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# From homemakers to breadwinners? How mandatory kindergarten affects maternal labour market attachment\*

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## Abstract

We analyse the effect of a mandatory kindergarten for four-year-old children on maternal labour supply in Switzerland by using two quasi-experiments: Firstly, we use a large administrative dataset and apply a non-parametric Regression Discontinuity Design to evaluate the effect of the reform at the birthday cut-off for entering the kindergarten in the same versus in the following year. Secondly, we complement this analysis by exploiting spatial variation and staggered treatment implementation of the reform across cantons (administrative units in Switzerland) in a Difference-in-Differences approach using a Swiss household survey. All in all, the results suggest that if anything, mandatory kindergarten increases the labour force attachment of mothers very moderately. The effects are driven by mothers earning less than the median annual work income of 15,706 CHF and by older mothers ( $>37$ ).

**Keywords:** Mandatory kindergarten, maternal labour supply, regression discontinuity design, difference-in-differences, variable selection

**JEL Classification:** H40, J13, J18, J21, J22

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# 1 Introduction

The labor force attachment of mothers has been receiving much attention in scientific and public discussions, given the large employment drops after giving birth observed in many countries, see for instance [Kleven et al. \(2019\)](#). In OECD countries, the average maternal employment rate amounts to 71% in 2019, with Iceland, Slovenia, and Sweden at the top with over 85%.<sup>1</sup> Also in Switzerland, which is the country considered in this study, mothers' employment rate declines after childbirth, ([Herrmann and Murier, 2016](#)), but it attains on average almost 78%.<sup>2</sup> Albeit this figure is above the OECD average, it is largely due to part-time (63%), rather than full-time employment (15%).<sup>3</sup>

A political instrument to encourage mothers' labour market attachment is the provision of childcare. For example, the opportunity of placing the first child in childcare increases the maternal labour force participation in the canton Bern ([Krapf et al., 2020](#)). Besides the labour force participation, the number of hours worked is also affected by the birth of a child: Mothers have a higher likelihood of working part-time than women without children ([Herrmann and Murier, 2016](#)). The provision of childcare influences also mothers' working hours positively: An expansion of childcare facilities rises maternal part-time hours ([Ravazzini, 2018](#)), while after-school care increases mothers' full-time employment ([Felfe et al., 2016](#)). Even maternal earnings may rise due to childcare availability, yet not the earnings of the total household ([Krapf et al., 2020](#)). However, attending childcare is voluntary and costly, so the comparison between mothers, whose children attend childcare and mothers whose children do not is generally plagued by selection bias. An interesting question in this context is how mothers' labor market behaviour and earnings react when childcare is mandatory and free of charge.

This paper examines the causal effect of mandatory kindergarten for four-year-old children on maternal labour behaviour and earnings in Switzerland. Since education policy is regulated at a cantonal level, there are differences in the implementation of the mandatory kindergarten and the birthday cut-off dates across cantons. We exploit these two cantonal differences in our

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<sup>1</sup>Reported by OECD. <https://www.oecd.org/els/family/database.htm>, retrieved 2021-08-24.

<sup>2</sup>Reported by OECD. <https://www.oecd.org/els/family/database.htm>, retrieved 2021-08-24.

<sup>3</sup>Reported by OECD. <https://www.oecd.org/els/family/database.htm>, retrieved 2021-08-24.

analysis by combining two different quasi-experiments and datasets.

The main part of the paper uses a large administrative dataset about the population of mothers with four-year-old children living in Switzerland. We link the Population and Households Statistics (STATPOP) with data about the Old-Age and Survivors' Insurance (OASI). Moreover, we have collected data about the implementation year of the policy, the birthday cut-off dates per year and the obligation to offer kindergarten years. This data stem from the cantonal departments of education, the Swiss Conference of Cantonal Departments of Education (EDK), and the cantonal laws. The State Secretariat for Economic Affairs (SECO) provided information about the cantonal unemployment rate on their homepage. Again we link this cantonal information to the administrative data. Methodologically, we exploit the random assignment at the birthday cut-off for entering the kindergarten in the same year (= treatment) versus the following year (= control) and apply a sharp Regression Discontinuity Design (RDD) because of the treatment rule. Since parents may postpone the kindergarten entry of their child by one year and we cannot observe the actual kindergarten entry, we rather assess the intention-to-treat (ITT) effect than the average treatment effect (ATE) at the threshold. We find that the mandatory kindergarten increases maternal employment by 1 percentage point on average over the outcome periods 2010 to 2017, this result is statistically significant at the 10% level. Likewise, the total annual work income rises by 1,337 CHF on average, statistically significant at the 1% level and the probability of earning the income from dependent employment increases by 1 percentage point, statistically significant at the 5% level. The effects are driven by low-income mothers and rather older mothers, who probably finished already their family planning.

A second evaluation strategy complementing the RDD approach exploits spatial variation and staggered implementation of the treatment, uses survey data and applies a Difference-in-Differences (DiD) approach. The survey data stems from the Swiss Household Panel (SHP) containing information about the mothers, their children, and the household. We link these data with the cantonal information like in the main part of the paper. To support the parallel trend assumption, we have data for at least six pre-treatment years in order to run a placebo test with unaffected years. Furthermore, we use fake treatment groups to conduct another placebo test. Methodologically, our paper distinguishes itself from the existing literature by using a double

machine learning approach as a robustness check. The results suggest that the implementation of mandatory kindergarten for four-year-old children affects mothers' market behaviour: We find that the maternal labour force rate increases after the implementation of the policy by 9 percentage points, statistically significant at the 5% level. Regarding employment levels, we detect a rise in part-time rates by 13 percentage points among mothers, statistically significant at the 1% level. The probability of earning the income from dependent employment increases by 9 percentage points, which is statistically significant at the 10% level. The findings are robust to sensitivity checks but imprecisely estimated.

This paper is related to a large and growing body of research assessing the effect of childcare policies on the employment behaviour of mothers. One strand of literature examines whether the provision of discounted childcare like in Austria ([Kleven et al., 2020](#)), Norway ([Hardoy and Schøne, 2015](#)), Sweden ([Lundin et al., 2008](#)), and the Netherlands ([Bettendorf et al., 2015](#)) impacts maternal employment. Further research work analyses the expansion of childcare places as done in Spain ([Nollenberger and Rodríguez-Planas, 2015](#)), Italy ([Carta and Rizzica, 2018](#)), Germany ([Geyer et al., 2015](#)), and Norway ([Kunze and Liu, 2019](#), ([Eckhoff Andresen and Havnes, 2019](#)) or a legal entitlement for it in Germany ([Bauernschuster and Schlotter, 2015](#)). The evaluation of these policies suggests a significantly positive effect on mothers' labour force participation, provided a low labour force participation rate and a small extent of childcare prior to the reform. Mothers' working hours were increased by a reduction in day-care fees in Germany ([Huebener et al., 2020](#)) and an extension from a half-day to a full-day kindergarten in Canada ([Dhuey et al., 2019](#)).

Additionally, this paper is related to research strands studying the impact of school laws on mothers labour market outcomes. In the U.S., [Gelbach \(2002\)](#) studies the effect of school enrolment, whereas [Barua \(2014\)](#) exploits the variation in school entry laws. In Norway, a decrease in the school starting age from seven to six years is analysed by [Finseraas et al. \(2017\)](#). The common result of these studies is a significant and positive effect of schooling on mothers' labour supply. Whereas the birth of a child shortly after the birthday cut-off leads to a negative impact on mothers' labour supply ([Zhu and Bradbury, 2015](#)). [Goux and Maurin \(2010\)](#) examine the effect of a decrease in the school starting age in France and find only an effect for the subgroup

of single mothers. The length of a school day also influences the labour market decisions of mothers: Thus, the extension from half-day to full-day school has a positive effect on the labour force participation of mothers (Berthelon et al., 2015 and Padilla-Romo and Cabrera-Hernández, 2019) and their hours worked (Padilla-Romo and Cabrera-Hernández, 2019).

The contribution of this paper is to narrow the gap between the childcare and the school law literature: In contrast to the former, we evaluate a mandatory kindergarten and not a voluntary decision of the parents to send their children to childcare. Furthermore, the Swiss kindergarten is free of charge, whereas childcare, like day care centres, is costly. In contrast to the majority of the school entry age literature<sup>4</sup>, we address younger children in the pre-school age. Furthermore, we contribute to the literature analysing countries with a rather high maternal labour force participation rate.

The remainder of this paper is organized as follows: In the next section, we provide information about the preschool policy in Switzerland. Thereafter, we present the data sources, descriptive statistics, and define the subsample. Then, we discuss the empirical strategy and the double machine learning approach. In the following section, we report the results and the robustness checks. Finally, we draw a conclusion of the empirical analysis.

## 2 Institutional background: Kindergarten reform

This section provides an overview of the initial situation and the reform of education policy in Switzerland. Switzerland regulates education policy at the cantonal level, consequently, the quality and quantity of education varies from canton to canton. For example, the school curriculum, entrance age, and the number of school years differ across the 26 cantons (SWI, 2006). A national referendum about an educational reform took place in 2006, which was accepted by 86% of the voters. One year later, an inter-cantonal "HarmoS" concordat was established, fifteen cantons entered into the concordat, while seven cantons rejected it.<sup>5</sup> The main goal of the concordat is to harmonize the mandatory school education among the cantons. This agreement

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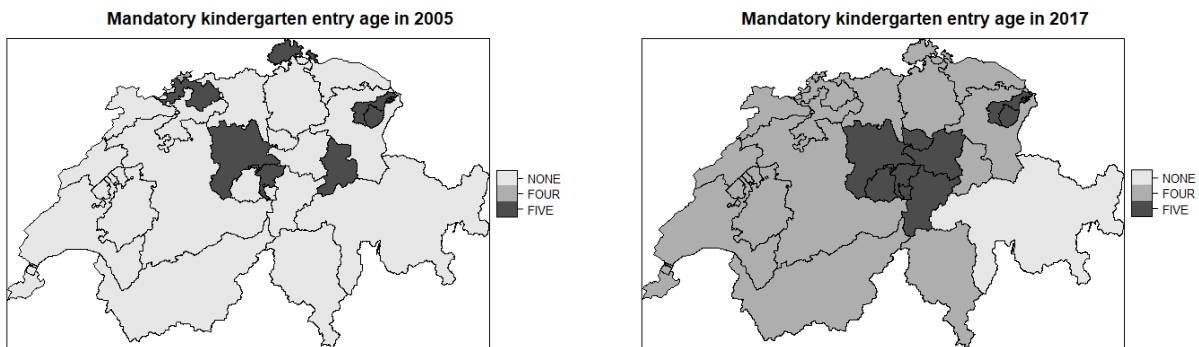
<sup>4</sup>Goux and Maurin (2010) analyse two-year-old children.

<sup>5</sup>as of 2019

also includes a decrease in the age for mandatory school attendance. All children turning four before the first of August must enter kindergarten ([Schweizerische Konferenz der kantonalen Erziehungsdirektoren, 2007](#)). But also in this case, neither the implementation year nor the shift of the birthday cut-off date is regulated on a central level.

Even before this inter-cantonal reform, several cantons had started to implement mandatory kindergarten for four-year-olds. Figure 2.1 gives an overview of the spread of this policy. Basel-Stadt was the first canton in which four-year-old children are supposed to enter kindergarten in 2005, the other cantons followed over time. In 2017, seventeen of twenty-six cantons had implemented an obligation to attend kindergarten for four-year-olds.<sup>6</sup> Eight cantons have implemented mandatory kindergarten for five-year-olds.<sup>7</sup> In the canton Grisons, children are generally not obliged to attend kindergarten. Not directly observable from Figure 2.1 is the change in the policy in Basel-Land, Glarus, and Schaffhausen. These cantons have implemented a mandatory kindergarten for five-year-old children at first and expanded the policy to four-year-olds.

Figure 2.1: Expansion of the kindergarten reform



Source: The map of Switzerland and its subdivisions is downloaded from the GADM homepage<sup>8</sup>, the information about the implementation of the mandatory kindergarten was collected by ourselves.

The mandatory kindergarten is part of early childhood education, which takes place prior to primary education ([OECD, 2015](#)) and corresponds to level 0 in the International Standard Classi-

<sup>6</sup>Aargau, Basel-Land, Basel-Stadt, Bern, Fribourg, Geneva, Glarus, Jura, Neuchâtel, St Gallen, Schaffhausen, Solothurn, Thurgau, Ticino, Vaud, Valais, and Zurich.

<sup>7</sup>Appenzell Outer-Rhodes, Appenzell Inner-Rhodes, Lucerne, Nidwalden, Obwalden, Schwyz, Uri, and Zug.

fication of Education (ISCED) (Eurydice, 2016/17). In the German- and Italian-speaking parts of Switzerland, kindergarten is rather a separate institution, whereas in the French-speaking part, kindergarten is represented as two additional classes in primary school (Schweizerische Konferenz der kantonalen Erziehungsdirektoren, 2016b). Kindergarten is, similar to a school curriculum, divided into lessons. The length and the numbers of lessons vary from canton to canton: At the lower bound is the canton Valais, where four-year-old children are supposed to attend 12 lessons per week.<sup>9</sup> Each lesson is 45 minutes long, so the children spend in sum 9 hours per week in the kindergarten. At the upper bound of kindergarten hours is the canton Ticino, where children attend the kindergarten on four days from the morning till the afternoon and an additional half-day.

Table 3.B.2.1 in the Appendix compares the implementation date of mandatory kindergarten (the so-called "Besuchsobligatorium") and the mandatory offer of kindergarten places (the so-called "Angebotsobligatorium") for four-year-old children. The second column reveals the year in which each canton has implemented a mandatory kindergarten for four-year-olds, if ever. The third column lists the year of the obligation to offer kindergarten. In some cases, the exact implementation year is not known because of data limitation, yet we know that the obligation to offer kindergarten exist at least since 2007. We can distinguish between the implementation behaviour of four groups: Cantons that have implemented mandatory kindergarten (Table 2.1) have either implemented the mandatory offer simultaneously or prior to the reform.<sup>10</sup> Cantons which have a voluntary kindergarten (Table 2.2) have either implemented the mandatory offer or not.

The attendance of the kindergarten is free of charge for both, the mandatory kindergarten and the voluntary kindergarten (Schweizerische Konferenz der kantonalen Erziehungsdirektoren, 2008a). Hence, the implementation of the kindergarten reform is not a subsidy, merely an obligation. According to a cantonal survey by the Swiss Conference of Directors of Education, almost 77% of four-year-old children attended kindergarten in the school year 2007/08 (Schweizerische Konferenz der kantonalen Erziehungsdirektoren, 2008b), although most cantons had not yet

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<sup>9</sup>EDK Kantonsumfrage 2016/17

<sup>10</sup>Since the canton Basel-Stadt has implemented the kindergarten policy already in 2005, we cannot ascertain whether the mandatory offer is implemented at the same time or before.



Table 2.1: Mandatory kindergarten &amp; Mandatory offer

	Mandatory offer	
	Simultaneously	Prior
Mandatory kindergarten	Aargau, Bern, Fribourg, and Valais	Basel-Land, Geneva, Glarus, Jura, Neuchâtel, St Gallen, Schaffhausen, Solothurn, Ticino, Thurgau, Vaud, and Zurich

Table 2.2: Voluntary kindergarten &amp; Mandatory offer

	Mandatory offer	
	Implemented	Not implemented
Voluntary kindergarten	Appenzell Outer-Rhodes, Appenzell Inner-Rhodes, Lucerne, Nidwalden, and Uri	Obwalden, Schwyz, and Zug

implemented the obligation. The aim of the kindergarten policy is twofold: Children’s development should be promoted, but also the compatibility of family and work should be encouraged ([Schweizerische Konferenz der kantonalen Erziehungsdirektoren, 2014b](#)). In this paper, we analyse the latter and examine whether the obligation to attend kindergarten at the age of four affects mothers’ work behaviour.

### 3 Hypothesis Development

As the literature review reveals, childcare reforms as well as a lower school starting age affect mothers’ work behaviour. In the former case, a subsidy either reduces the financial burden or provides more childcare places. In the latter case, the effect comes from the obligation to attend school at a younger age. In contrast, we evaluate the impact of the mandatory kindergarten on mothers’ work behaviour. Therefore, the analysis combines features from both literature strands: The kindergarten is an obligation and is free of charge like in the school starting age literature. On the other side, we evaluate a policy that affects children in the childcare age<sup>11</sup>. This chapter presents the hypothesis and the possible channels.

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<sup>11</sup>four-years-olds

In the main part of the paper, we evaluate the effect of the mandatory kindergarten on maternal labour market behaviour within a specific age window: We compare mothers with slightly older children who were born prior to a specific birthday cut-off and are supposed to enter kindergarten in the same year (= treated) with mothers whose children are slightly younger, born after a specific birthday cut-off and who are supposed to enter kindergarten in the following year (= control).<sup>12</sup> Mothers with a slightly younger child may spend their time by taking care of their child or must organize a (costly) childcare to allocate their time to work. Contrarily, mothers, whose child is supposed to enter kindergarten, can spend their time on other activities, for example, labor market participation. Consequently, a mother who would take care of her child in the absence of the reform can rearrange her time: Following the model of the allocation of time [Becker \(1965\)](#), the mother can spend more time in market work and/or leisure activities. [Swart, van den Berge and van der Wiel \(2019\)](#) find that mothers increase their labour supply after the school enrolment of their youngest child, given that they take care of their children prior to the school start. Therefore, we expect in our setting that only one subgroup of mothers increase their labour supply: Mothers whose children are supposed to enter kindergarten, but who would take care of them if no policy is implemented. Concerning the total effect, we expect a positive, but small effect because the effect is zero for mothers who would send their child to kindergarten anyway.

In a second evaluation approach, we evaluate the same research question, yet the treatment is defined on the cantonal instead of the individual level. Some cantons<sup>13</sup> have implemented a mandatory kindergarten (= treated) and others offer only a voluntary kindergarten (= control) in a specific year. Mothers living in a canton with voluntary kindergarten may spend their time by taking care of their child or can send their child to kindergarten to be able to work. A condition for the use of the voluntary kindergarten is the availability of sufficient places. Due to a shortage of data, we cannot determine whether the demand equals the supply of kindergarten places prior to the reform. However, we know that the obligation to offer kindergarten places (the so-called "Angebotsobligatorium") was not enforced in all cantons. In our dataset 42.15% of the cantons had not implemented an obligation of kindergarten places when kindergarten

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<sup>12</sup>Exception Ticino: kindergarten offer for 3-year-old children.

<sup>13</sup>The number of cantons varies over time

attendance was voluntary. We can conclude that the kindergarten reform at least guaranteed enough kindergarten places. In case the mother lives in a canton with a mandatory kindergarten, the child is supposed to enter kindergarten and the mother can allocate her time between leisure and market work. We rely on the same argumentation as in the main part above and hypothesize that mothers whose children are supposed to enter kindergarten, but who would take care of them conditional no policy would be implemented, increase their labour supply.

## 4 Data

### 4.1 Data for the Regression Discontinuity Design

Our main evaluation strategy is a non-parametric regression discontinuity design (RDD) to examine the effect of the reform at the birthday cut-off for entering the kindergarten in the same versus in the following year. It relies on a large administrative dataset including the universe of mothers with four-year-old children living in Switzerland, the so-called Population and Households Statistics (STATPOP). We link the latter with data coming from the old-age and survivors' insurance (OASI) in Switzerland by a unique identifier. STATPOP provides information on the stock and structure of the Swiss resident population from 2010-2018. This enables us to examine the population of mothers whose youngest child is four-year-old and living in a canton with mandatory kindergarten. Furthermore, the data contain personal characteristics from the mother, their children, and the father of the four-year-old child. Two variables are central for our regression discontinuity approach (RDD): Firstly, the canton of residence to determine whether the four-year-old lives in a canton with mandatory kindergarten in the current year. Secondly, the exact date of the birth of the four-year-old determines whether a child was born prior (= treated) or after (= control) a specific birthday cut-off date. Additionally, other personal characteristics such as the marital status, country of birth, resident permit of the mother, father, and the children are also contained in the dataset.

The OASI data contain information about the income, unemployment benefits, disability indemnities for every insured person in Switzerland from 2010 - 2017. Based on this data, we

calculated the "Income from work" as the total income minus transfer payments (for instance, unemployment insurance and welfare benefits) and minus other indemnities (like the compensation for military service) for the following subgroups: i) employees whose employer resides in Switzerland, ii) voluntary insured employees whose employer is not liable for contributions in Switzerland, and iii) self-employed persons<sup>14</sup>. Subsequently, we generated the variable "In labor force", which is 1 if a person earns income from work or if she receives unemployment benefits and 0 otherwise. We calculated the dummy "Out of labour force" as 1 minus the value of "In labour force". Furthermore, we constructed the dummy variable "Employed", which turns 1 if the person earns an income from work and participates in the labor market and is 0 otherwise. Vice versa, a person is indicated as "Unemployed" if she participates in the labour force but is not employed. The variable "Income from dependent employment" gives the sum of the income for mothers working in dependent employment, whereas the variable "Income from dependent employment (binary)" indicates whether a mother works in dependent employment. In the last step, we matched this database with cantonal information about the unemployment rates, the yearly birthday cut-off date of the mandatory kindergarten, and an indicator of whether a canton joined the "HarmoS" concordat.

The database contains a total of 687,749 mothers with at least one four-year-old child between 2010 and 2018. Since the last period in the OASI data is 2017, we drop the STATPOP observations from 2018 such that 606,434 observations remain. Another data restriction is based on the empirical literature, because childcare seems to not affect the mothers' labour market outcomes when there is a younger sibling of the four-year-old child in the household (see for example [Berlinski and Galiani \(2007\)](#), [Fitzpatrick \(2012\)](#), and [Gelbach \(2002\)](#)). We overcome this concern by restricting the subsample to mothers whose youngest child is four years old, which decreases the sample size to 360,234 mothers. Three cantons with mandatory kindergarten did not determine a birthday cut-off date, hence we exclude observations from Aargau, Fribourg prior to 2013, and all the municipalities of the canton Bern except the federal city of Bern, after that 289,728 observations are in the sample. Furthermore, we drop observations with negative income entries<sup>15</sup> in the OASI data and keep 289,631 observations. We exclude observations,

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<sup>14</sup>excluding agriculture

<sup>15</sup>f.ex. due to adjustment entries

which live in cantons without a mandatory kindergarten for four-years-old. Since two sources<sup>16</sup> reported two consecutive days as birthday cut-off dates in Basel-Stadt, we took the later cut-off date and excluded observations on the previous day from our analysis<sup>17</sup>. Finally, 201,993 observations remain in our evaluation dataset.

Figure 4.1 provides a timeline for the measurement of the key variables in our analysis, with  $t$  denoting a specific year. The running variable will be henceforth denoted by  $D$  and depicts whether the child turns four prior ( $D = 1$ ) or after the cut-off date ( $D = 0$ ), measured in the baseline period ( $t = 0$ ). The outcome periods start in the year the child turns four ( $t = 0$ ) and end five years later ( $t = 5$ ). Personal characteristics (e.g. number of children), are measured one year before the child turns four ( $t = -1$ ).

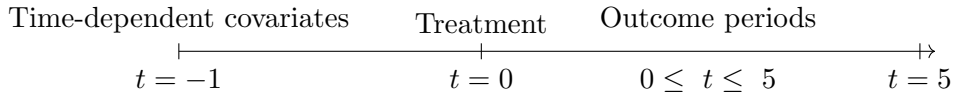


Figure 4.1: Timeline of measured variables

We check the balance of labour market relevant covariates as suggested in Lee (2008), by running several RDD estimations where each covariate serves as an outcome. Table 4.1 reports the sample mean, the balance checks, and the relative number of missings for each variable at the threshold. Mothers, fathers and cantonal covariates are balanced, which points to randomization at the threshold. We also see from the table that mothers close to the threshold have on average 1.87 children, are predominately born in Switzerland and live in a partnership. Furthermore, 73% of these mothers are employed, earn on average 32,343 Swiss francs (CHF) per year, which mainly stems from dependent employment. Fathers labor market characteristics show with 90% a very high employment rate, an annual mean gross income of 93,193 CHF, and 86% of this income comes from dependent employment. Moreover, we see a rather low mean unemployment rate (3.6%) and a high probability that the canton participates in the HarmoS concordat (94%).

<sup>16</sup>see "Regierungsratsbeschluss betreffend Stichtag für die Einschulung für die Schuljahre 2011/12 bis 2015/16 (§56 Abs. 1 Schulgesetz)" and "EDK/IDES-Kantonsumfrage" for the years 2011 to 2016.

<sup>17</sup>to run a Donut-RDD for this canton

Table 4.1: Descriptive statistics

	Sample mean	Coefficient	Standard error	P-value	Relative number of missings in %
<i>Mother's characteristics</i>					
Number Children	1.87	0.00	0.02	0.93	0.00
Born in Switzerland	0.52	0.00	0.01	0.99	2.76
<i>Nationality</i>					
Resident permit B	0.11	-0.00	0.01	0.88	6.70
Resident permit C	0.26	0.01	0.01	0.59	6.70
Other resident	0.63	-0.00	0.01	0.99	6.70
<i>Age</i>					
Age	35.62	0.01	0.14	0.92	6.70
<i>Partnership</i>					
In Partnership	0.82	-0.01	0.01	0.240	6.70
Not in Partnership	0.14	0.01	0.01	0.21	6.70
Terminated Partnership	0.05	0.00	0.01	0.92	6.70
<i>Mother's labour market characteristics</i>					
Employed (binary)	0.73	0.00	0.01	0.72	5.90
Unemployed (binary)	0.01	0.00	0.00	0.75	5.90
Out of labor force (binary)	0.26	0.01	0.01	0.67	5.90
Total income from work (in CHF)	32,343.25	-1,330.49	932.60	0.15	5.90
Income from dependent employment (in CHF)	30,806.61	96.418	1,045.20	0.93	5.90
Income from dependent employment (binary)	0.70	-0.01	0.01	0.53	5.90
<i>Father's labour market characteristics</i>					
Employed (binary)	0.90	0.00	0.01	0.84	5.90
Unemployed (binary)	0.01	0.00	0.00	0.97	5.90
Out of labor force (binary)	0.09	0.00	0.01	0.86	5.90
Total income from work (in CHF)	93,192.48	-470.86	2,477.32	0.85	5.90
Income from dependent employment (binary)	0.86	0.01	0.01	0.49	5.90
Income from dependent employment (in CHF)	87,861.57	-52.86	2,550.93	0.98	5.90
<i>Cantonal characteristics</i>					
HarmoS member	0.94	0.00	0.01	0.98	0.00
Unemployment rate	3.55	0.04	0.03	0.25	0.00

Sources: STATPOP (2010 - 2017) and OASI (2010 - 2017); calculations are done by ourselves

Table 4.2 reports the balance checks for cantonal dummies at the threshold. Due to the different birthday cut-off dates in the German- and the French-speaking part of the canton Valais (Upper Valais and Central/Lower Valais), we analyze these regions separately for the canton Valais. We find that the cantonal dummies are balanced around the birthday cut-off with two exceptions: Basel-Stadt that implemented as the first canton ever the mandatory kindergarten and Ticino, which is the only canton implementing mandatory kindergarten offer for 3-year-old children, are unbalanced around the birthday cut-off.

Table 4.3 shows the mean outcomes per period for the whole sample. We see that the outcome means are rather stable over time: On average 76% of mothers work in the labour

Table 4.2: Descriptive statistics: Cantonal dummies

	Sample mean	Coefficient	Standard error	P-value
Basel-Stadt	0.04	-0.00	0.01	0.60
St Gallen	0.10	-0.01	0.01	0.59
Thurgau	0.06	-0.00	0.01	0.98
Zurich	0.32	0.00	0.02	0.81
Fribourg	0.05	-0.00	0.01	0.80
Geneva	0.10	0.01	0.01	0.16
Glarus	0.01	-0.00	0.00	0.60
Neuchâtel	0.04	0.00	0.01	0.76
Basel-Land	0.05	-0.02	0.01	0.01
Jura	0.01	-0.00	0.00	0.40
Solothurn	0.04	-0.01	0.01	0.30
Bern	0.02	0.00	0.00	0.87
Vaud	0.12	0.00	0.01	0.67
Schaffhausen	0.01	0.00	0.00	0.11
Ticino	0.03	0.01	0.00	0.00
Upper Valais	0.01	0.00	0.00	0.91
Central and Lower Valais	0.02	-0.01	0.01	0.37

Sources: STATPOP (2010 - 2017); calculations are done by ourselves

market and almost all of them are employed. They earn an average annual gross income of 33,412 CHF, including other non-working income (like recipients of daily sickness and accident benefits) they have a slightly higher income of 34,210 CHF, and including the indemnities leads to a total average annual gross income of 35,008 CHF.

Table 4.3: Mean outcomes per period

	Mean					
	Period 0	Period 1	Period 2	Period 3	Period 4	Period 5
Employed (binary)	0.75	0.76	0.76	0.77	0.77	0.76
Unemployed (binary)	0.01	0.01	0.01	0.01	0.01	0.01
Out of labor force (binary)	0.24	0.23	0.23	0.22	0.22	0.23
Total income from work	33,412.11	33,953.14	34,429.04	35,022.00	35,371.85	35,216.20
Income from dependent employment (binary)	0.72	0.73	0.73	0.74	0.74	0.74
Income from dependent employment	31,883.95	32,364.92	32,828.14	33,449.17	33,873.49	34,001.38

Sources: OASI (2010 - 2017); calculations are done by ourselves

## 4.2 Data for the Difference-in-Differences Approach

For the second evaluation strategy of the paper, we link data from different sources. Firstly, we use survey data from the Swiss Household Panel (SHP). This panel starts in 1999 and has drawn households by NUTS II regions. The sample is representative with respect to social groups across Switzerland. In the years 2004 and 2013 refreshment samples are added. The main aim of the SHP is to follow the participants over a longer period of time in order to study social change (Voorpostel et al., 2017). For this reason, each person has a unique identifier (id). In our analysis, we use data about mothers' demographic variables and their labour market behaviour. This information comes from the individual dataset. The number of children in the household and the residential canton are provided by the household dataset. The birth month and year of birth of the children as well as the id number of the mother stem from the grid dataset.

Secondly, data concerning the mandatory kindergarten entry age, the year of policy implementation, and the birthday cut-off dates are collected from the cantonal departments of education, the cantonal laws, and the Swiss Conference of Cantonal department of education (EDK). Furthermore, the EDK conducts a yearly survey among the cantons. From these documents, we extract the information about the obligation to offer kindergarten. However, this information is only available for the time span between 2007 and 2017. The EDK provided also data about the member cantons of the HarmoS concordat. Thirdly, cantonal data about the unemployment rate in each year was provided by the Staatsekretariat für Wirtschaft (SECO). Information about the official languages in each canton stems from cantonal laws.

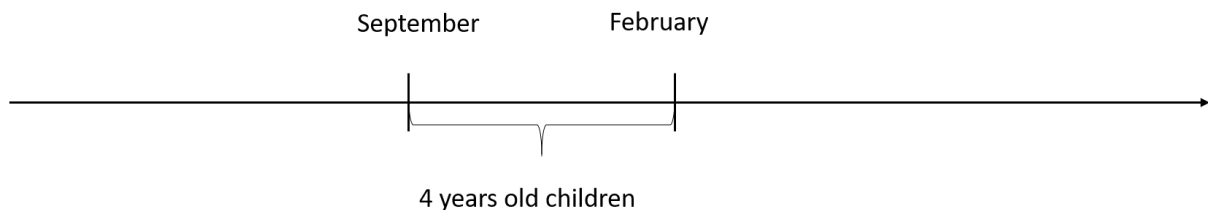
To get one dataset, we merge all waves of the individual SHP data and the unique grid dataset by the identification number of a person. We use the same procedure for all waves of household data. In this case, the unique identifier is the household number. Then we merge this data with the masterfile. Afterwards, we merge the masterfile with the other collected data by canton and year. This approach was not possible for the canton Valais because the birthday cut-off dates differ between the German-speaking part (Upper Valais) and the francophone part (Central and Lower Valais). Therefore, we request the community numbers from the SHP additionally. We



matched these administrative numbers with the two parts of the canton Valais. Subsequently, we merge the data about the kindergarten policy with this file and append it to the master file.

In a next step, we restrict the sample to mothers whose youngest child is four years old between September and February, living in the same household for the years 2004 to 2017. The reasons for our approach are threefold: First, the empirical literature finds no effect for mothers with younger children in the same household. See for example [Berlinski and Galiani \(2007\)](#), [Fitzpatrick \(2012\)](#), and [Gelbach \(2002\)](#). Second, the birthday cut-off dates differ from canton to canton and from year to year. Like table [3.B.1.1](#) in the Appendix shows, the earliest birthday cut-off date ever was the 28th of February and implemented in the German-speaking part of the canton Valais which is called "Upper Valais". Whereas the latest birthday cut-off date ever (31.08.) was implemented in the French-speaking part of the canton Valais, the so-called "Central and Lower Valais". To get a constant treatment and control group, we define the birthday cut-off as a time span from the latest ever implemented birthday cut-off to the earliest ever implemented birthday cut-off as in Figure [4.2](#).

Figure 4.2: Definition of the age span



Since we use this time span, we exclude rather older children from our sample. Table [4.4](#) shows the treatment and control group: The age of the children is the same in both groups: They turn four before September and turn five after February. However, the children differ by their canton of residence. The treated children live in a canton in which the mandatory kindergarten for four-year-olds is implemented. Whereas the children in the control group live in a canton in which none or mandatory kindergarten for five-year-olds is implemented.

Third, Basel-Stadt was the first canton that implemented mandatory kindergarten for four-

Table 4.4: Treatment and control group

	Treatment	Control
Age & birthday cut-off	Children turning four before September and five after February	Children turning four before September and five after February
Canton	Mandatory kindergarten for four-year-old children implemented	None or mandatory kindergarten for five-year-old children implemented

year-olds. To have one pre-period, we use the time span from 2004 to 2017 (last year with available data). After this restriction procedure, we end up with 953 observations, out of which 394 observations are treated and 559 are part of the control group.

Table 4.5 provides the descriptive statistics for the treatment and control group separately. The latter contains never treated observations as well as observations that are still in the pre-period, yet treated later on. Since the treatment is implemented over time in the cantons, the number of observations in the treatment group is smaller than in the non-treated group. Consequently, the treated cases are observed later on average. Considering mothers' characteristics, the average age of a mother whose child is treated is 39 years old, whereas mothers of non-treated children are on average younger. Mothers in the treatment group are also higher educated than the control group. A value of 6 represents A-level as the highest degree.

Almost 90% of mothers are married, yet mothers are more likely to be single in the treated group. Furthermore, the share of foreign mothers is higher in the treated group. The average political attitude of mothers is at the center of the political spectrum and does not differ significantly between the groups. The self-reported health status is good, yet better among the treated mothers. Two children live in an average household. The cantonal characteristics reveal an unemployment rate of 3.31% on average in the treated observations. There is no statistically significant difference in the control group. However, the treated cantons are more likely to be a HarmoS member (89%) and less likely to have implemented one kindergarten year (4%) over time. The second part of table 4.5 gives an overview of the descriptive statistics of the outcome variables. Mothers in the treatment group are more likely to take part in the labour force, have a

Table 4.5: SHP: Descriptive statistics

	Treated		Non-treated		Mean difference	p-value
	Mean	Std.dev	Mean	Std.dev		
	Time					
Year	2014.09	2.52	2008.11	3.52	5.98	0.00
	Control variables					
	mean	std.dev	mean	std.dev	mean difference	p-value
<i>Mothers' characteristics</i>						
Age	39.17	4.28	38.12	3.86	1.05	0.00
Highest education	6.53	2.84	5.91	2.72	0.62	0.00
Married	0.86	0.35	0.89	0.31	-0.03	0.11
Single	0.10	0.30	0.04	0.19	0.06	0.00
Foreign	0.17	0.37	0.13	0.34	0.04	0.10
Political attitude	4.51	2.02	4.62	1.87	-0.11	0.38
Self-reported health status	1.85	0.60	1.95	0.59	-0.10	0.02
Number of children	2.14	0.78	2.18	0.84	-0.04	0.45
<i>Cantonal characteristics</i>						
Unemployment rate	3.31	0.92	3.26	1.31	0.05	0.48
Mandatory kindergarten for 5-year-olds	0.04	0.19	0.23	0.42	-0.20	0.00
HarmoS member	0.89	0.31	0.69	0.46	0.20	0.00
	Outcome variables					
Out of labour force	0.18	0.38	0.23	0.42	-0.05	0.04
Unemployed	0.02	0.12	0.01	0.11	0.00	0.73
Employed	0.81	0.39	0.76	0.43	0.05	0.06
Part-time employed	0.77	0.42	0.70	0.46	0.07	0.02
Full-time employed	0.04	0.20	0.06	0.24	-0.02	0.26
Income from dependent employment	0.78	0.42	0.72	0.45	0.06	0.04
Number of observations	394		559			

Source: Swiss Household Panel (SHP)

higher probability of being employed, work more often in part-time work, and earn their income more frequently from dependent employment in comparison to the control group. There are no statistical differences in the probability of being unemployed or working full-time.

## 5 Econometric approach

### 5.1 The Regression Discontinuity Design

In this section, we discuss the two methods used for analysing the effect of mandatory kindergarten. The first part of the paper uses a Regression Discontinuity Design (RDD) for evaluating the effect of mandatory kindergarten on mothers' labor market outcomes. The setting of our main part in the paper implies a sharp RDD, because of the following rule of the treatment (D): All children who turn four prior to or exactly at the cut-off are supposed to enter kindergarten in the current year ( $D=1$ ), whereas children who turn four after the cut-off are supposed to enter

kindergarten in the following year ( $D=0$ ). In this case, we identify the average treatment effect (ATE) at the threshold, (see [Lee and Lemieux \(2010\)](#)), i.e. among mothers, whose children are born at the cut-off. Due to this rule, it is not possible to observe children born on the same day, simultaneously in the treatment and control group. To make the treatment and control group as comparable as possible in terms of covariates, we analyse only those observations within a specific bandwidth around the cut-off date.

One concern of identifying the ATE at the cut-off in our setting is the so-called "red shirting behaviour", i.e. parents postpone the kindergarten entrance of their children by one year. Since there is no micro-data about kindergarten attendance in Switzerland, we can neither evaluate whether red shirting takes place nor instrument the kindergarten attendance (see for example [Hahn et al. \(2001\)](#)) if this is the case. Therefore, we can only identify the intention-to-treat-effect (ITT) in our setting.

The identification of the effect relies on the assumption that the potential outcomes must be continuous around the cut-off ([Hahn et al., 2001](#)). This assumption implies the comparability of mothers in their characteristics on both sides of the cut-off. In our setting, the assumption holds if the difference between the cut-off date and children's birth date is as good as randomly assigned at the cut-off. In other words, the continuity assumption is fulfilled if parents cannot precisely sort their children in the treatment or the control group and is violated otherwise.

In our case, the running variable specifies the difference between the birth date of the child and the cut-off date and is therefore a discrete value. A standard test for checking the plausibility of the continuity of the potential outcomes at the cut-off is the Mc Crary test which analyses the continuity of density of the running variable at the cut-off. A discontinuity of the density at the cut-off would point to selective sorting (also known as bunching) and therefore likely violate the continuity of potential outcomes. This test works well if the running variable is continuous, yet it is in general inconsistent when the running variable is discrete. For this reason, we use Frandsen's test instead, which delivers a consistent result even in the presence of a discrete running variable. Frandsen's test uses only data points, which are exactly at or adjoining to the threshold, whereas the Mc Crary Test extrapolates in areas away from the threshold, which

lead to inconsistency if the running variable is discrete (Frandsen, 2017).

Table 5.1 reports the results of Frandsen’s test, where  $K$  corresponds to our self-defined maximum of the probability mass function (pmf) curve which is still considered as ”non-sorting,” see Frandsen (2017).  $K$  must at least equal zero, meaning that a nonlinearity is not allowed in the pmf (Frandsen, 2017), which represents our first specification. Furthermore, to allow for a non-linearity,  $K$  equals 0.1 and 0.2, as robustness tests in the following two specifications. The associated p-value indicates whether there is a statistical difference in the pmf of both sides of the threshold. Table 5.1 indicates no significant deviation from the expected density of the running variable, regardless of the specification of  $K$ . In short, we do not find any sorting behaviour around the cut-off.

Table 5.1: Frandsen’s test

K	p-value
0	0.49
0.1	0.88
0.2	1.0

Sources: STATPOP (2010 - 2017) and OASI (2010 - 2017);  
calculations are done by ourselves

If the continuity assumption holds, the ATE (or in our case the ITT) at the threshold is non-parametrically identified given the bandwidth approaches zero, see Hahn et al. (2001). Equation (5.1) presents the identification result, where  $\gamma$  gives the parameter of interest,  $Y_d$  denotes the potential outcome (e.g. hypothetical employment) under treatment status  $d \in \{1, 0\}$ ,  $R$  the running variable, and  $r$  the cut-off value ( $r = 0$ ).

$$\gamma = E[Y_1 - Y_0 | R = r] \tag{5.1}$$

We use the following local linear regression to estimate the parameter of interest within a specific data window around the cut-off of the running variable:

$$Y_i = \alpha + \beta_0 D_i + \beta_1 R_i + \beta_2 R_i * D_i + \beta_3 Z_i + \epsilon_i \tag{5.2}$$

$Y_i$  denotes the outcome (e.g. labour force status),  $D_i$  represents the treatment status, and  $R_i$  the running variable centered around the cut-off ( $=0$ ) of individual  $i$ .  $\beta_0$  is the average treatment effect of the mandatory kindergarten on mother's labour market behaviour at the threshold and  $\beta_1$  and  $\beta_2$  give the average effect of the running variable on the outcome.  $Z_i$  is a vector of additional covariates for individual  $i$ .<sup>18</sup> We control for covariates to get lower standard errors, which is possible because the covariates (e.g. maternal total income one year earlier) are good predictors of the outcomes (e.g. maternal total income one year later). For the estimation, we use the "rdrobust" command from the eponymous package for the statistical software R (Calonico et al., 2021), with a uniform kernel function and a heteroskedasticity-robust plug-in residuals variance estimator without weights. We estimate a local linear regression ( $p = 1$ ), use a local quadratic regression ( $q = 2$ ) to perform bias-correction, and use the MSE-optimal bandwidth selector where the bandwidth  $h$  is data-driven computed by the companion command "rdbwselect".

## 5.2 The Difference-in-Differences Approach

Further, we describe the econometric approach of the second evaluation strategy of the paper: The SHP is an unbalanced panel with refreshment samples in 2004 and 2013. As preschool policies are decided on the cantonal level, we apply a difference-in-differences (DiD) strategy to exploit treatment variation across cantons and over time. As there has been a staggered adoption of mandatory kindergarten across cantons over time, we use a two way fixed effects model rather than the basic DiD setting with one pre- and one post-treatment period. The DiD estimation with different timings gives a weighted average of all DiD estimators across groups and times. We use ordinary least square estimation (OLS) to estimate the results.

Equation (5.3) shows the estimated model in our empirical analysis. The subscript  $i$  denotes the individual,  $s$  the canton, and  $t$  the time.  $Y_{ist}$  is a vector of the following binary dependent variables: "Out of labour force", "Unemployed", "Employed", "Part-time employment", "Full-

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<sup>18</sup>We include all covariates listed in Table 4.1 and 4.2.

time employed”, ”Income from dependent work.”

$$Y_{ist} = \alpha + \gamma_s + \lambda_t + \delta D_{st} + \beta X'_{ist} + \epsilon_{ist} \quad (5.3)$$

The right hand side of equation (5.3) presents the independent variables. We include canton ( $\gamma_s$ ) and time fixed effects ( $\lambda_t$ ), the former controls for time-invariant differences between the cantons, the latter for differences across years which are common to all cantons.  $D_{st}$  represents the treatment dummy indicating whether the child is eligible for the treatment, i.e. mandatory kindergarten in the respective canton and the current year. The parameter  $\delta$  gives the effect we are interested in, the average treatment effect on the treated (ATT).  $\epsilon_{ist}$  corresponds to time-varying unobservables. We include a vector of included time-variant as well as time-invariant covariates ( $X_{ist}$ ) to make the common trend assumption more plausible.

The common trend assumption is the key identifying assumption in a DiD approach. Intuitively speaking, the labour market outcomes of the treatment and control group would follow the same time trend in the absence of the treatment, i.e. the mandatory kindergarten. Therefore, we need to control for those maternal and cantonal covariates which would lead to a different time trend. For instance, younger and less educated mothers are usually more affected by business cycles than older and better educated ones, such that the time trend differs among these groups. Another example represents mothers with a poorer health status who might be laid off faster than healthy mothers in times of crisis. We must also control for cantonal covariates which might affect the employment trend of treated and control groups differently like the unemployment rate measure one year prior to the treatment year. We provide placebo tests conditional on covariates in Table 6.7 and 6.8 in Section 6.2.

To check the robustness of the results, we use three specifications: First, we include socio-demographic characteristics of the mother which can affect the labour market situation. This subset of covariates is commonly used in the childcare literature. Like Felfe et al. (2016), we include mothers’ age, education, civil status, and the number of children in the household. In line with Ravazzini (2018), we also add mothers’ citizenship as control. Since Cai and Kalb (2006)

reports that a better health status leads to a rise in the likelihood of participating in the labour market, we control for mothers' self-reported health status additionally. Conservative mothers are more likely to stay out of the labour force and work less if they decide to participate in the labour market (Stam et al., 2014). For this reason, we control for mothers' political attitude. Second, we extend the former specification by adding cantonal characteristics to "control for forces that lead policies to change" Besley and Case (2000). Like Felfe et al. (2016) we use the cantonal unemployment rate as control. In the context of our study, we control for the implementation of a mandatory kindergarten for five-year-old children and the membership in the HarmoS concordat. Third, we use double machine learning to pick the most important confounders, interactions, and higher order terms from the socio-demographic and cantonal characteristics in a data-driven way.

The latter approach prevents both overfitting (too many included covariates) and omitted variable bias (too few included variables). The variable selection is done by a least absolute shrinkage and selection operator (LASSO) of the R package "hdm", developed by (Chernozhukov et al., 2016). Since we aim to do linear inference, we use the command "rlassoEffects" and estimate the results with the method "double selection", option "POST=TRUE".<sup>19</sup> This procedure selects the relevant covariates first and does inference afterwards. Table 3.B.2.2 in the Appendix gives an overview of the included covariates in each specification.

## 6 Results

### 6.1 Findings from a Regression Discontinuity Design

This chapter provides the estimated results as well as the robustness checks. Starting with the results of the paper's main part, Table 6.1 reports the ITT estimates at the threshold for pooled outcome periods when including covariates and corresponding missing dummies. It suggests the sample mean of the outcome (not only at the threshold), the effects along with standard errors and p-values, the bandwidth size, the number of observations in total as well

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<sup>19</sup>The "rlassoEffects" command provides also the option "partialling-out". We have estimated the effects with this option too. The point estimates remain the same, in some cases the standard errors differ.



as in the bandwidth. We find that mandatory kindergarten increases the probability of being employed by 1 percentage point, this effect is statistically significant at the 10% level. The effect on the total annual work income amounts to 1,336.86 CHF (which corresponds to 3,90% of the sample mean). This income effect is highly statistically significant but rather small in the effect size. Finally, the probability of earning the income from dependent employment goes up by 1 percentage point, this result is statistically significant at the 5% level. We test multiple hypotheses, which can lead to finding more statistically significant results than actually exist, the so-called false-positive rate (Benjamini and Hochberg, 1995). To overcome this concern in our paper, we apply the Benjamini-Hochberg (B-H) procedure, which results in one statistically significant effect at the 10% level.<sup>20</sup>

We check the robustness of our results by estimating the RDD approach without covariates (Table 3.B.1.2 in the Appendix) and find rather similar point estimates, yet the precision decreases. Another check consists of varying the optimal bandwidth, we multiplied this by 1.5 and 2/3 (Table 3.B.1.3 and 3.B.1.4 in the Appendix). We find that the effect sizes stay rather similar except for the effects on "Total annual work income" and "Annual income from dependent employment."

In a next step, we analyse the effects in separate and consecutive outcome periods, relative to the year the child turns four. Figures 6.1 show the results of the different outcomes, starting in period 0 (i.e. the year the child turns four) and ending in period 5. The dots depict the ITT at the threshold and the bands represent the point-wise 95% confidence intervals based on robust standard errors. We do not find any statistically significant effect for the outcomes in comparison to the pooled estimates. This might be caused by lower power due to a decrease in the number of observations. Moreover, we find unstable point estimates, both the sign and the effect size change over periods. The plots reveal further that the effects are, especially in the later periods, imprecisely estimated because of the decrease in the number of observations. Figure 3.B.1.1 in the Appendix depict the plots without covariates and shows similar results in terms of significance, point estimates, and precision.

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<sup>20</sup>R-package "sgof", command "BH", where alpha = 0.1

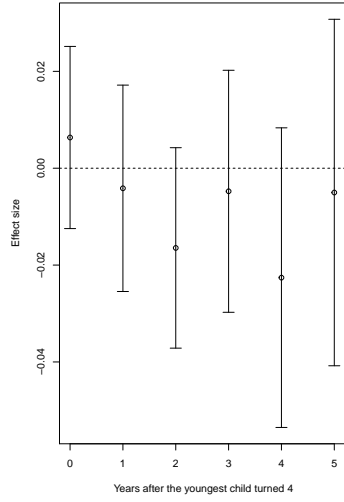
Table 6.1: RDD: Empirical results with covariates

	Sample mean	Coefficient	Standard error	P-value	Bandwidth	Observations	Observations within band-width
Out of labour force (binary)	0.23	-0.01	0.00	0.26	69.99	735,520	285,555
Employed (binary)	0.76	0.01	0.01	0.10	63.18	735,520	261,065
Unemployed (binary)	0.01	0.00	0.00	0.24	48.13	735,520	198,603
Annual work income (in CHF)	34,273.01	1,336.86	500.07	0.01	32.55	735,520	133,198
Income dependent employment (binary)	0.73	0.01	0.01	0.05	51.93	735,520	210,465
Annual income from dependent employment (in CHF)	32,515.20	150.48	331.57	0.65	56.65	735,520	232,017

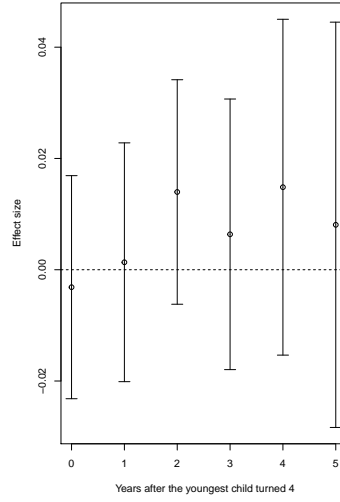
Sources: STATPOP (2010 - 2017) and OASI (2010 - 2017); calculations are done by ourselves

We subsequently analyse whether the effect of mandatory kindergarten is different for specific subgroups. Firstly, we split the sample at the median "maternal annual work income" (15,706 CHF), which is measured one year before the child turns four. Table 6.2 represents the results for mothers, who have an income lower than the median, including not working mothers, whereas Table 6.3 reports the effects for mothers earning an annual income equal or higher than the median. The findings suggest that the effects are driven by mothers whose income is lower than the median. All six effects in Table 6.2 remain statistically significant at the 10% level after applying the B-H procedure. Furthermore, our finding coincides with [Finseraas et al. \(2017\)](#) who analyse how school enrolment affects maternal labour supply finding that low-income mothers are the driver of the effect. Table 6.3 reveals that mothers who earn equal or more than the median wage, experience even a decrease of 2.3% of the median annual work income. This effect remains statistically significant after applying the B-H procedure. Since mandatory kindergarten is mainly half-day care, a possible explanation might be that mothers reduce their working hours, for example in the afternoon, to take care of their child.

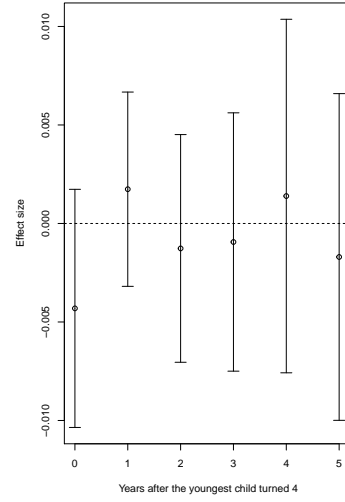
Secondly, we split the sample size in younger (age < 38, Table 6.4) and older mothers (age > 37, Table 6.5) and find that the implementation of the mandatory kindergarten has rather an effect on older mothers than younger ones. The B-H procedure supports the statistical significance at the 10% level for all four effects in Table 6.5. This finding is in line with [Nollenberger and Rodríguez-Planas \(2015\)](#) who explain that older mothers may already have reached their desired number of children and may react stronger to the implementation of the mandatory kindergarten for their youngest child.



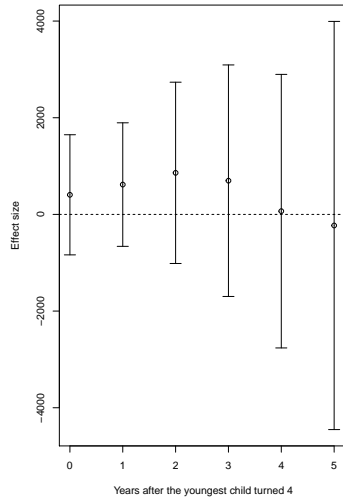
(a) Out of labour force (binary)



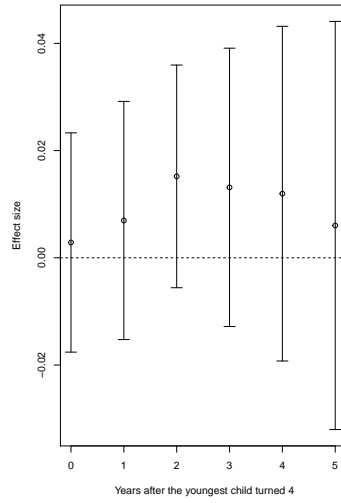
(b) Employed (binary)



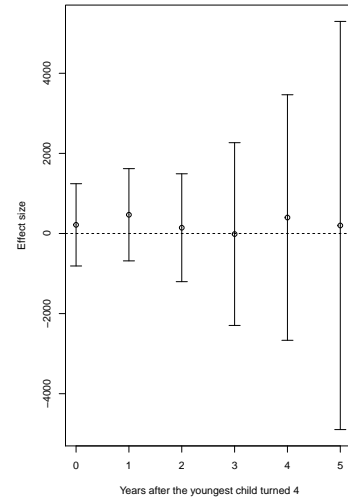
(c) Unemployed (binary)



(d) Total annual work income



(e) Income from dependent employment (binary)



(f) Income from dependent employment

Figure 6.1: RDD: Effects over years with covariates

Note: Dots represent ITTs at the threshold, bands correspond to 95% confidence intervals

Table 6.2: RDD: Heterogenous effects: Annual work income < 15,706 CHF

	Sample mean	Coefficient	Standard error	P-value	Bandwidth	Observations	Observations within band-width
Out of labour force (binary)	0.43	-0.02	0.01	0.07	43.72	367,756	88,982
Employed (binary)	0.56	0.03	0.01	0.00	51.17	367,756	105,192
Unemployed (binary)	0.01	0.00	0.00	0.09	52.04	367,756	107,357
Annual work income (in CHF)	13263.18	2840.36	687.82	0.00	32.45	367,756	66,282
Income dependent employment (binary)	0.53	0.03	0.01	0.00	45.63	367,756	93,150
Annual income from dependent employment (in CHF)	11975.20	1344.59	490.21	0.01	49.37	367,756	101,318

Sources: STATPOP (2010 - 2017) and OASI (2010 - 2017); calculations are done by ourselves

Table 6.3: RDD: Heterogenous effects: Annual work income  $\geq$  15,706 CHF

	Sample mean	Coefficient	Standard error	P-value	Bandwidth	Observations	Observations within band-width
Out of labour force	0.03	0.00	0.00	0.38	52.92	367,764	107,475
Employed (binary)	0.96	0.00	0.00	0.82	68.31	367,764	141,222
Unemployed (binary)	0.01	0.00	0.00	0.80	57.01	367,764	118,450
Annual work income (in CHF)	55282.39	-1296.93	518.08	0.01	67.57	367,764	139,264
Income dependent employment (binary)	0.93	0.00	0.00	0.99	57.03	367,764	118,450
Annual income from dependent employment (in CHF)	53054.75	-597.02	533.40	0.26	42.52	367,764	87,027

Sources: STATPOP (2010 - 2017) and OASI (2010 - 2017); calculations are done by ourselves

Table 6.4: RDD: Heterogenous effects: Age &lt; 38

	Sample mean	Coefficient	Standard error	P-value	Bandwidth	Observations	Observations within band-width
Out of labour force (binary)	0.23	-0.01	0.01	0.52	39.98	404,134	89,164
Employed (binary)	0.76	0.01	0.01	0.42	41.98	404,134	93,837
Unemployed (binary)	0.01	0.00	0.00	0.16	55.15	404,134	125,829
Annual work income (in CHF)	28,634.51	-537.23	503.83	0.29	32.49	404,134	73,391
Income dependent employment (binary)	0.74	0.00	0.01	0.67	50.90	404,134	114,070
Annual income from dependent employment (in CHF)	27,586.16	-448.19	462.97	0.33	29.20	404,134	66,923

Sources: STATPOP (2010 - 2017) and OASI (2010 - 2017); calculations are done by ourselves

Table 6.5: RDD: Heterogenous effects: Age &gt; 37

	Sample mean	Coefficient	Standard error	P-value	Bandwidth	Observations	Observations within band-width
Out of labour force (binary)	0.24	-0.02	0.01	0.03	35.83	331,386	65,354
Employed (binary)	0.75	0.02	0.01	0.06	36.09	331,386	66,996
Unemployed (binary)	0.01	0.00	0.00	0.91	55.178	331,386	101,745
Annual work income (in CHF)	41,149.32	488.04	689.56	0.48	59.91	331,386	109,256
Income dependent employment (binary)	0.72	0.03	0.01	0.00	36.38	331,386	66,996
Annual income from dependent employment (in CHF)	38,526.30	3,987.08	909.57	0.00	27.33	331,386	50,674

Sources: STATPOP (2010 - 2017) and OASI (2010 - 2017); calculations are done by ourselves

## 6.2 Findings from a Difference-in-Differences Approach

Subsequently, we report the results of the complementing DiD analysis. Table 6.6 presents the main results of the kindergarten reform for mothers with four-year-old children. We display three specifications: Firstly, "socio-demographic X" controls only for socio-demographic characteristics, secondly, "all X" controls for all characteristics represented in Table 4.5 in Section 4.2, and finally we let LASSO pick the controls "lasso picked X". The standard errors are clustered to control for cantonal specific effects in the first two specifications. However, clustering is not possible in the LASSO approach.<sup>21</sup> Therefore, we use heteroscedasticity robust standard errors in the last specification. The last column provides the number of observations "obs". The latter varies because of missing data in the outcome variables which leads to the exclusion of these observations.

Table 6.6: DiD: Empirical results

	socio-economic X			all X			lasso-picked X			obs
	effect	se	pval	effect	se	pval	effect	se	pval	
Out of labour force	-0.09	0.04	0.04	-0.10	0.04	0.02	-0.09	0.05	0.05	953
Unemployed	0.02	0.02	0.28	0.02	0.02	0.28	0.02	0.02	0.22	953
Employed	0.07	0.05	0.14	0.08	0.05	0.10	0.07	0.05	0.14	953
Part-time employed	0.11	0.06	0.04	0.12	0.06	0.03	0.13	0.05	0.01	948
Full-time employed	-0.04	0.02	0.13	-0.04	0.02	0.11	-0.04	0.03	0.15	948
Income from dependent employment	0.06	0.05	0.26	0.07	0.05	0.13	0.09	0.05	0.09	952

Note: Standard errors of lasso-based estimation do not account for clustering.

Source: Swiss Household Panel (SHP)

The findings suggest a positive effect of the reform on the labour force supply of mothers: If the youngest child is supposed to enter kindergarten at the age of four, mothers' probability of being in the labour force increases by 9 percentage points. Therefore, the share of employed (unemployed) mothers increases by 7 (2) percentage points, yet not significantly. Results on the employment level reveal a significant effect on mothers' part-time by 11 percentage points. In contrast, mothers' full-time rate decreases by 4 percentage points and is not significant. The obligation to attend kindergarten has no significant effect. It gets significant at the 10 % level in the LASSO specification. The B-H procedure suggests one (two) statistically significant effect

<sup>21</sup>We estimated the first two specifications without clustered standard errors as well. The standard errors were not much different

at the 10% level for the lasso (all X) specification, but none for the socio-economic specification.

To support the parallel assumption we conduct two placebo tests. In Table 6.7, we use a fake treatment group, namely mothers whose children are too young to be eligible for the reform. In the first specification, the youngest child in the household is between 0 and 1 year old. The other two columns use alternative specifications regarding the age of the children: 1 and 2 years, as well as 2 and 3 years. We control for the socio-demographic as well as the cantonal covariates in the placebo test. We do not find any statistically significant effect of the reform on mothers' whose children are younger than the eligibility age. In Table 6.8, we test whether there was an anticipatory effect in the periods prior to the very first implemented reform (1999 - 2004). For this analysis, we split the pre-treatment periods in a fake pre-treatment period (1999 - 2001) and a fake post-treatment period (2002 - 2004). Since the p-values are too large to be significant, we cannot reject the null hypothesis and find no support for an anticipatory effect.

Table 6.7: DiD: Placebo tests with unaffected age groups

	0 and 1 years old				1 and 2 years old				2 and 3 years old			
	est	se	pval	obs	est	se	pval	obs	est	se	pval	obs
Out of labour force	0.04	0.05	0.42	440	0.02	0.04	0.68	680	-0.00	0.05	0.95	732
Unemployed	0.02	0.04	0.70	440	-0.00	0.03	0.90	680	-0.00	0.02	0.86	732
Employed	-0.06	0.05	0.28	440	-0.01	0.05	0.77	680	0.01	0.05	0.89	732
Part-time employed	-0.11	0.09	0.19	439	-0.02	0.06	0.76	676	-0.01	0.05	0.86	728
Full-time employed	0.05	0.05	0.28	439	0.00	0.02	0.93	676	0.02	0.02	0.31	728
Income from dependent employment	-0.05	0.04	0.25	440	-0.04	0.04	0.34	680	-0.00	0.06	0.96	732

Note: Controlling for all  $X$ .

Source: Swiss Household Panel (SHP)

To test the robustness of the results, we run several checks. At first, we report the results with a changed control group. In Table 6.9, we exclude mothers whose children have to attend kindergarten at the age of five. In other words, only mothers with children living in a canton without the kindergarten obligation are in the control group. The effect on the labour force participation gets more precise and bigger in absolute terms. The effect on being unemployed stays insignificant and is close to zero, whereas the effect on being employed increases slightly in comparison to the main results. Even in this robustness check, the effect on part-time rate



Table 6.8: DiD: Placebo tests with unaffected periods

Pre-treatment period: 1999-2001; fake post-treatment period: 2002-04

	treated: intro after 2008			treated: intro after 2010			treated: intro after 2012			obs
	est	se	pval	est	se	pval	est	se	pval	
Out of labour force	0.06	0.06	0.34	0.04	0.06	0.52	-0.01	0.07	0.90	490
Unemployed	-0.01	0.03	0.62	-0.01	0.03	0.74	0.01	0.02	0.81	490
Employed	-0.05	0.07	0.51	-0.03	0.07	0.65	0.00	0.07	0.95	490
Part-time employed	-0.03	0.08	0.75	-0.03	0.08	0.66	0.00	0.08	0.99	485
Full-time employed	-0.02	0.04	0.69	0.01	0.04	0.88	0.02	0.02	0.53	485

Note: Controlling for all  $X$ .

Source: Swiss Household Panel (SHP)

is highly significant. The effect on the full-time rate stays insignificant. The effect on the dependent work gets highly significant.

Table 6.9: DiD: Empirical results with changed control group (only eventually treated)

	socio-economic $X$			all $X$			lasso-picked $X$			obs
	est	se	pval	est	se	pval	est	se	pval	
Out of labour force	-0.11	0.05	0.03	-0.12	0.05	0.01	-0.10	0.05	0.03	825
Unemployed	0.02	0.02	0.34	0.02	0.02	0.26	0.02	0.02	0.31	825
Employed	0.09	0.06	0.12	0.10	0.05	0.07	0.08	0.05	0.10	825
Part-time employed	0.12	0.07	0.08	0.12	0.06	0.04	0.13	0.06	0.02	821
Full-time employed	-0.02	0.03	0.37	-0.02	0.03	0.39	-0.03	0.03	0.39	821
Income from dependent employment	0.11	0.04	0.01	0.10	0.05	0.03	0.11	0.05	0.04	824

Note: Standard errors of lasso-based estimation do not account for clustering.

Source: Swiss Household Panel (SHP)

For another robustness check, we exclude cases in which the information about the implementation year of a mandatory kindergarten was uncertain. This was the case for the cantons Glarus (2001-2010), Jura (1999-2011), and Ticino (1999-2014). The results are provided in Table 3.B.2.3 in the Appendix. The effect on the labour force remains positive and significant. Whereas the impact of the kindergarten policy on the part-time employment is only in the lasso-picked specification significant. The effect on unemployment, and full-time employment and the income from dependent employment is insignificant.

In order to make the sample representative, we use individual cross-sectional weights which keep the sample size of the current year in a next step. The weights are provided by the SHP and refer to the population of the whole sample. Therefore, we have to adapt the weights, because

we use a subsample of mothers with children of a specific age. We followed the approach of [Antal \(2016\)](#) to adapt the weights. Furthermore, there are no weights available for only the second sample. For this reason, we use combined weights for the first and second sample and exclude observations that are not part of the survey from the beginning. Table [3.B.2.4](#) in the Appendix shows that the effect on part-time employment (controlling for all X) remains significant and the negative impact of the reform on full-time employment gets highly significant.

Since mothers with different educational levels could react differently to the implementation of mandatory kindergarten, we split the sample to analyse effect heterogeneity. However, we could not find any evidence for a different effect among the groups. This could also be the case because of the limited data size.

All in all, the DiD results suggest a positive effect of mandatory kindergarten on mothers' employment, compared to rather insignificant effects in the RDD approach. However, we need to put our findings from these two different evaluation strategies into perspective: Firstly, the results from the RDD approach are based on administrative data, such that the number of observations is larger and the quality of data is better than the survey data used in the DiD part. Due to the smaller sample size, the findings of the DiD approach are imprecisely estimated. Secondly, the RDD identifies in general the Average Treatment Effect (ATE) at the threshold, whereas the DiD identifies the effect for the sub-population of treated individuals (ATT), such that these effects might not be directly comparable. Finally, we analyse the effect in both evaluation strategies on rather different outcomes.

## 7 Conclusion

This paper analyses the impact of mandatory kindergarten concerning four-year-old children on maternal labour market behaviour. One common threat of effect identification is that mothers self-select their children either into the treatment or the control group. We overcome this concern by two methods for assessing natural experiments: Our first identification strategy draws on Swiss administrative data, exploits cut-off dates as a source of random assignment, and uses an RDD approach. The findings point to a small increase in the employment probability by

1 percentage point. Furthermore, mothers earn on average a 1.337 CHF higher income, which corresponds to 3.9% of their mean income. Finally, the probability that this income stems from dependent employment increases by 1 percentage point. The effects are driven by mother's earning less than the median annual work income of 15,706 CHF and by older mothers ( $>37$ ).

Our second approach complements the main analysis by using spatial variation as well as staggered implementation of the mandatory kindergarten as exogenous variation. This analysis is based on survey data and applies a DiD approach, which is run in three specifications: First, we include socio-demographic variables of the mother and the household. Second, we add cantonal characteristics to the socio-demographic specification. Third, we use a double machine learning approach to pick the relevant covariates in a data-driven way. Even with this method, we find a robust, but imprecisely estimated impact on mothers' labour market behaviour: The probability of being in the labour market increases, for mothers whose child is treated, by 9 percentages points. At the same time, the part-time employment of mothers rises by 11 percentages points. Strictly speaking, these effects should be interpreted as intention to treat effect because some parents may decide to redshirt and postpone kindergarten entry by one year.

Our findings contribute to closing the gap between the literature on childcare and school entry by analysing the mandatory and free of charge kindergarten. Current literature suggests that attending childcare or school affects maternal labour force participation positively in countries with a low maternal labour force participation and low childcare attendance rate (see for example [Nollenberger and Rodríguez-Planas \(2015\)](#) or [Bauernschuster and Schlotter \(2015\)](#)). In Switzerland, maternal labour force participation exceeds the OECD average, hence we find in our study, if anything, a very moderately effect of mandatory kindergarten on maternal labour force attachment. This result is in line with other studies analysing the effect of childcare on maternal employment in countries with an even higher maternal labour force participation (see for example [Lundin et al. \(2008\)](#)). From a policy perspective, it would be interesting to examine whether a full-day kindergarten impacts maternal working hours and earnings.

# Appendices

## A Acknowledgement

The first part of the study has been realized using the data from the Federal Statistical Office (FSO) and the Central Compensation Office (CCO). We are grateful for the data provision as well as the linking and anonymizing of the data conducted by the FOS.

The second part of the study has been realized using the data collected by the Swiss Household Panel (SHP), which is based at the Swiss Centre of Expertise in the Social Sciences (FORS). The project is financed by the Swiss National Science Foundation.

## B Appendix

### B.1 Main part

Table 3.B.1.1: Cut-off dates

Canton	Year(s)	Cut-off date
Basel-Stadt	2005 - 10	30.04.
	2011	16.05.
	2012	01.06.
	2013	16.06.
	2014	01.07.
	2015	16.07.
	2016	31.07.
	2017	31.07.
St Gallen	2008 - 17	31.07.
Thurgau	2008	31.05. <sup>22</sup>
	2009	30.06.
	2010-17	31.07.
Zurich	2008-13	30.04.
	2014	15.05.
	2015	31.05.
	2016	15.06.
	2017	30.06.
Fribourg	2009 - 17	31.07.
Geneva	2011 - 17	31.07.
Glarus	2011 - 17	31.07.
Neuchâtel	2011 - 17	31.07.
Basel-Land	2012	15.05.
	2013	31.05.
	2014	15.06.
	2015	30.06.
	2016	15.7.
	2017	31.7.
Jura	2012 - 17	31.07.
Solothurn	2012	31.05.
	2013	30.06.
	2014 - 17	31.07.
Bern	2013	31.05.
	2014	30.06.
	2015 - 17	31.07.
Aargau	2013 - 17	30.04. - 31.07. <sup>23</sup>
Vaud	2013 - 17	31.07.
Schaffhausen	2014	30.06.
	2015 - 17	31.07.
Ticino	2015 - 17	31.07.
Upper Valais	2015	28.02.
	2016	30.04.
	2017	30.06.
Central and Lower Valais	2015	31.08.
	2016	31.07.
	2017	31.07.

**Note:** Dates in *blue* represent the earliest or the latest cut-off ever.

Sources: Regierungsrat Basel-Stadt (2010), Erziehungsdepartement des Kantons Basel-Stadt (2013), Grosser Rat des Kantons Basel-Stadt (2010), Grosser Rat des Kantons St.Gallen (2007), Regierungsrat Thurgau (2007), Kantonsrat Zürich (2007), Schule Küsnacht (no date), Grosser Rat des Kantons Freiburg (2008), Secrétariat du Grand Conseil Genève (2010), Landsgemeinde Glarus (2009), Le Grand Conseil de la République et Canton de Neuchâtel (2011), Regierungsrat des Kantons Basel-Landschaft (2011), Parlement de la République et Canton du Jura (2011), Kanton Solothurn - Amt für Volksschule und Kindergarten (2012), Kanton Solothurn - Amt für Volksschule und Kindergarten (2013), Kanton Aargau - Departement Bildung, Kultur und Sport - Abteilung Volksschule (2010), Grosser Rat des Kantons Schaffhausen (2014), Gran Consiglio Repubblica e Cantone Ticino (2011), and 1815.ch (2014).

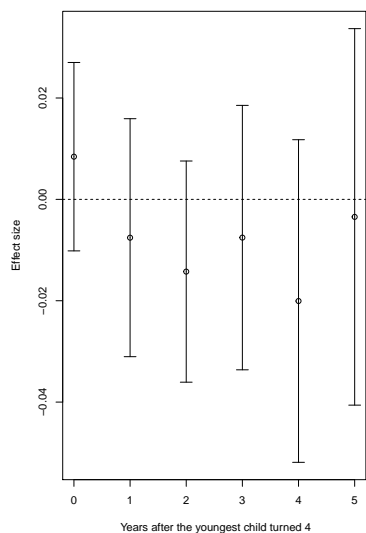
<sup>22</sup>Municipalities can decide whether they use the 31.7. as cut-off date from 2008 an or use this stepwise approach: <https://www.tg.ch/news/news-detailseite.html/485/news/2817/newsarchive/1>, last downloaded 2019/10/07.

<sup>23</sup>Transition period of 6 years.

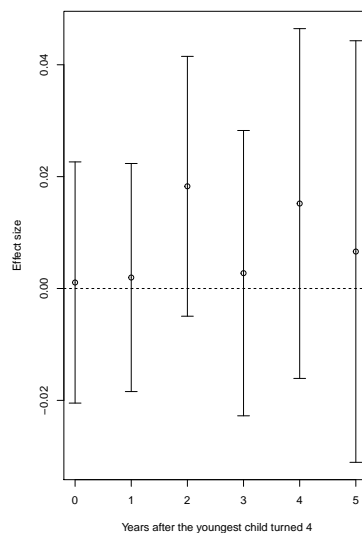
Table 3.B.1.2: RDD: Empirical results without covariates

	Sample mean	Coefficient	Standard error	P-value	Bandwidth	Number of observations	Number obs within bw
Out of labour force (binary)	0.23	-0.00	0.01	0.56	65.44	735,520	269,236
Employed (binary)	0.76	0.00	0.01	0.63	71.25	735,520	293,733
Unemployed (binary)	0.01	-0.00	0.00	0.24	48.27	7355,20	198,603
Total annual work income	34,273.01	-705.56	537.68	0.19	70.98	735,520	289,613
Income dependent employment (binary)	0.73	0.01	0.01	0.05	50.63	735,520	206,519
Annual income from dependent employment	32,515.20	-897.07	508.05	0.08	69.65	735,520	285,555

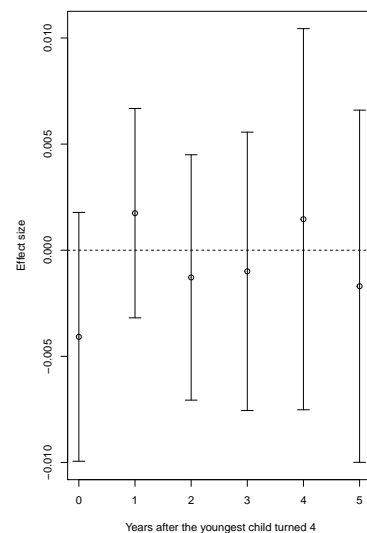
Sources: STATPOP (2010 - 2017) and OASI (2010 - 2017); calculations are done by ourselves



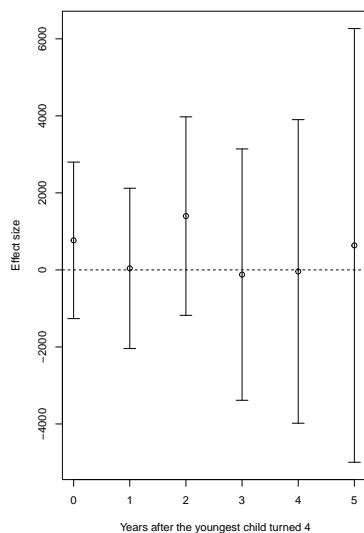
(a) Out of labour force (binary)



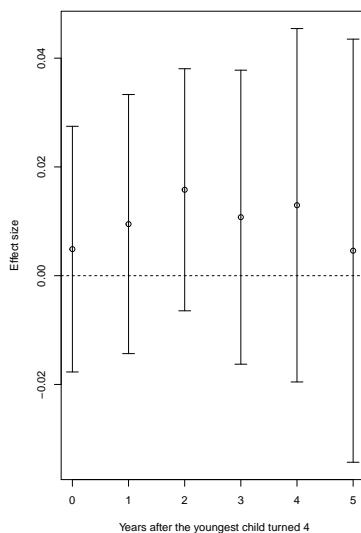
(b) Employed (binary)



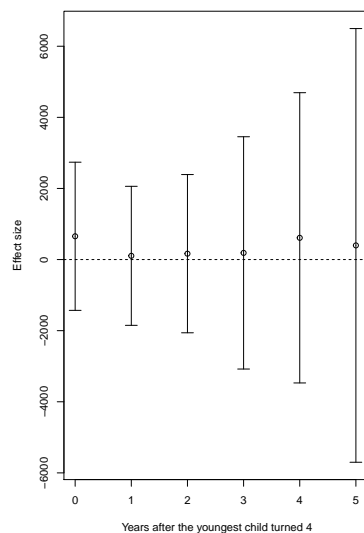
(c) Unemployed (binary)



(d) Total annual work income



(e) Income dependent employment



(f) Income dependent employment (binary)

Figure 3.B.1.1: RDD: Effects over years without covariates

Note: Dots represent ITTs at the threshold, bands correspond to 95% confidence intervals



Table 3.B.1.3: RDD: Robustness: Bandwidth \* 1.5

	Sample mean	Coefficient	Standard error	P-value	Bandwidth	Number of observations	Number of observations within bandwidth
Out of labour force (binary)	0.23	-0.01	0.00	0.02	104.98	735,520	427,429
Employed (binary)	0.76	0.01	0.00	0.01	94.77	735,520	386,618
Unemployed (binary)	0.01	-0.00	0.00	0.37	72.19	735,520	297,628
Annual work income (in CHF)	34273.01	343.63	400.36	0.39	48.82	735,520	198,603
Income dependent employment (in CHF)	32515.20	383.23	273.76	0.16	84.97	735,520	346,165
Dummy income dependent employment (binary)	0.73	0.01	0.00	0.09	77.89	735,520	317,350

Sources: STATPOP (2010 - 2017) and OASI (2010 - 2017); calculations are done by ourselves

Table 3.B.1.4: RDD: Robustness: Bandwidth \* 2/3

	Sample mean	Coefficient	Standard error	P-value	Bandwidth	Number of observations	Number of observations within bandwidth
Out of labour force (binary)	0.23	-0.00	0.01	0.38	46.66	735,520	190,654
Employed (binary)	0.76	0.01	0.01	0.32	42.12	735,520	174,017
Unemployed (binary)	0.01	-0.00	0.00	0.20	32.09	735,520	133,198
Annual work income (in CHF)	34273.01	659.40	608.50	0.28	21.70	735,520	88,336
Income dependent employment (in CHF)	32515.20	726.81	411.33	0.08	37.76	735,520	153,294
Dummy income dependent employment (binary)	0.73	0.02	0.01	0.00	34.62	735,520	141,416

Sources: STATPOP (2010 - 2017) and OASI (2010 - 2017); calculations are done by ourselves

## B.2 Complementary part

Table 3.B.2.1: Implementation dates

Canton	Mandatory kindergarten for four-year-old children	Mandatory kindergarten extended	Obligation to offer kindergarten for four-year-old children	Mandatory offer extended
Aargau	2013	no	2013	yes
Appenzell Outer-Rhodes	-	no	at least since 2007	at least not since 2007
Appenzell Inner-Rhodes	-	no	at least since 2007	at least not since 2007
Basel-Land	2012	yes	at least since 2007	at least not since 2007
Basel-Stadt	2005	no	at least since 2005	at least not since 2005
Bern	2013	no	2013	yes
Fribourg	Stepwise since 2009/10	no	2009	yes
Geneva	2011	no	at least since 2007	at least not since 2007
Glarus	2011	yes	at least since 2007	at least not since 2007
Grisons	-	no	2013	yes
Jura	2012	no	at least since 2007	at least not since 2007
Lucerne	-	no	2011	no
Neuchâtel	2011	no	at least since 2007	at least not since 2007
Nidwalden	-	no	2008	no
Obwalden	-	no	-	no
St Gallen	2008	no	at least since 2007	at least not since 2007
Schaffhausen	2014	yes	at least since 2007	at least not since 2007
Schwyz	-	no	2017	no
Solothurn	2012	no	at least since 2007	at least not since 2007
Ticino	2015	no	at least since 2007	at least not since 2007
Thurgau	2008	no	at least since 2007	at least not since 2007
Uri	-	no	2016	no
Vaud	2013	no	at least since 2007	at least not since 2007
Valais	2015	no	2015	yes
Zug	-	no	-	no
Zurich	2008	no	at least since 2007	at least not since 2007

Sources: Schweizerische Konferenz der kantonalen Erziehungsdirektoren (2008b), Schweizerische Konferenz der kantonalen Erziehungsdirektoren (2009), Schweizerische Konferenz der kantonalen Erziehungsdirektoren (2010), Schweizerische Konferenz der kantonalen Erziehungsdirektoren (2011), Schweizerische Konferenz der kantonalen Erziehungsdirektoren (2012), Schweizerische Konferenz der kantonalen Erziehungsdirektoren (2013), Schweizerische Konferenz der kantonalen Erziehungsdirektoren (2014a), Schweizerische Konferenz der kantonalen Erziehungsdirektoren (2015), Schweizerische Konferenz der kantonalen Erziehungsdirektoren (2016a), Schweizerische Konferenz der kantonalen Erziehungsdirektoren (2017), and Schwyz (2012).

Table 3.B.2.2: DiD: Picked covariates

socio-economic $X$	all $X$	lasso-picked $X$
<i>Mothers' characteristics</i> age age <sup>2</sup> education citizenship civil status political attitude political attitude <sup>2</sup> self-reported health	<i>Mothers' characteristics</i> age age <sup>2</sup> education citizenship civil status political attitude and political attitude <sup>2</sup> self-reported health	<i>Mothers' characteristics</i> age  education citizenship  self-reported health
<i>Household characteristics</i> number of children in the household	<i>Household characteristics</i> number of children in the household	<i>Household characteristics</i> number of children in the household
	<i>Cantonal characteristics</i> unemployment rate unemployment rate <sup>2</sup> mandatory kindergarten for five-year-old children canton is member of Harmos	<i>Cantonal characteristics</i> unemployment rate  mandatory kindergarten for five-year-old children canton is member of Harmos

Table 3.B.2.3: DiD: Empirical results dropping ambiguous kindergarten entry

	socio-economic $X$			all $X$			lasso-picked $X$			obs
	effect	se	pval	effect	se	pval	effect	se	pval	
Out of labour force	-0.07	0.04	0.10	-0.08	0.05	0.08	-0.08	0.04	0.08	915
Unemployed	0.02	0.02	0.14	0.02	0.02	0.14	0.02	0.02	0.19	915
Employed	0.05	0.05	0.31	0.06	0.05	0.26	0.06	0.05	0.22	915
Part-time employed	0.09	0.06	0.11	0.10	0.06	0.09	0.11	0.05	0.03	910
Full-time employed	-0.04	0.03	0.19	-0.04	0.03	0.18	-0.04	0.03	0.16	910
Income from dependent employment	0.05	0.06	0.39	0.06	0.05	0.20	0.05	0.05	0.32	914

Note: Standard errors of lasso-based estimation do not account for clustering.

Table 3.B.2.4: DiD: Empirical results with weights

	socio-economic $X$			all $X$			obs
	est	se	pval	est	se	pval	
Out of labour force	-0.05	0.05	0.37	-0.07	0.05	0.13	946
Unemployed	0.02	0.01	0.14	0.02	0.01	0.07	946
Employed	0.03	0.06	0.57	0.05	0.05	0.32	946
Part-time employed	0.10	0.07	0.13	0.13	0.06	0.04	941
Full-time employed	-0.07	0.03	0.03	-0.07	0.03	0.02	941
Income from dependent employment	0.02	0.06	0.69	0.04	0.06	0.54	945

Note: Standard errors of lasso-based estimation do not account for clustering.

Table 3.B.2.5: Restriction of the sample size

Description	Sample size
All mothers with children in the SHP dataset	35.337 observations
children turning 4 before September and 5 after February	3.510 observations
Selected children must be youngest child in household	2.006 observations
Periods from the year 2004 onwards	1.472 observations
Exclude observations with missings in the outcomes or controls	953 observations in evaluation sample

Table 3.B.2.6: Variable description

Dependent variable	Description
Out of labour force	Dummy working status "not in labor force", constructed from wstat==3
Unemployed	Dummy working status "unemployed", constructed from wstat==2
Employed	Dummy working status "active occupied", constructed from wstat==1
Part-time employed	Dummy working in part-time, constructed from pw39==1 <i>"Currently, in your main job, do you work parttime or 100% ?"</i>
Full-time employed	Dummy working in full-time, constructed from pw39==2 <i>"Currently, in your main job, do you work parttime or 100% ?"</i>

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