the children. The Law on Childcare in 2005 was the start of the reforms we consider in our empirical analysis below, but the unification of the subsidies and the extension to guestparent care had only a minor (initial) effect on

[6]All data on the use of formal childcare in Section 2 are from Statistics Netherlands (http://statline.cbs.nl).

[7]The subsidy is per hour of formal childcare.

Figure 3: Share of children in formal childcare (in %)



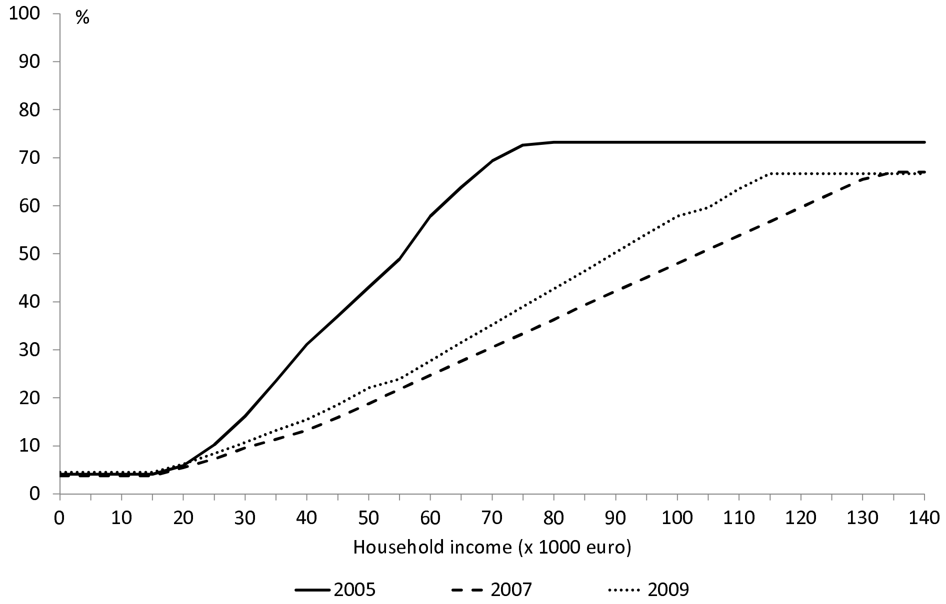
Source: Statistics Netherlands.

Table 1: Public spending on childcare and EITCs for parents (millions of euro)

Year	2002	2003	2004	2005	2006	2007	2008	2009
Childcare subsidies	725	755	1028	1,001	1,343	2,058	2,825	3,034
EITCs for parents	410	460	738	830	871	984	971	1,290
– *Combinatiekorting*[a]	410	460	479	484	314	324	247	0
– *Inkomensafhankelijke combinatiekorting*[b]	0	0	259	346	557	660	724	1,290

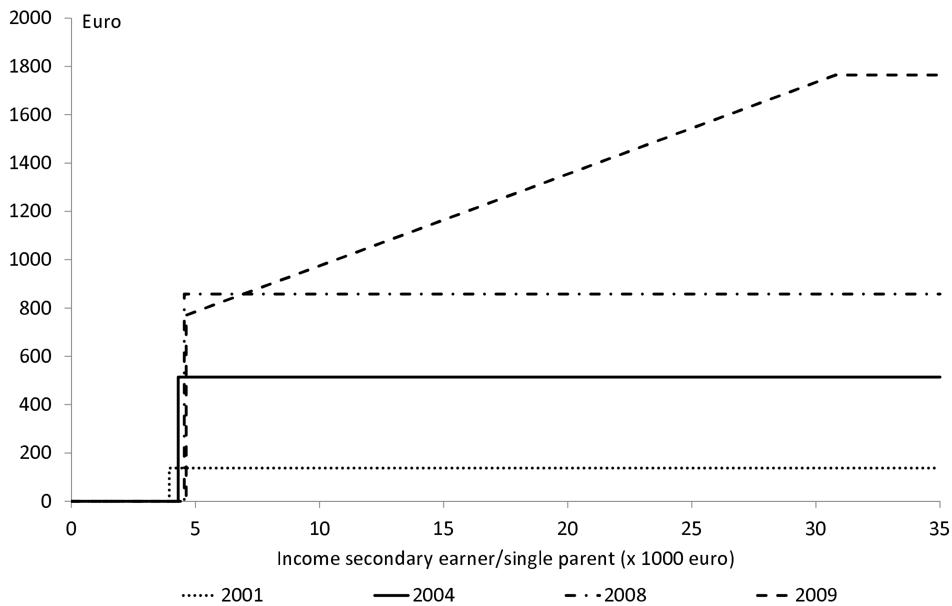
Source: Ministry of Finance (2010) and own calculations (imputation of employers' contribution for childcare up to 2007 with data from the Ministry of Social Affairs and Employment (personal communication) and split of the EITCs for parents in its two components using the MIMOSI model of CPB). [a]The *Combinatiekorting* applies to primary earners, secondary earners and working single parents with a youngest child up to 12 years of age. [b]The *Inkomensafhankelijke combinatiekorting* applies to secondary earners and working single parents with a youngest child up to 12 years of age.

Figure 4: Parental contribution rate for the first child



Source: own calculations using publicly available subsidy tables.

Figure 5: EITC secondary earners and single parents



Source: Tax Office.

public spending on formal childcare; see Table 1. Indeed, presumably because the subsidy was slightly reduced for the highest incomes,[8] public spending actually fell slightly from 2004 to 2005. Figure 3 shows that the share of children in formal childcare in 2005 hardly changed relative to the preceding period. Hence, in our empirical analysis we do not expect to find significant labour supply effects for 2005.

In 2006 and 2007 the subsidy rate was increased drastically. Figure 4 shows the resulting changes in the parental contribution rate for the 'first child' between 2005 and 2007.[9] First, note that the parental fee depends on the income of the household. In all years, households with the lowest income receive the highest subsidy (up to 96% of the full price). For households with a low income the subsidy rate hardly changed between 2005 and 2007. For middle income households the subsidy rate went up by 20 to 40 percentage points, whereas the increase in the subsidy for the highest income households was smaller than for middle income households. On average, the parental cost share in the full price dropped from 37% in 2005 to 18% in 2007.[10] Indeed, parents were the main beneficiaries of the reform as average prices of formal childcare places grew more or less in line with the CPI, despite the steep increase in the subsidy rate.[11] Hence, the increase in the subsidy rate was not counteracted by a rise in the full price of childcare charged by childcare institutions to parents. Next to the drop in parental fees, from 2007 onwards schools were obliged to act as an intermediary for parents and childcare institutions to arrange out-of-school care. Finally, in 2009, we saw a small reversal of the policy change, as the government reduced the subsidy for parents to some extent (see Figure 4), but compared to 2005 there was still a large drop in the parental fee for middle and high income households.[12]

Figure 3 shows that the dramatic drop in the contribution rate in 2006 and 2007 spurred the growth in the use of formal childcare in 2006 and beyond.[13] Due to the rise in the subsidy per child and the higher participation in formal childcare, public spending on childcare rose quickly from 1 billion euro in 2005 to 3 billion euro in 2009; see Table 1.

In DD analyses it is crucial to consider other policies that might influence the outcome variables for the treatment or control group (differently).[14] We carefully examined

[8] See Plantenga et al. (2005).

[9] The Tax Office defines the first child as the child for which the parents have the highest childcare expenditures.

[10] Source: Tax Office data provided by the Ministry of Social Affairs and Employment (personal communication).

[11] Over 2005–2009 the average full price for an hour of daycare and out-of-school care grew by 9.6% and 6.0%, respectively (Ministry of Education, Culture and Science, 2009), while the CPI grew by 6.5% (CPB, 2012).

[12] Since childcare subsidies expanded more for middle income households than for high and low income households, larger labour supply effects are expected for the former households. Unfortunately, we cannot perform this exercise, because data on household income is not included in our dataset (the Labour Force Survey).

[13] Survey results from Berden and Kok (2009) indicate that there was a large shift from informal to formal care between 2004 and 2008: for children 0–3, 4–7 and 8–12 years of age, the share of parents using formal care in the total of parents using formal and informal care rose from respectively 58 to 77%, 22 to 54% and 21 to 44%.

[14] Another concern might be that what we see is not the result of an increase in childcare demand but the result of a drop in rationing on the formal childcare market. However, the available data on waiting

various changes in taxes and subsidies and found that, apart from one, there were no substantial changes in taxes or subsidies targeted at the treatment or control group. The only complication comes from changes in the EITCs for parents with a youngest child up to 12 years old, the *Combinatiekorting* (Combination credit) and the *Inkomensafhankelijke combinatiekorting* (Income dependent combination credit). These EITCs are also targeted exclusively at our treatment group. Figure 5 shows the change in the sum of the *Combinatiekorting* and *Inkomensafhankelijke combinatiekorting* for secondary earners and single parents (mostly women) over the period 2001–2009. Table 1 gives the changes in aggregate 'spending' (revenue losses) on these EITCs. Between 2001 and 2004 these credits increased from 138 to 514 euro, and public expenditures increased from 410 to 738 million euros between 2002 and 2004. Between 2004 and 2008 the individual subsidy increased from 514 euro to 858 euro for secondary earners and single parents, and in 2009 there was another increase for secondary earners and single parents with relatively high earnings.[15] In 2009, the maximum credit was 1,765 euro, where the maximum was reached at 30,803 euro of gross individual income (for comparison, in 2009 the minimum wage of a fulltime worker was 16,776 euro). Since these credits target the same group as childcare subsidies we can only determine the joint effect of the changes in childcare subsidies and these credits.[16] The EITC reform presumably served to increase the labour supply of the treatment group, meaning that this does not affect our conclusion that the effect on employment was modest given the budgetary impulse.[17]

3 Methodology

We estimate the effect of the reform on labour participation using a DD strategy (see *e.g.* Blundell and Costa Dias, 2009; Imbens and Wooldridge, 2009). This method estimates the effect of a reform by comparing the change in outcomes of the treatment group before and after the reform, using the change in outcomes of a control group to control for common time effects. Our treatment group consists of parents influenced by the change in childcare costs, which are parents with a youngest child up to 12 years of age.

As the control group we use parents with a youngest child 12 to 18 years of age (living at home). These parents are not eligible for childcare subsidies but are otherwise quite similar to the treatment group. The DD estimator requires that in the absence of the

lists suggest that these are rather small, and that the change in waiting lists was much smaller than the change in filled childcare places. For example, the survey data reported in Van Rens and Smit (2011) suggest that the waiting list for daycare (out-of-school care) dropped from 10% (11%) of filled places in 2007 (the first year of the survey) to 7% (6%) of filled places in 2009. The drop in waiting lists is much smaller than the increase in the number of children going to daycare and out-of-school care, which increased by 49% (19%) and 139% (55%) respectively between 2005 and 2009 (2007 and 2009).

[15] The tax credit for working primary earners (mostly men) with young children was phased out over the period 2005–2009.

[16] Also note that the change in the credits for working parents in 2009 was mostly targeted at middle and high income earners, like the childcare reform.

[17] De Boer et al. (2014, Table 5) simulate the EITC reform with a structural model. They find that the reform increased both the participation rate and the hours worked per week of mothers with a youngest child 0–11 years of age living in couples.

policy reform the treatment and control group face a common time trend in labour force participation. This assumption cannot be tested. However, we have ten years of pre-reform data, so we can check whether the groups have a similar trend in the pre-reform period. In general, we estimate an event history specification, allowing leads and lags around the reform to have different coefficients.

This assumption could be violated if the government was anticipating a change in behaviour when deciding to pass the new law. Also, if parents anticipated the policy change and adapted their behaviour in advance, this too would create a problem for identification of the treatment effect. In our case, both issues are unlikely. First, inspection of the data shows that there is no change in the long-term trend in the years before the reform that could have induced the policy changes from 2005 onwards. Second, the most important policy change is the reduction of the parental fee in 2006–2007. Since this reduction was not included in the Law on Childcare of 2005, parents were unable to anticipate these changes before 2005. Both assumptions are supported by the outcomes of the placebo tests that we report in Section 5.

Finally, the common trend assumption is violated if the composition of characteristics related to our outcomes within the groups is not stable. This could happen if for example the childcare reform led to a change in fertility rates (since our treatment group is defined by having a young child this would alter the composition of the treatment group).[18] We constructed and inspected fertility rates but found no evidence of a change in trends after 2005.[19]

To estimate the treatment effect on participation, we regress participation status on year fixed effects (α_t), group fixed effects (γ_g), individual characteristics (X_i) and a set of treatment dummies for each year after the reform (D_{gs}):

$$y_{igt} = \alpha_t + \gamma_g + X_i\beta + \sum_{s=2005}^{2009} \delta_s D_{gs} + \epsilon_{igt}. \tag{1}$$

D_{gs} is a set of dummies equal to one if individual i has a youngest child up to 12 years of age in year s. The common trend of the treatment and control group is captured by the year fixed effects, while the constant difference in participation between the treatment and control group is captured by the group fixed effects. We include different group fixed effects for parents with a youngest child 0–3, 4–7 and 8–11 years of age. Individual

[18] Figure A.1 shows that each characteristic develops smoothly for both the treatment and control group. The proverbial exception is the ethnicity dummy, the increase in the share of immigrants in 2000 is caused by a change in the definition of this group, however our results are very similar when we exclude the ethnicity dummy from the regressions. We have also run regressions of the covariates on year fixed effects, the group dummies and the treatment dummies. The results for the treatment dummies in Table A.4 indicate that some covariates are correlated with the treatment dummies. However, it is unlikely that these results reflect endogenous responses to the reform (e.g. education, ethnicity and age are presumably fixed), rather they indicate small differences in the trend growth of the covariates between the treatment and control group. Regression results indeed show that it is important to control for these individual characteristics. A concern is then that there are also unobserved differential changes in the treatment and control group. However, we do not find support for this hypothesis since the placebo treatment dummies are not significantly different from zero.

[19] None of the other quasi-experimental studies discussed in Section 6 that look at fertility finds a significant effect on fertility.

characteristics are included to control for observable changes in the composition of the groups over time. In equation (1) we allow the treatment effect to be different in each year after the policy change. When the annual treatment effects are not significantly different from one another, we instead estimate two treatment effects: a treatment effect for 2005–2007 (short-run) and a treatment effect for 2008–2009 (medium-run).

According to the modified Breusch-Pagan test, the hypothesis of homoskedasticity is strongly rejected for both the participation and hours equation. Therefore, we use population weights in estimation to correct for heteroskedastic error terms.[20] Table A.5 and Table A.6 show that the estimates of the treatment effects are very similar when we apply standard OLS without weights.

To further correct for potential heteroskedasticity we report robust standard errors. For the main regressions, in the online appendix we also report standard errors clustered at the year-group level,[21] and standard errors clustered at the group level,[22] (see Table A.5 and Table A.6). In the majority of cases, clustered standard errors are smaller than robust standard errors. We prefer to be conservative and report the larger (robust) standard errors (as suggested by Angrist and Pischke, 2009). Furthermore, the conclusions are robust across the alternative specifications for the standard errors.

Participation is a discrete variable, so equation (1) is a linear probability model for participation. We also estimate the effect on hours worked per week. We follow Angrist and Pischke (2009) and estimate a linear model with the same sample of individuals that we use in the participation equation. So we estimate equation (1) with y denoting the number of hours worked per week, potentially zero.

4 Data

We use data from the Dutch Labour Force Survey (*Enquête Beroepsbevolking*) of Statistics Netherlands. This is an annual survey which includes approximately 80,000 individuals per year. We have repeated cross-sections for the period 1995–2009. The reform started in 2005, so we have a long data series preceding the policy change to study the common trend assumption crucial in DD analyses. The finding of a common trend before the reform is taken as an indication that the trend would remain the same in the absence of the reform. The survey includes labour supply information (participation and hours worked per week), individual characteristics (age, education level, native/immigrant, couple/single) and household characteristics (number of children, age of the children).[23]

From this dataset we select our treatment group of mothers with a youngest child up to 12 years of age. Furthermore we restrict the analysis to mothers 20 to 50 years of age.

[20] Following the recommendation of Solon et al. (2013).

[21] 64 clusters: 4 groups, youngest child 0–3, 4–7, 8–11 and 12–17 years of age, times 16 time periods.

[22] 4 clusters: youngest child 0–3, 4–7, 8–11 and 12–17 years of age.

[23] For each year we restrict our sample to individuals that were interviewed in person. Apart from these, there were three follow-up interviews of the same individual within one year by telephone. Since these are considered less reliable and are basically the same observation (see Statistics Netherlands, 2009), we decided to use only the data from the interviews in person. Unfortunately, we could not make this distinction for 2009, so we have about four times more observations in 2009 than in the other years, but the sample weights correct for this.

Table 2: Descriptive statistics treatment and control group (1995–2009)

	Treatment group	Control group
Participation	0.664	0.709
Hours worked per week	14.44	16.63
Age	35.73	44.00
Lower educated	0.292	0.400
Middle educated	0.461	0.423
Higher educated	0.247	0.177
Single	0.095	0.144
Immigrant	0.207	0.165
Household size	3.918	3.812
Age youngest child	4.459	14.361
Observations	202,104	61,125

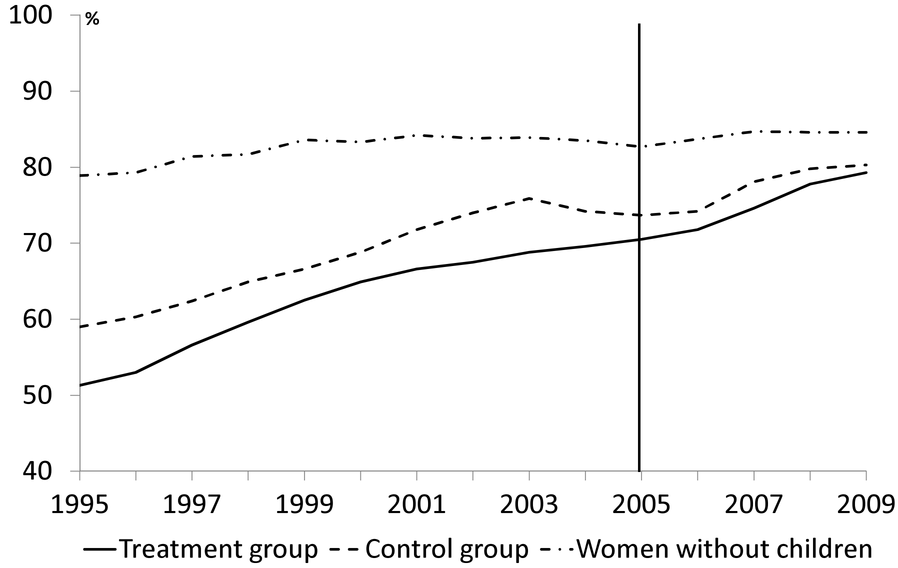
Values are means weighted with sample weights. Source: Labour Force Survey (Statistics Netherlands).

This gives us 202,104 observations for the treatment group (for the full sample period). As a control group we use mothers with a youngest child 12 to 18 years of age. Restricting the control group to mothers 20 to 50 years of age we have 61,125 observations in the control group. We also considered women without children as a potential control group. However, as we will see below, this is not a valid control group since they have a different pre-reform trend in the participation rate and hours worked than the treatment group.

Table 2 gives descriptives statistics for the treatment and control group. The table shows the outcome variables participation and hours worked per week, and the covariates age (in the regression we use 5-year category dummies), education (in categories lower, middle and higher educated), a dummy for being single, a dummy for being an immigrant, the size of the household (in the regression we include dummies for families with one, two or three and more children below the age of 12) and the age of the youngest child (with separate dummies for 0–3, 4–7 and 8–11 years of age). For most variables, the treatment and control group are quite similar. Mothers in the control group are somewhat more likely to be single, and are also somewhat more likely to be lower educated. The share of immigrants is slightly higher in the treatment group, which could be explained by the higher fertility rate of immigrants. There are sizeable differences between the groups with respect to the age of the mother and the age of the youngest child, however this is inevitable considering the definition of the groups.

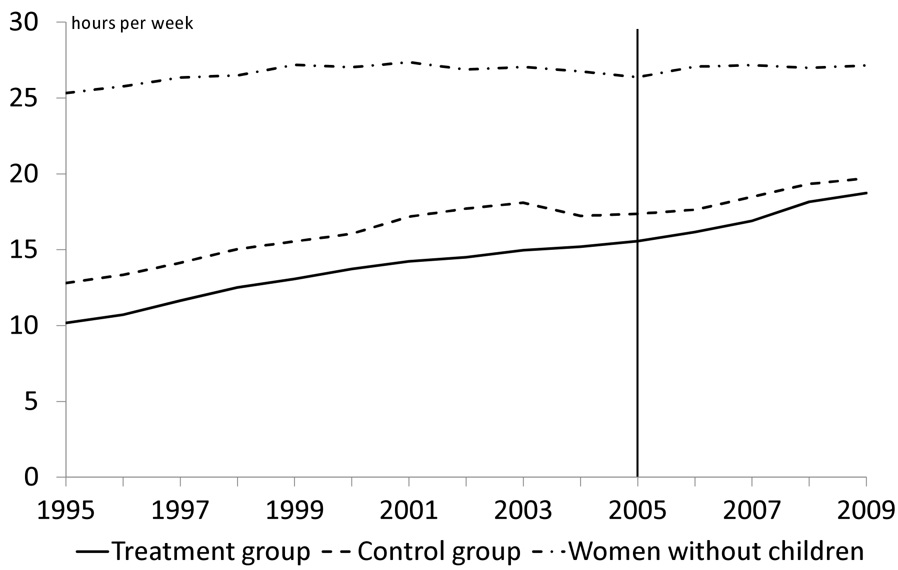
In the DD method we compare the outcomes of the treatment and control group over time. In Figure 6 we plot participation rates of the women in the treatment group (youngest child 0–11), the control group (youngest child 12–17) and for women without children (a potential control group). The solid vertical line marks the start of the policy reform. We see that both the treatment and control group exhibit an upward trend before the policy change, while participation is always higher for the control group. Furthermore, the rate of growth is very similar for the two groups, whereas women without children

Figure 6: Participation rate



Source: Labour Force Survey (Statistics Netherlands).

Figure 7: Hours worked per week



Source: Labour Force Survey (Statistics Netherlands).

clearly have a different pre-reform trend.[24] This suggests that women with an older child are an appropriate control group for our DD analysis, whereas women without children are not.

In Figure 7 we plot the average number of hours worked per week. Again we see that there is a clear upward trend, both in the treatment group and our control group, whereas the upward trend is absent in the group of women without children. Again, women with an older child seem an appropriate control group, and women without children do not.

5 Estimation results

5.1 Participation rate

We first present the estimation results for the effect of the reform on the participation rate of all women in the treatment group, and subsequently consider the results for subgroups.

We first estimate equation (1) for the participation rate, without any individual or household characteristics and with two treatment effects, one for 2005–2007 and one for 2008–2009.[25] Estimates are presented in column (1) in Table 3. In the first three years the effect is 2.7 percentage points, while in the last two years it is 4.2 percentage points, both significantly different from zero. In column (2) we include individual characteristics. Controlling for changes in the observed characteristics, the effects drop to 1.5 percentage points and 2.3 percentage points (a 3.0% increase), respectively.[26,27]

A concern might be that some women that were in the treatment group in the early years are in the control group in the later years. When there is a treatment effect on the participation rate of mothers extending beyond the treatment period (due to for example a career effect), part of the treatment effect may be masked by an effect on the control group.[28] Therefore we also estimate the model using only parents with a child 16–17 years of age in the control group so that individuals in the control group that were previously

[24]There appears to be somewhat of a wobble in the participation rate in the control group around 2003–2004. However, when we add a placebo treatment dummy for 2003–2004, it is not significantly different from zero, and the treatment effect is virtually unchanged, see below in Table 3. As an additional check we also looked at the mean values for the covariates for the control group, but these show no sudden changes around 2003, see Figure A.1 in the online appendix.

[25]Results of estimating the model with annual treatment dummies for 2005–2009 are shown in Table A.1 in the online appendix. We can not reject that the treatment effects per year are equal. However, the results suggest a difference between the effects in 2005–2007 and 2008–2009. We therefore decided to estimate and present results with separate treatment effects for 2005–2007 and 2008–2009. These might be considered the short- and medium-run effects of the reform.

[26]The estimates of the coefficients of the control variables are all significant and in line with expectations, see Table A.1 in the online appendix. The linear probability model may predict values outside the [0,1] interval. We find that only 0.14% of the predicted values are outside this interval.

[27]For the two treatment effects we still cannot reject that they are equal. When we estimate one single treatment effect for 2005–2009, we obtain a treatment effect of 1.8 percentage points (significant at the 1% level).

[28]The results on career effects of the studies discussed in Section 6 are mixed. Lefebvre et al. (2009) find significant long-run effects for lower-educated mothers, Nollenberger and Rodríguez-Planas (2015) find that the effect on participation reaches its maximum two years after the treatment and then fades away.

Table 3: Effect on participation rate: all women

	(1) WLS 95–09 No covariates	(2) WLS 95–09	(3) WLS 95–09 No overlap[a]	(4) WLS 95–09 With placebo	(5) M-DD 95–09	(6) WLS 95–09 Simple diff. quadr. trend
Placebo 00–02				–0.001		
				(0.007)		
Placebo 03–04				–0.011		
				(0.008)		
Treat 05–07	0.027***	0.015**	0.025**	0.012*	0.016*	-0.008
	(0.006)	(0.006)	(0.011)	(0.007)	(0.008)	(0.006)
Treat 08–09	0.042***	0.023***	0.034***	0.021***	0.031***	0.033***
	(0.006)	(0.006)	(0.010)	(0.007)	(0.007)	(0.011)
Observations	263,229	263,229	219,961	263,229	263,229	202,104

Robust standard errors in parentheses, standard errors in column (5) are bootstrapped, * denotes significant at 10% level, ** at 5% level and *** at 1% level. Individual characteristics (except in (1)), group fixed effects and year fixed effects are included but not reported. [a]The control group consists of parents with a youngest child aged 16–17, so we exclude individuals in the control group that were previously in the treatment group.

Table 4: Effect on participation rate: subgroups of women (WLS)

	(1) Single women	(2) Women in couples	(3) Youngest child 0–3	(4) Youngest child 4–7	(5) Youngest child 8–11
Treat 05-07	0.030	0.011*	0.018***	0.015**	0.012
	(0.019)	(0.006)	(0.007)	(0.008)	(0.008)
Treat 08-09	0.047***	0.020***	0.025***	0.028***	0.015**
	(0.018)	(0.006)	(0.007)	(0.007)	(0.007)
Observations	26,453	236,776	155,163	119,429	110,887

Robust standard errors in parentheses, * denotes significant at 10% level, ** at 5% level and *** at 1% level. Individual characteristics, group fixed effects and year fixed effects are included but not reported.

in the treatment group are excluded. This results in the effects reported in column (3). The effect is somewhat larger than our base results, though not significantly different.

The validity of our estimates depends critically on the common trend assumption. To further assess the plausibility of this assumption we also estimate a placebo treatment effect. Specifically, we estimate a treatment effect for some years before 2005. Since no relevant policy change occured in this period (that differs between the treatment and control group) we should not find a significant effect. We estimate a placebo treatment effect for 2000–2002 and for 2003–2004. The placebo effects and the two treatment effects are reported in column (4). Both placebo treatment effects are not significantly different from zero, while both treatment effects are hardly affected by the inclusion of the placebo dummies.[29]

As another robustness check we report estimates based on a matching-differences-in-differences approach (see for example Blundell and Costa Dias, 2009). The combination of matching and differences-in-differences weakens the required assumptions of each of these methods separately. We create cells based on marital status, ethnicity, education level and number of children. We calculate the average participation in each cell-year combination for both the control and the treatment group and compute the differences-in-differences estimate for each cell. We then average over all estimates, weighting by the population share of the cell in the treatment group.[30] The estimates are presented in column (5) and are very close to the baseline effects.

Finally, we show results of a simple differencing model for only the treatment group, including a quadratic time trend (column (6)). This leads to a zero effect in 2005–2007 and a positive effect of 3.3 percentage points in 2008–2009.

We also estimate the effect for the following subgroups of women: i) single women, ii) women in couples, iii) women with a youngest child 0–3 years of age (pre primary

[29] Table A.7 in the online appendix shows that using 2003–2004 as the base years and including a placebo treatment dummy for 1995–1999 and 2000–2002 again results in insignificant placebo treatment dummies.

[30] The exact estimator is defined as follows. Define cells $j \in J$ for each combination of the values of the covariates (there are 36 cells in our application). The variable of interest (participation or hours worked) of individual i, in year t belongs to one particular cell j and to the treatment group ($d = 1$) or the control group ($d = 0$), and is denoted by Y^d_{ijt}. We define three periods $p = 0, 1, 2$, which are the pre-reform period (1995–2004), the short-term post-reform period (2005–2007) and the medium-term post-reform period (2008–2009). The average outcome in cell j of group d in period p is given by:

$$\bar{Y}^d_{j,p} = \frac{1}{\sum_t I(t \in p)} \sum_{t \in p} \Big[\sum_{i \in j} k_{i,t} Y^d_{i,j,t} \Big]$$

With $k_{i,t}$ is the individual's weight within a cell, which is based on the population weights that we use in all regressions. The treatment effect estimator (for the short-term effect, $p = 1$) is given by the weighted average over the differences-in-differences estimates in each cell:

$$\delta_p = \sum_{j=1}^{J} \alpha_j \Big[(\bar{Y}^{treat}_{j,1} - \bar{Y}^{treat}_{j,0}) - (\bar{Y}^{control}_{j,1} - \bar{Y}^{control}_{j,0}) \Big]$$

And similar for the medium-term effect, $p = 2$. The weight of each cell in the sample is denoted by α_j and is based on the distribution of covariates of the treated individuals in the post-reform period.

school), iv) 4–7 years of age (first years in primary school), and v) 8–11 years of age (last years in primary school). We do this by estimating equation (1) for each subsample, thereby allowing differences in coefficients for all covariates between subgroups. Results are reported for these groups in Table 4. In columns (1) and (2) we find that the effect on the participation rate of single women is higher than for women in couples. Next, columns (3), (4) and (5) suggest that the effect on the participation rate is larger for women with a child in daycare or a child in the first grades of primary school than for women with a child in the later grades of primary school. Since older children are less likely to go to childcare, and we are estimating an intention-to-treat effect, this is in line with expectations.[31] The placebo treatment dummies for 2000–2004 are insignificant for all subgroups except for women with a youngest child aged 0-3.[32]

5.2 Hours worked per week

In addition to participation we are also interested in the effect on hours worked per week. Again we start with the results for all women, and subsequently consider the results for subgroups.[33]

As discussed in Section 3 we estimate equation (1) with average hours worked per week as the outcome variable and including all women in this regression, both working women and non-working women. We estimate a separate treatment dummy for 2005–2007 and for 2008–2009. Results for the estimation without covariates are reported in column (1) in Table 5. We find a significantly positive effect of 1.0 hours per week in 2005–2007, and 1.6 hours per week in 2008–2009. When we include covariates, in column (2), the treatment effects drop again, but remain positive and significant at the 1% level in both periods. In 2005–2007 the estimated effect is an increase of 0.7 hours per week. In 2008–2009 the effect is 1.1 hours per week. Given the average number of hours worked per week for women of 18.4 in 2008–2009, these effects are more substantial in percentage terms (6.2% in 2008–2009) than the effects on the participation rate (3.0% in 2008–2009).

When we restrict the control group to mothers with a youngest child 16–17 years of age, such that they were never in the treatment group, we find that the estimated effect on hours is again somewhat larger; see column (3). Column (4) presents the effects on hours worked when we add two placebo treatment dummies, for 2000–2002 and 2003–2004. The first placebo dummy is negative and significant at the 10% level, the second is not significantly different from zero.[34] Both the short- and medium-run treatment effects are somewhat lower than in column (2).

[31]In Figures A.2 and A.3 in the online appendix we show treatment effects per age of the youngest child, for 2005–2007 and 2008–2009, respectively. These results also show a declining pattern with age of the youngest child.

[32]These results are available in Table A.2 in the online appendix.

[33]For the hours worked analysis we restrict the sample period to 1997-2009. Inspection of the trends showed that in 1995-1996 trends might be slightly different, such that we decided to exclude these early years from the hours analysis.

[34]Table A.7 in the online appendix shows that using 2003–2004 as the base years and including a placebo treatment dummy for the periods before 2003 results in insignificant placebo treatment dummies for hours worked per week.

Table 5: Effect on hours worked per week: all women

	(1) WLS 97–09 No covariates	(2) WLS 97–09	(3) WLS 97–09 No overlap[a]	(4) WLS 97–09 With placebo	(5) M-DD 97–09	(6) WLS 97–09 Simple diff. quadr. trend
Placebo 00–02				–0.447*		
				(0.263)		
Placebo 03–04				–0.436		
				(0.271)		
Treat 05–07	1.033***	0.661***	0.775**	0.384	0.817***	-0.134
	(0.207)	(0.198)	(0.356)	(0.246)	(0.237)	(0.173)
Treat 08–09	1.570***	1.075***	1.402***	0.798***	1.278***	1.136***
	(0.203)	(0.194)	(0.336)	(0.243)	(0.250)	(0.344)
Observations	231,097	231,097	192,962	231,097	231,097	177,286

Robust standard errors in parentheses, * denotes significant at 10% level, ** at 5% level and *** at 1% level. Individual characteristics (except in (1)), group fixed effects and year fixed effects are included but not reported. [a]The control group consists of parents with a youngest child aged 16–17, so we exclude individuals in the control group that were previously in the treatment group.

Table 6: Effect on hours worked per week: subgroups of women (WLS)

	(1) Single women	(2) Women in couples	(3) Youngest child 0–3	(4) Youngest child 4–7	(5) Youngest child 8–11
Treat 05–07	1.090*	0.552**	1.009***	0.472**	0.309
	(0.623)	(0.207)	(0.218)	(0.241)	(0.254)
Treat 08–09	1.680***	0.962***	1.418***	1.226***	0.408
	(0.609)	(0.203)	(0.215)	(0.239)	(0.250)
Observations	23,945	207,152	135,545	105,170	98,004

Robust standard errors in parentheses, * denotes significant at 10% level, ** at 5% level and *** at 1% level. Individual characteristics, group fixed effects and year fixed effects are included but not reported.

Table 7: Effects on labour supply: all men

	(1) Participation WLS	(2) Hours worked WLS
Treatment 05-07	0.005	–0.108
	(0.004)	(0.238)
Treatment 08-09	0.003	–0.344
	(0.004)	(0.216)
Observations	224,674	195,879

Robust standard errors in parentheses, * denotes significant at 10% level, ** at 5% level and *** at 1% level. Individual characteristics, group fixed effects and year fixed effects are included but not reported.

In column (5) we present results from the matching-differences-in-differences estimator as defined in the previous section. The results are very similar to the baseline results in column (2). In column (6) we present results from a simple difference model, only including the treatment group and a quadratic trend. The treatment effect for 2005–2007 is not significantly different from zero, the treatment effect for 2008–2009 is close to our baseline estimate.

For subgroups of women we report estimates in Table 6. The pattern is similar to the results for participation. The effect for single women is again larger than for women in couples and the total effect can be attributed mainly to women with a child younger than 8 years of age. For hours worked, the placebo treatment dummies are insignificant for all subgroups except women in couples and women with a youngest child 0–3 years old.[35] Recall we are identifying the treatment effects of the joint reform and not solely of the childcare reform. The differential effects across subgroups therefore might be confounded by heterogenous effects of the EITC expansion.

5.3 Results for men

The effects for men are much less pronounced. We briefly report the main results in Table 7.[36] We find no significant effect on the participation rate of men in any treatment year in any specification.[37] We also check for an effect on the participation rate for subgroups. We find no significant effect for most subgroups.

We find a negative coefficient on hours worked per week for men, increasing in magnitude to –0.3 hours in 2008-2009. However, the coefficient is not significantly different from zero. For men with a youngest child 0–3 years old we find a significant negative effect on hours worked of –0.5 hours per week. The drop in hours worked by these men

[35]Placebo treatment effects can be found in Table A.3 in the online appendix.

[36]Detailed results can be found in Table A.8–A.11 in the online appendix. Figures A.6 and A.7 plot the participation rates and hours worked per week for men, respectively.

[37]When we limit the control group to men with a youngest child 16 or 17 years old, the coefficient on the treatment effect becomes slightly negative for 2008–2009 and significant at the 10% level.

may be the result of the increase in the labour supply of their partners.

6 Discussion

How do our results compare to the findings of related studies? Table 8 gives an overview of quasi-experimental studies that study the effect of changes in subsidized childcare or eligibility for (pre-)school on the labour force participation of parents.[38] For each study we report subsequently: the country under consideration, a brief description of the reform/instrument and the treatment group, the pre-reform or counterfactual participation rate and hours worked per week (including the zeros for the non-participants), the sample period, the share of part-time employment in total employment, and finally the treatment effect on the participation rate (in percentage points) and on hours worked per week.[39]

We divide the studies into two groups, 'intention-to-treat' studies and 'IV' studies. The reported treatment effects for the IV studies measure the effect corresponding to an increase in the enrollment rate of children in childcare or pre-school by 1 percentage point (multiplied by 100 for the participation rate). The reported treatment effects for the intention-to-treat studies correspond to different changes in enrollment rates of children in childcare or pre-school. Therefore, to ease the comparison, below we also present back-of-the-envelope calculations of an 'IV' treatment effect for some intention-to-treat studies using information on changes in the enrollment rates of children in childcare or pre-school.

First, consider the effect on the participation rate. Looking at the column 'TE PR' (treatment effect, participation rate) of Table 8, we find that our treatment effect takes an intermediate position. For the whole group of mothers with a youngest child 0–11 years of age, we find an increase in the participation rate of 2.3 percentage points. This is larger than the effects reported for the reforms in Sweden and Norway by respectively Lundin et al. (2008) and Havnes and Mogstad (2011a), comparable to the effects reported by Nollenberger and Rodríguez-Planas (2015) for the reform in Spain, but substantially smaller than the effects reported by Baker et al. (2008) and Lefebvre and Merrigan (2008) for the reform in Quebec.[40]

However, the comparison is complicated by the fact that we are comparing treatment effects for different impulses, and for different treatment groups. To ease the comparison with other studies we can calculate the increase in the participation rate of mothers per percentage point increase in the enrollment rate of children in childcare or pre-school, following e.g. Cascio (2009). Furthermore, most other studies focus on mothers with young children, so we will do the calculation for mothers with a youngest child 0–3 years of age. For this group we find a treatment effect of 2.5 percentage points. The data underlying Figure 3 show that the enrollment rate for children 0–3 years of age increased

[38]The exact references of the reported treatment effects can be found in Table A.12 in the online appendix.

[39]The data on part-time employment in total employment are taken from the OECD Labour Force Statistics. The data are for all women and men. Unfortunately, the OECD does not report part-time shares for the subgroups we consider.

[40]The treatment effect of Lundin et al. (2008) is not directly comparable to the other numbers since it measures the childcare price elasticity of the employment rate of mothers. However, what is the most relevant here is that the number is small and insignificantly different from zero.

Table 8: Comparison with related quasi-experimental studies[a]

Study	Country	Reform/instrument	Treatment group	Pre PR	Pre H/W	Sample period	Share PT	TE PR	TE H/W
'Intention-to-treat' studies									
This study	NL	Parental fee from 37 to 18%,	Mothers 0–11	71	15.2	95–09	60	2.3***	1.1***
		extens. to guestparent care,	Mothers coupl. 0–11	72	15.3	95–09	60	2.0***	0.9***
		increase EITC work. parents	Single moth. 0–11	56	14.2	95–09	60	4.7***	1.7***
			Mothers young. 0–3	70	15.3	95–09	60	2.5***	1.4***
			Mothers young. 4–7	69	14.4	95–09	60	2.8***	1.2***
			Mothers young. 8–11	74	16.0	95–09	60	1.5**	0.4***
			Fathers 0–11	94	38.2	95–09	15	0.3	–0.3
Nollenberger and Rodríguez-Planas (2014)	ESP	Expansion of subsidized childcare	Mothers young. 3	29	10.9	87–97	12	2.3***	0.9***
Felfe et al. (2013)[b]	SWI	Difference in after-school care	Mothers 0–12	68	-	10	47	7	-
		in neighbouring cantons	Fathers 0–12	98	-	10	9	–2	-
Havnes and Mogstad (2011a)	NOR	Staggered intro. childcare	Married moth. y. 3–6	25	-	{76,79}	41	1.1***	-
Schlosser (2011)	ISR	Staggered intro. pre-school	Arab mothers 2–4	6	1.5	98–03	-	7.1**	2.8**
Goux and Maurin (2010)	FRA	Eligibility for pre-school	Mothers coupl. 3	78	-	99	25	0.4	-
			Single moth. 3	80	-	99	25	3.6**	-
Cascio (2009)	US	Staggered intro. public school	Married moth. y. 5	36	12.7	50–90	-	–1.1	–0.3
			Single moth. y. 5	58	21.9	50–90	-	6.9**	2.4*
Baker et al. (2008)	CAN	Expansion childcare Quebec, parental fee from 50 to 20%	Women coupl. 0–4	53	-	94–02	29	7.7***	-
Lefebvre and Merrigan (2008)	CAN	Expansion childcare Quebec, parental fee from 50 to 20%	Mothers y. 1–5	61	19.8	93–02	29	8.1***	4.5***
Lundin et al. (2008)	SWE	Price cap childcare prices, 55% drop in parental fee	Mothers coupl. 1–9	70	-	{01,03}	21	–0.2	-
Berlinski and Galiani (2007)	ARG	Staggered increase pre-school	Mothers 3–5	39	12.5	92–00	-	7.4	-
'IV' studies									
Fitzpatrick (2012)	US	Eligibility for public school,	Married moth. y. 5	60	25.3	00	19	2.7	0.0
		using date of birth	Single moth. y. 5	68	32.9	00	19	12.2**	3.0
Fitzpatrick (2010)	US	Eligibility for pre-kindergarten	Mothers young. 4	70	25.6	00	19	0.5	0.1
		in Georgia and Oklahoma,	Married moth. y. 4	-	-	00	19	0.5	0.2
		using date of birth	Single moth. y. 4	-	-	00	19	0.2	0.0
Gelbach (2002)	US	Eligilibility for public school,	Married moth. y. 5	41	13.5	80	22	5.0***	1.5***
		using quarter of birth	Single moth. y. 5	52	17.9	80	22	5.1**	2.7***

[a]Column 4 indicates the treatment group, where the numbers (e.g. 0–11) indicate the age of the child, and youngest child is abbreviated to young. or y. Columns 5–10 give respectively (5) the pre-reform or counterfactual participation rate, (6) the pre-reform or counterfactual hours worked per week (including zeros for non-participants), (7) the sample period, (8) the share of part-time workers for all women or men in a country from the OECD in the year before the reform (DD studies) or the year before the year of the data (RD studies), (9) the treatment effect on the participation rate (in percentage points) and (10) the treatment effect on hours worked per week (in hours per week). The exact reference for the treatment effects for each study are given in the online appendix. We present the treatment effect estimates along with their level of significance, where * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. [b]Felfe et al. (2013) present IV estimates of the treatment effect, but the treatment effect does not measure the increase in the participation rate of the parents per increase in enrollment in out-of-school care, and is therefore not directly comparable to the IV estimates presented in the lower part of the table.

by 13.2 percentage points over the period 2004–2009 (the last year of the pre-reform period to the last year of the reform period for which we have data).[41] This suggests a 0.19 (0.025/0.132) percentage point increase in the participation rate of mothers with a youngest child 0–3 years of age per percentage point increase in the enrollment of children 0–3 years of age in formal childcare.[42] This is larger than the 0.06 calculated for Norway by Havnes and Mogstad (2011a), comparable to the 0.18 calculated for Spain by Nollenberger and Rodríguez-Planas (2015) but substantially smaller than the 0.55 which we can calculate for the reform in Quebec of Baker et al. (2008).[43]

One reason that can explain why we find smaller effects than Baker et al. (2008) for Quebec is that we consider data from a relatively recent period, where the pre-reform participation rate of mothers was already relatively high. As argued by e.g. Cascio (2009) and Fitzpatrick (2012), studies that use data from a later period are therefore more likely to find smaller effects, as childcare subsidies are then more likely to be inframarginal to the participation decision.[44] However, we should note that some studies (e.g. Goux and Maurin, 2010; Havnes and Mogstad, 2011a) have also expressed concerns about the studies on the reform in Quebec, where pre-reform trends seem to differ between the treatment province and the control provinces and there were also other reforms occuring during the same period as the childcare reform.[45]

A potential explanation for why the effect is larger than the effects reported by Lundin et al. (2008) and Havnes and Mogstad (2011a), is that both workers and non-workers are eligible for subsidized childcare in Sweden and Norway, whereas only working parents are

[41]This may include an increase in the enrollment rate due to other reasons than the reform. However, since there is no control group for the use of formal childcare (we consider a nationwide reform), it is hard to determine what the increase in enrollment would have been in the absence of the reform.

[42]A similar calculation for mothers with a youngest child 4–7 and 8–11 years of age is complicated by the fact that we only have information on the enrollment rate of children for both age groups combined. The enrollment rate of children in formal childcare in this age range increased by 9.6 percentage points over the period 2004–2009. Applying this increase to both groups we obtain a 0.29 (0.028/0.096) and 0.16 (0.015/0.096) percentage point increase in the participation rate of mothers with a youngest child 4–7 and 8–11 years of age, respectively, per percentage point increase in the enrollment of their children in formal childcare. However, since the enrollment rate of children 4–7 years of age is typically larger than the enrollment rate of children 8-11 years of age, and hence probably also the increase in enrollment rate in percentage points, the number is more likely to be smaller than 0.29 for mothers with a youngest child 4–7 years of age and more likely to be larger than 0.16 for mothers with a youngest child 8-11 years of age. Unfortunately, we do not have information on the enrollment rate of formal childcare for the subgroups of single mothers and mothers in couples, so we can not do the calculation for these subgroups.

[43]Baker et al. (2008, pp.711-713) report an additional increase in the enrollment rate in formal childcare in Quebec of 14 percentage points relative to the rest of Canada. This suggests a 0.55 (0.077/0.14) percentage point increase in the participation rate of mothers per percentage point increase in the enrollment rate of children in formal childcare.

[44]Blau and Kahn (2007) and Heim (2007) show that labour supply elasticities of women in the US have fallen over time as their participation rate has increased. Cross-country support for the hypothesis that labour supply elasticities are lower when participation rates are higher can be found in Bargain et al. (2014).

[45]Indeed, when comparing the treatment effects with placebo reforms, Baker et al. (2008, p.731) note that "[B]y this method, the increase in care in Quebec is far outside anything seen in other provinces, but the increase in mothers' employment lacks significance."

eligible for subsidized childcare in the Netherlands.[46] Indeed, only working single parents and two-earner couples qualify for formal childcare subsidies in the Netherlands. This may also explain why the effect for women in couples is bigger than the effect found by e.g. Goux and Maurin (2010) and the effects of the US studies (Gelbach, 2002; Fitzpatrick, 2010, 2012), noting that for the US studies we should compare the treatment effects with e.g. our back-of-the-envelope calculation of the 'IV' treatment effect (multiplied by 100) of 19 percentage points for mothers with a youngest child 0–3 years of age. These studies all focus on pre-school reforms, the (implicit) subsidy therefore does not have a work requirement on the part of the parents.

Turning to the effect on hours worked per week, again our results take an intermediate position. However, the effect we find for hours worked per week is relatively large when compared to the effect on the participation rate.[47] This may be related to the large share of women that work part-time in the Netherlands. Indeed, despite the relatively high participation rate, the hours worked per week per person in the treatment group (including the zeros for non-participants) is still rather low. This leaves a lot of room for the intensive margin to respond.

In line with the other studies (e.g. Cascio, 2009; Goux and Maurin, 2010; Fitzpatrick, 2012) we find that the treatment effect is larger for single mothers than for women in couples.[48]

Finally, there is one other study that also reports effects on fathers. Using differences in enforcement of out-of-school care in neighbouring cantons in Switzerland, Felfe et al. (2013) find that whereas the share of full-time working women is higher when there is more out-of-school care, the share of full-time working men is lower. This is in line with our finding for fathers, they seem to have reduced their working hours in response to the Dutch reform. However, note that the effect is only statistically significant for men with a youngest child 0–3 years age.

7 Conclusion

Many countries seek to increase formal labour force participation of mothers. Policymakers often point to Scandinavia, where public spending on childcare is high and the maternal employment rate is high as well. However, our analysis of a large recent reform in the Netherlands, which cut the parental fee for formal childcare in half, suggests that such a correlation can not necessarily be interpreted as a causal relation. We conclude that the large policy reform in the Netherlands increased participation of women with young children by a modest 2.3 percentage point or 3.0%. The hours worked effect is

[46] Working parents and individuals actively looking for work or enrolled in active labour market policies.

[47] Again, we can do a back-of-the-envelope calculation of the effect on hours worked per week per percentage point increase in the enrollment rate of children in formal childcare. For mothers with a youngest child 0–3, 4–7 and 8–11 years of age this yields respectively 10.6 (1.4/0.132), 12.5 (1.2/0.096) and 4.2 (0.4/0.096). This is on average somewhat higher than Nollenberger and Rodríguez-Planas (2015) who calculate an effect of 7.53 hours per week per percentage point increase in the enrollment rate of children in childcare.

[48] According to Meghir and Phillips (2010) it is a stylized fact of empirical labour economics that single parents are relatively responsive to financial incentives.

larger though, an increase of 1.1 hours per week or 6.2%. This is partly counteracted by a decrease in average hours worked by men of –0.3 per week, although the estimate for men is not significantly different from zero. Recall that all these effects should be interpreted as joint effects, as the government also increased EITCs for parents with young children over the same period.

Our findings are quantitatively in between the findings of recent studies for Sweden (Lundin et al., 2008) and Norway (Havnes and Mogstad, 2011a) that find very small effects, and studies for Canada (Baker et al., 2008; Lefebvre and Merrigan, 2008) and some other countries that find substantial effects. We believe that our results are particularly relevant for many other highly developed OECD countries that face quite similar starting conditions in terms of the maternal employment rate and public spending on childcare. We have also shown that it is important to look beyond participation and also look at hours worked per week.

In this paper we use the Dutch reform to study the relation between childcare subsidies and labour force participation. However, the reform could also be used to investigate a number of other relevant questions. Indeed, Baker et al. (2008) argue that a full evaluation of publicly financed childcare requires answers to three questions, which we take up below.

First, how does public financing affect the quality and quantity of formal childcare, and to what extent does it lead to substitution of informal childcare? This requires microdata on the price and use of formal and informal childcare over time. One of the side effects of the policy reform is that since 2005 we have potentially good microdata on the use of formal childcare, since all subsidies now run via the Tax Office. However, finding reliable informal childcare data remains a challenge.

Second, how do childcare subsidies affect labour force participation and what is the net cost to the government? We have answered the first part of this question. For the second part one needs to link the labour force participation data to the childcare data, and to link these data to a tax-benefit calculator to determine the effects on government receipts and expenditures.[49] We do not have the data to do this exercise. However, the expansion seems to have been rather costly. Between 2005 and 2009 expenditures on childcare subsidies and EITCs for parents with young children increased by 2.6 billion euro in total. This seems a rather large amount given that the increase in participation in persons and in fulltime equivalents was about 30 thousand.[50] Even after controlling for some trend growth in these expenditures, the additional public expenditure per additional working person or per additional working fulltime equivalent seems rather large.

Third, what is the effect of expanding formal childcare on children and families? There are a number of papers that use the same reforms used in the analysis of labour force participation to study the effects on children and families (see *e.g.* Loeb et al., 2007; Baker et al., 2008; Havnes and Mogstad, 2011b). For the moment no such study exists for the Netherlands. However, a number of recent studies suggest that this might be an important element to consider in the Dutch reform. Vermeer et al. (2005) and Kruif

[49]Despite the substantial rise in female participation found in Baker et al. (2008) they still calculate the net effect on government finances to be negative, in part due to substantial substitution of informal care by formal care.

[50]Part of the increase in hours worked by mothers is counteracted by the drop in hours worked by fathers.

et al. (2009) use a large number of internationally comparable indicators for the quality of daycare, and find a disturbing trend.[51] On a scale from 1 (bad) to 7 (excellent), their sample scored on average 4.8 in 1995, 4.3 in 2001, 3.2 in 2005 and a meager 2.8 in 2008. Furthermore, in 2008, 49% of daycare centres got a rating 'insufficient' and 51% got a rating of 'poor', while none of the 200 daycare centres got a rating of 'good'. Hence, it seems important to study how the policy reform affected children and families, and how participation in formal childcare affects children and families in general.

We are also interested in how these effects may differ in the short- and long-run. In particular, we have used data up to 2009. Since the major changes in the parental fee took place in 2006 and 2007, we consider our results medium-run effects. It would be interesting to study what happened after 2009. Faced with the dramatic rise in public expenditures on formal childcare, the current government has substantially decreased subsidies for formal childcare. Indeed, by 2015 the average parental fee will rise to 34% (Ministry of Social Affairs and Employment, 2011). However, this will provide us with an interesting new natural experiment, to study *e.g.* whether the response of parents is symmetric for decreases and increases in the parental fee.

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[51]Specifically, they use the ITERS-R (Infant/Toddler Environment Rating Scale - Revised) for 0-2.5 year olds, and the ECERS-R (Early Childhood Environment Rating Scale - Revised) for 2.5 to 5 year olds.

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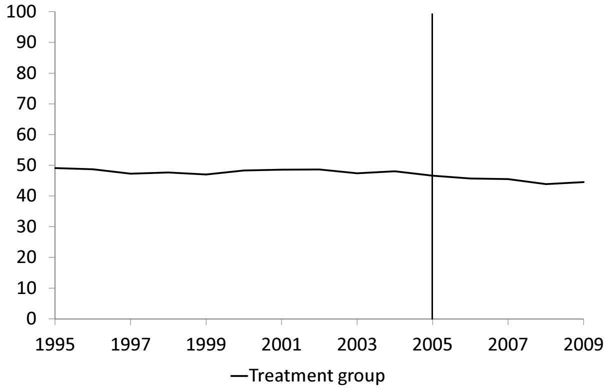
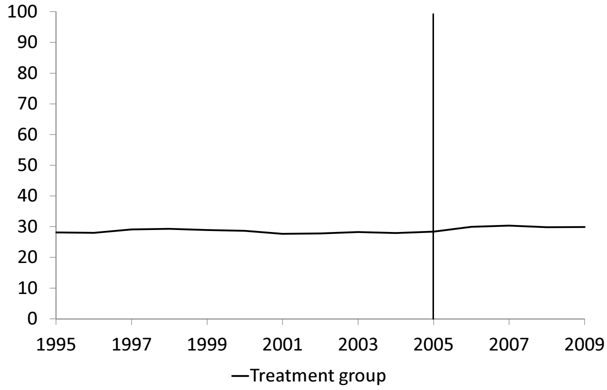
Online appendix

Figure A.1: Averages of the covariates per year by treatment and control groups

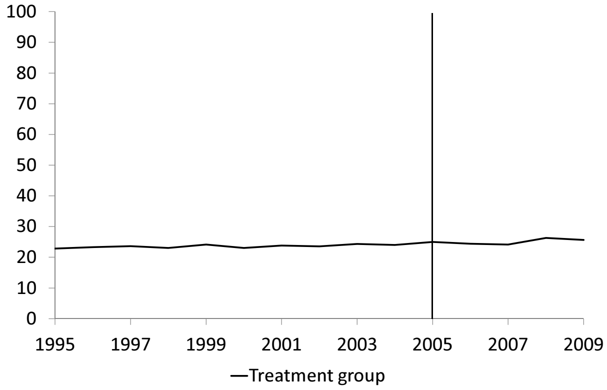
(a) Child 0–3



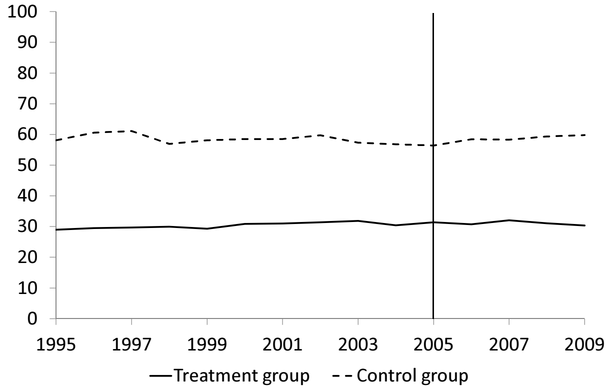
(b) Child 4–7



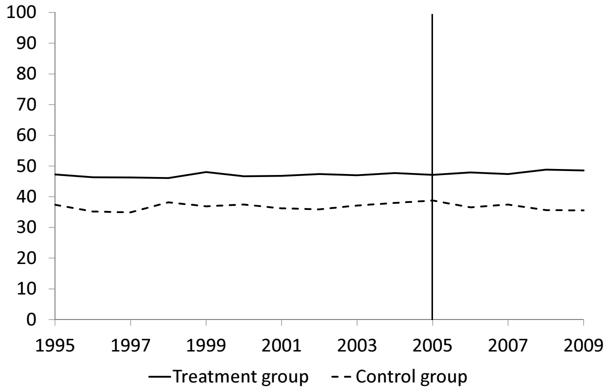
(c) Child 8–11



(d) 1 child under 18



(e) 2 children under 18



(f) more than 2 children under 18

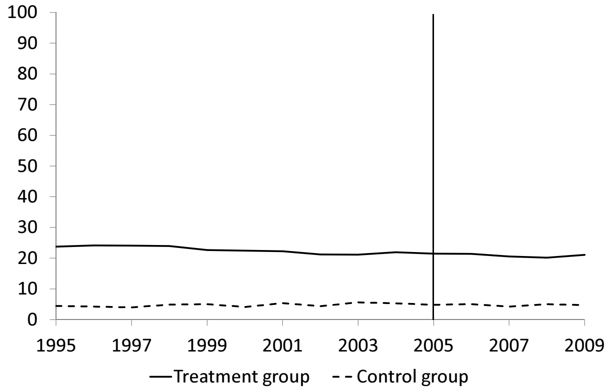
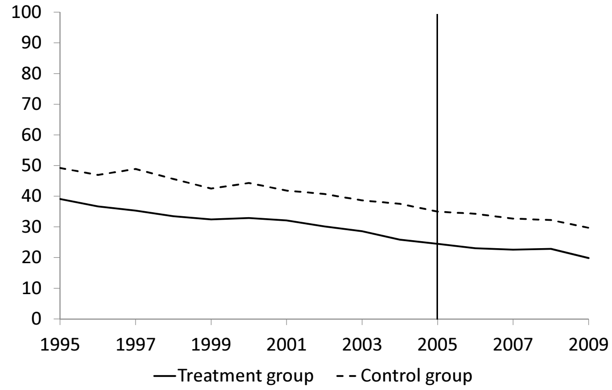
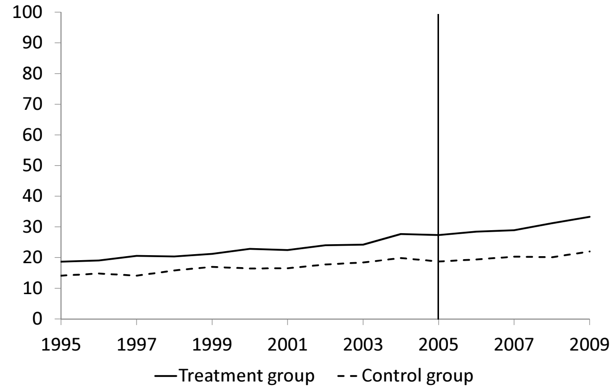


Figure A.1: Continued

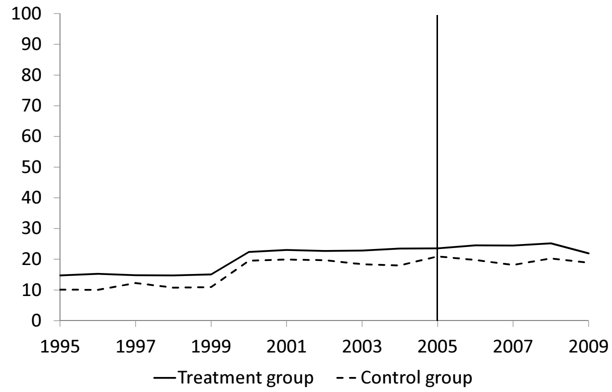
(g) Lower educated



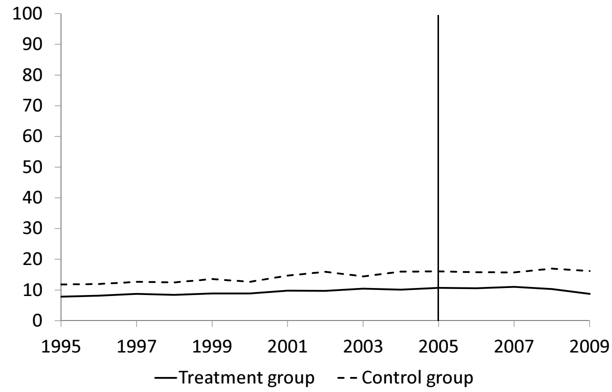
(h) Higher educated



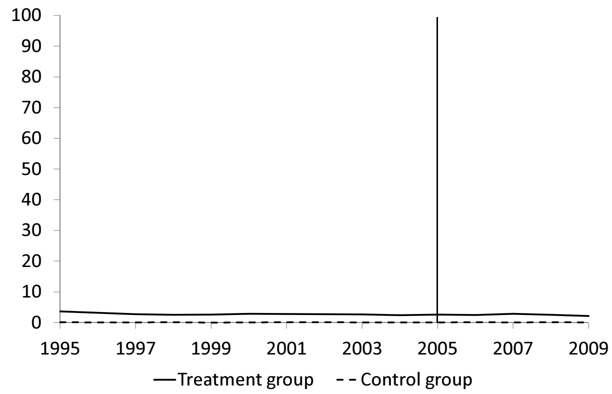
(i) Immigrant



(j) Single



(k) Age 20–25



(l) Age 25–30

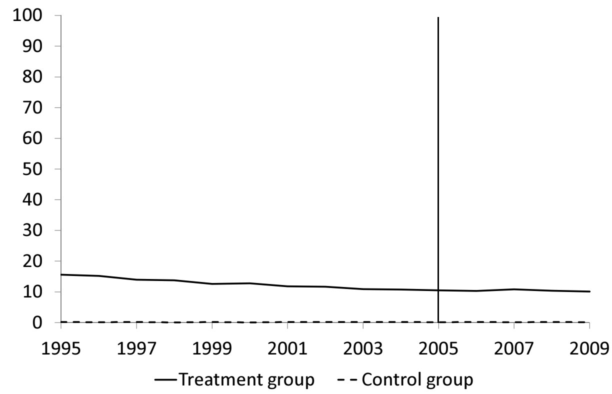
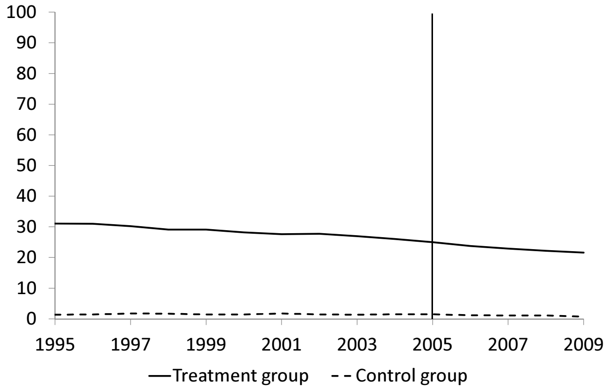
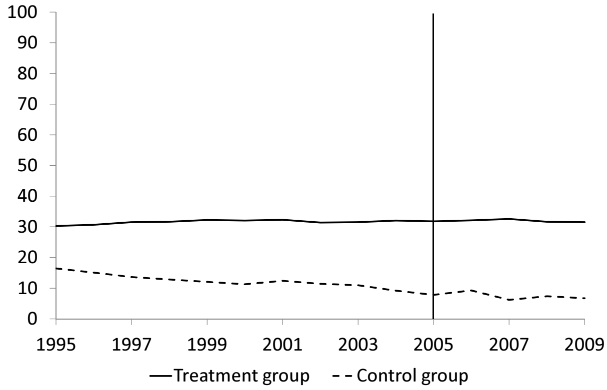


Figure A.1: Continued

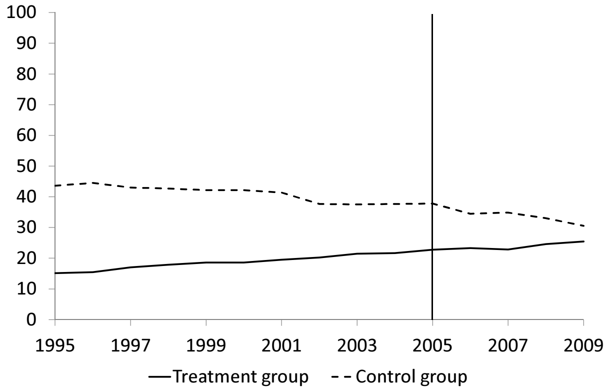
(m) Age 30–35



(n) Age 35–40



(o) Age 40–45



(p) Age 45–50

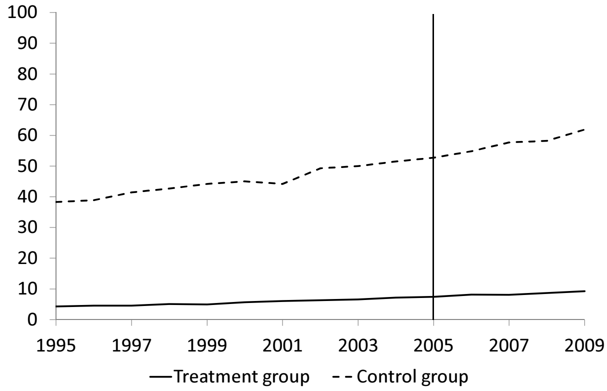
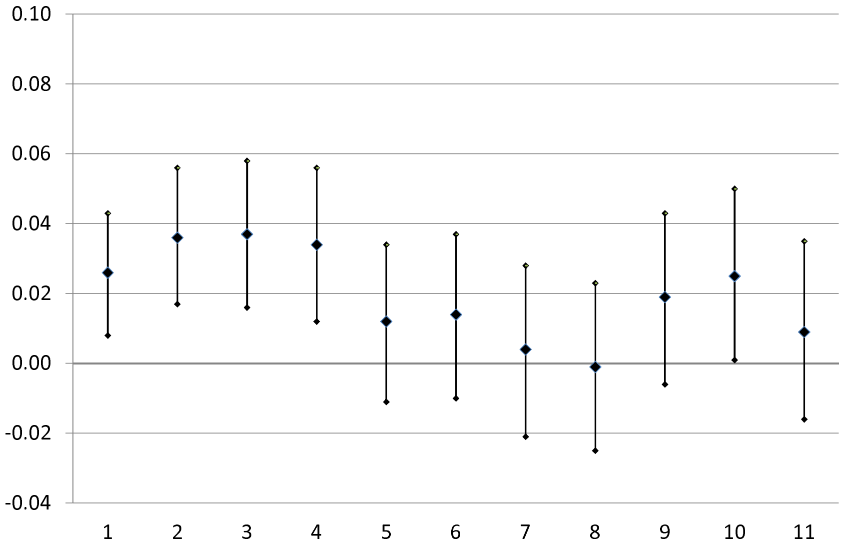
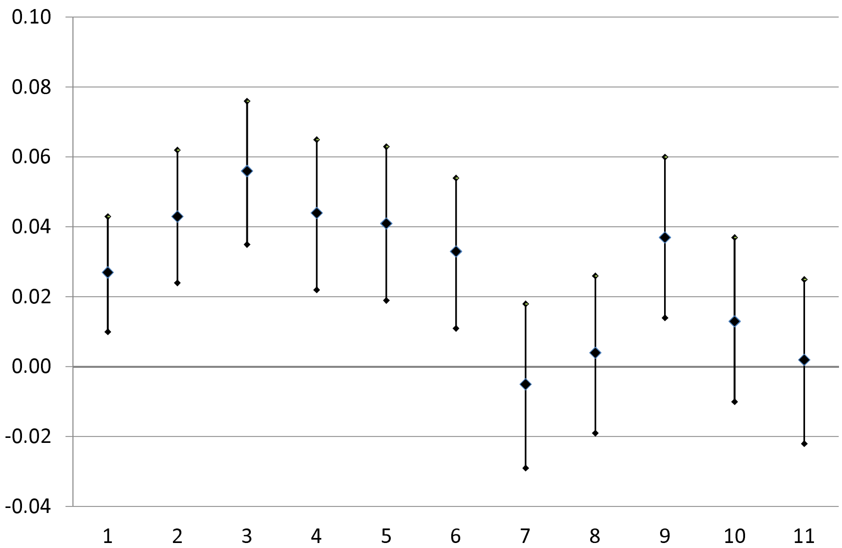


Figure A.2: Treatment effect 2005–2007 on part. of women, by age of the youngest child



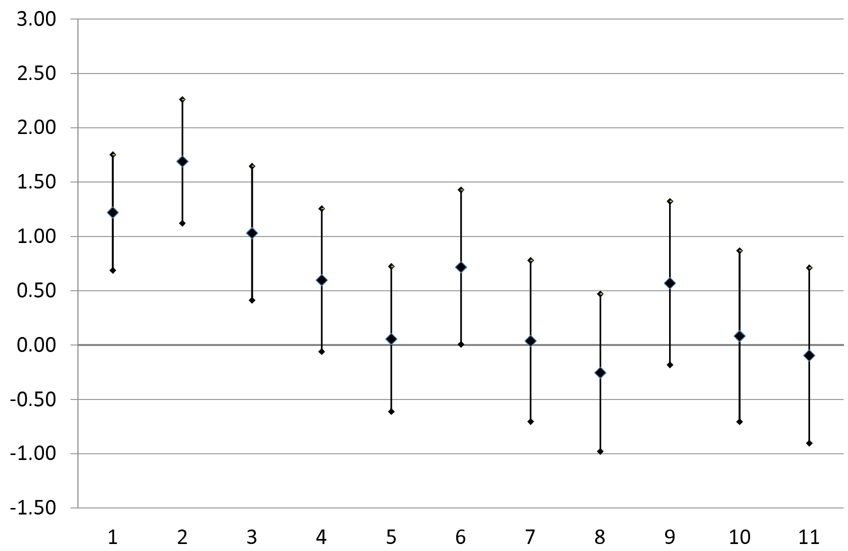
Estimates result from regressions identical to column (2) in Table 3, with the treatment effect interacted with the age of the youngest child. Lines indicate 95% confidence intervals of the coefficients.

Figure A.3: Treatment effect 2008–2009 on part. of women, by age of the youngest child



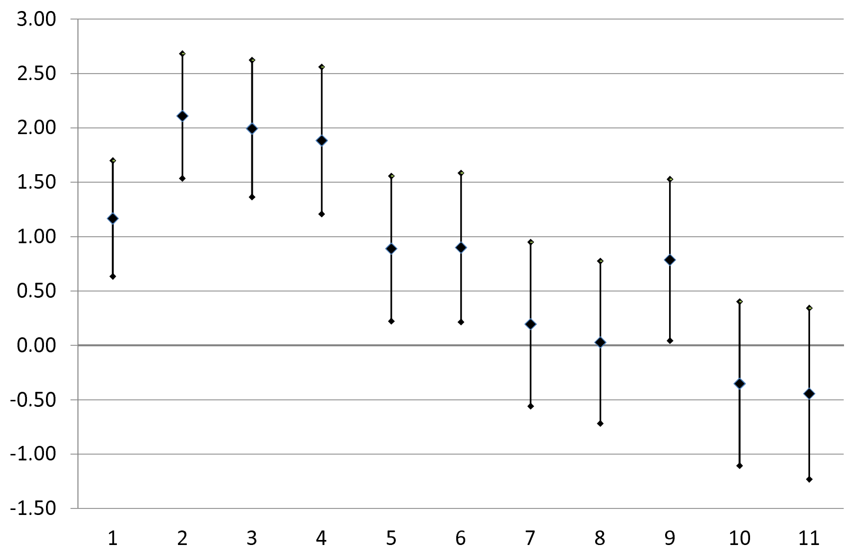
Estimates result from regressions identical to column (2) in Table 3, with the treatment effect interacted with the age of the youngest child. Lines indicate 95% confidence intervals of the coefficients.

Figure A.4: Treatment effect 2005–2007 on hours women, by age of the youngest child



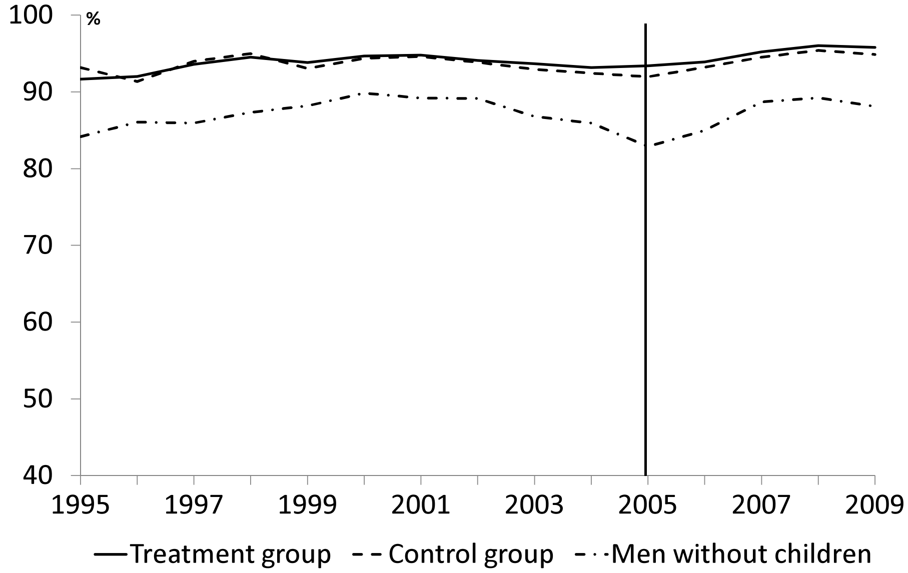
Estimates result from regressions identical to column (2) in Table 3, with the treatment effect interacted with the age of the youngest child. Lines indicate 95% confidence intervals of the coefficients.

Figure A.5: Treatment effect 2008–2009 on hours women, by age of the youngest child



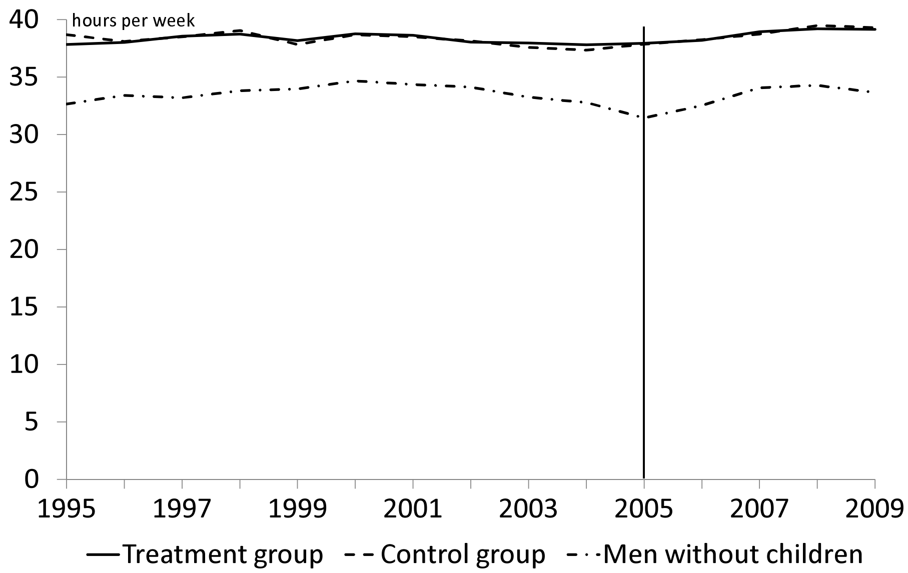
Estimates result from regressions identical to column (2) in Table 3, with the treatment effect interacted with the age of the youngest child. Lines indicate 95% confidence intervals of the coefficients.

Figure A.6: Participation rate men



Source: Labour Force Survey (Statistics Netherlands).

Figure A.7: Hours worked per week men



Source: Labour Force Survey (Statistics Netherlands).

Table A.1: Effect on participation rate of women

	(1) WLS	(2) WLS
Treatment 2005	0.011	
	(0.009)	
Treatment 2006	0.020**	
	(0.010)	
Treatment 2007	0.013	
	(0.010)	
Treatment 2008	0.021**	
	(0.009)	
Treatment 2009	0.026***	
	(0.005)	
Treatment 05–07		0.015**
		(0.006)
Treatment 08–09		0.023***
		(0.006)
Child 0–3	–0.090***	–0.090***
	(0.005)	(0.005)
Child 4–7	–0.069***	–0.069***
	(0.004)	(0.004)
Child 8–11	–0.027***	–0.027***
	(0.004)	(0.004)
1 child under 18	0.140***	0.140***
	(0.003)	(0.003)
2 children under 18	0.093***	0.093***
	(0.003)	(0.003)
Lower educated	–0.179***	–0.179***
	(0.003)	(0.003)
Higher educated	0.110***	0.110***
	(0.002)	(0.002)
Immigrant	–0.153***	–0.153***
	(0.003)	(0.003)
Single	–0.093***	–0.093***
	(0.004)	(0.004)
Age 20–25	0.107***	0.107***
	(0.009)	(0.009)
Age 25–30	0.008	0.008
	(0.006)	(0.006)
Age 30–35	0.066***	0.066***
	(0.005)	(0.005)
Age 35–40	0.075***	0.075***
	(0.004)	(0.004)
Age 40–45	0.056***	0.056***
	(0.003)	(0.003)
Observations	263,229	263,229
P-value test equal treatment effects	0.573	0.233

Robust standard errors in parentheses, * denotes significant at 10% level, ** at 5% level and *** at 1% level. Year fixed effects are included but not reported.

Table A.2: Effect on participation rate: subgroups of women with placebo treatment (WLS)

	(1) Single women	(2) Women in couples	(3) Youngest child 0–3	(4) Youngest child 4–7	(5) Youngest child 8–11
Placebo 00–02	0.010	–0.001	–0.013	0.017*	0.005
	(0.023)	(0.008)	(0.008)	(0.009)	(0.010)
Placebo 03–04	–0.050*	–0.004	–0.021**	–0.001	–0.001
	(0.024)	(0.008)	(0.009)	(0.010)	(0.010)
Treat 05–07	0.021	0.010	0.010	0.020**	0.015
	(0.021)	(0.007)	(0.008)	(0.009)	(0.009)
Treat 08–09	0.038*	0.018***	0.018**	0.034***	0.019**
	(0.020)	(0.007)	(0.007)	(0.008)	(0.009)
Observations	26,453	236,776	155,163	119,429	110,887

Robust standard errors in parentheses, * denotes significant at 10% level, ** at 5% level and *** at 1% level. Individual characteristics, group fixed effects and year fixed effects are included but not reported.

Table A.3: Effect on hours worked per week: subgroups of women with placebo treatment (WLS)

	(1) Single women	(2) Women in couples	(3) Youngest child 0–3	(4) Youngest child 4–7	(5) Youngest child 8–11
Placebo 00–02	0.798	–0.566**	–0.905***	0.008	0.019
	(0.861)	(0.273)	(0.284)	(0.317)	(0.342)
Placebo 03–04	–1.537*	–0.219	–0.721**	–0.203	–0.062
	(0.887)	(0.281)	(0.295)	(0.330)	(0.351)
Treat 05–07	0.966	0.289	0.486*	0.423	0.300
	(0.802)	(0.256)	(0.270)	(0.298)	(0.318)
Treat 08–09	1.555**	0.699***	0.894***	1.177***	0.399
	(0.790)	(0.253)	(0.268)	(0.296)	(0.315)
Observations	23,945	207,152	135,545	105,170	98,004

Robust standard errors in parentheses, * denotes significant at 10% level, ** at 5% level and *** at 1% level. Individual characteristics, group fixed effects and year fixed effects are included but not reported.

Table A.4: Regression of covariates on treatment dummies

Dependent variable	(1) Lower educated	(2) Higher educated	(3) Immigrant	(4) Single	(5) Aged 20–25	(6) Aged 25–30
Treat 05–07	0.002	0.033***	0.006	–0.007	–0.001	–0.02***
	(0.007)	(0.006)	(0.006)	(0.005)	(0.001)	(0.002)
Treat 08–09	0.011	0.058***	0.001	–0.028***	–0.003***	–0.02***
	(0.007)	(0.006)	(0.006)	(0.005)	(0.001)	(0.002)
Observations	263,229	263,229	263,229	263,229	263,229	263,229
Dependent variable	(7) Aged 30–35	(8) Aged 35–40	(9) Aged 40–45	(10) One child	(11) Two children	
Treat 05–07	–0.04***	0.051***	0.094***	0.023***	–0.006	
	(0.003)	(0.005)	(0.007)	(0.007)	(0.007)	
Treat 08–09	–0.052***	0.052***	0.147***	0.001	0.024***	
	(0.003)	(0.005)	(0.006)	(0.007)	(0.007)	
Observations	263,229	263,229	263,229	263,229	263,229	

Robust standard errors in parentheses, * denotes significant at 10% level, ** at 5% level and *** at 1% level. Group fixed effects and year fixed effects are included but not reported.

Table A.5: Alternative standard errors and weighting: participation women

	(1)	(2)	(3)	(4)	(5)	(6)
Weighted	Y	Y	Y	N	N	N
Std errors	Robust	Clustered group-year[a]	Clustered group[b]	Robust	Clustered group-year[a]	Clustered group[b]
Treat 05–07	0.015**	0.015**	0.015***	0.013**	0.013***	0.013***
	(0.006)	(0.004)	(0.002)	(0.005)	(0.004)	(0.002)
Treat 08–09	0.023***	0.023***	0.023***	0.021***	0.021***	0.021**
	(0.006)	(0.005)	(0.003)	(0.004)	(0.004)	(0.004)
Observations	263,229	263,229	263,229	263,229	263,229	263,229

Standard errors in parentheses, * denotes significant at 10% level, ** at 5% level and *** at 1% level. Individual characteristics, group fixed effects and year fixed effects are included but not reported. [a]Standard errors clustered on the group-year level (64 clusters: 4 groups (youngest child 0-3, 4-7, 8-11, 12-17) and 16 years). [b]Standard errors clustered on the group level (4 clusters (youngest child 0-3, 4-7, 8-11, 12-17)).

Table A.6: Alternative standard errors and weighting: hours worked women

	(1)	(2)	(3)	(4)	(5)	(6)
Weighted	Y	Y	Y	N	N	N
Std errors	Robust	Clustered group-year[a]	Clustered group[b]	Robust	Clustered group-year[a]	Clustered group[b]
Treat 05–07	0.661***	0.661***	0.661*	0.424**	0.424***	0.424*
	(0.198)	(0.174)	(0.209)	(0.174)	(0.144)	(0.148)
Treat 08–09	1.075***	1.075**	1.075***	0.921***	0.921***	0.921**
	(0.194)	(0.204)	(0.261)	(0.145)	(0.176)	(0.234)
Observations	263,229	263,229	263,229	263,229	263,229	263,229

Standard errors in parentheses, * denotes significant at 10% level, ** at 5% level and *** at 1% level. Individual characteristics, group fixed effects and year fixed effects are included but not reported. [a]Standard errors clustered on the group-year level (64 clusters: 4 groups (youngest child 0-3, 4-7, 8-11, 12-17) and 16 years). [b]Standard errors clustered on the group level (4 clusters (youngest child 0-3, 4-7, 8-11, 12-17)).

Table A.7: Alternative placebo specifications: all women (WLS)

	(1)	(2)
	Participation	Hours worked
Placebo 95–99	0.011	0.150
	(0.008)	(0.265)
Placebo 00–02	0.010	–0.087
	(0.009)	(0.260)
Treat 05–07	0.023***	0.763***
	(0.008)	(0.243)
Treat 08–09	0.032***	1.150***
	(0.008)	(0.239)
Observations	263,229	231,097

Robust standard errors in parentheses, * denotes significant at 10% level, ** at 5% level and *** at 1% level. Individual characteristics, group fixed effects and year fixed effects are included but not reported.

Table A.8: Effect on participation rate: all men

	(1)	(2)	(3)
	WLS	WLS	WLS
	95–09	95–09	95–09
		No overlap[a]	With placebo
Placebo 00–02			0.003
			(0.004)
Placebo 03–04			0.005
			(0.006)
Treat 05–07	0.005	0.013	0.007
	(0.004)	(0.010)	(0.005)
Treat 08–09	0.003	–0.012*	0.005
	(0.004)	(0.007)	(0.004)
Observations	224,674	191,846	224,674

Robust standard errors in parentheses, * denotes significant at 10% level, ** at 5% level and *** at 1% level. Individual characteristics, group fixed effects and year fixed effects are included but not reported. [a]The control group consists of parents with a youngest child aged 16–17, so we exclude individuals in the control group that were previously in the treatment group.

Table A.9: Effect on participation rate: subgroups of men (WLS)

	(1)	(2)	(3)	(4)	(5)
	Single Men	Men in couples	Youngest child 0–3	Youngest child 4–7	Youngest child 8–11
Treat 05–07	–0.030	0.008*	0.003	0.003	0.011*
	(0.043)	(0.004)	(0.005)	(0.005)	(0.005)
Treat 08–09	–0.008	0.004	0.004	0.002	0.002
	(0.038)	(0.004)	(0.004)	(0.004)	(0.005)
Observations	3,247	221,427	133,423	95,605	85,900

Robust standard errors in parentheses, * denotes significant at 10% level, ** at 5% level and *** at 1% level. Individual characteristics, group fixed effects and year fixed effects are included but not reported.

Table A.10: Effect on hours worked per week: all men

	(1) WLS 97–09	(2) WLS 97–09 No overlap[a]	(3) WLS 97–09 Placebo
Placebo 00–02			0.093
			(0.267)
Placebo 03–04			0.400
			(0.302)
Treat 05–07	–0.108	0.084	0.024
	(0.238)	(0.482)	(0.281)
Treat 08–09	–0.344	–0.474	–0.211
	(0.216)	(0.363)	(0.262)
Observations	195,879	167,350	195,879

Robust standard errors in parentheses, * denotes significant at 10% level, ** at 5% level and *** at 1% level. Individual characteristics, group fixed effects and year fixed effects are included but not reported. [a]The control group consists of parents with a youngest child aged 16–17, so we exclude individuals in the control group that were previously in the treatment group.

Table A.11: Effect on hours worked per week: subgroups of men (WLS)

	(1) Single Men	(2) Men in couples	(3) Youngest child 0–3	(4) Youngest child 4–7	(5) Youngest child 8–11
Treat 05–07	0.312	–0.060	–0.341	0.006	0.278
	(1.930)	(0.239)	(0.260)	(0.286)	(0.301)
Treat 08–09	–0.056	–0.345	–0.514**	–0.231	–0.111
	(1.774)	(0.215)	(0.237)	(0.263)	(0.279)
Observations	2,973	192,906	115,693	83,370	75,048

Robust standard errors in parentheses, * denotes significant at 10% level, ** at 5% level and *** at 1% level. Individual characteristics, group fixed effects and year fixed effects are included but not reported.

Table A.12: Exact references treatment effects related studies in Table 8

Study	Treatment group	Treatment effect participation rate	Treatment effect hours worked per week
Nollenberger and Rodríguez-Planas (2014)	Mothers young. 3	p. 32, Table 2, row 'DDD', column (1)	p. 32, Table 2, row 'DDD', column (1)
Felfe et al. (2013)	Mothers 0–12	p. 18, Table 2, row 'Employment', column (3)	–
	Fathers 0–12	p. 18, Table 2, row 'Employment', column (3)	–
Havnes and Mogstad (2011a)	Mothers moth. y. 3–6	p. 1461, Table 2, row 'Employment', column (1)	–
Schlosser (2011)	Arab mothers 2–4	p. 41, Table 4, row 'A. mean effect in post-law, period', column (3)	p. 42, Table 5, row 'A. mean effect in post-law period', column (3)
Goux and Maurin (2010)	Mothers coupl. 3	p. 956, Table 1a, row 'Year of birth=1996', column 'Mother's participation'	–
	Single moth. 3	p. 957, Table 1b, row 'Year of birth=1996', column 'Mother's participation'	–
Cascio (2009)	Married moth. y. 5	p. 153, Table 4a, row 'Worked last week', column (5)	p. 153, Table 4a, row 'Hours last week', column (5)
	Single moth. y. 5	p. 153, Table 4a, row 'Worked last week', column (5)	p. 153, Table 4a, row 'Hours last week', column (5)
Baker et al. (2008)	Women coupl. 0–4	p. 724, Table 2, row 'Mother works', column 'ELIG dummy'	–
Lefebvre and Merrigan (2008)	Mother y. 1–5	p. 540, Table 5, row 'Participation', column 'β_{2002}'	p. 540, Table 5, row 'Annual hours worked', column 'β_{2002}'
Lundin et al. (2008)	Mothers coupl. 1–9	p. 657, Table 2, row 'Employment', column (5)	–
Berlinski and Galiani (2007)	Mothers 3–5	p. 677, Table 6, row 'New Stock', column (2)	–
Fitzpatrick (2012)	Married moth. y. 5	p. 593, Table 4, row 'Current employment', column 1	p. 593, Table 4, row 'Usual hours per week in prior year', column 1
	Single moth. y. 5	p. 592, Table 3, row 'Current employment', column 1	p. 592, Table 3, row 'Usual hours per week in prior year', column 1
Fitzpatrick (2010)	Mothers young. 4	p. 72, Table 5, row 'Worked last year', column 'Coefficient'	p. 72, Table 5, row 'Hours per week last year', column 'Coefficient'
	Married moth. y. 4	p. 74, Table 6, row 'Worked last year', column 'With no younger children'	p. 74, Table 6, row 'Hours per week last year', column 'With no younger children'
	Single moth. y. 4	p. 74, Table 6, row 'Worked last year', column 'With no younger children'	p. 74, Table 6, row 'Hours per week last year', column 'With no younger children'
Gelbach (2002)	Married moth. y. 5	p. 315, Table 9, row 'Current employment', column (3)	p. 315, Table 9, row 'Hours last week', column (3)
	Single moth. y. 5	p. 315, Table 7, row 'Current employment', column (3)	p. 315, Table 7, row 'Hours last week', column (3)