After-school Care and Maternal Labor Supply:

Evidence from Norway

Tevje Dolve Hetlelid

Master Thesis

Master Thesis handed in 1st of June 2019

Master's Degree in Economics

University of Bergen, Department of Economics

[June 2019]

UNIVERSITETET I BERGEN

Foreword

On a bright August day in 2013, I came to Nygårdshøyden and entered the Institute of Economics. Numbers and figures and formulas organize the world around us, and I came to Bergen to make sense of it all:

The Greek unemployment rate had just reached a record high 27.8 percent; the Norwegian government was planning to propose a national budget based on a structural, non-oil deficit of NOK 135 billions; and the French professor Thomas Piketty was about to publish his assessment of the dynamics behind wealth distribution by presenting the inequality $r > g$.

I wished to learn how to decipher the mighty calculations that oversee and dictate policy, and how this knowledge should best be put to use.

My supervisor and professor Kjell Vaage has assisted me in my efforts and guided me through my final work, which has become exactly what I had wanted to achieve, and taught me so much. Thank you.

And thank you, Madeleine. Everything – anything – feels better and gets more exciting when sharing it with you.

To Gry, Narve, Olaug and Tuva: a warm hug.

Berlin, 1st of June 2019

Tevje Dolve Hetlelid

Abstract

In their pursuit to boost female workforce participation, governments have introduced family policy measures with a two-fold goal: to stimulate more mothers into the labor market, and for more working mothers to work full-time. In this master thesis, I show that publically facilitated after-school care in Norway has met only the first target.

I use observations from a Norwegian reform from 1997, which gave a large group of mothers in 44 municipalities sudden accessibility to after-school care. Within a difference-in-differences research design, I separately examine the extensive and intensive margin of maternal employment. I find positive and statistically significant effects on mothers' likelihood of entering the labor market. Simultaneously, however, the reform does not stimulate part-time working mothers to increase their labor supply.

All empirical results have been computed using the statistical software Stata. [1]

[1] (En del av) de data som er benyttet i denne publikasjonen er hentet fra kommunedatabasen til NSD — Norsk senter for forskningsdata AS. NSD er ikke ansvarlig for analyse av dataene eller for de tolkninger som er gjort her.

List of Tables

List of Figures

Index

1 Introduction

"If the female employment rate in Norway was reduced to the average level in the OECD, the value of the production loss would equal Norway's total oil wealth."

Jens Stoltenberg (Aftenposten, 2012, own translation)

Because labor is considered the most important resource in the Norwegian economy (St.Meld. 29 2016-2017, p. 151), a generally high employment rate is a public policy objective (NOU 2012: 15, p. 144). As Mr. Stoltenberg, the former prime minister of Norway, illustrated, the female labor supply represents a huge value to society. In addition to its vast economic potential, encouraging inclusion of women in the workforce also serves the purpose of gender equality: every citizen, regardless of their sex, should have equal rights and equal access to opportunities.

A high female employment rate is often attributed to strong family policies; cash-benefits or in-kind services aimed at facilitating the everyday life of families enable parents, and especially mothers, to balance family life with a career. Publically provided after-school care for the youngest schoolchildren is one such policy instrument, and it is recommended by the OECD, who urge governments to "step up investments in out-of-school-hours care services, which can help parents with school-aged children participate in paid work full-time" (OECD, 2017, p. 213).

In this master thesis, I analyze the effects of after-school childcare on maternal employment by evaluating a Norwegian care program introduced in the 1990s, partly intended at bringing mothers into the workforce.

In Figure 1.1, I compare the female employment rate in Norway (black) to the average female employment rate in the OECD (gray) from 1990 to 2015. The Norwegian rate lies above the OECD average during the whole period, even increasing the initial gap of 9 percentage points in 1990 to 16 percentage points in 2015. A noticeable characteristic of the consistently higher overall employment rate is the correlation with a high share of part-time employed women (dashed lines).

During the period, however, as Norway's female employment rose and then stabilized around 70 percent, the share of part-time employed women simultaneously decreased from 25 percent in 1990 to 19 percent in 2015, while the OECD-share rose from 12 to 13 percent.

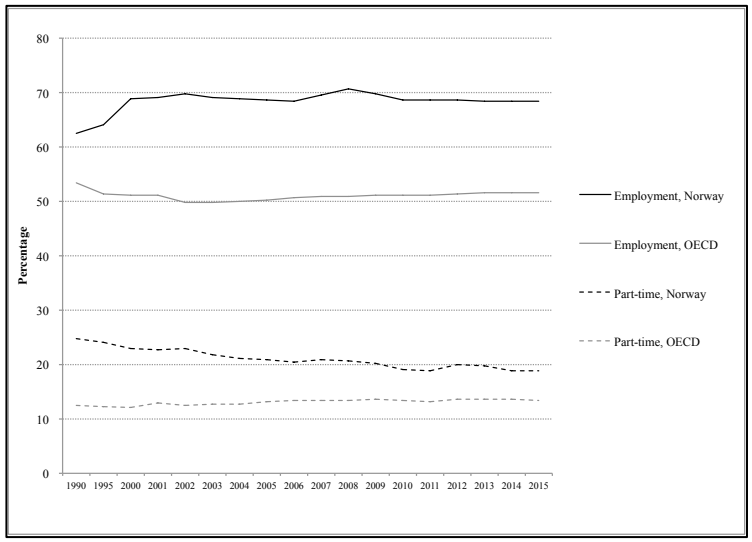


Figure 1.1. Female employment rates: Norway and the OECD, 1990 to 2015. Source: OECD.

In the same timeframe as observed in Figure 1.1, the Norwegian after-school program *Skolefritidsordningen* (SFO) developed. This public childcare scheme, directed at schoolchildren less than 10 years of age, was introduced gradually at a local level from 1990 onwards.

In a report by the Norwegian Ministry of Children and Equality from 2007, the authors recognize the rise in the female employment rate during the same period and suggest after-school care as a positive contributor (Barne- og likestillingsdepartementet, p. 17). Another government white paper from 2008 reports that mothers whose youngest child was between 7 and 10 years old on average increased their weekly working hours by 4.2 hours from 1991 to 2005, while mothers to children between 11 and 15 years of age increased their work load by only 1.7 hours per week.

This thesis will investigate the aforementioned correlations in seeking to answer the following research question:

What is the effect of after-school care on maternal labor supply? [1]

The paper is structured in the following manner: in Chapter 2, I summarize the existing research on parental labor supply and school-level childcare arrangements. In Chapter 3, I make up the background of the analysis by describing the Norwegian after-school program and briefly outline the labor market trends of the most relevant policy-targets, mothers of school-aged children. In Chapter 4, I put forward the empirical strategy. In Chapter 5, I describe and assess the available data, before presenting the estimation results in Chapter 6. In Chapter 7, I perform specification checks and in Chapter 8, I give my concluding remarks.

2 Literature Review

To my knowledge, no one has investigated research question [1] with Norwegian data prior to this thesis. Only one study has evaluated Norway's after-school program (Kvalle and Wendelborg, 2002), but the aspect of parental employment was excluded from the review. In fact, the lack of systemized knowledge has led Norwegian parliamentarians to request a white paper on the after-school program from the government (Innst. 69 S, 2016–2017). There are, however, studies of similar programs from other countries. I present a selection of these articles in Section 2.1. Further, in Section 2.2, I review two articles for methodological reasons. Both articles have empirically analyzed kindergarten expansion and maternal labor supply in Norway. At last, in Section 2.3, I summarize the effects of after-school care found on mothers' workforce participation.

Following the literature, I distinguish between two different measures of change in employment, which I will continue to use in the rest of the text. Employment at the extensive margin considers any type of employment in contrast to unemployment. By using the measure of employment level at the extensive margin, an estimated model examines whether more individuals enter the labor market. Employment at the intensive margin, on the other hand, is based on much time employed individuals spend in the labor market. With this employment definition, the estimated models report whether the average working individual reduces or increases her labor supply.

2.1 Evidence from other countries

According to Blau and Currie (2006), there are three causes for public interference in the childcare market: (i) equity considerations, (ii) parental labor supply stimulation and (iii) market failures (p. 1196). Regarding the second cause, the authors suggest two motives. Firstly, childcare subsidies cost the taxpayer less than having parents on a welfare program, as government-run childcare can stimulate employment, thus increasing the tax base in the economy. Secondly, the employment of low-income earning mothers can have positive externalities (p. 1197). Blau and Currie's survey reports positive effects of childcare access on the employment of parents with very young children, but finds no effect of after-school care for school-aged children. The article summarizes the economic research on childcare and labor supply as characterized by weak research designs, small sample sizes and lack of measurements of childcare quality (p. 1264).

Berthelon et al. (2015) study after-school care and mothers' labor force participation by examining a Chilean school reform from 1996, which extended the school day for the country's primary school pupils. The extension of the school day is considered an equivalent to after-school care. The Chilean reform was announced in 1996, but was administered at a local level, resulting in a gradual implementation of full-day school spreading geographically.

Berthelon et al. look at the period from 2004 to 2009, during which the national share of full-day schools increased from 34.7 percent to 55.4 percent. The maternal labor supply effect of full-day school is estimated only at the extensive margin, implying that the authors examine whether after-school care increases mothers' workforce participation. Berthelon et al. identify the causal effect of school day extension by using municipal share of full-day schools as a representation of policy access, and then estimate a fixed-effects model. The main specification of the model contains fixed individual, municipal and regional characteristics to control for unobserved factors: holding time-invariant factors fixed, the mothers' labor market response to a change in childcare access is the outcome of reform.

Berthelon et al. report a statistically significant, positive effect of 11.9 percentage points in response to a municipality reaching complete full-day school coverage, finding strong indications of after-school care encouraging mothers to enter the labor market.

In her working paper, Nemitz (2015) estimates a bivariate probit model using data from a German reform dated 2003. The reform involved the federal government allocating resources to the different states in order for the states to facilitate all-day school. Through information from questionnaires, Nemitz identifies a sample of mothers making use of the voluntary after-school care scheme and mothers who do not. This serves to compare these two samples of mothers, defining the first one as a treatment group and the second one as a control group.

The geographical variation in after-school care availability allows Nemitz to estimate an average treatment effect of the reform. Nemitz employs the number of schools at county-level receiving reform funds from the federal government as an instrument for the policy. The average treatment effect in her estimation is the average probability of employment post-reform among mothers putting their child in after-school care subtracted the corresponding probability of mothers not making use of the scheme. Nemitz finds a 26 percentage points higher probability of employment for treated mothers relative to non-treated mothers, but no effect on full-time employment.

Considering the same German 2003-reform, but approaching with an empirical matching strategy, Gambaro et al. (2018) find positive effects of after-school care both on mothers' employment at the extensive margin as well as at the intensive margin. Gambaro et al. observe mothers at two stages: in the year before their child enters primary school and in the year after. The policy treatment is defined as making use of the after-school program during that school year between the observations. The treatment effect is the change in employment of treated mothers relative to the change in employment of non-treated mothers, before and after the child starts in first grade.

In order to make the two groups more statistically similar, thereby more comparable, Gambaro et al. apply nonparametric entropy balancing, which weighs all the included variables of the control group so that every variable's mean and variance is equal to the equivalent variable in the treatment group. The authors have highly detailed individual-level information, and exploit this by using a wide set of control variables, which in most studies are unobserved, e.g. the individual's desired working hours. Gambaro et al. find that after-school care had a positive effect of 11.4 percentage points at the extensive margin. They also report a positive effect at the intensive margin, as treated mothers increase their weekly working hours by 2.6 hours.

In Felfe et al. (2016) the authors emphasize the endogeneity of after-school care implementation, as policy provision might have been driven by local preferences. If after-school care access originates from endogenously made decisions, it will be difficult to identify the scheme's true effects on maternal labor supply, because of unobserved underlying trends. Felfe et al. specifically mention the difference-in-differences method as vulnerable to this endogeneity concern, if the research design is not cautiously organized (p. 67).

Felfe et al. restrict their sample to populations living in what they have classified as homogenous economic areas in Switzerland, which are divided by cantonal borders. Because after-school care facilitation is decided at a cantonal level and the sample population is categorized as equal in terms of preferences, the set up is more likely to detect causal effects. After confirming the cantonal variations in after-school care provision, Felfe et al. apply the 2SLS strategy, using the number of after-school centers per 100 children as the instrument for cantonal enforcement of policy. In their main model, the authors find no effect at the extensive margin of maternal employment, but a 0.9 percentage point increase at the intensive margin. Even though the estimate holds a p-value of 10.4, implying an only nearly significant

coefficient, Felfe et al. conclude that one more after-school center allows one more mother to work full-time (p. 72).

2.2 Methodological approaches

The methodological approach by Havnes and Mogstad (2011) resembles the analytical framework of my own study. In their article, Havnes and Mogstad use the difference-in-differences method to investigate the effects of Norwegian kindergarten coverage expansion in the 1970s, centered around a 1975 reform, on mothers' employment. The authors construct a treatment group and a control group based on the pre-reform municipal coverage in childcare for 3- to 6-year-olds. They list all municipalities from the lowest coverage to the highest and define the treatment group as mothers living in municipalities with a coverage rate lower than the median rate, while the remaining mothers constitute the control group.

Within the difference-in-differences research design, the causal effect is estimated as an average treatment effect, which distinguishes between the treatment group and the control group, and between the post-reform period and the pre-reform period. Estimating their model, Havnes and Mogstad find practically no effect of kindergarten expansion reform on maternal labor supply; universal kindergarten access did not stimulate mothers' workforce participation, nor did it encourage full-time employment. The authors suspect that publically organized childcare may rather have crowded out other informal arrangements (p. 1464).

Havnes and Mogstad also emphasize that the difference-in-differences method only identifies the immediate labor market response to reform, and that there might be substantial long run effects of women adjusting their human capital investments to the now accessible childcare.

Revisiting the research question of Havnes and Mogstad (2011), Andresen and Havnes (2018) conduct a more sophisticated analysis, applying the 2SLS method on data from a Norwegian 2002-kindergarten, focusing on mothers to children aged 1 year or 2 years old. In contrast to Havnes and Mogstad (2011), the authors do not identify only married mothers, but also cohabiting mothers, cohabiting fathers, as well as single mothers and non-resident fathers.

Using the number of available childcare slots in the municipality as their instrument variable, Andresen and Havnes find significant positive effects of childcare on maternal employment, simultaneously as no effects on paternal employment. For cohabiting mothers, the effect is a 29.4 percentage points' increase, where almost every one of these going into full-time

employment. In the case of single mothers, there is an employment increase of 22.5 percentage points, with practically all of them entering part-time employment. The considerable positive effects-results of Andresen and Havnes (2018) contrast the findings of Havnes and Mogstad (2011), and demonstrate how different methods and different data availability might reach different findings.

2.3 Summary

In Table 2.1, I summarize the empirical findings of the literature review. With the exception of Felfe et al. (2016), all reviewed articles find that after-school care schemes encourage mothers' labor market participation. Berthelon et al. (2015) and Gambaro et al. (2018) both find estimates around 12 percentage points increase in maternal employment at the extensive margin, while Nemitz (2015) reports a 26 percentage points higher probability of employment.

The effects of policy at the intensive margin of maternal employment, considering the findings of Gambaro et al. (2018) and Felfe et al. (2016) in particular, are also positive. The consistently positive effects found in the literature suggest that similar childcare arrangements can expect similar results.

Havnes and Mogstad (2011), however, who apply a difference-in-differences strategy on Norwegian data, find no effect of childcare on maternal labor supply.

Table 2.1 *Summary of literature review results*

Article	Research design	Observations	Estimation method	Main results	
				Ext.margin	Int.margin
Berthelon et al. (2015)	Chile: Full-day school reform and mothers' employment. Reform from 1996.	6 453	Fixed effects-model	+ 11,9 pp.[1]	---
Nemitz (2015)	Germany: Full-day school and mothers' employment. Reform from 2002.	5 010 / 3 047 [2]	Bivariate probit-model	+ 26 pp.[3]	No effect.
Gambaro et al. (2018)	Germany: Full-day school *and* after-school care, mothers' employment.	1 711 / 2 543 [4]	Matching technique	+ 11,4 pp.	+ 2,6 hours[5]
xFelfe et al. (2016)	Switzerland: After-school care. Cantonal differences, but homogenous economic areas.	4 412	2SLS	No effect.	+ 0.9 pp.
Havnes and Mogstad (2011)	Norway: Kindergarten expansion and mothers' employment. Reform from 1972.	252 699	Difference-in-difference	No effect.	No effect.
Andresen and Havnes (2018)	Norway: Kindergarten reform and parental employment. Reform from 2002.	283 868 / 33 288 [6]	2SLS	+29 pp. / +23 pp.[7]	+22 pp./---

[1] By reaching full coverage.
[2] First sample consists of mothers
[3] Probability of being employed.
[4] First sample consiscts of mothers not employed pre-treatment, second sample consists of mothers already employed.
[5] Hours per week.
[6] Cohabiting mothers first, single mothers second.
[7] Cohabiting mothers first, single mothers second.

3 Institutional Setting

3.1 History of the after-school program

Skolefritidsordningen (SFO) is a municipal-level administered childcare scheme for schoolchildren between 6 and 9 years of age, which in the Norwegian school system corresponds to the 1st to the 4th grade of primary school (Utdanningsdirektoratet, 2015). The after-school program provides supervision from the end of the children's school day until the end of the parents' workday. Attending the program is voluntary, and a child can either participate on a full-time basis (20 hours per week) or on a part-time basis (10 hours per week). Local authorities are responsible for financing the scheme, and make widespread use of parental payments. I hence refer to publically after-school care *availability*, in contrast to public after-school care *provision*.

The total nationwide participation in the program from 1974 to 2017 is illustrated in Figure 3.1. The black line represents the total number of schoolchildren in the relevant age group, in other words, the youngest pupils in primary school. In the old primary school structure, lasting from 1974 to 1996, this group consisted of children aged 7, 8 or 9 years old. The 1997 school reform, marked by the vertical red line, warranted the entrance of 6-year-olds in primary school. From that year on, the black line also includes all children aged 6. The sharp decline in cohorts from 1974 to 1984 can be attributed to a decrease in the Norwegian birth rate from 2.75 in the late 1960s to 1.75 a decade later, signaling a change in family structures (Statistics Norway).

The origins of the after-school program tells of a decentralized policy: a service that appeared at different times in different localities, with the first *fritidshjem* (leisure time center) established in Oslo in 1952, the demands of working mothers (NOU 1979: 29, p. 16). In 1974, as Figure 3.1 shows, after-school care was practically non-existing; only 250 children had access to organized supervision after the short schoolday was over. Around the same time, the Norwegian government recognized the general lack of childcare as the main obstacle facing women wanting to enter the labor market. The government, therefore, set out a new family policy, shifting the focus over to developing universally accessible services as an alternative to pure cash benefits (St. Meld. 51 1973-1974).

Despite this new service-oriented approach, after-school care was not the target of any nationwide policy until 1990, when the different kinds of local after-school care schemes

were gathered under the term "*skolefritidsordning*" or *SFO* and the central government began allocating funds to build facilities for the service.

Figure 3.1 shows the rise in participants from 8 900 in 1989 to 49 000 only five years later. The efforts by the then ruling government to develop the after-school program were at the core of their new family policy; gender equality was a supreme priority and public childcare was sharply in focus (St. Meld. 70 1991-1992). National provision of the after-school program was considered an important child-, school- and gender equality reform (p. 26).

In 1993, the government announced an upcoming school reform, aimed at modernizing education. The expansion of the after-school program was to be a significant component of the reorganization of the primary school. It was argued that it would have positive effects not only for the children, but also for the parents (St. Meld. 40 1992-1993). The reform, named *Reform97,* was supposed to better reflect the structures of modern society, prepare the children for a modernized economy and build down educational differences originated in social inequalities (p. 7). The after-school program was, in 1992, already present in 342 of Norway's 435 municipalities - a promising indication to the feasibility of the reform (p. 47).

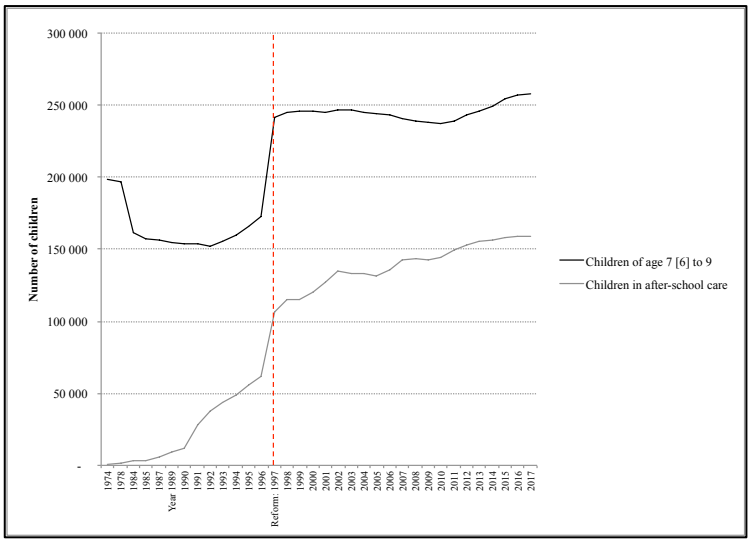


Figure 3.1. After-school care participation in total numbers, 1974 to 2017. Data sources: St. meld. nr. 51.(1973-1974); NOU 1979: 20; NOU 1984: 20; Statistics Norway; Utdanningsdirektoratet.

In 1997, as 6-year-olds entered the 1st grade of primary school, the total number of children eligible for the after-school program grew from 170 000 to 240 000 in a year (the black line in

Figure 3.1). In the same year, the after-school program was nationalized by legislation: every municipality was to facilitate the service. Meanwhile, the number of participants grew from 60 000 to 105 000 children (the gray line in Figure 3.1). Since 1997, the number of children aged 6 to 9 has been stable around the 250 000 mark, while the number of after-school program attendees has steadily grown from the aforementioned 105 000 to 160 000 in 2017.

The rise in national after-school program participation observed in Figure 3.1 can be illustrated by a coverage rate. Figure 3.2 graphs the steep elevation in the share of eligible schoolchildren taking part in the program, during the first half of the 1990s. A government white paper outlining the after-school program push of *Reform97* reports that 88 percent of the country's municipalities offered after-school care in 1996, but that the coverage rate varied geographically (St. Meld. 55 (1996-1997), p. 2).

The after-school program coverage experienced a sharp increase from 36 percent to 44 percent in the year 1997. The development of the after-school program, however, was most rapid in the early 1990s. The figure, looking at coverage rate and not total participation numbers, indicate that participation did not only rise due to belated municipalities introducing it in 1997, but steadily rose among the municipalities that had already provided the scheme prior to 1997. Since the reform, coverage has continued to rise, eventually stabilizing around 60 percent in 2017.

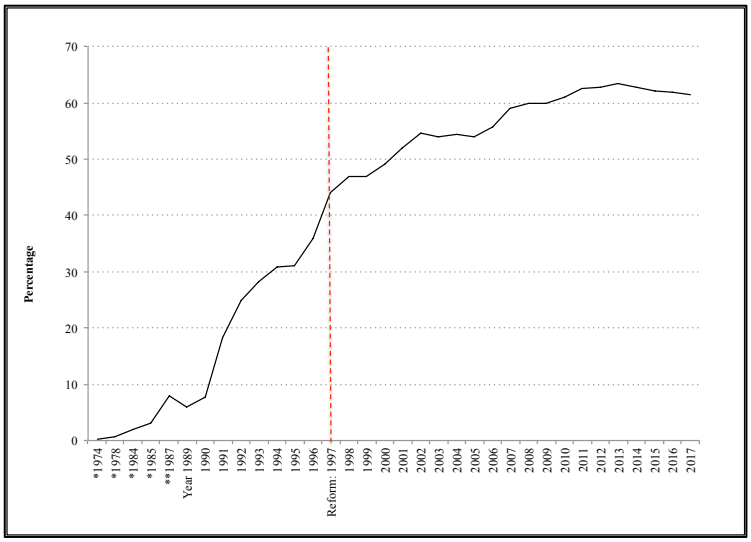


Figure 3.2. After-school program coverage, 1974 to 2017.
Data sources: St. meld. nr. 51.(1973-1974); NOU 1979: 20; NOU 1984: 20; Statistics Norway; Utdanningsdirektoratet.

After-school program participation is, furthermore, contingent upon the child's age. The youngest schoolchildren, aged 6 and 7, are the most frequent users of after-school care. Figure 3.3 graphs the development of the coverage rate by children's age around the time of the reform. Children aged 6 did not attend primary school before 1997. Therefore, in the years 1995 and 1996, I have graphed the share of 6-year-olds being full-time in an educational program specifically designed for 6-year-olds (Statistics Norway, 1996). Participation growth is not limited to 6-year-olds, meaning their entry into primary school is not the sole driving force of the collective growth. The positive evolution of other age cohorts from 1996 to 1997 shows a wider tendency to use after-school care to a larger degree after *Reform97*.

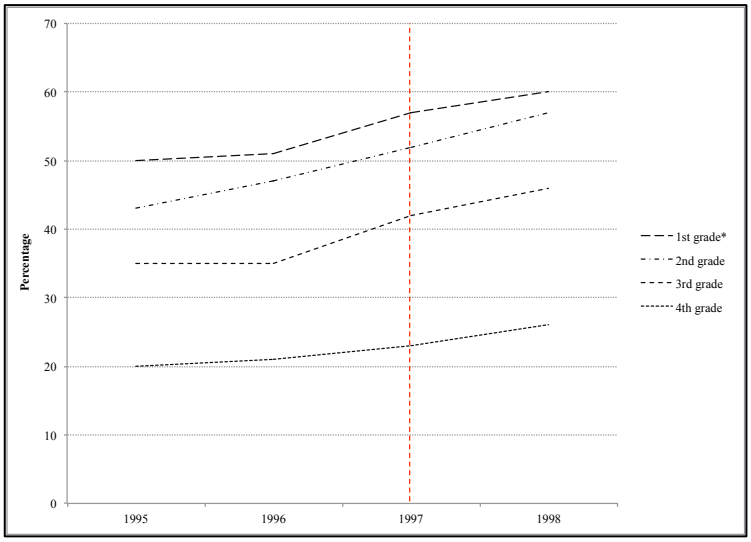


Figure 3.3. Share of children in 1st to 4th grade in after-school care or equivalent schemes, 1995-1998.

Data source: Statistics Norway.

3.2 Female employment in Norway

Norway's economy is characterized by a high female employment rate. In 2015, 68 percent of women between 16 and 65 years old participated in the labor market. Only Iceland (80 percent) and Sweden (70 percent) had larger shares of women employed (OECD, 2019).

Figure 3.4 compares the employment rates of Norwegian men and women between 15 and 75 years old, respectively, in the time period between 1986 and 2010. The figure – where male employment is black and female employment is gray – shows employment for both genders

follow similar short-term trend lines. However, on the longer term, from 1985 to 2010, the share of employed men decreased from 76 percent to 71 percent, whereas the share of employed women increased from 58 percent to 68 percent, narrowing the gender gap by 15 percentage points.

The simultaneous rise in both employment rates around the reform year 1997, and in particular the rise in male employment, indicates an overall boom in the Norwegian economy at the time. This suggests that other forces than family policy exclusively, can be driving factor behind growing female workforce participation. However, a childcare scheme like the after-school program might actually enable the relevant mothers to respond to these underlying economic changes and take part in the labor market, so the scheme becomes, through this mechanism, a contributing factor to the increase.

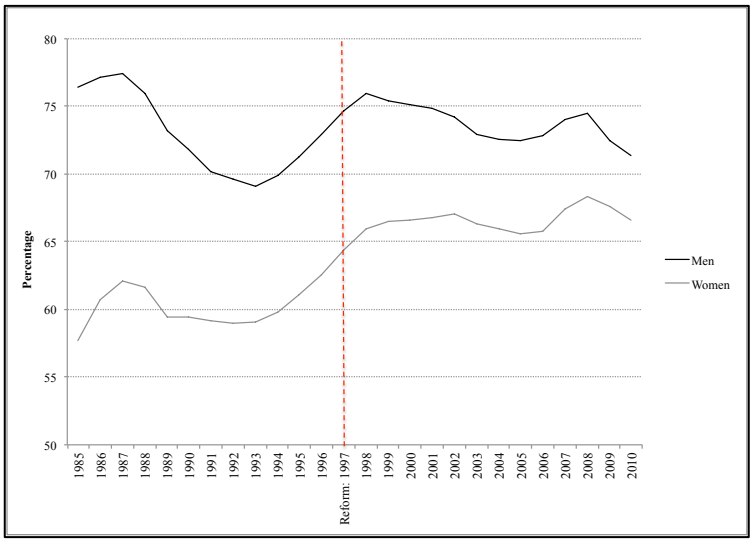


Figure 3.4. Employment rates in Norway, 1985 to 2010.
Data source: Statistics Norway.

Breaking down the female employment rate by age group, employment trends can be narrowed down to the women most likely to be affected by the after-school care reform. Figure 3.5 displays the age group of mothers to 6-year-olds in 1997. Of all children born in 1991, 36 of them were born by mothers who in 1997 are between 30 and 34 years old. Two other age groups of mothers also stand out: 20 percent of mothers are 25 to 29 years old, and 29 percent of mothers are 35 to 39 years old. A smaller group of mothers, 11 percent, are 40

to 44 years old. Only very few mothers younger than 25 or older than 45 years old have children eligible for the after-school program.

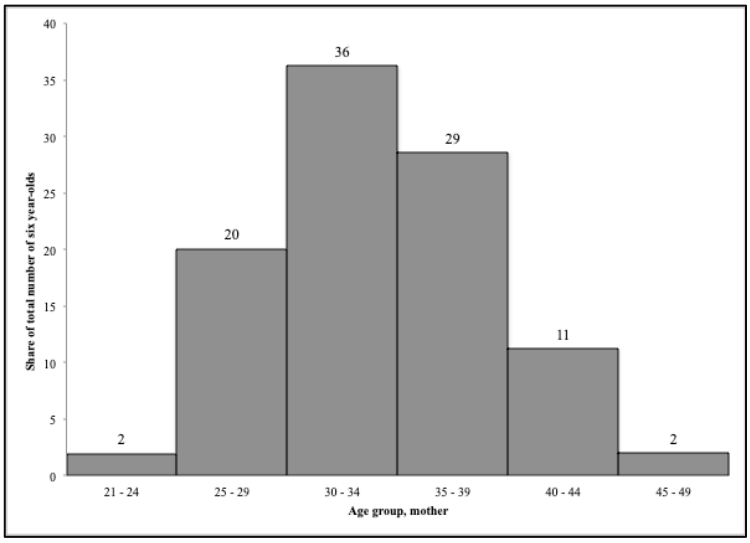


Figure 3.5. Proportional size of age group to mothers of all six-year-olds, year 1997. Data source: Statistics Norway.

Figure 3.6 highlights the employment trends of the defined age groups (solid lines), as well as the employment rates of other age groups (dashed lines) for all women, mothers of children all ages and non-mothers combined.

The 25 to 44 year olds are generally the most active on the labor market. If the reform has brought mothers to young schoolchildren to the labor market, mothers aged between 25 and 44 years should see a rise in employment relative to mothers in other age groups. Not all women are mothers of young children at any given time and these overall employment rates may thus be skewed. This notwithstanding, I expect the employment of women and mothers of each age group to follow similar trend lines, with changes for young children's mothers being reflected in the overall employment for women.

There seems to be a long-term upward trend among the age groups from 25 to 44 years old, as their employment rates in the 2000s stabilize at a higher level than in the late 1980s. Only the age group of 25- to 29-year-olds (the green line) sees a sudden upturn in employment after 1997. However, this group's trend was positive also before 1997.

Even though after-school care of children is thought to alleviate the burden on mothers, this cannot be concluded based on the overall employment trends observed in Figure 3.6. Publically organized childcare could instead replace private day-care services or informal arrangements, with nearby-living grandparents, for example. Alternatively, as the after-school program developed from 1990 onwards, the employment trends might reflect effects of the early efforts ahead of the 1997-reform. Again, no conclusion on this matter can be drawn based on Figure 3.6.

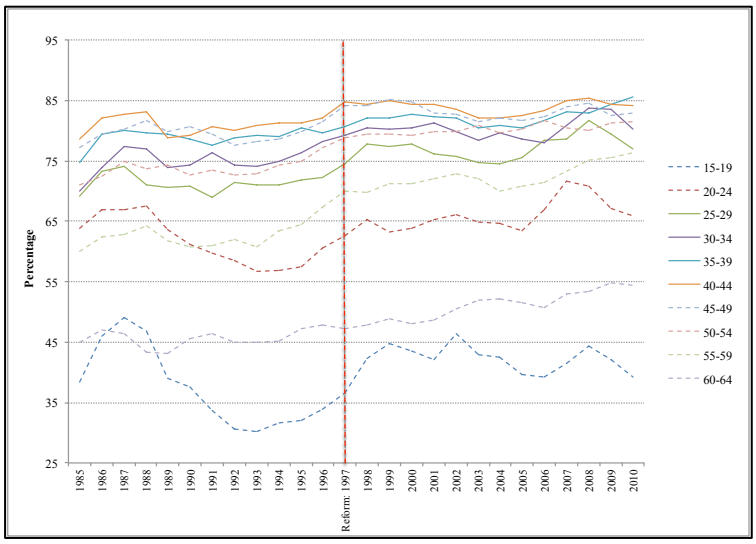


Figure 3.6. Female employment rates of different age groups, Norway, 1985 to 2010. Data source: Statistics Norway.

4 Empirical Strategy

I examine the research question [1] using a methodological framework similar to that of Havnes and Mogstad (2011). I consider the Norwegian school reform of 1997 a natural experiment and estimate the effect of the subsequent after-school care access on maternal labor supply with a difference-in-differences (DD) estimate.

Because it is the central government that imposed the policy - requiring municipalities to provide after-school care - I am able to isolate and study causal effects of sudden after-school care availability. With the DD method, I compare maternal employment outcomes of two groups of mothers over time. One group gains unique access to after-school care through *Reform97*, while the other group of mothers does not. Thus, I have a policy-receiving treatment group and an unaffected control group to compare it with.

The DD method reports an average treatment effect, which distinguishes the average post-reform outcome of the treatment group from the average post-reform outcome of the control group, and is described by [2] (Wooldridge, 2012, p. 369). The effect of a policy can be understood as the average change in post-reform employment outcome of the treatment group, denoted by *T*, subtracted by the equivalent change of the control group, denoted by *C*[2].

$$\hat{\delta}_{DD} = [\bar{y}_{T,POST} - \bar{y}_{T,PRE}] - [\bar{y}_{C,POST} - \bar{y}_{C,PRE}] \qquad [2]$$

A central feature of the difference-in-differences research design is that it does not require equal employment levels between the comparison groups prior to reform for the average treatment effect to be legitimate. The validity of the results rests upon satisfying the common trends assumption (Angrist and Pischke 2015, p. 179), which is described in Figure 4.1.

If the employment trends of the two groups prior to reform are similar, it is reasonable to assume that the future trends would continue to be similar in the counterfactual case of no reform. Correspondingly, the factual post-reform trend of the control group represents the hypothetical post-reform trend of the treatment group. The DD estimate of [2], marked by the

[2] The estimate can also be understood in the following way: $\hat{\delta}_{DD} = [\bar{y}_{T,POST} - \bar{y}_{T,PRE}] - [\bar{y}_{C,POST} - \bar{y}_{C,PRE}]$

red line[3], tells us how mothers acquiring sudden access to after-school care respond to policy in the labor market.

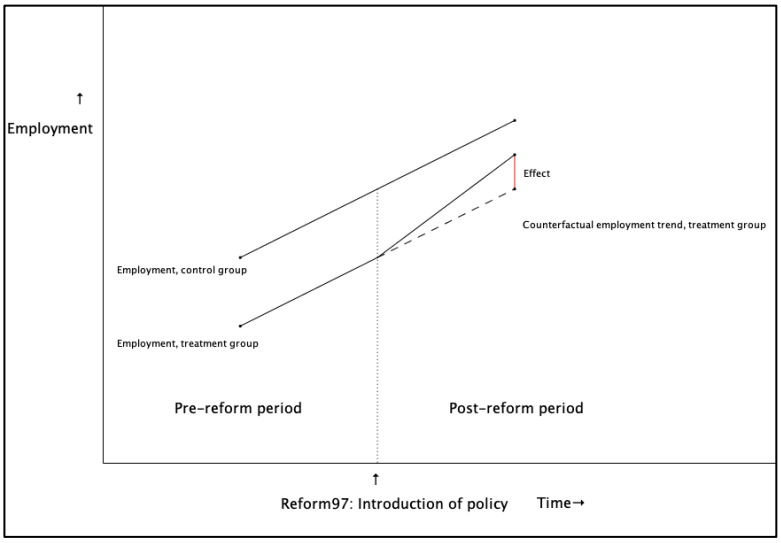


Figure 4.1. An illustration of the difference-in-difference method.
Source: own.

4.1 Identification of treatment

Two constraints have shaped my identification strategy. The first is access to data; I neither have data on individual use of after-school care, nor on municipal coverage. I only have knowledge of the municipalities' general after-school care offer. Statistics Norway provides employment data for the years 1996 to 2003 on mothers who gave birth between 1982 and 1995. The data observations thus start one year before the reform, and includes mothers of children eligible for the reform, and of children in adjacent age ranges - too young or too old.

The other constraint is the existence of local after-school care provision prior to *Reform97*. According to St. Meld. 55 (1996-1997, p. 2), 88 percent of Norwegian municipalities already organized public after-school by the fall of 1996. That leaves me to consider only individuals in the remaining 12 percent of municipalities, which *Reform97* had a direct impact on. By lack of a better term – because *Reform97* as a school reform did affect all municipalities – I will refer to these municipalities as *Reform97*-municipalities.

[3] Note that the estimated average treatment effect will not equal the value of the red line's graphical length, but rather report the average value of each yearly post-reform change combined.

A request was sent to 99 percent of Norwegian municipalities in early 2018 – all for which viable contact information was found – to provide the date and extent of the introduction of public after-school care. Table 4.1 presents an overview of the survey's metadata, while the responding local authorities' self-reported first year of offering after-school care is graphed in Figure 4.2. Of the 314 municipalities that confirmed receiving my inquiry, 107 gave an exact year of after-school care introduction, and thus qualified to be included in the analysis.

Table 4.1 *Overview of local authorities' response to after-school care inquiry*

Number of municipalities in total (2018)	422
Number of municipalities reached by e-mail	417
Number of municipalities confirmed receiving e-mail	314
Number of municipalities with response	141
Number of municipalities providing information	129
Number of municipalities included in analysis	107
Number of *Reform97*-municipalities	44
Percentage of all Norwegian municipalities included in analysis	**25.4**

Source: own.

Figure 4.2 strongly suggests a direct impact from *Reform97* on local after-school program provision: 36 municipalities implemented it in 1997, significantly more than in any other year. Eight, mostly small, municipalities did not implement the reform in 1997, but shortly thereafter, in 1998 or in 1999. I consider these 44 latecomer municipalities to have implemented the after-school care program as a direct consequence of Reform97. Only for these municipalities is the policy exogenously applied and the treatment randomized.

Knowing that 12 percent of municipalities had no after-school care prior to 1997 (St. Meld. 55, 1996-1997, p. 2), there were 50 or 51 *Reform97*-municipalities. The 44 identified *Reform97*-municipalities represent 10 percent of Norway's municipalities and 86 to 88 percent of the true total of *Reform97*-municipalities.

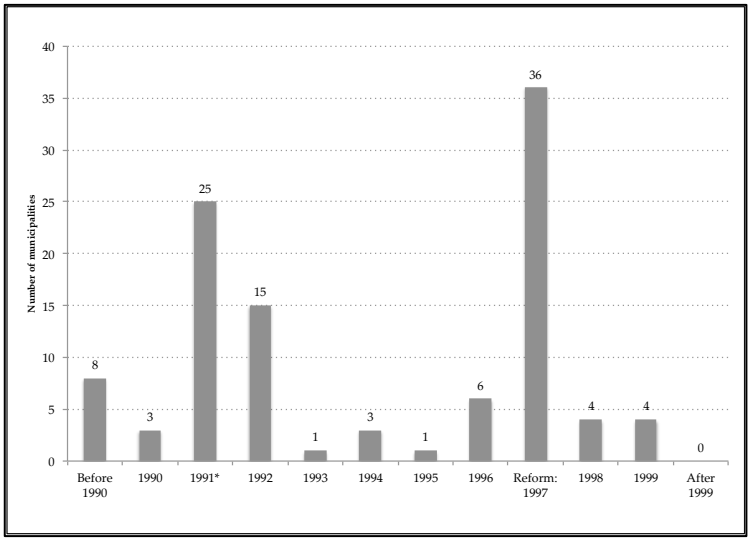


Figure 4.2. Introduction of after-school program over time. Number of municipalities per year (from group of included responders)[4]. Source: own.

A descriptive cross-section regression shows how the *Reform97*-municipalities compare to the 63 other existing municipalities. It uses *Reform97*-status as the outcome variable and a set of municipal characteristics, such as political preferences, demographic composition and the state of the economy, collected from the Regional Database at the Norwegian Social Research Centre as input variables.

The municipal share of conservative votes in the 1996 parliamentary election is used to proxy political preferences, i.e. the political affiliation and values of the municipal population, not the political realities of after-school care implementation. If the *Reform97*-municipalities tend to be more conservative it could be correlated with a preference for traditional gender roles, whereby women work less. Thus, a public demand for after-school care is weak, which might explain why local authorities have avoided after-school care introduction.

The demographic input variable is the share of women aged 50 or more, because these potential grandmothers may represent an alternative care arrangement, and hence, a smaller need for public after-school care. The economic input variables are the unemployment rate for men and for women, respectively, and the average income level, each per municipality in the year 1997.

[4] Municipalities responding with ”early 1990s” were considered offering the service from 1991 onwards.

For each characteristic, municipalities are given a dummy variable. The dummy is equal to one if the municipality is above the sample's median value, or zero if it is below that median. The regression results can be found in Table A.1 in the appendix and report no significant effects of any of the municipal characteristic on being a *Reform97*-municipality. The *Reform97*-municipalities are statistically similar to other municipalities.

The main data sample consists of mothers living in the 44 *Reform97*-municipalities from 1996 to 2003. Any mother to a child who would be eligible for after-school care in 1996 and in 1997 (being 6, 7 or 8 years old in 1996) is defined as in the treatment group. The treatment is the availability of after-school care for their child in 1997.

The control group is made to be as similar to the treatment group as possible in terms of underlying characteristics; yet they must differ in their access to after-school care. Access is decided by a strict age limit, despite age varying gradually. I thus choose mothers to primary school-aged children who would be non-eligible in 1996 and in 1997, i.e. children aged 10, 11 and 12 years old in 1996, as the control group.

To summarize: First, I follow the same mothers over time, from 1996 to 2003. Second, all mothers come from the 44 *Reform97*-municipalities; none of them had access to public after-school care prior to 1997. Third, without municipal coverage as an instrumental variable, I measure the effects of the intention to treat and not of the treatment received. For that reason, I refer to the estimates as effects of after-school care *availability*, keeping in mind that the program was, and still is, based on voluntary participation and parental payments.

4.2 Threats to the empirical strategy

Three threats could undermine the estimation results: a violation of the common trends assumption, selection problems, and data interference from competing policies.

The DD design is convenient because it allows for differences between groups. However, if the condition of common trends pre-reform is not satisfied, the control group's post-reform employment trend is not a legitimate counterfactual. With observations starting one year before the reform, the pre-reform trends of the two groups can neither be assessed, nor compared.

To overcome this shortcoming, I use aggregate Norwegian data that resembles my treatment and control groups from the *Reform97*-municipalities, and test the common trends assumption

with it instead. Figure 4.3 shows full-time employment of mothers with children aged between 7 and 10 years old (black), and the development of mothers with children aged 11 to 15 years old (gray) from 1991 to 2004. The first group serves as an equivalent of the treatment group and the second, an equivalent of the control group. The vertical line marks *Reform97*.

Figure 4.3 brings forward two points. Firstly, mothers of young children work consistently less full-time than their counterparts with older children, potentially indicating a higher need of childcare. Secondly, and most importantly, despite short-term differences, the employment rates roughly move in the same upwards direction. I consider the pre-reform trends of the two groups to be sufficiently similar to satisfy the condition of common trends.

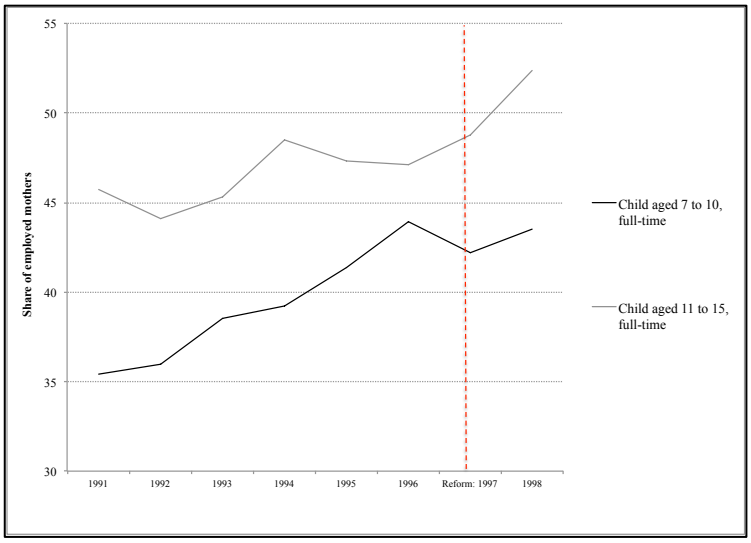


Figure 4.3. Employment trends for employed mothers with children aged 7 to 10 years old and employed mothers with children aged 11 to 15 years old. 1991 to 1998.
Data source: Barne- og likestillingsdepartementet, 2007.

The second threat to the research design is a methodological critique proposed by Felfe et al. (2016). It questions the DD strategy's ability to handle potential selection problems. Selection occurs at two levels: an individual mother could select into the treatment or control groups, or at the policy level, when the policy is introduced to the treatment group non-randomly. For example, when treatment is conditioned upon geography, there could be selective migration (individual level), and local underlying characteristics could drive provision of after-school

care (policy level). These violations of the randomness assumption would make the DD estimate positively biased.

With the proposed setup, however, individual selection bias would only occur if women actively chose between giving birth in 1984-1986 or rather in 1988-1990 (regardless of their own age), which is highly unlikely. There is a higher possibility for a policy selection bias: children aged 10 to 12 are, as a matter of fact, older, and more likely to be independent. There would be reasons for the policy to target the youngest schoolchildren, and there could be substantial differences in how the policy truly affects their mothers.

The third threat is influence from other policies on the same mothers' employment. If these are being introduced, enhanced or developed in the same period as the after-school program, they can lead to an overestimation of the true effect of after-school care availability. Note, however, that a bias-inducing policy must apply specifically to the mothers in the treatment group. If all mothers, on the other hand, are affected by it, the policy does not compromise the validity of the results. To my knowledge, no such policy was introduced simultaneously with *Reform97* (Vollset, 2011).

4.3 An alternative approach

Children's age is not the only possible distinction between treatment and control. Another alternative is to use mothers with SFO-eligible children living in *Reform97*-municipalities in 1996 as a treatment group and the equivalent mothers in other municipalities as a control group.

This approach breaks with a standard DD set up because the control group has received treatment prior to reform and continues being treated after reform. For such a strategy to be valid, the employment trends between treatment municipalities and control municipalities must be parallel after *Reform97*.

The strategy is appealing because it compares mothers with children of the same age. These mothers are expected to resemble in terms of underlying characteristics. However, it has a serious flaw: the possible endogeneity of treatment. Underlying factors affecting local childcare policy come in different shapes. On the demand side, mothers in a certain area might have a stronger preference for care services, or may have selectively migrated to municipalities that would offer it. On the supply side, some municipalities introduced the

policy voluntarily, and likely planned and budgeted for its introduction, possibly affecting volume, price, quality and publicity. These disturbances would lead mothers in control municipalities to have a stronger treatment received than mothers in the treatment group. This importance difference, however, would not be captured by the analysis, but stay hidden behind an intention-to-treat, which would completely similar across locations.

The descriptive statistics and results for this alternative are presented in the appendix, together with corresponding figures. The results chapter will include comments on these results, as a means of comparison with the main results.

5 Descriptive Statistics and Model

The detailed nature of Statistics Norway's data have allowed me to narrow down to mothers living in the 44 identified *Reform97*-municipalities who gave birth in the period 1982 to 1995, and to control for individual factors such as immigration background, education level and age. Employment figures, given annually from 1996 to 2003, are classified into four workload bins, according to average weekly working hours. A mother is yearly registered either as (i) unemployed, (ii) short part-time employed (4 to 19 working hours per week on average), (iii) long part-time employed (20 to 29 hours) or (iv) full-time employed (over 30 hours).

In order to follow the same mothers over time, the data sample is restricted to mothers with a child aged 6, 7, 8, 10, 11 or 12 in 1996. Mothers of children aged 9 year in 1996 are excluded from the data sample, as explained in Section 4.1. The treatment group includes 5 388 mothers and the control group, 7 636 mothers.

Table 5.1 displays a comparison of the two groups based on mothers' individual characteristics and reveals the two groups to have fairly similar features. Trivially, mothers in the control group (with younger children) are on average younger than their control group counterparts (with older children), and the average age in both groups increases with 7 years over the period.

Table 5.1 *Descriptive statistics of main sample*

Mean values of:	Treated 1996	Non-treated 1996	Treated 2003	Non-treated 2003
Employment variables				
Employment	54.96	59.68	88.83	88.45
Full-time employed	45.76	42.71	59.26	60.78
Part-time employed, 4-19 hours	27.97	28.87	18.99	17.81
Part-time employed, 20-29 hours	26.27	28.42	21.63	21.40
Control variables				
Age, mother	33.15	37.90	40.15	44.90
Higher education, share of mothers	58.76	50.89	57.63	50.89
Immigrant, share of mothers	5.77	4.77	5.77	4.77
Age, father[1]	36.11	40.87	43.04	47.76
Higher education, share of fathers	62.25	58.00	61.36	58.03
Observations	5 388	7 636	5 388	7 636

[1]Number of observations for fathers in equal horizontal order: 5 205; 7 327; 5 234; 7 388.

The employment statistic reports an average pre-reform gap in total employment of 4.72 percentage points. However, treatment group mothers' employment is to a larger degree characterized by full-time employment.

The largest gap between the two groups, of 8 percentage points, occurs for the pre-reform share of higher education (being education beyond high school, i.e. for over 12 years). Mothers of younger children enter higher education more frequently perhaps a general trend occurring over time, driven by their, on average, slightly younger age.

From 1996 to 2003, both groups experience overall employment growth (34 percentage points for treated mothers, 29 for untreated). Relatively, however, the employment gap is reduced. In 2003, for both groups, more employed mothers work full-time (increase of 13.5 percentage points for treated, 18 for untreated). Not only is the gap between the groups reduced, it actually reverses the advantage: working mothers in the treatment group go from being the most full-time employed mothers, to instead undertake part-time employment to a larger degree.

The observed differences between the groups fall into the realm over expected variations and are considered acceptable for a truthful and credible comparative analysis.

5.1 The equation

Equation [3] describes the employment outcome *y* of a mother *i* in year *t.* All estimations will be based on it.

$$y_{it} = \beta_0 + \beta_1 Treated_i + \beta_2 Post_t + \beta_3 Treated_i \times Post_t + \beta_4 X_{it} + \varepsilon_{it} \qquad [3]$$

The difference-in-differences model is characterized by two dummy variables. *Post* divides the analysis period into a post-reform period by holding value 1 for all observations from 1997 to 2003, and a pre-reform period, holding value 0 for observations in 1996. As seen in Figure 4.1, eight municipalities introduce after-school care after 1997[5]. In their case, *Post*

[5] These municipalities include: Hemsedal, Lesja, Vega, Ørland (1998) and Hol, Kvinnherad, Svelvik, Utsira (1999).

only holds the value 1 from the year they introduce after-school care, i.e. 1998 or 1999, and onwards.

The variable *Treated* is equal to 1 for the identified treatment group and 0 for the control group. The average treatment effect of [3] is thereby estimated by the interaction of *Post* and *Treated*, which reports the post-reform change in outcome exclusively for the treated sample.

The *Post* estimate is the average outcome of the control group and to be understood as the counterfactual post-reform outcome of the treatment group. The interaction estimate of β_3 is therefore to be interpreted in relation to the coefficient of *Post*.

A set of individual control variables is included in the vector X in order to isolate the treatment's effect on the outcome variable. These are four dummy variables: whether the individual is born in Norway, whether the individual has more than 12 years of education and two variables describing age; the first is true if the individual was younger than 30 years old in 1996, the second is true if the individual was older than 39 years old.

The last input variable, ε, is an error term. To control for unobserved heterogeneity, I also include fixed effects at a municipal level. The fixed effects control for time-invariant unrevealed characteristics. In the given context, this could include local employment trends or after-school care prices.

I now turn to the output variable y. Employment may change in two ways: mothers may enter or exit the workforce (the extensive margin), and working mothers can increase or reduce the amount of hours worked (the intensive margin). Both of these effects are of societal interest, as is a third one: the share of employed women in full-time employment. Equation [3] can be designed according to each of these three types of changes in employment, forming three separate regression analyses.

First, I estimate employment at the extensive margin. In this case, all observations from the main sample are included and the outcome variable is a dummy variable equal to 1 if the individual is employed, or to 0 if the individual is unemployed. The estimation at the extensive margin reveals whether the reform brings new mothers into the labor market.

In the second set of estimations, I examine how after-school access encourages employment at the intensive margin, i.e. if the average mother increases her labor supply. The data sample is restricted to mothers working part-time prior to reform. The individual can either move from short part-time employment to long part-time employment or full-time employment, or

move from long-term employment into full-time employment. The outcome dummy y_{it} equals 1 if the registered labor supply in year t is higher than the labor supply of individual i in 1996, and equals 0 if it is equal to or lower than in 1996. The intensive margin effect is measured as the likelihood of working more hours per week post-reform.

In the third set of estimations, I consider only the effect on full-time employment, which is a policy objective. The outcome variable of equation [3] is equal to 1 if the individual is full-time employed and 0 otherwise. The data sample contains all mothers of the main sample employed in 1996. This regression investigates whether *Reform97* has shifted the gravity of maternal employment towards more full-time employment.

Because all the possible output variables and the input variables are dummy variables, the resulting coefficients can be understood as percentage point effects. All estimates result from OLS regressions on [3], which implies linear probability. All estimations are computed using Stata.

6 Empirical Results

The empirical results from the estimations of equation [3] show a two-sided effect of after-school care availability on maternal employment. After-school care access has a positive effect at the extensive margin of employment. As intended, the reform seems to encourage more mothers to enter the labor market. At the intensive margin of employment, however, the results suggest that after-school care discourages maternal labor intensity. Employed mothers in the treatment group appear to be less likely to increase their labor supply, and also less likely to start working full-time. This contradicts the intention of the policy. The empirical findings are consistent over a large set of subsamples.

6.1 Main sample

Difference-in-differences estimations report three coefficients to consider. In Table 6.1, the estimates of *Treated* reveal the added employment probability of being a treatment group mother versus being a control group mother. This is an expression of the pre-reform employment gap between the two groups. The coefficient of *Post-reform* represents the post-reform employment increase of the control group, i.e. the likely counterfactual post-reform trend of the treatment group. The *Diff-in-diff* estimate reports the change in outcome after reform exclusively for the treated mothers, in relation to the *Post-reform* estimate, and can be interpreted as the effect of policy.

Table 6.1 displays the estimation results from regressions on the main sample, with mothers of older primary-school children as the control group. Each column represents a specified model. The models differ in the employment measure used as the outcome variable, the sample used, and the inclusion of individual control variables and of municipal fixed effects.

In columns (1) and (2), I use the whole main sample and consider employment at the extensive margin. In columns (3) and (4), I present the labor supply response at the intensive margin of mothers working part-time in 1996. Columns (5) and (6) exhibit the effect of reform in full-time employment, using a sample of mothers employed in 1996.

The model in column (1) indicates a significant pre-reform employment gap of -4.44 percentage points, an overall employment growth of 19.10 percentage points and an average treatment effect of 3.58 percentage points. When including individual control variables and municipal fixed effects in column (2), the pre-reform employment gap is -5.00 percentage

points, the post-reform employment growth is of 19.12 percentage points, and the average treatment effect is statistically significant and equals 3.55 percentage points.

The policy effect in column (2) is noticeably lower than the policy effects uncovered by Berthelon et al. (2015), Gambaro et al. (2018) and Nemitz (2015). In comparison to the no-effect result of Felfe et al. (2016), it is clearly larger.

The interpretation of the result should be understood in the particular context of this analysis. After-school care is based on voluntary participation and its sole availability implies an intention-to-treat effect. Furthermore, mothers of eligible children are not perfectly comparable to mothers of older schoolchildren, as the initial need for treatment may be lower in the latter group. Controlling for education and age, both of which influence employment opportunities, the identified treatment reduces the pre-reform employment gap by 71 percent[6].

Table 6.1 *Main regressions. Main sample. Extensive margin, intensive margin and full-time employment, respectively. OLS.*

	Extensive margin		**Intensive margin**		**Full-time employment**	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Treated	-.0444***	-.0500***	-.0075**	-.0233***	.0161	.0139
	(.0070)	(.0067)	(.0034)	(.0048)	(.0135)	(.0131)
Post-reform	.1910***	.1912***	.3962***	.3974***	.1199***	.1200***
	(.0087)	(.0086)	(.0161)	(.0160)	(.0071)	(.0072)
Diff-in-diff	.0358***	.0355**	-.0248	-.0245	-.0253*	-.0252*
	(.0060)	(.0059)	(.0171)	(.0171)	(.0135)	(.0135)
Intercept	.6047***	.5700***	.0319**	.0078	.4226***	.3691***
	(.0115)	(.0065)	(.0142)	(.0107)	(.0136)	(.0099)
N	104 192	104 192	33 736	33 736	60 144	60 144
R^2 *within*	.0289	.0455	.0796	.0828	.0060	.0185
R^2 *between*	.0103	.0569	.0014	.0144	.0018	.0058
R^2 *overall*	.0282	.0456	.0770	.0802	.0057	.0178
Individual controls	No	Yes	No	Yes	No	Yes
Municipal FE	No	Yes	No	Yes	No	Yes

Clustered standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Columns (3) and (4) indicate a negative maternal labor supply response to after-school care availability at the intensive margin. Treated mothers have an overall lower labor intensity, as indicated by the statistical significant negative coefficient of *Treated*. *Post-treatment* indicates

[6] Column (2) of Table 6.1: *Diff-in-diff* coefficient as share of *Treated* coefficient.

a general move into more intensive employment. The estimated average treatment effect is negative with -2.45 percentage points. However, it is not statistically significant, so the result can only be understood as an indication.

Contrary to the findings of Gambaro et al. (2018) and Felfe et al. (2016), which both report positive effects, models (5) and (6) find a negative effect of after- school care access on full-time employment. Employed treatment group mothers are in general more likely to be full-time employed, but following the introduction of public after-school care, the likelihood decreases by -2.53 percentage points relative to their hypothetical potential.

Figures 6.1 to 6.4 are graphical illustrations of the results in Table 6.1. Figure 6.1 depicts the employment rates of treated versus non-treated mothers, with a black line and a gray line, respectively. The figure presents the initial gap in employment between the two groups, and shows how the two lines converge after reform, with the employment rate of treated mothers eventually surpassing the one of non-treated mothers. Figure 6.1 is an indication of after-school care access meeting its target of stimulating maternal employment.

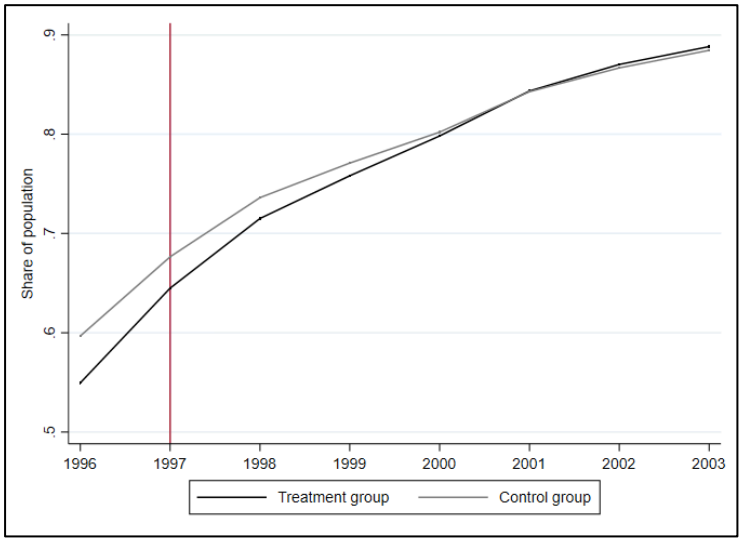


Figure 6.1: Employment rate from 1996 to 2003. Treatment group (black lines) vs. Control group (gray lines). Main sample.

Figures 6.2 and 6.3 describe the employment patterns of employed mothers. Figure 6.2, considers pre-reform part-time employed mothers only, while Figure 6.3 includes all pre-reform employed mothers. In both figures, both types of part-time employment see a general downward trend, with a corresponding upward trend in full-time employment.

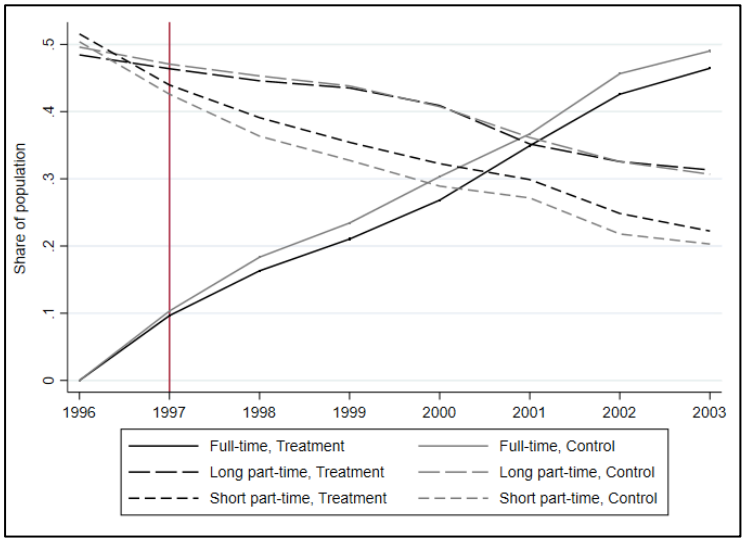


Figure 6.2: Employment status from 1996 to 2003.
Treatment group (black lines) vs. Control group (gray lines). From main sample: Part-time employed in 1996.

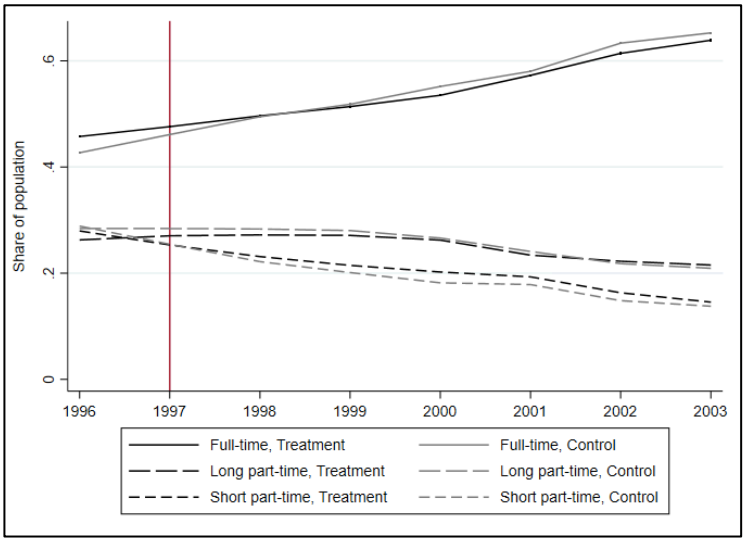


Figure 6.3: Employment status from 1996 to 2003.
Treatment group (black lines) vs. Control group (gray lines). From main sample: Employed in 1996.

Differences between the comparison groups also become evident. Figure 6.2 corroborates the indication by model (4): the move into a more intensive labor supply is weaker among treated mothers than among non-treated mothers. While the majority of mothers in both groups enter full-time employment, non-treated mothers do so more frequently. The share of treated

mothers working long part-time increases after 1997, which is consistent with the results in model (6).

In Figure 6.4, the four employment statuses are given as shares of the whole main sample. The treatment group increases its labor supply relative to the control group by reducing unemployment rates and growing its share of part-time work, not of full-time work.

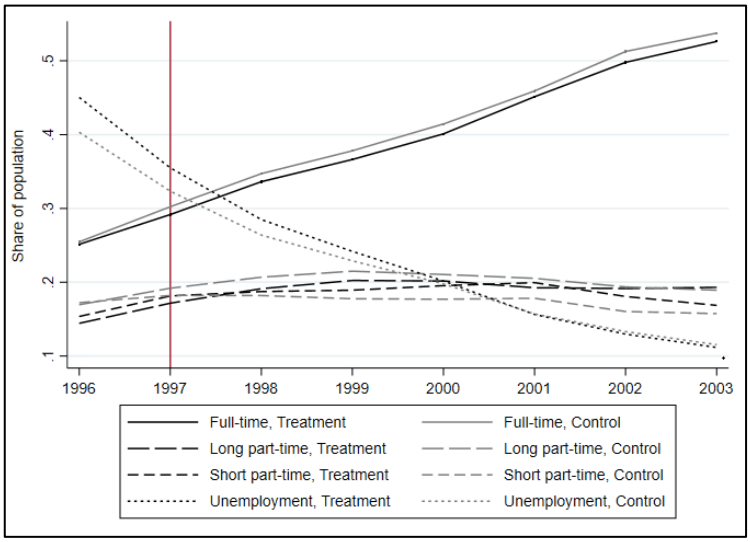


Figure 6.4: Employment status from 1996 to 2003.
Treatment group (black lines) vs. Control group (gray lines). Main sample.

The negative effect on the intensive margin (columns (3) to (6) in Table 6.1) might be explained by an income effect (Vuri, 2016). Before the introduction of publically organized childcare, employed mothers might have worked full-time while also privately financing some other supervision arrangement. If the new public option is cheaper, thanks to subsidies or economies of scale, some mothers may reduce their labor supply because they now can generate the same amount of net income by working fewer hours (Vuri, 2016 p. 4).

The generality of the results have been tested for each of the three employment definitions. Equation [3] has been re-estimated using different subpopulations of the main sample. The six subsamples are based on relevant individual characteristics: being foreign-born, having higher education, having high-school education, being younger than 30 years of age, being between 30 and 39 years of age, as well as being older than 39 years old (age ranges conditioned on the year 1996).

The extensive margin of employment for each subsample is shown in Table 6.2, the intensive margin, in Table 6.3, and the empirical results with a full-time employment measure, in Table 6.4. Overall, the subsample regression results are consistent with the results from the main sample. After-school care availability stimulates employment, but has a negative effect on labor intensity in general and full-time employment in particular.

The main results appear to be most applicable for mothers with higher education, which by each employment measure have statistically significant results. After-school care access impacts foreign-born mothers the most; *Reform97* reduces their likelihood of increasing their labor supply by 11.53 percentage points and their likelihood of full-time employment by 7.04 percentage points.

Table 6.2 *Subsample regressions I. Extensive margin. OLS.*

	(1) Foreign-born	(2) Higher education	(3) High-school education	(4) Age < 30	(5) 30 ≤ Age ≤ 39	(6) 39 < Age
Treated	-.1218***	-.0497***	-.0506***	-.0102	-.0235**	-.0355
	(.0380)	(.0088)	(.0101)	(.0316)	(.0102)	(.0225)
Post-reform	.3004***	.1862***	.1959***	.2770***	.2095***	.1518***
	(.0194)	(.0103)	(.0083)	(.0254)	(.0095)	(.0076)
Diff-in-diff	.0183	.0305***	.0459**	.0048	.0059	.0118
	(.0252)	(.0069)	(.0100)	(.0234)	(.0075)	(.0136)
Intercept	.4302***	.6677**	.5732***	.4573***	.5654***	.5682***
	(.0336)	(.0081)	(.0104)	(.0280)	(.0084)	(.0097)
N	5 400	56 416	47 776	14 416	61 472	28 304
R^2 *within*	.0735	.0368	.0317	.0689	.0417	.0433
R^2 *between*	.0086	.0359	.0161	.0338	.0463	.0018
R^2 *overall*	.0757	.0408	.0315	.0682	.0415	.0449
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes	Yes	Yes	Yes

Clustered standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 6.3 *Subsample regressions II. Intensive margin. OLS.*

	(1) Foreign-born	(2) Higher education	(3) High-school education	(4) Age < 30	(5) 30 ≤ Age ≤ 39	(6) 39 < Age
Treatment	.0361	-.0054	-.0351***	-.0030	-.0114***	-.0044
	(.0476)	(.0065)	(.0080)	(.0202)	(.0039)	(.0061)
Post-reform	.4338***	.4309***	.3618***	.4385***	.4063***	.3798***
	(.0327)	(.0175)	(.0174)	(.0482)	(.0177)	(.0149)
Diff-in-diff	-.1153*	-.0507**	.0025	-.0314	-.0502***	-0095
	(.0598)	(.0198)	(.0189)	(.0534)	(.0187)	(.0279)
Intercept	.1129***	.0269**	.0353***	.0379	.0061	-.0200
	(.0351)	(.0111)	(.0124)	(.0245)	(.0151)	(.0151)
N	1 096	18 104	15 632	4 136	20 024	9 576
R^2 *within*	.1124	.0900	.0769	.0908	.0838	.0848
R^2 *between*	.0048	.0004	.0069	.0821	.0474	.0430
R^2 *overall*	.0945	.0863	.0748	.0873	.0817	.0799
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes	Yes	Yes	Yes

Clustered standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 6.4 *Subsample regressions III. Full-time employment. OLS.*

	(1) Foreign-born	(2) Higher education	(3) High-school education	(4) Age < 30	(5) 30 ≤ Age ≤ 39	(6) 39 < Age
Treatment	.0643	.0343**	-.0104	-.0485	.0527***	-.0257
	(.0491)	(.0158)	(.0286)	(.0459)	(.0148)	(.0242)
Post-reform	.1090***	.1244***	.1143***	.1581***	.1256***	.1096***
	(.0274)	(.0093)	(.0099)	(.0411)	(.0087)	(.0074)
Diff-in-diff	-.0704**	-.0437***	-.0058	-.0276	-.0528***	.0088
	(.0339)	(.0137)	(.0187)	(.0509)	(.0133)	(.0208)
Intercept	.4518***	.4685***	.3825***	.4106***	.3640***	.3676***
	(.0297)	(.0100)	(.0112)	(.0453)	(.0134)	(.0126)
N	2 168	35 424	24 720	6 976	35 800	17 368
R^2 *within*	.0082	.0090	.0073	.0152	.0144	.0336
R^2 *between*	.0003	.0485	.0006	.0527	.0151	.1603
R^2 *overall*	.0071	.0083	.0072	.0134	.0140	.0337
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes	Yes	Yes	Yes

Clustered standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

6.1 The alternative approach

The empirical results and related figures of the proposed alternative approach can be found in the appendix. At the extensive margin, the most specified model presents a statistical significant negative average treatment effect of -1.91 percentage points. This result implies that after-school care in fact dissuades mothers from entering the workforce.

Such a response is difficult to justify, even by an income effect. It seems more plausible that the common trends assumption is violated. The control group municipalities are, in fact, still absorbing the effects from the previously introduced SFO supply. The DD method would have been able to overcome this shortcoming, had it not been for another concern, namely, that for the control group municipalities, the policy is endogenous and hence, positively biased.

With reference to these potential issues, I have decided not to emphasize the results from the alternative approach. However, as my concerns are not proven to be true, I choose to give the results and illustrative figures in the appendix.

7 Specification Checks

A varied set of specification checks can further explain the main results, as well as test their validity. This chapter includes seven specification checks on different aspects of the methodology and theme. A standard placebo test had to be excluded for lack of necessary data[7].

Even though none of the specification checks reject the empirical findings, they emphasize a need for a more precise research design and a more detailed treatment identification than this study's year of intention to treat.

7.1 Specification check I: Heterogeneity

The first specification check relates to the subsample regressions of Chapter 6. Each individual control variable is interacted with the DD variable to understand how the group characteristics drive the results. The effect of SFO access on maternal employment can thus be analyzed along three dimensions: before and after reform, between treated and non-treated mothers, and between different subsamples.

The estimation results are presented in Table 7.1, which is organized by the three different outcome variables. To correctly interpret the total treatment effect in the subsample, the single group-interaction coefficients from Table 7.1 must be added to the main DD estimate.

The extensive margin in column (1) reveals that a high-educated mother in general would be 10.57 percentage points more likely to be employed, but if she is a treatment group mother the likelihood decreases by -4.23 percentage points. The model reports that a high-educated treatment group mother after reform increases her probability of employment by 1.02 percentage points relative to her hypothetical outcome[8]. Following this as a procedure of interpretation, column (1) reveals that the employment of foreign-born mothers responds negatively to SFO availability, while mothers less than 30 years of age respond highly positively. The effects on older mothers are not statistically significant.

The intensive margin in column (2) shows positive average treatment effects for the oldest mothers, and negative or insignificant average treatment effects for every other subsample.

[7] A placebo-tests checks whether the applied reform year is relevant by simulating reform in a year prior to reform. The main results pass the placebo-test if the test results are zero. I have data from only one year prior to reform and can, therefore, not produce such a test.
[8] In column (1): Estimate of Row 9 added to the estimate of Row 3.

Mothers with higher education are the only ones to increase their labor supply in the post-reform period, but treated mothers with higher education do so to a lesser degree.

The full-time employment share in column (3) increases post reform for all subsamples. However, all the DD coefficients are negative and not statistically significant.

Overall, the results from Table 7.1 reflect the main empirical findings of chapter 6: SFO availability has a positive effect on maternal labor supply at the extensive margin, but seems to discourage labor supply at the intensive margin. The effects are the strongest among high-educated mothers.

7.2 Specification check II: Year-dummies

One concern of the current research design is the construction of the comparison groups; the treated mothers and the non-treated mothers are likely to have different pre-reform SFO demand. Hence, as the children grow older, the positive employment trend of the treatment group might reflect a natural tendency towards higher employment. This implies that the main findings, instead of measuring policy effects, are driven by an endogenous positive employment trend in the treatment group.

To investigate this matter, the employment development of the treatment group is examined and compared to the control group year by year in the post-reform period. Dummy variables representing each post-reform year are interacted with the DD variable. This enables me to understand how the groups behave relative to each other on a yearly basis. If the DD-year interactions closest to the reform year zero out the main DD estimate and the DD-year interactions furthest away from reform lose relevance in relation to the main DD estimate, the specification check might signal the natural tendency of increased employment as children grow older.

In column (1), the DD estimate of Row (3) is highly statistically significant with a value of 8.89 percentage points. This indicates a general positive employment evolution in the treatment group relative to the control group. By considering the DD-year interactions, however, it becomes evident that positive employment effects do not occur until 1999.

In column (2) the negative effects appear to be immediate, even though the main DD estimate is not statistically significant and a definite conclusion cannot be drawn. Looking at column (3), the negative effects manifest themselves for years after the reform.

I consider the specification test results not to reject the main results completely, because policy can actually have long-term effects. Nevertheless, the results of Table 7.2 are suggesting that a more comparable comparison groups and a stronger treatment-received instrument are preferable.

7.3 Specification check III: Instant impact

In columns (1), (3) and (5) of Table 7.3, I look at the instant impact of reform in another way than by using a sample consisting of observations from the years 1996, 1997 and 1998 only. The results reproduce the overall effects from the main findings, indicating a relatively immediate response. This is especially true at the extensive margin of employment, estimated at 1.68 percentage points, which is also statistically significant. This effect reduces the initial employment gap by 47 percent[9]. For the intensive margin and full-time employment, estimates are not statistically significant. These results are graphically illustrated by Figures A.5 to A.8 in the appendix and demonstrate a sudden increase in employment.

7.4 Specification check IV: Mothers to 6-year-olds

As previously addressed, the chosen research design uses intention-to-treat as treatment, and not treatment received; all mothers with eligible children receive the *Reform97* policy treatment in the year SFO is introduced in their municipality, whether their child in reality attends SFO or not.

While SFO attendance is not known for the children of my sample mothers, Figure 4.4 shows that the SFO coverage rate was highest generally among 6-year-olds and 7-year-old. More recent official figures reveal that this feature of SFO consumption is similar geographically (Utdanningsdirektoratet, 2019). Differently expressed, mothers to 6 or 7 year old children are more often true treatment receivers than mothers to older children. Possible causes for this are higher demand for SFO for younger children, or potentially preferential treatment by the municipalities, or perhaps the knowledge that the child is eligible to attend SFO in several years to come.

[9] Calculated from 1.68 as a share of 3.59.

I have interpreted previously found effects of intention-to-treat on employment as driven by children actually attending SFO. For this hypothesis to hold, mothers who are truly higher receivers of treatment should see a higher average effect on employment than the full sample.

I have estimated [3] using mothers of 6-year-olds as a subsample and the same control group as in the main regressions. The estimation results in columns (2), (4) and (6) in Table 7.3 are similar to the main findings, but have a stronger magnitude. The results are illustrated in Figures A.9 to A.12 in the appendix and show a quick labor market response to reform – at every employment measure. This supports the validity of intention-to-treat as a proxy for treatment received, but also emphasize the need to know the extent of policy received to better understand maternal workforce dynamics.

7.5 Specification check V: Simulation

In this specification check, I examine whether there are underlying trends other than caused by *Reform97* driving the main findings. For that, I estimate [3] using the same definition of treatment and control group, but with a sample consisting of mothers living in the 63 registered municipalities that are not defined as *Reform97*-municipalities, but already had organized SFO before 1997. These mothers are in the same situation at the same time, and their employment should be driven by the same underlying trends, with the exception of trends happening specifically and exclusively across the 44 *Reform97*-municipalities. I have named this specification check "Simulation", as it resembles a simulation of *Reform97*, even though these 63 municipalities in fact also were affected by the school reform. However, they did not introduce after-school care as a direct response to *Reform97*.

Columns (1), (3) and (5) in Table 7.4 display highly similar estimates as the main results of Table 6.1. The similarity of these results suggests a general upwards employment trend of mothers with young primary school children during the late 1990s, which might challenge the validity of the identification strategy.

On the other hand, *Reform97* did in fact impact all the municipalities in 1997. As illustrated by Figure 3.3, there was a general positive trend in SFO coverage in the period from 1996 to 1998. The findings from specification check V might also be explained by a broader impact of *Reform97*.

7.6 Specification check VI: Urban drivers

Employment characteristics often diverge between individuals residing in urban centers versus in district areas, and that may also be the case in this analysis. To investigate this, the models have been reestimated without individuals living in municipalities housing more than 25 000 inhabitants, namely Sandnes and Ringerike.

The results seem not to be greatly affected. The DD estimate of the intensive margin (in column (4) of Table 7.4), however, is much smaller than its main sample equivalent (in Table 6.1). Given that mothers with higher education dominate the effects at the intensive employment margin, the specification check results are likely due to female higher education being an urban area phenomenon.

7.7 Specification check VII: Larger control group

The control group of the main regressions has, until now, contained mothers with children aged 10, 11 or 12 in 1996. The group was defined as such to ensure that control group mothers could not receive treatment after reform, nor had access to it prior to reform.

In this specification check, I construct a new control group: all mothers who do not have any children aged 6, 7 or 8 years in 1996 are included. This approach has a clear weakness: the newly added control group mothers with a child below 6 years old in 1996, will see their child be eligible for SFO during the time period of my analysis. This will interfere with the results.

Nevertheless, I estimate the model and find that the results display similar, but smaller effects than the main results. These are presented in Table 7.5. The smaller effects are likely to be explained by new control group mothers also having children eligible for SFO.

Table 7.1 *Specification check I: Heterogeneity interactions included. Extensive margin, intensive margin and full-time employment, respectively. OLS.*

	(1) Extensive margin	(2) Intensive margin	(3) Full-time employment
Treatment	-.0423***	-.0219***	.0093
	(.0083)	(.0081)	(.0134)
Post-reform	.1912***	.3974***	.1200***
	(.0086)	(.0160)	(.0072)
Diff-in-diff	.0397***	-.0179	.0065
	(.0128)	(.0203)	(.0150)
Foreign-born	-.0818***	.0207	.0239
	(.0172)	(.0225)	(.0272)
High-educated	.1057***	.0559***	.1208***
	(.0089)	(.0123)	(.0095)
Younger mom	-.0528***	.0240	.0114
	(.0180)	(.0270)	(.0256)
Older mom	-.0174	-.0218**	.0225**
	(.0104)	(.0087)	(.0090)
Foreign-born*Diff-in-diff	-.0456**	-.0648	-.0242
	(.0223)	(.0540)	(.0490)
High-educated*Diff-in-diff	-.0195*	-.0295**	-.0308
	(.0105)	(.0143)	(.0222)
Younger mom*Diff-in-diff	.0419**	.0260	-.0336
	(.0199)	(.0306)	(.0217)
Older mom*Diff-in-diff	-.0145	.0356***	-.0188
	(.0200)	(.0270)	(.0265)
Intercept	.5661***	.0054	.3612***
	(.0075)	(.0118)	(.0083)
N	104 102	33 736	60 144
R^2 *within*	.0461	.0834	.0188
R^2 *between*	.0649	.0079	.0027
R^2 *overall*	.0462	.0809	.0181
Individual controls	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes
Heterogeneity int.	Yes	Yes	Yes

Clustered standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 7.2 *Specification check II: Year-dummies included. Extensive margin, intensive margin and full-time employment, respectively. OLS.*

	(1) Extensive margin	(2) Intensive margin	(3) Full-time employment
Treatment	-.0512***	-.0230***	.0125
	(.0065)	(.0049)	(.0135)
Post-reform	.2258***	.4927***	.1722***
	(.0231)	(.0510)	(.0223)
Diff-in-diff	.0889***	.0578	-.0017
	(.0152)	(.0451)	(.0278)
Year 97	-.1544***-.	-.3584***	-.1536***
	(.0187)	(.0404)	(.0195)
Year 98	-.0767**	-.2030***	-.1056***
	(.0294)	(.0673)	(.0251)
Year 99	-.0741***	-.2040***	-.1042***
	(.0129)	(.0337)	(.0154)
Year 2000	-.0430***	-.1121***	-.0706***
	(.0124)	(.0303)	(.0133)
Year 2001	-.0022	-.0412	-.0421***
	(.0137)	(.0307)	(.0122)
Year 2002	.0217	.0518*	.0110
	(.0136)	(.0305)	(.0127)
Year 1997*Diff-in-diff	-.0890***	-.0879*	-.0054
	(.0192)	(.0455)	(.0255)
Year 1998*Diff-in-diff	-.0944***	-.1476**	-.0349
	(.0277)	(.0676)	(.0273)
Year 1999*Diff-in-diff	-.0560***	-.0818**	.0207
	(.0130)	(.0326)	(.0177)
Year 2000*Diff-in-diff	-.0471***	-.0978**	-.0330*
	(.0129)	(.0330)	(.0176)
Year 2001*Diff-in-diff	-.0425***	-.0771**	-.0241
	(.0134)	(.0342)	(.0155)
Year 2002*Diff-in-diff	-.0397***	-.0923***	-.0356**
	(.0132)	(.0309)	(.0149)
Intercept	.5812***	.0343**	.3824***
	(.0075)	(.0147)	(.0109)
N	104 102	33 736	60 144
R^2 *within*	.0684	.1600	.0301
R^2 *between*	.0493	.0153	.0061
R^2 *overall*	.0683	.1563	.0291
Individual controls	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes

Clustered standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 7.3 *Specification checks III and IV. Extensive margin, intensive margin and full-time employment, respectively. OLS.*

	Extensive margin		Intensive margin		Full-time employment	
	(1) Period: 96-98	(2) 6-year-olds in 1996	(3) Period: 96-98	(4) 6-year-olds in 1996	(6) Period: 96-98	(5) 6-year-olds in 1996
Treatment	-.0359***	-.0592***	-.0154***	-.0403***	.0193	.0219
	(.0071)	(.0114)	(.0047)	(.0116)	(.0126)	(.0176)
Post-reform	.1009***	.1908***	.1944***	.3975***	.0391***	.1220***
	(.0086)	(.0087)	(.0158)	(.0160)	(.0051)	(.0063)
Diff-in-diff	.0168**	.0458***	-.0149	-.0327	-.0071	-.0425**
	(.0051)	(.0081)	(.0134)	(.0255)	(.0103)	(.0175)
Intercept	.5561***	.5685***	.0215**	.0098***	.3717***	.3647***
	(.0062)	(.0080)	(.0096)	(.0130)	(.0105)	(.0080)
N	39 072	75 288	12 651	24 960	22 554	44 184
R^2 *within*	.0323	.0430	.0678	.0848	.0133	.0196
R^2 *between*	.0287	.0523	.0439	.0369	.0005	.0160
R^2 *overall*	.0325	.0434	.0613	.0823	.0128	.0190
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes	Yes	Yes	Yes

Clustered standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 7.4 *Specification checks V and VI. Extensive margin, intensive margin and full-time employment, respectively. OLS.*

	Extensive margin		Intensive margin		Full-time employment	
	(1) Simulation	(2) Urban drivers	(3) Simulation	(4) Urban drivers	(5) Simulation	(6) Urban drivers
Treatment	-.0548***	-.0482***	-.0124***	-.0246***	.0400***	.0076
	(.0084)	(.0091)	(.0034)	(.0061)	(.0115)	(.0170)
Post-reform	.2050***	.1833***	.4546***	.3849***	.1382***	.1135***
	(.0033)	(.0083)	(.0088)	(.0182)	(.0047)	(.0082)
Diff-in-diff	.0411***	.0348***	-.0442***	-.0053	-.0514***	-.0098
	(.0041)	(.0077)	(.0118)	(.0060)	(.0098)	(.0105)
Intercept	.5652***	.5751***	-.0222**	.0205	.4278***	.3761***
	(.0057)	(.0081)	(.0102)	(.0128)	(.0070)	(.0095)
N	237 457	72 392	68 320	23 648	137 368	42 088
R^2 *within*	.0468	.0468	.0937	.0820	.0157	.0181
R^2 *between*	.0977	.0934	.1531	.0187	.0102	.0058
R^2 *overall*	.0472	.0470	.0933	.0787	.0171	.0173
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes	Yes	Yes	Yes

Clustered standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 7.5 *Specification check VII. Extensive margin, intensive margin and full-time employment, respectively. OLS.*

	(1) Extensive margin	(2) Intensive margin	(3) Full-time employment
Treatment	-.0288***	-.0064**	-.0229**
	(.0051)	(.0027)	(.0108)
Post-reform	.2093***	.3872***	.0981***
	(.0085)	(.0139)	(.0063)
Diff-in-diff	.0177***	-.0162	-.0041
	(.0043)	(.0137)	(.0113)
Intercept	.5509***	.0077	.4048***
	(.0064)	(.0113)	(.0074)
N	236 376	71 800	134 648
R^2 *within*	.0459	.0798	.0200
R^2 *between*	.0158	.0053	.0332
R^2 *overall*	.0457	.0781	.0196
Individual controls	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes

Clustered standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

8 Concluding Remarks

After-school care is a family policy instrument intended to help parents combine career and family life. It is considered particularly relevant for mothers, whose inclusion in the workforce is high on the political agenda.

In this master thesis, I have used a difference-in-differences research design to evaluate the effects of after-school care availability on maternal employment. Following a 1997 school reform in Norway, a considerable amount of mothers saw their municipalities be obliged to facilitate public after-school care.

The main contribution of this study is two-fold. First and foremost, the Norwegian reform encourages more women to enter the labor market. Simultaneously, however, after-school care seems to discourage part-time employed mothers to pursue more work. The second result might suggest an income effect, whereby mothers remain part-time employed, not because they must care for children, but because they are subject to cheap childcare arrangements and hence, have a lower need for additional income.

The presented estimates are limited to the short-term benefits of after-school care. The DD method seeks immediate labor market results, while the adaptation of mothers to after-school care availability might react over the course of several years, including through affecting the birth rate of working mothers.

The identification strategy involves estimating intention-to-treat effects, giving only an indication as to the true effects of the policy. The conclusions from a wide range of specification checks emphasize the need for a more accurate identification mechanism of the policy treatment. The findings from these tests do not categorically reject the main empirical findings, but uncover some underlying trends, which might drive the estimations, both within subsamples of the treatment group, and across the treatment and control groups. These secondary results should be investigated further.

To whom the necessary data becomes available, I would recommend following the comprehensive empirical strategy of Andresen and Havnes (2018), and to use a more sophisticated instrument, which better captures the treatment-received effect. No less importantly, analyzing the long-term effects of affordable, broadly provided, high quality after-school care could reveal benefits yet to come.

Literature

Andresen, M. E. and Havnes, T. (2018) Child Care, Parental Labor Supply and Tax Revenue. IZA Institute for Labor Economics. Discussion paper series no. 11576.

Angrist, J. D. and Pischke, J. (2015) *Mastering 'Metrics. The Path from Cause to Effect.* Princeton University Press.

Barne- og likestillingsdepartementet (2007) Yrkesdeltakelse og arbeidstid blant mødre og fedre.

Berthelon M., Kruger D. and Oyarzun M. (2015) The effects of longer school days on mothers' labor force participation. IZA discussion paper, 9212

Blau, D., Currie, J. (2006) Pre-School, Day Care, and After-School Care: Who's Minding the Kids? In: Hanushek, E.,Welch, F. (Eds.), *Handbook of the Economics of Education Vol. 2.* Elsevier, pp. 1163–1278

Felfe, C., Lechner, M. and Thiemann, P. (2016) After-school care and parents' labor supply. *Labor Economics 42*, pp. 64–75

Gambaro, L., Marcus, J. and Peter, F. (2018) School entry, afternoon care, and mothers' labor supply. *Empir Econ* (2018). https://doi.org/10.1007/s00181-018-1462-3

Havnes, T. and Mogstad, M. (2011) Money for nothing? Universal child care and maternal employment. *Journal of Public Economics 95*, pp. 1455 – 1465.

Kvalle, Ø and Wendelborg, C. (2002) Nasjonal evaluering av skolefritidsordningen. Rapport 4. Nord-Trøndelagsforsking.
From: https://naku.no/sites/default/files/files/Evaluering%20av%20SFO_nasjonal.pdf (Downloaded: 12.04.2019)

Nemitz, J. (2015) The Effect of All-Day Primary School Programs on Maternal Labor Supply. ECON Working Paper Series no. 213. University of Zürich.

NOU 1979: 20. Forsøk med fritidshjem.

NOU 2012: 15. Politikk for likestilling.

OECD (2017) "Childcare supports: Helping both parents in paid work" in The Pursuit of Gender Equality. An Uphill Battle. OECD Publishing. Paris.
From: https://read.oecd-ilibrary.org/social-issues-migration-health/the-pursuit-of-gender-equality_9789264281318-en#page212 (Downloaded: 12.04.2019)

Stortingsinnstilling. Innst. 69 S (2016–2017). Innstilling fra kirke-, utdannings- og forskningskomiteen om Representantforslag fra stortingsrepresentantene Trine Skei

Grande, Iselin Nybø og Abid Q. Raja om å endre skolefritidsordningen til en aktiv fritidsordning og styrke kvaliteten i tilbudet

St. Meld 7 (1991 – 1992) Likestillingspolitikk for 1990-åra.

St. Meld. 40 (1992 – 1993) …vi smaa, en Alen lange. Om 6-åringer i skolen – konsekvenser for skoleløpet og retningslinjer for dets innhold.

St. Meld. 55 (1996 – 1997) Om skolefritidsordningen.

Utdanningsdirektoratet (2015) Regelverk for skolefritidsordningen (SFO). From: https://www.udir.no/regelverkstolkninger/opplaring/Leksehjelp/Regelverk-for-skolefritidsordningen-SFO (Downloaded: 12.04.2019)

Vollset, G. (2011) Familiepolitikkens historie – 1970 til 2000. Report 1/2011. NOVA – Norwegian Social Research.

Vuri, D. (2016) Do childcare policies increase maternal employment? *IZA World of Labor 2016: 241.*

Wooldridge, J. M. (2014) *Introduction to Econometrics. Europe, Middle East and Africa Edition*. Cengage Learnong EMEA.

Statistics

OECD

OECD.stat. Employment: Labor force participation rate, by sex and age group.
Downloaded from: https://stats.oecd.org/index.aspx?queryid=54741# [20.03.2019]

Official reports

NOU 1979: 20.
Page 12: 1 400 children in leisure time centers in 1978.

NOU 1984: 20.
Page 19: 3 150 children in leisure time centers in 1984.

NOU 1996: 13.
Table 9.8.1: "Antall barn i 1.-3. klasse med plass i skolefritidsordning."

St. meld. nr. 51. (1973-1974)
Page 48: 250 children in an after-school care arrangement.

St. meld. nr. 40. (1992-1993)
Page 47: 342 municipalities with after-school care program.

Statistics Norway

(1976) Statistical Yearbook 1976.
Table 445: Pupils by type of school and age and as percentage of registered population.

(1987) Statistical Yearbook 1987.
Table 85: Pupils by type of school and age and in per cent of registered population.

(1980) Statistical Yearbook 1980.
Table 441: Pupils by type of school and age and as percentage of registered population.

(1988) Statistical Yearbook 1988.
Table 85: Pupils by type of school and age and in per cent of registered population.

(1990) Statistical Yearbook 1990.
Table 89: Pupils by type of school and age and in per cent of registered population.

(1991) Statistical Yearbook 1991.
Table 91: Pupils by type of school and age and in per cent of registered population.

(1992) Statistical Yearbook 1992.
Table 91: Pupils by type of school and age and in per cent of registered population.

(1993) Statistical Yearbook 1993. Table 89: Pupils by type of school and age and in per cent of registered population.

(1986) Barnehager og fritidshjem 1985.
Table 33: Persons employed in leisure time centres, by position. Children per person employed. County.

(1988) Barnehager og fritidshjem 1987.
Table 31: Children in leisure time centres by ownership of the establishment. County.

(1990) Barnehager og fritidshjem 1989.
Table 33: Children in leisure time centres, by ownership of the establishment. County.

(1996) Child Care Institutions and Educational Programmes for 6-Year Olds 1995.
Table 31: Educational programmes for 6 year olds. Number of programmes and children. County.

(1997) Child Care Institutions and Educational Programmes for 6-Year Olds 1996.
Table 31: Educational programmes for 6-year olds. Number of programmes and children. County.

(1999) Aktuell utdanningsstatistikk: Utdanning i Norge. Nøkkeltall 1999.
Table 2.10: Grunnskolen. Deltakelse i skolefritidsordning, etter klassetrinn. 1995-1998.

From Statbank, Statistics Norway

Table 01610: Employees 16-74 years, by place of residence, sex, age and settled working hours per week (M) (closed series) 1990 – 2001.

Table 07459: Population, by sex and one-year age groups (M) 1986 - 2019

Table 03781: Employed persons, by age and sex 1972 – 2018.

Table 04232: Total fertility rate, women 1968 – 2018.

Utdanningsdirektoratet

Grunnskolens informasjonssystem. Key word: ”Elever”. (Downloaded July 31st 2018.)

Grunnskolens informasjonssystem. Key word: “SFO”. (Downloaded July 31st 2018.)

Appendix

A.1 Municipalities included in analysis (treated municipalities in bold)

Arendal	**Gran**	**Masfjorden**	**Skjervøy**	**Vågsøy**
Aurskog-Høland	**Granvin**	Meland	Smøla	Vågå
Balestrand	Grue	**Midsund**	**Snåsa**	Våler
Ballangen	**Halsa**	Nedre Eiker	Sola	**Ørland**
Berg	Hammerfest	Nesset	**Solund**	Øygarden
Berlevåg	**Hemne**	Nome	Spydeberg	**Årdal**
Bjerkreim	**Hemsedal**	Nordkapp	Stange	Ås
Bodø	Herøy	Nordre Land	Stavanger	
Bykle	**Hjartdal**	Oppegård	**Stor-Elvdal**	
Bø	Hobøl	**Rakkestad**	Stordal	
Bømlo	**Hol**	Randaberg	**Sula**	
Eide	Hornindal	Rindal	Surnadal	
Eidfjord	Horten	**Ringerike**	**Sveio**	
Eidsberg	**Hurdal**	Ringsaker	**Svelvik**	
Etnedal	Høyanger	Risør	Sør-Aurdal	
Fauske	Jondal	**Rygge**	Sørum	
Fitjar	**Kvinnherad**	Saltdal	Time	
Fjell	**Lenvik**	**Sandnes**	Trysil	
Flakstad	**Lesja**	**Sauherad**	Tysnes	
Frei	Lier	Sel	**Utsira**	
Froland	Lierne	Selje	**Vega**	
Fræna	Lund	**Seljord**	**Vegårshei**	
Fyresdal	Lunner	Siljan	**Vestre Slidre**	
Gjerstad	**Lyngen**	Ski	**Vestre Toten**	
Gloppen	**Marker**	Skiptvet	Våga	

A.2 Variables

Variable name	Description
emp0	Dummy equal 1 if unemployed.
emp1	Dummy equal 1 if weekly hours worked between 4 and 19 hours.
emp2	Dummy equal 1 if weekly hours worked between 20 and 29 hours.
emp3	Dummy equal 1 if weekly hours worked over 30 hours. Full-time employment.
parttime	Dummy equal 1 if part-time employed.
working	Dummy equal 1 if employed.
morejob	Dummy equal 1 if the year's labor supply is higher than the pre-reform level.
immigrant	Dummy equal 1 if not born in Norway.
highedmother	Dummy equal 1 if years of education over 12 years.
youngermom	Dummy equal 1 if age under 30 years in 1996.
midoldmom	Dummy equal 1 if age between 30 and 39 years in 1996.
oldmom	Dummy equal 1 if age 40 years and over in 1996.
age6	Dummy equal 1 if child aged 6 years old.
age7	Dummy equal 1 if child aged 7 years old.
age8	Dummy equal 1 if child aged 8 years old.
age9	Dummy equal 1 if child aged 9 years old.
age10	Dummy equal 1 if child aged 10 years old.
age11	Dummy equal 1 if child aged 11 years old.
age12	Dummy equal 1 if child aged 12 years old in.
Reform97	Dummy equal 1 if treatment municipality, 0 if control municipality.
treatment	In a treatment municipality: Dummy equal 1 if mother to child aged 6 to 9 years old in 1996.
control	In a treatment municipality: Dummy equal 1 if mother to child aged 10 to 12 years old in 1996.
postreform	Dummy equal 1 for all years from 1997 onwards.
did	(1) Interaction between *treated* and and *postreform.* (2) Interaction between *Reform97* and *postreform.*
conservative	Dummy equal 1 if the number of votes for Conservative Party or Progress Party as share of total votes in the municipality during the parliament election of 1996 was higher than the equivalent median share of votes among the selected municipalities.
oldwomen	Dummy equal 1 if the number of women aged 50 and above as share of all women in the municipality was. higher than the equivalent median share among the selected municipalities. Year 1997
lowerincome	Dummy equal 1 if the share of people aged 17 and abover in the municipality earning below 300 000 NOK was higher than the equivalent share among the selected municipalities. Year 1997.
femaleunemp	Dummy equal 1 if the municipal share of unemployed women between 16 and 66 years of age is above the equivalent median share of selected municipalities. Year 1997.
maleunemp	Dummy equal 1 if the municipal share of unemployed men between 16 and 66 years of age is above the equivalent median share of selected municipalities. Year 1997.

A.3 Comparing *Reform97*-municipalities to other municipalities

Table A.1 *Describing the* Reform97-*municipalities. Outcome variable: Status as* Reform97-*municipality. OLS.*

	(1) Reform97
Conservative vote	-.1498
(Share of votes for Conservative Party or Progressive Party 1996-election)	(.1143)
Women over 50 years old	.1660
(Share of women over 50 years of age of all women)	(.1085)
Low average income	-.1162
	(.1133)
Female unemployment rate	.1191
	(.1038)
Male unemployment rate	-.0665
	(.1081)
Intercept	.3506***
	(.1325)
N	107
R^2	.0668

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

3) Empirical results from the alternative strategy

Table A.2 *Descriptive statistics of alternative sample.*

Mean values of:	Treated 1996	Non-treated 1996	Treated 2003	Non-treated 2003
Employment variables				
Employment	54.77	55.70	88.66	89.52
Full-time employed	45.13	51.99	59.52	66.38
Part-time employed, 4-19 hours	28.04	25.51	18.81	15.49
Part-time employed, 20-29 hours	26.82	22.50	21.67	18.13
Control variables				
Age, mother	33.75	34.09	40.75	41.09
Higher education, share of mothers	57.63	61.19	57.63	61.19
Immigrant, share of mothers	5.78	7.18	5.78	7.18
Age, father[1]	36.72	36.96	43.64	43.91
Higher education, share of fathers	61.21	65.94	61.36	65.68
Observations	7 735	17 513	7 735	17 513

[1]Number of observations for fathers in equal horizontal order: 7 112; 16 768; 7 156; 16 904

Table A.3 *Alternative sample regressions. Extensive margin, intensive margin and full-time employment, respectively. OLS.*

	Extensive margin		**Intensive margin**		**Full-time employment**	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Treated	-.0001		.0483***		-.0360	
	(.0149)		(.0146)		(.0236)	
Post-reform	.2461***	.2461***	.4104***	.4104***	.0868***	.0868***
	(.0046)	(.0046)	(.0094)	(.0094)	(.0065)	(.0065)
Diff-in-diff	-.0191**	-.0191**	-.0379**	-.0369**	.0034	-.0033
	(.0088)	(.0088)	(.0155)	(.0155)	(.0121)	(.0121)
Intercept	.5607***	.5209***	-.0182***	-.0166**	.4640***	.4610***
	(.0085)	(.0053)	(.0068)	(.0082)	(.0157)	(.0116)
N	147 136	147 136	39 960	39 960	80 976	80 976
R^2 *within*	.0362	.0507	.0794	.0815	.0037	.0112
R^2 *between*	.0073	.0680	.0001	.0001	.0276	.0095
R^2 *overall*	.0356	.0508	.0769	.0797	.0066	.0123
Individual controls	No	Yes	No	Yes	No	Yes
Municipal FE	No	Yes	No	Yes	No	Yes

Clustered standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

*Note: As treatment is defined as the individual's municipality of residence's status as Reform97, the inclusion of municipal fixed effects removes the coefficient because of collinearity.

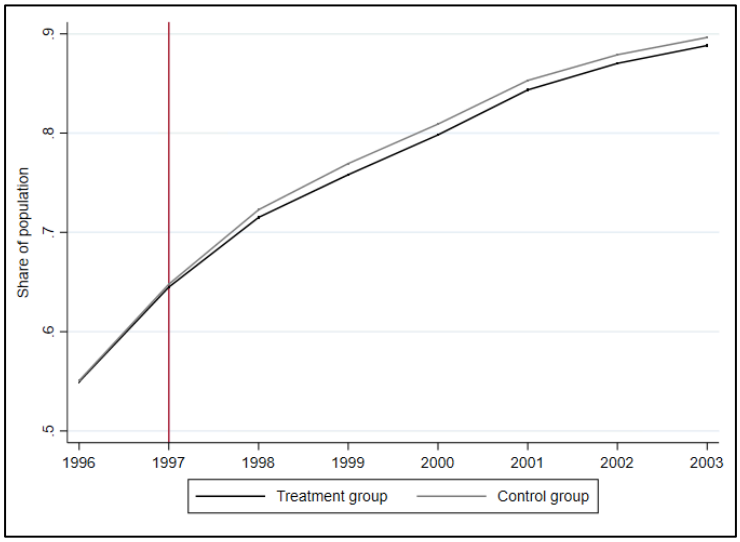


Figure A.1 Employment rate from 1996 to 2003. Treatment group vs. Control group. Alternative sample.

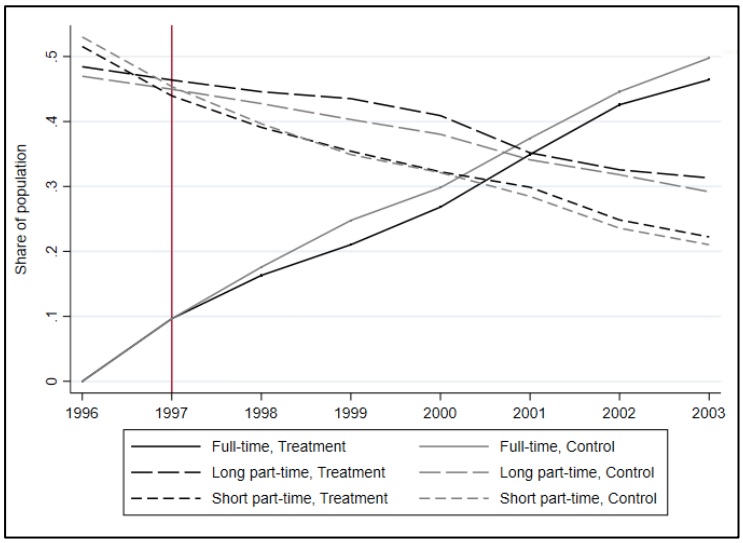


Figure A.2 Employment statuses from 1996 to 2003. Treatment group vs. Control group. From the alternative sample: Part-time employed in 1996.

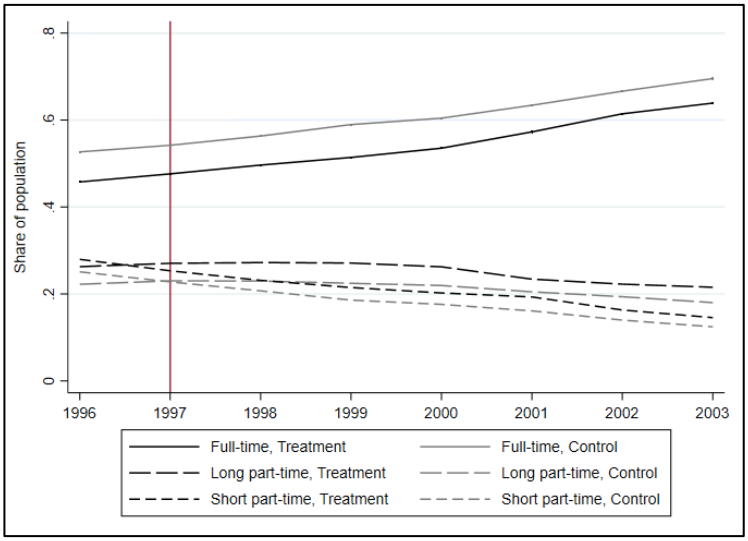
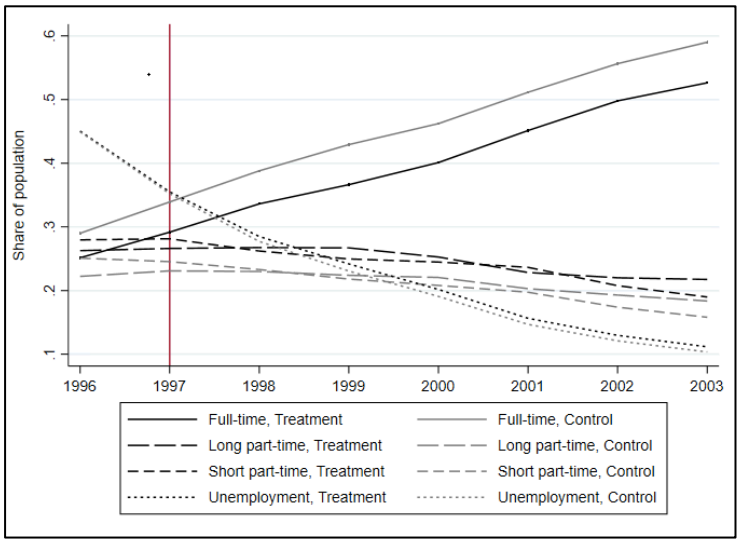


Figure A.3 Employment statuses from 1996 to 2003. Treatment group vs. Control group. From the alternative sample: Employed in 1996.



A.4 Employment statuses from 1996 to 2003. Treatment group vs. Control group. Alternative sample.

A.4 Figures related to the specification checks

A.4.1 Specification check III: Shorter time periode: 1996 to 1998

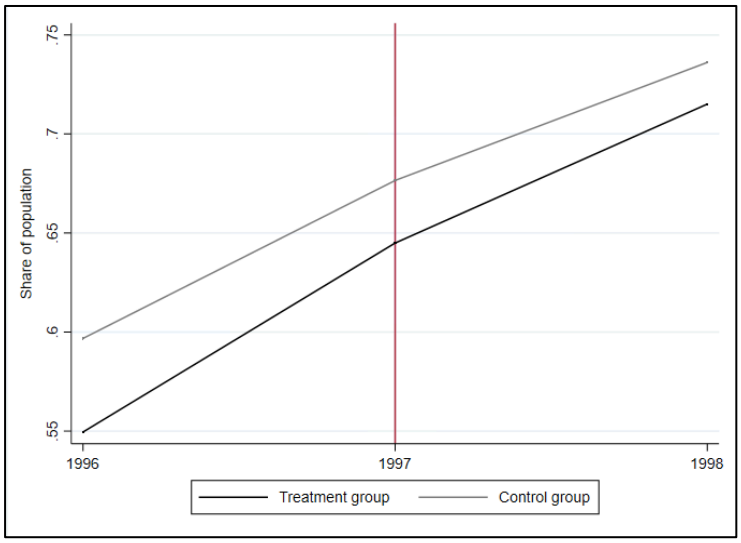


Figure A.5 Employment rate from 1996 to 1998.
Treatment group vs. Control group. Main sample.

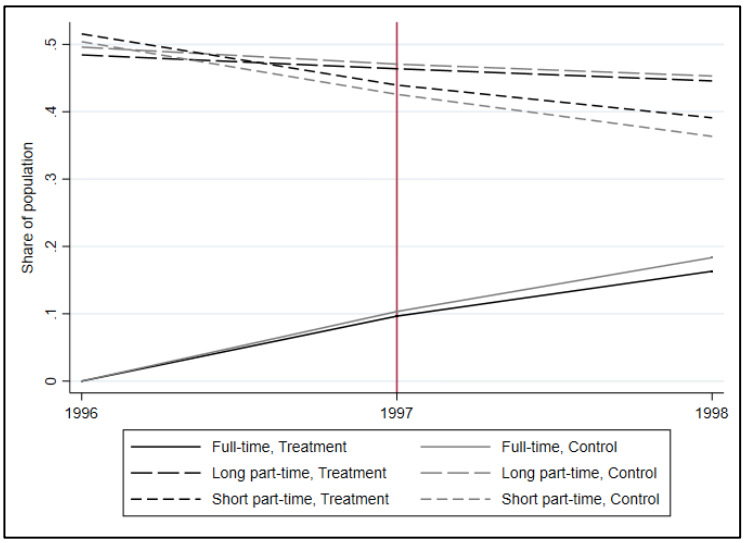


Figure A.6 Employment statuses from 1996 to 1998. Treatment group vs. Control group.
Main sample: Part-time employed in 1996.

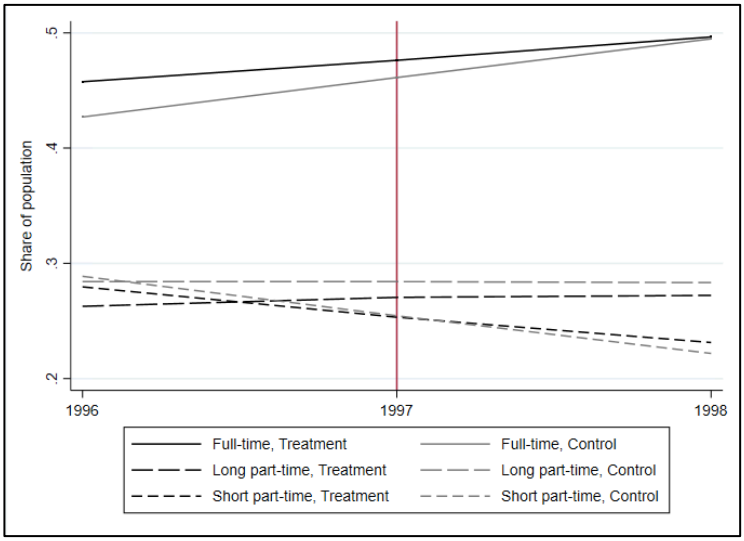


Figure A.7 Employment statuses from 1996 to 1998. Treatment group vs. Control group. Main sample: Employed in 1996.

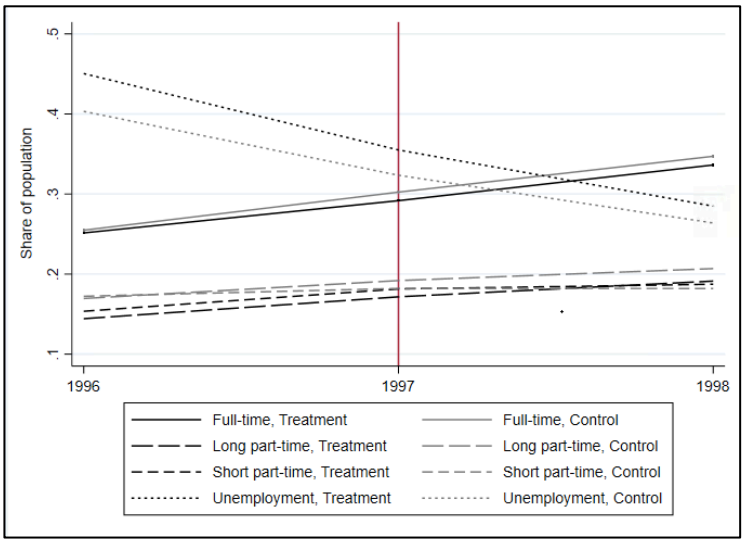


Figure A.8 Employment statuses from 1996 to 1998. Treatment group vs. Control group. Main sample.

A.4.2 Specification check IV: Mothers with children aged 6 in 1996

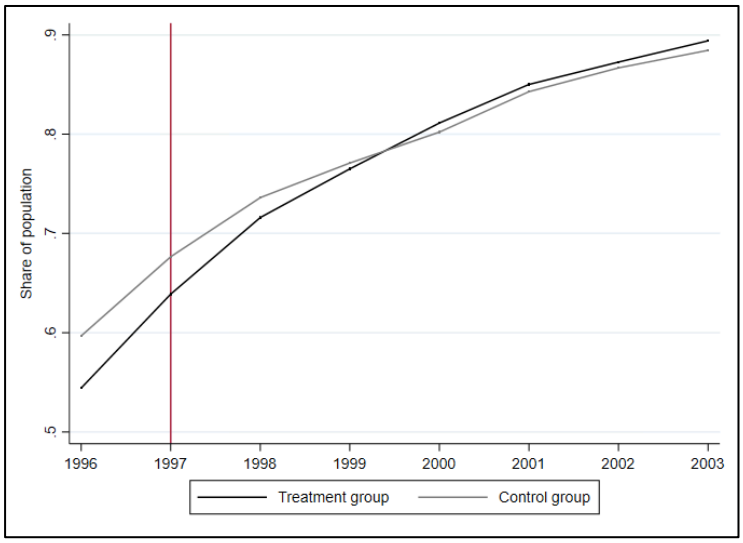


Figure A.9 Employment rate from 1996 to 2003. Treatment group vs. Control group. New treatment group: Mothers with children aged 6 in 1996. From main sample.

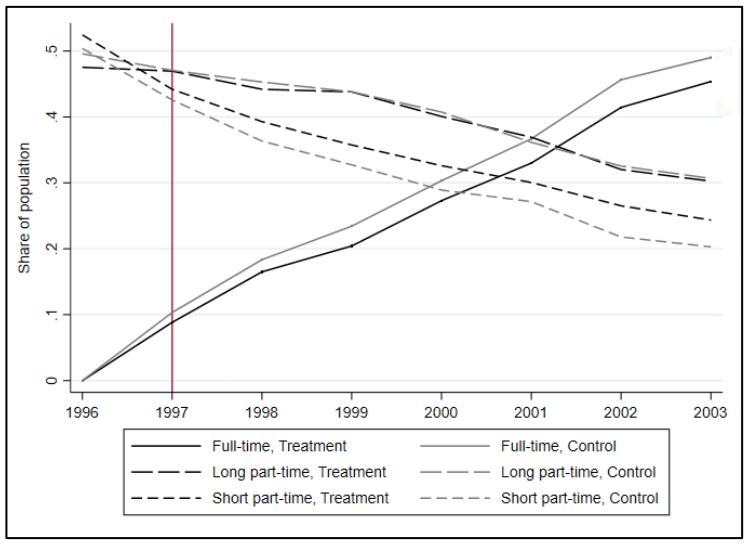


Figure A.10 Employment statuses from 1996 to 2003. Treatment group vs. Control group. New treatment group: Mothers with children aged 6 in 1996. From main sample: Part-time employed in 1996.

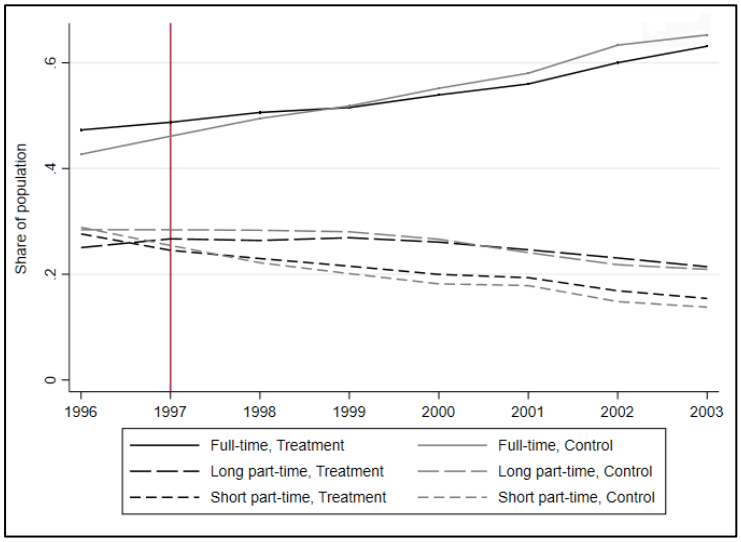


Figure A.11 Employment statuses from 1996 to 2003 Treatment group vs. Control group. New treatment group: Mothers with children aged 6 in 1996. From main sample: Employed in 1996.

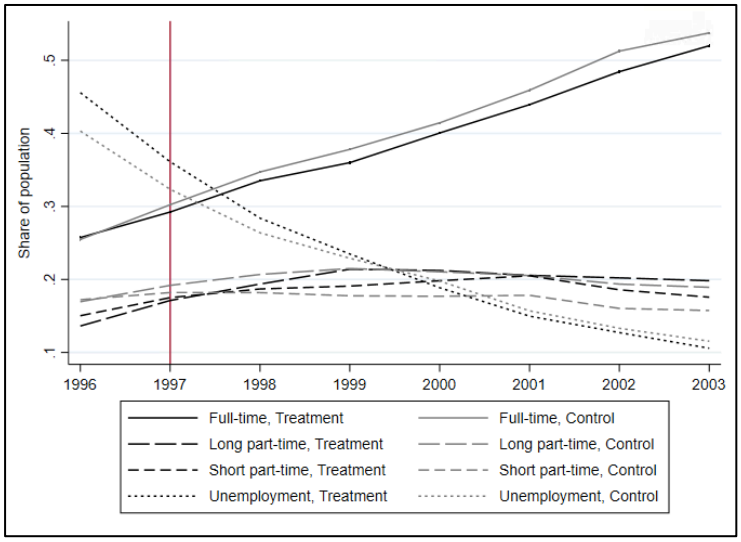


Figure A.12 Employment statuses from 1996 to 2003. Treatment group vs. Control group. New treatment group: Mothers with children aged 6 in 1996. From main sample.

A.4.3 Specification check VII: Expanded control group

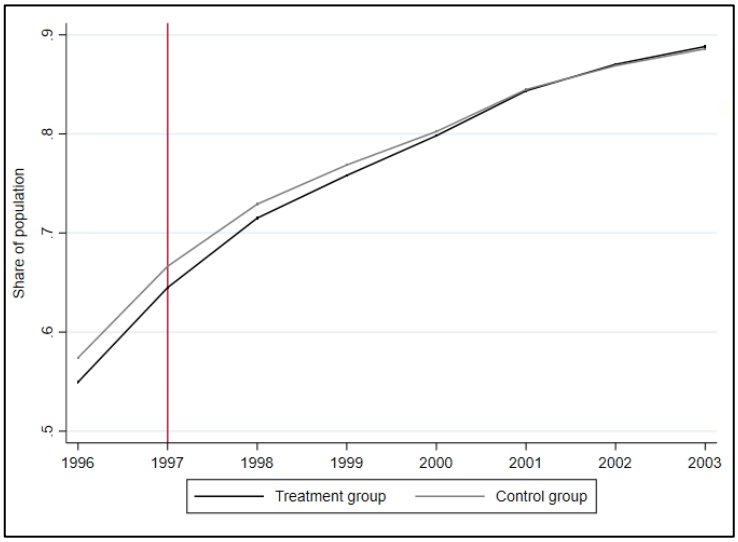


Figure A.13 Employment rate from 1996 to 2003. Treatment group vs. Control group. Expanded control group.

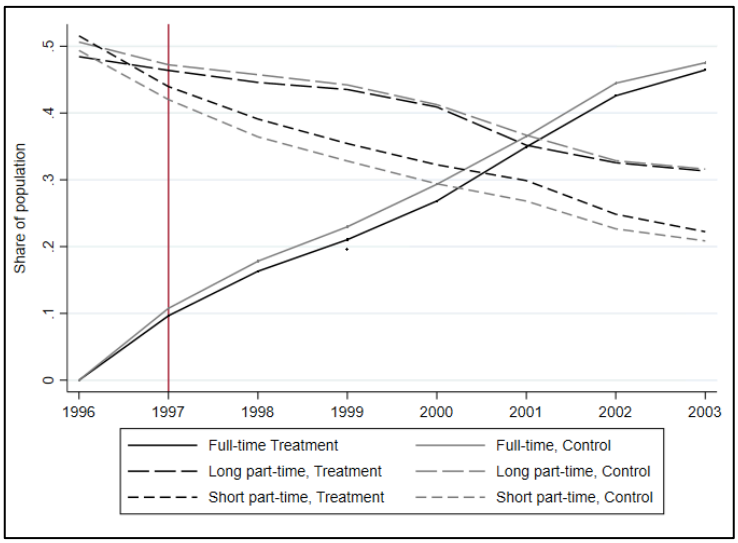


Figure A.14 Employment statuses from 1996 to 2003. Treatment group vs. Control group. Expanded control group; Part-time employed in 1996.

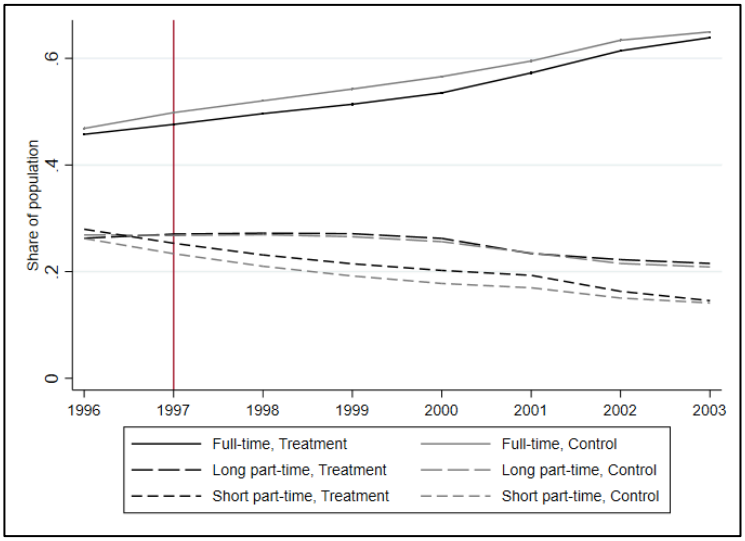


Figure A.15 Employment statuses from 1996 to 2003. Treatment group vs. Control group. Expanded control group; Employed in 1996.

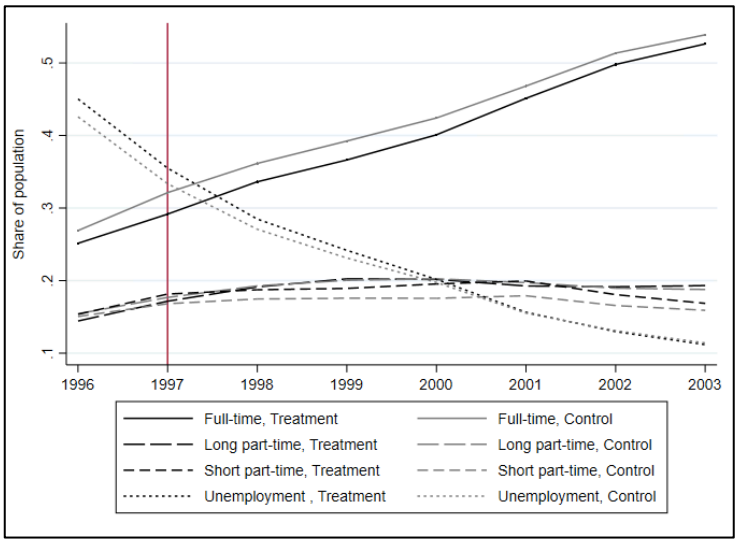


Figure A.16 Employment status from 1996 to 1998. Treatment group vs. Control group. Expanded control group.