Evaluation of Public Policies targeting Disadvantaged and Vulnerable Groups in Developed Countries

Doctoral Thesis

presented to the Faculty of Management, Economics and Social Sciences
at the University of Fribourg (Switzerland),
in fulfillment of the requirements for the degree of
Doctor of Economics and Social Sciences (Dr.rer.pol.)

submitted by

Selina GANGL

from Germany

Accepted by the Faculty of Management, Economics and Social Sciences
on 28.02.2022 at the proposal of

Prof. Martin Huber, Ph.D. (first supervisor) and
Prof. Andreas Steinmayr, Ph.D. (second supervisor)

Fribourg, Switzerland, 2022

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Acknowledgements

I would like to express my sincere gratitude to all those who have supported me during my Ph.D. studies. Especially, I am thankful to Prof. Martin Huber, Ph.D. for the chance to pursue my Ph.D. at his chair and for his constructive comments that greatly improved my work. Furthermore, I am grateful to Prof. Andreas Steinmayr, Ph.D. for the valuable co-supervision, sharing his insightful experiences in the econ job market with me and the great opportunity to visit his lab. A special thank goes to Prof. Dr. Berno Büchel who is a co-author of one chapter of this thesis and has been a mentor for me in many aspects of academic life.

I very much appreciate the hospitality of Karin Büchel, who warmly welcomed me into her home during the data collection in Liechtenstein. Additionally, I am also grateful for Laura Ravazzini's well-considered comments on one chapter of this thesis. I would also like to thank for the support and happy moments during my visit in Innsbruck: Katia Gallegos, with whom I luckily shared an office, Elisabeth Gsottbauer and Sebastian Butschek who discussed the econ job market with me and who gave fruitful feedback to my job market presentation. I would also like to extend my thank to Luisa Wallossek for her helpful comments.

Last but not least, I am deeply indebted to my family and friends who provided particularly, but not only, emotional support. My partner Joël Duss got never tired to encourage me during the Ph.D. journey and set every event into perspective: For him no low-point was very low and no high-point very high. My best friend Johanna Staffner always nudged me to face new challenges and to be persistent. She and her partner Manuel Mayrl provided not only moral support to me, both kindly helped me to move from Innsbruck to Bern. Finally, my biggest fan - my Mum! She greatly supported me in all my decisions and inspired my brother and me to think big since our childhood: "Die Welt steht euch offen."

November, 2021

Selina Gangl

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1 Introduction

The announcement of David Card, Joshua Angrist, and Guido Imbens as current Nobel Prize winners in the field of economics, led to considerable and international media attention on natural experiments (see, for example, Pischke (2021), Smialek (2021), and ZEIT ONLINE (2021)). Natural experiments identify a causal effect in settings where a randomized control trial (RCT) is not feasible or is socially unaccepted due to ethical concerns. Instead of a laboratory experimenter randomly assigning a drug (treatment) or placebo (control) to an individual, natural experiments use other methods of random assignment, such as geographic variation of a treatment. Based on this seminal work, many applied microeconomic studies emerged and methodological work contributed to the extension and improvement of this literature. As for the latter, weakening the assumptions in standard parametric models led to more flexible methods for causal inference.

This dissertation builds on the so-called "credibility revolution" in economics by exploiting natural experiments and using flexible methods. In particular, it studies the causal effect of policies targeting vulnerable or disadvantaged groups in developed countries. Vulnerable groups, like children or migrants, are affected by policies in their daily life, but can hardly influence it, because they are excluded from the right to vote. Mothers can be seen as disadvantaged group in the labour market, because they generally bear the brunt of care responsibilities but often lack suitable childcare options. This dissertation consists of three independent essays, which primarily study the effect of labour market policies but also one health policy. The reason for the latter is that nutritional behaviour in childhood may lead to consequences in the later employment.

Chapter 2 examines the effect of soda taxes on consumption behaviour and health of school-aged children in France and Hungary. Childhood obesity is a worldwide problem and may lead to the development of non-communicable diseases. Reducing the intake of sugar decreases the probability of getting obese. Additional taxes may decrease the demand for sodas if the price increase (due to tax and pass-through) is high enough and sodas have a large enough price elasticity. This might be especially the case for subgroups with a very restricted budget. To the best of my knowledge, this is the first paper analysing the effect of soda taxes on school-aged children. I study two different soda taxes: Hungary imposed a "Public Health Product Tax" on several unhealthy products in 2011, whereas France introduced solely a tax on sodas, containing sugar or artificial sweeteners, in 2012. I use survey data and exploit spatial variation by using a semi-parametric difference-in-differences (DID) approach. Since the policies differ in Hungary and France, I analyse the effects separately by using a neighbouring country without a soda tax as a control group. The results point to a counter-intuitive positive effect of the tax on soda consumption in Hungary and no effect in France. The body mass index (BMI) is not affected by the tax in any country. Reasons for this finding might be a substitution of unhealthy products as well as the decreased amount of sugar in sodas in the case of Hungary and the low tax burden in the case of France.

The remaining two chapters address policy evaluation in the sub-field of labour economics. Chapter 3 is joint work with Martin Huber. It analyses the effect of mandatory kindergarten for four-year-old children on maternal labour market attachment in Switzerland. This paper narrows the gap between childcare (which is voluntary) and school entry age (which concerns rather older children) literature. We hypothesise that mothers whose youngest child is obliged to attend kindergarten may re-allocate their time towards labour market participation if they would take care of their child in absence of the mandatory kindergarten. Since voluntary kindergarten was already available, free of charge, and well attended before the reform, we expect a small positive effect. We combine two quasi-experiments and two Swiss datasets in the paper: Firstly, we use a large administrative dataset and apply a flexible Regression Discontinuity Design (RDD) 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 in a Difference-in-Differences (DiD) approach using a Swiss household survey. Our results from the RDD suggest that the reform has increased the maternal employment probability and the income from work slightly. These effects are driven by mothers earning less than the median annual work income of 15,706 CHF and by older mothers (>37). The findings from the complementary DiD approach point to a rise in maternal labour force participation and dependent employment too. Since kindergarten is mainly half-day care, we find additionally an increase in the share of part-time working mothers. However, these results are imprecisely estimated. All in all, the findings suggest that mandatory kindergarten increases the labour force attachment of mothers in general very moderately, but more strongly for older and low-income mothers.

Chapter 4 is written in collaboration with Berno Büchel and Martin Huber. It examines whether residence permits affect the labour market attachment of foreign workers. The paper exploits a unique migration lottery in Europe, taking place in Liechtenstein. The paper analyses labour migration between European Economic Area (EEA) nationals and Liechtenstein. This analysis appears relevant because a large share of labour mobility takes place between rather developed nations competing for skilled labour. Furthermore, most employees are cross-border commuters, so we estimate a lower bound for the effect of a residence permit. Methodologically, the paper uses a non-parametric Instrumental Variable (IV) approach to identify the effect of the subgroup of compliers, i.e. lottery participants who would move to Liechtenstein if they win the lottery and who would not move otherwise. The findings suggest that workers winning a residence permit and moving to Liechtenstein are more likely to work, raise their employment level, and extend the duration in the labour market in Liechtenstein. These effects are mainly driven by individuals not working in Liechtenstein before the lottery participation. However, even for previous cross-border commuters, positive employment effects emerge in the longer run. We find the labour market effect to be persistent even several years after the lottery with no sign of fading out. These results suggest that granting resident permits to foreign workers can be effective to foster labour supply despite the alternative of cross-border commuting from adjacent regions.

2 Do soda taxes affect the consumption and health of school-aged children? Evidence from France and Hungary*†

Abstract

This paper examines the effect of two different soda taxes on consumption behaviour and health of school-aged children in Europe: Hungary imposed a Public Health Product Tax (PHPT) on several unhealthy products in 2011. France introduced solely a soda tax, containing sugar or artificial sweeteners, in 2012. In order to exploit spatial variation, I use a semi-parametric Difference-in-Differences (DID) approach. Since the policies differ in Hungary and France, I analyse the effects separately by using a neighbouring country without a soda tax as a control group. The results suggest a counter-intuitive positive effect of the tax on soda consumption in Hungary. The reason for this finding could be the substitution of other unhealthy beverages, which are taxed at a higher rate, by sodas. The effect of the soda tax in France is as expected negative, but insignificant which might be caused by a low tax rate. The body mass index (BMI) is not affected by the tax in any country. Consequently, policy makers should think carefully about the design and the tax rate before implementing a soda tax.

Keywords: Soda tax, consumption, health, semi-parametric difference-in-differences, HBSC

JEL Classification: H25, I12, I18, L66

* I have benefited from comments by seminar participants at the Swiss Public Health Conference 2020, the EuHEA PhD Student-Supervisor & Early Career Researcher Conference 2020, the Annual Congress of the Verein für Socialpolitik 2020, and a research seminar at the University of Fribourg. I thank participants, in particular Edel Doherty, Reiner Eichenberger, Martin Huber, Marco Portmann, Giannina Vaccaro, Christian Zihlmann, and two anonymous reviewers of SMYE 2021 for their helpful comments.

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Declaration of conflicts of interest: none.

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† A previous version of the paper was published as a working paper on EconStor.

2.1 Introduction

Childhood obesity is a worldwide problem and implies risks like staying obese in adulthood as well as developing non-communicable diseases (WHO, 2020). Therefore, ways and means are searched to mitigate or even prevent childhood obesity. Since an association between childhood obesity and soda consumption exists, the intake of sugar-sweetened drinks among children should be reduced (James and Kerr, 2005). For example, a substitution of sugar-sweetened drinks by sugar-free drinks showed a decrease in weight among children (de Ruyter et al., 2012).

Hungary and France have implemented a tax on sodas, yet the design and the tax rate differed among the countries: Hungary introduced a broad tax on unhealthy products, in which sugar-sweetened sodas were taxed by converted 1.83 Eurocents per litre[1] from 2011 on (Ecorys, 2014). One year later, France imposed a soda tax of 7.16 Eurocents per litre but included sodas with artificial sweetener too (Ecorys, 2014). These taxes are designed to increase the price and aim to decrease the consumption of sodas in the population. The existing literature focuses on the consumption behaviour of the household, whereas little is known about the response of children to a soda tax (Wilson and Hogan, 2017).

Hence, this paper analyses the effect of a soda tax on consumption behaviour and the body mass index (BMI) of school-aged children in Hungary and France. Since the policies differ among the countries, I analyse the effect separately. In the first analysis, Hungary forms the treatment group, while neighbouring country Croatia, which does not levy a soda tax, serves as the control group. In the second analysis, France constitutes the treatment group and Spain, the control group without a soda tax. Methodologically, I exploit spatial variation of the tax and use a semi-parametric Difference-in-Differences (DiD) approach to evaluate the policy. This method uses inverse probability weighting (IPW) to control for differences in observable characteristics between the treatment and control group as well as over time.

Since this paper aims at a cross-country comparison, I use data from the cross-national survey Health-Behaviour in School-Aged Children (HBSC) which ensures that the same question is asked in each country. This survey is conducted in cooperation with the World Health Organization (WHO)

[1] converted into Euros at the rate on 01.09.2011

Europe and takes place on a quadrennial basis. In the setting of this natural experiment, the year 2010 constitutes the pre-treatment period and 2014 post-treatment period. Furthermore, I use the survey years 2006 and 2010 as pre-treatment years to provide evidence for the parallel trend assumption.

The results suggest a counter-intuitive positive and significant effect of the tax on soda consumption in Hungary. Considering the nature of the tax might explain this finding. Since the prices of other unhealthy products, like energydrinks, increase as well, the substitution behaviour of children could explain this result. Soda consumption is not affected by the tax in France. A reason for this finding might be the low soda tax rate of 7 Eurocents per litre. Children's body mass index is not influenced by the tax in any country.

This paper relates to two strands of literature. The first one addresses the impact of a soda tax on adults or households in different countries and several U.S. jurisdictions. For example, Falbe et al. (2016) focus on poorer districts in North California and study the effect of a converted 31 Eurocents per litre[2] soda tax in Berkeley in 2015, compared to similar areas without such a tax in Oakland and San Francisco. They find a decrease in soda consumption by 21% after the tax implementation and an increase in water consumption by 63%. In Philadelphia, the soda tax amounts to converted 49 Eurocents per litre[3] in 2017. Zhong et al. (2018) find that Philadelphians, compared to citizens of close and similar cities without such a tax, drink 40% fewer sodas directly after the tax implementation, yet the effect diminishes one year later (Zhong, 2020). However, Wilson and Hogan (2017) criticize that the soda tax can be easily circumvented, if the tax is implemented very locally, such as in a particular city. For this reason, Wilson and Hogan (2017) caution against over-interpreting the results of these studies.

There is little literature that relates to France and Hungary which are analysed in this paper. The soda tax literature based on France reports a pass-through of the soda tax to the consumer between 39% (Etilé et al., 2018) and 100% (Capacci et al. (2019) and Berardi et al. (2016)). Capacci et al. (2019) analyse the number of purchased sodas at the household level in France compared to close regions in Italy. They apply a differences-in-differences approach and find a very small but imprecisely estimated decrease in soda consumption and explain their result with the low soda tax level. Bíró

[2]converted into litre and Euros (at the rate on 01.03.2015)
[3]converted into litre and Euros (at the rate on 01.01.2017)

(2016) evaluates the effect of the unhealthy product tax in Hungary but sodas are excluded from the analysis due to data restrictions. Kurz and König (2021) examine both the soda tax in France and the PHPT in Hungary using data on purchased sodas and apply a synthetic control group analysis. Their findings suggest a slight decline in soda consumption in France and a decrease in Hungary in the short run that fades away two years later. However, these results are imprecisely estimated due to the small number of observations.

The second strand of literature discusses the effect of the soda tax on children's and adolescents' outcomes in other countries than France and Hungary. The evidence is mixed in these studies: Fletcher et al. (2010) finds a small decline in soda consumption of children and adolescents due to the implementation of a soda tax across all U.S. states, which had an average soda tax between 1.5% and 2.3%. Whereas Sturm et al. (2010) ascertain that a low soda tax ($\leq 4\%$) has neither an effect on soda consumption nor on obesity rates of children in the U.S. states. In Philadelphia, a tax of converted 49 Eurocents per litre[4] does not affect children's soda consumption (Cawley et al., 2019). However, subgroups like overweighted children, children from families with low incomes (Sturm et al., 2010) or children with a high soda consumption prior to the reform (Cawley et al., 2019) are more likely to react to a soda tax. Regarding the effect of the soda tax on BMI, Powell et al. (2009) find no significant change among adolescents in different U.S. states. In Peru, a price war among the manufactures lead to a reduction of soda prices at the end of the nineties, which increased the obesity rate among children (Ritter, 2018).

To the best of my knowledge, this is the first paper analysing the impact of two different soda taxes on soda consumption and health of school-aged children. A cross-country comparison is difficult to draw because countries collect their data by themselves with different questions and methods (Jou and Techakehakij, 2012). Using the health behaviour in school-aged children (HBSC) dataset enables me to draw a cross-country comparison.

The remainder of this paper is organized as follows: In the next section, I provide information about the implementation of the soda tax in France and Hungary. Thereafter, I present the data source, descriptive statistics, and define the subsample. Then, I discuss the empirical strategy and show the results. Finally, I conclude the empirical analysis.

[4]converted into litre and Euros (at the rate on 01.01.2017)

2.2 Institutional background: Soda taxes

Soda taxes represent a policy tool to combat sugar intake on a country and local level. Several U.S. cities, as well as various countries, have implemented a tax on sugar-sweetened sodas in recent years (Allcott et al., 2019). The first two countries which imposed a soda tax in Europe were Finland in 1940 followed by Norway in 1981 (Asen, 2019).

Thirty years after Norway, Hungary levied a "Public Health Product Tax" (PHPT) on salted snacks, condiments, flavoured alcohol, fruit jams, confectionery, energy drinks, and also on sugar-sweetened beverages (Ecorys, 2014). Every product category reveals a different tax level, even among the sugar-containing beverages exist differences: Syrups are taxed by 200 Hungarian Forint (HUF)[5] per litre, whereas other sugar-sweetened sodas are taxed by 7 HUF[6] per litre. Additionally, the original tax level for sodas amounted to 5 HUF in 2011 and was increased to 7 HUF in 2012 (Bíró, 2016). This soda tax only affects sodas exceeding a sugar content of 8 grams per 100 millilitres, sodas with less sugar are not taxed. To make this threshold more apparent, an original Coca-Cola contains more than 10 grams of sugar per 100 millilitres.[7] The reason for the implementation of this tax was a public health crisis (WHO, 2015). Non-communicable diseases have been a widespread cause of premature death, whereby the main risk factor was unhealthy diet (Institute for Health Metrics and Evaluation, 2010). One year prior to the implementation of the health policy, the share of overweight adults[8] amounted to 61.7% (WHO, 2017c) which exceeded the European average of 58.7% (WHO, 2017d). Likewise, the share of obese adults was higher (25.3%) (WHO, 2017a) than the European average (22.3%) (WHO, 2017b).

According to the law, the consumer bears the tax burden, by paying retail price including the soda tax (Ecorys, 2014). To answer the question whether the soda tax was passed through to the consumer, I tracked the development of the annual average price of two litres of Coca-Cola. Table 2.1 reports a price increase of Coca-Cola from the soda tax implementation in 2011 onwards. With this data at hand, I calculated the yearly price increase and the price index based on the previous year. The price of two litres of Coca-Cola rose from the pre-treatment year 2010 to the implementation year 2011 by

[5] equals 73 Eurocents on 01.09.2011.
[6] equals 2.2 Eurocents on 01.01.2012
[7] https://www.coca-cola.co.uk/our-business/faqs/how-much-sugar-is-in-coca-cola, last retrieved on 07.11.2021.
[8] Persons 18 years and older with a body mass index equal or bigger than 25.

21 HUF. To see whether the price increase was driven by inflation or the soda tax, I compared the Coca-Cola price index with the consumer price index (CPI) of non-alcoholic beverages. The price of Coca-Cola increased more than the price of non-alcoholic beverages in general, which points to the pass-through of the soda tax. In 2012, the tax was slightly raised from 5 HUF to 7 HUF per litre and comparing the two different price indexes shows an even greater difference than in the year before. The price indexes started to converge in the year 2013 and approximated each other in 2014.

Table 2.1: Price development of Coca-Cola and consumer price index (CPI) of non-alcoholic beverages

	2010	2011	2012	2013	2014
Coca-Cola average price (2l)	295 HUF	316 HUF	351 HUF	366 HUF	373 HUF
Coca-Cola price increase (2l)	0 HUF	21 HUF	35 HUF	15 HUF	7 HUF
Coca-Cola price index	100	107.1	111.1	104.3	102.0
CPI of non-alcoholic beverages	100.1	101.9	105.9	101.9	100.5

Source: Data concerning the Coca Cola annual average price and the CPI of non-alcoholic beverages stems from the Hungarian Central Statistical Office. The Coca-Cola price increase and index is self-calculated.

A beverage tax was implemented in France in January 2012 and it was originally intended to apply to sugar-sweetened sodas only. However, this was, according to Ecorys (2014), not possible, because the customs codification classifies sodas with added sugar and sweetener into the same category. Whereas Le Bodo et al. (2019) reports that sodas with artificial sweeteners were included in the tax to generate higher tax revenues for the farm sector. Consequently, both kinds of sodas are taxed, independently of their quantity of sugar or sweetener. The tax increased over time from 7.16 Eurocents per litre in 2012, to 7.31 Eurocents in 2013, and reached 7.45 Eurocents in 2014 (Ecorys, 2014). The aim of the soda tax is twofold: Firstly, it is designed to collect additional revenue for the health (Ecorys, 2014) and farm sector Le Bodo et al. (2019). Secondly, the soda tax is supposed to reduce the obesity rate among French citizens (Ecorys, 2014). One year prior to the implementation of the health policy, France revealed a share of overweight adults exceeding the European average by almost 2 percentage points (WHO (2017c) and WHO (2017d)). Every fifth adult had a BMI equal to or over 30 which indicates obesity among the WHO definition, yet the share of obese adults in France is lower than the European average (WHO (2017a) and WHO (2017b)). Moreover, the National Nutrition and Health Programme 2011 formulated the goal to decrease the share of children drinking more than half a glass of soda a day by at least a quarter in the following five years (Ministère du Travail, de l'Emploi et de la Santé, 2011). The discussion about the implementation of the soda tax lasted from 2005 to 2011. In August 2011 the tax was decided unexpectedly, the implementation followed five months later (Le Bodo et al., 2019). The pass-through of the soda tax to the consumer is reported between 39% (Etilé et al., 2018) and 100% (Capacci et al. (2019) and Berardi et al. (2016)).

Table 2.2 highlights the main differences in the taxes in Hungary and France. Hungary's Public Health Product Tax includes a bunch of unhealthy products and not only sodas. However, exclusively sugar-sweetened sodas exceeding the threshold of 8 g sugar per 100 millilitres are taxed, whereas sodas with less sugar or artificial sweetener are exempt from the tax. France taxes sugar-sweetened soft drinks (regardless of sugar content) and soft drinks with artificial sweeteners. The tax level is different among the countries, yet both countries raised the tax after implementation.

Table 2.2: Comparison of taxes in Hungary and France

	Hungary	France
Implementation year	2011	2012
Tax	Unhealthy product tax	Beverage tax
Products	Sodas Syrups Energy drinks Confectionery Salted snacks Condiments Flavoured alcohol Fruit jams	Sodas
Kind of sodas	Sugar-sweetened	Sugar-sweetened or artificial sweetener
Threshold of sugar	$>$ 8 grams per 100 millilitres	None
Tax level in implementation year	5 HUF ($\sim$ 1.8 Eurocents)	7.16 Eurocents
Tax level in evaluation year (2014)	7 HUF ($\sim$ 2.4 Eurocents)	7.45 Eurocents
Tax level increase	40%	4%

Source: The information concerning Hungary and France stems from Ecorys (2014), the table was created by myself.

2.3 Hypothesis Development

Soda taxes aim to raise the price and to decrease the consumption of sugar-sweetened beverages (Wilson and Hogan, 2017). In theory, manufacturers collect the retail price including the tax from the customer and transfer it to the state. In practice, the manufacturers might decide to reduce their margin and to pass only part of the tax to the customer. Empirical evidence ranges from a partly pass-through of 39% (Etilé et al., 2018) to 100% (Capacci et al. (2019) and Berardi et al. (2016)) in France. Table 2.1 reveals a price increase of Coca-Cola greater than the inflation after the implementation of the soda tax in Hungary. Consequently, the soda prices increased for the consumer in both countries. Figure 2.1 illustrates the increase in the soda price due to the positive pass-through of the tax.

$$\boxed{\text{Soda tax}} + \boxed{\text{Pass-through} > 0\%} \Rightarrow \boxed{\text{Soda price} \uparrow}$$

Figure 2.1: Soda price development

Whether the increased price leads to a decrease in demand depends on the price elasticity of sodas (Wilson and Hogan, 2017). Sodas are goods with a rather high price elasticity because they do not belong to staple food. Hence, I expect a reduction of soda consumption as shown in Figure 2.2).

$$\boxed{\text{Soda price} \uparrow} + \boxed{\text{High price elasticity}} \Rightarrow \boxed{\text{Soda consumption} \downarrow}$$

Figure 2.2: Reduction of soda consumption

In a next step, this hypothesis is adjusted to the subgroup of school-aged children which is presented in Figure 2.3. The crucial question is: How do children get mainly access to sugar-sweetened sodas? Either parents provide these beverages or children spend (part of) their pocket money on sodas. In the first case, children's soda consumption should also decrease when household consumption decreases due to the soda price increase. In the second case, children are even more affected by a price increase, because of their limited budget. Hence, I expect a decrease in soda consumption among children.

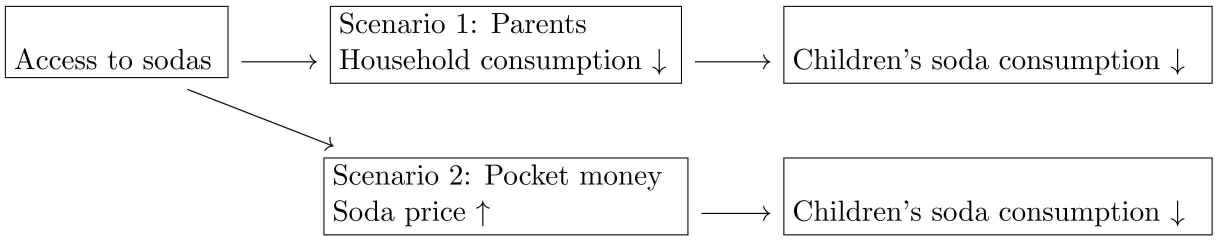


Figure 2.3: Children's access to sodas

The second hypothesis addresses the effect of the soda tax on children's BMI and depends on the substitution behaviour of the children: If children consume less sugar due to a lower soda intake and this amount of sugar is not substituted by other sugar-containing products, the average BMI of children might decline. For example, a substitution of sugar-sweetened drinks by sugar-free drinks pointed to a decrease in weight among children (de Ruyter et al., 2012). However, if children substitute the saved sugar with other sugary products, the soda tax does not affect the BMI. Since Powell et al. (2009) find no significant effect of a soda tax on children's BMI in the U.S., I expect at least not an increase in BMI due to the soda tax.

2.4 Data

To undertake a cross-country comparison, I use data about the health behaviour in school-aged children (HBSC). This survey is conducted in cooperation with the World Health Organization (WHO) Europe on a quadrennial basis since 2001. The advantage of this survey is the use of the same questionnaire in each country. One question captures the frequency of sugar-sweetened soda consumption on a 7-point scale from "never" to "more than once daily". Another section addresses body measures like children's height and weight. Based on this information, the body mass index (BMI) is calculated and reveals whether a child is overweight. The random sampling is made class-wise which implies a repeated cross-sectional design.

Regarding the sample restriction, I excluded pupils with missing observations in the dependent variable like soda consumption and body mass index. In the survey year 2006 is no information about the BMI available, so I calculate it by the following formula BMI = bodyweight/(body height in m)^2.[9] Furthermore, I have excluded children with missing information about their age, sex, TV consumption on weekdays, and having their own bedroom. Moreover, observations are excluded from the sample that do not inform about the number of computers in the household, ownership of a car, family wealth, and whether the mother or the father lives in the main home.

An appropriate control group is similar to the treatment group but without the implemented policy. This approach enables us to attribute the change in the outcome to the implemented policy in the treatment group (Taillie et al., 2017). Therefore, I use a neighbouring country, without a soda tax in force, as a control group. As Table 2.3 shows, Croatia constitutes the control group for Hungary and Spain for France. Hungary has implemented the tax in 2011 and France followed one year later, yet the data is available every four years. Therefore, the survey year 2010 constitutes the pre-treatment period and 2014 the post-treatment period. In the treatment as well as in the control group are 11 to 15 years old surveyed pupils.

Table 2.3: Treatment and control groups

Panel A: Hungary and Croatia

	Treatment		Control	
	Country	Survey year	Country	Survey year
Pre-treatment	Hungary	2010	Croatia	2010
Post-treatment	Hungary	2014	Croatia	2014

Panel B: France and Spain

	Treatment		Control	
	Country	Survey year	Country	Survey year
Pre-treatment	France	2010	Spain	2010
Post-treatment	France	2014	Spain	2014

Table 2.4 reports the descriptive statistics for the treated children living in France and the untreated children living in Spain separately. For either group, the mean and the standard deviation (std.dev) of the variables are provided. The last two columns contain the mean differences across groups as well as the p-values. In France, the share of boys and girls in the survey is almost bal-

[9]https://projekte.uni-hohenheim.de/wwwin140/info/interaktives/bmi.htm

anced and children are on average 13.5 years old. The children watch on average 2 hours TV on a weekday and the majority has their own bedroom. Most children live together with their mother and father at home. The family posses on average 1.7 cars and 2.3 computers, and reports well-being between "quite well of" and "average". All these before mentioned control variables are statistically significantly different across children in France and Spain. The lower part of Table 2.4 presents the descriptive of the outcome variables. The frequency of consumed sodas is measured as a categorical variable, a value of four corresponds to a soda consumption on two to four days a week. A body mass index (BMI) of 19 is within the normal range.[10] The mean differences of the two outcome variables are statistically significant between children in France and Spain. Regarding the sample size, 8,821 children participated in the survey in France and 9,744 children participated in Spain which sums up to 18,565 observations in total.

Table 2.4: Descriptive statistics: France (treated) and Spain (non-treated)

	Treated		Non-treated			
	mean	std.dev	mean	std.dev	mean difference	p-value
	Time					
Year	2011.97	2.00	2012.32	1.97	-0.35	0.00
	Control variables					
Child characteristics						
Female (Dummy)	0.51	0.50	0.52	0.50	-0.02	0.04
Age (in years)	13.54	1.65	13.68	1.61	-0.14	0.00
TV consumption on a weekday (categorical)	2.09	1.77	2.00	1.60	0.09	0.00
Household characteristics						
Mother living at main home (Dummy)	0.92	0.27	0.96	0.20	-0.04	0.00
Father living at main home (Dummy)	0.75	0.44	0.83	0.37	-0.09	0.00
Number of family cars	1.69	0.53	1.54	0.58	0.15	0.00
Own bedroom (Dummy)	0.85	0.36	0.83	0.38	0.02	0.00
Number of computers per family	2.30	0.80	2.23	0.83	0.07	0.00
Family well-off (categorical)	2.28	0.85	2.93	0.50	-0.65	0.00
	Outcome variables					
Frequency of sodas (categorical)	3.97	1.87	3.85	1.76	0.12	0.00
Body mass index (BMI)	18.97	3.18	19.94	3.30	-0.97	0.00
Number of observations	8,821		9,744			

Source: Health behaviour of school-aged children (HBSC)

Table 2.5 reports the descriptive statistics for the second country-pair. Children living in Hungary belong to the treatment group and children living in Croatia form the control group. TV consumption is slightly higher among the children in Croatia and it is more likely that the father lives in the main household in Croatia. Families in Croatia have a higher probability to have more than one car in

[10]https://www.stanfordchildrens.org/en/topic/default?id=determining-body-mass-index-for-teens-90-P01598

Table 2.5: Descriptive statistics: Hungary (treated) and Croatia (non-treated)

	Treated		Non-treated			
	mean	std.dev	mean	std.dev	mean difference	p-value
	Time					
Year	2011.74	1.98	2011.64	1.97	0.09	0.00
	Control variables					
Child characteristics						
Female (Dummy)	0.52	0.50	0.51	0.50	0.00	0.64
Age	13.59	1.65	13.68	1.66	-0.10	0.00
TV consumption on a weekday (categorical)	2.04	1.65	2.48	1.74	-0.43	0.00
Household characteristics						
Mother living at main home (Dummy)	0.95	0.23	0.98	0.13	-0.04	0.00
Father living at main home (Dummy)	0.74	0.44	0.94	0.24	-0.20	0.00
Number of family cars	1.04	0.71	1.34	0.59	-0.30	0.00
Own bedroom (Dummy)	0.73	0.44	0.67	0.47	0.07	0.00
Number of computers per family	1.83	0.87	1.73	0.85	0.10	0.00
Family well-off (categorical)	2.40	0.83	2.15	0.91	0.25	0.00
	Control variables					
Frequency of sodas (categorical)	4.03	1.98	3.94	1.84	0.09	0.00
Body mass index (BMI)	19.54	3.49	19.90	3.24	-0.36	0.00
Number of observations	7,544		9,919			

Source: Health behaviour of school-aged children (HBSC)

comparison to families in Hungary, whereas children in Hungary are more likely to have their own bedroom. The number of computers at home is slightly higher in Hungary than in Croatia, whereas the families in Croatia score higher in being well off. Children in Hungary consume slightly more frequently sodas than children in Croatia. The BMI is slightly higher in Croatia than in Hungary. There are 7,544 treated children in Hungary and 9,919 non-treated children in Croatia, so 17,463 children in total.

2.5 Econometric approach

In this chapter, I discuss the Difference-in-Differences (DiD) strategy for identifying the Average Treatment Effect on the Treated (ATET) (see e.g. Lechner (2010)), i.e. among children living in Hungary or France in 2014. The potential outcome Y (e.g. frequency of consumed sodas) depends on the time period t $\in \{0, 1\}$ and the potential treatment state d $\in \{0, 1\}$. The notation Y_t^d indicates the potential outcome in the potential treatment state d and in time period t. For example, the potential outcome of the treatment group (d = 1) in the pre-treatment period (t = 0) is represented by Y_0^1. This notation facilitates to state the identifying assumptions of the DiD framework, see Lechner (2010):

The first assumption, formulated in equation 2.1, implies the exogeneity of the covariates (X).

This assumption would be violated if the soda tax affects the characteristics of the children or the household. Time-independent covariates, like gender, cannot be affected by the soda tax because they are constant over time. Time-dependent variables may be affected by the treatment, especially if these variables are measured after the implementation of the soda tax. Since I use repeated cross-sections, the covariates are measured in 2014, whereas the soda tax is in force since 2011 or 2012 respectively. However, it is rather unlikely that the soda tax affects, for example, children's TV consumption or whether the mother lives at the main home or not.

$$X^1 = X^0 = X;\ \forall x \in \chi. \tag{2.1}$$

The main identifying assumption in the context of DiD is the common trend assumption, formally stated in equation 2.2. Intuitively speaking, the soda consumption and the BMI of children living in Hungary and Croatia, would follow the same time trend in the absence of the soda tax.[11] For this reason, I need to control for child and household covariates that would lead to different time trends. For example, boys and children from low-income families consume more sugar-sweetened sodas than girls and children from more privileged families (Schröder et al., 2021), hence the time trend differs between these groups. I provide a placebo test conditional on covariates using unaffected periods in Table 2.7 in Section 2.6 to support this assumption.

$$\begin{aligned}
&E[Y_0^1|X = x, D = 1] - E[Y_0^0|X = x, D = 1] = \\
&E[Y_0^1|X = x, D = 0] - E[Y_0^0|X = x, D = 0] = \\
&E[Y_0^1|X = x] - E[Y_0^0|X = x];\ \forall x \in \chi.
\end{aligned} \tag{2.2}$$

A further assumption rules out an anticipatory effect (θ) of the policy in the pre-treatment period (t = 0) as formulated in equation 2.3. Accordingly, children in the treated countries Hungary and France must not anticipate the effect of the soda tax in 2010 and their soda consumption must not change prior to the implementation of the tax. Since the tax was discussed from 2005 to 2011 in the

[11] This assumption must hold for France and Spain too.

parliament in France, it might have raised the awareness of unhealthy beverages among the French children. However, the decision to pass this law was unexpected and the implementation time of five months was rather short (Le Bodo et al., 2019). In Hungary, the law was passed one and a half months before it came into force (Ecorys, 2014), which represents also a short period for anticipatory behaviour.

$$\theta_0(x) = 0;\ \forall x \in \chi. \tag{2.3}$$

The last assumption is known as the common support assumption and is formulated in equation 2.4. It ensures that for each child in Hungary in the year 2014, another child exists with the same characteristics in the following three groups: i) Hungary in 2010, ii) Croatia in 2010, and iii) Croatia in 2014.[12] Under assumptions 2.1 - 2.4, the ATET is identified.

$$\begin{aligned} &P[TD = 1|X = x, (T, D) \in (t, d), (1, 1)] < 1; \\ &\forall (t, d) \in \{(0, 1), (0, 0), (1, 0)\};\ \forall x \in \chi. \end{aligned} \tag{2.4}$$

A standard DiD approach models a linear relationship between the policy and the outcome, in this case, the outcome variable is continuous. The variable "Frequency of sodas" is measured as a categorical variable in the HBSC dataset. Therefore, this variable is a limited dependent variable, implying a non-linear relationship between the policy and the outcome. However, considering the non-linearity may lead to the violation of the identifying assumption of the DiD, the common trend assumption (Lechner, 2010). To deal with this issue, I use a semi-parametric approach to model the relationship in a more flexible way than a parametric approach.

Equation 2.5 describes the identification of the semi-parametric ATET based on inverse probability weighting (Huber, 2019). The outcome variable Y is multiplied by an inverse probability weight, where Π gives the share of treated observations in the post-treatment period and $\rho_{d,t}(X)$ is the probability of

[12]This assumption holds for France in 2014 too. In this case, the three groups are i) France in 2010, ii) Spain in 2010, and iii) Spain in 2014.

being in the treatment state d and in the time period t, conditional on covariates X. This propensity score is estimated by probit.

$$E\left[\left\{\frac{DT}{\Pi} - \frac{D(1-T)\rho_{1,1}(X)}{\rho_{1,0}(X)\Pi} - \left(\frac{(1-D)T\rho_{1,1}(X)}{\rho_{0,1}(X)\Pi}\right) - \frac{(1-D)(1-T)\rho_{1,1}(X)}{\rho_{0,0}(X)\Pi}\right)\right\} Y\right],$$
$$where\ \Pi = Pr(D=1, T=1),\ \ \rho_{d,t}(X) = Pr(D=d, T=t|X). \tag{2.5}$$

To ensure that the common trend assumption holds, I include the following covariates (X) in the estimation: On the individual level, I control for age and sex of the child, because older children reveal a different soda consumption than younger children and boys differ in their consumption behaviour from girls (Vereecken et al., 2005). Since TV consumption was associated with soda consumption (see for example Andreyeva et al. (2011) and Grimm et al. (2004)), I control for television consumption on a weekday. On the household level, I take into account several characteristics: Firstly, I control for the household structure, in particular, whether the mother or the father lives in the same household as the child. Secondly, I control for the wealth of the family, because it is associated with different soda consumption levels (Drewnowski et al., 2019). I use the following proxies for family's wealth: Ownership of a family car, number of computers in the household, well-off of the family, a dummy indicating whether the child has his/her own bedroom. Furthermore, soda consumption increases with wealth in Eastern European countries, whereas it decreases in Western European countries (Vereecken et al., 2005). Country-specific characteristics, like the growth of the Gross Domestic Product (GDP), may affect the soda consumption of its inhabitants and thus bias the results. Controlling for country-specific covariates could serve as a solution for this problem, yet this is not possible because of the multi-collinearity with the treatment. Therefore, I inspect the GDP growth of each country pair in Section 2.6.

For the estimation, I use the didweight command of the causalweight package in R, with the default number of bootstrap replications of 1,999 to calculate the standard errors, and the default trimming rule of 0.05 to drop observations with an extreme propensity score from the sample. Furthermore, I use no clusters in the analysis. Clustered standard errors would help to get consistent standard errors given the number of clusters is large enough. However, in this setting there is only one country in the treatment group. I do not use clustered standard errors on the country level because whenever bootstrap does not draw the one treated country, the observation is dropped. In the Appendix, school-year specific clustering is used as a robustness check. Since the didweight command is designed for one pre- and one post-treatment-period, I use the survey years 2010 and 2014 in the estimation. Several pre-treatment years are available to test the parallel trend assumption. I use the survey years 2006 and 2010 and run the estimation with a fake treatment in the latter.

2.6 Results

This chapter provides the estimated results as well as the sensitivity analysis. Table 2.6 presents the effects of the implemented tax, the standard errors are estimated by bootstrap, the p-values are obtained from t-tests. Panel A reports the effect of the policy package in Hungary, whereas Croatia constitutes the control group. The findings point at the first glance to a counter-intuitive positive and significant ($p < 0.01$) effect of the tax on consumption behaviour among school-aged children.[13] Since a range of products are taxed, the substitution of sugar-sweetened products might drive this result. For example, a survey among adults who changed their nutritional behaviour due to the PHPT suggested that 52% substituted energy drinks with sodas (Martos et al., 2016), which might be driven by the higher tax on energy drinks.[14] However, another driver of this result might be the relative stronger GDP growth in Hungary compared to Croatia (see Figure 2.B.1 in the Appendix), i.e. children in Hungary could spend more money for sodas.[15] The second outcome, children's BMI is not affected by the tax in Hungary. Panel B in Table 2.6 reports the effects of the soda tax on the frequency of sugar-sweetened soda consumption and the BMI for French children (treated) and Spanish children (untreated). The effects have the expected negative sign but are insignificant. This result is consistent with analyses of large quantities of soft drinks purchased at the aggregate level such as households (Capacci et al., 2019) and industry (Kurz and König, 2021), which find only a small and imprecisely estimated decrease in soft drinks purchased. But Table 2.B.2 shows larger GDP growth in Spain than in France over time which might affect the result.

[13] The effect size is not directly interpretable because soda consumption is measured as a categorical variable.

[14] 250 HUF/l if taurin > 100 mg per 100 ml or 40 HUF/l if no taurin but methylxanthine > 15 mg per 100 ml see Bíró (2015).

[15] Due to the multicollinearity with the treatment, I cannot control for the GDP growth.

Table 2.6: Empirical results

Panel A: Hungary and Croatia

	Effect	Standard error	P-value	Number of observations
Frequency of sodas	0.35	0.07	0.00	18,712
Body Mass Index (BMI)	0.12	0.13	0.36	17,553

Panel B: France and Spain

	Effect	Standard error	P-value	Number of observations
Frequency of sodas	-0.08	0.08	0.31	20,951
Body Mass Index (BMI)	-0.07	0.15	0.66	18,723

Note: Standard errors are estimated by bootstrap.

A downside of non-clustered standard errors in a DiD setting is the possibility of underestimating the standard errors (Bertrand et al., 2004). To check whether the empirical results in Table 2.6 are robust, I have re-estimated the results with clustered standard errors. The most conservative approach would be to cluster standard errors on an aggregate level. However, it is not possible to cluster on the country level in this setting, because of the few numbers of treated countries. Whenever cluster bootstrap does not draw the treatment group, the procedure does not work. Hence I cluster on the next lowest level which has variation in the data: the school-year level. Table 2.B.1 in the Appendix shows that the clustered standard errors are almost equal to the robust standard errors in Table 2.6.

The identifying assumption of the DiD approach is the parallel trend assumption, implying that, conditional on the covariates, the treatment and control group follow the same time trend in the absence of the treatment. This assumption is not testable, yet it is possible to conduct a placebo test to support this assumption: I use the two pre-treatment periods 2006 and 2010 and pretend that in the latter a 'fake treatment' was implemented. Table 2.7 reports large p-values in both panels, which supports the parallel trend assumption.

As a robustness test, I use another neighbouring country as an alternative control group in each panel: I substitute Croatia with Slovakia and Spain with Switzerland. Table 2.B.2 and Table 2.B.3 in the Appendix report the descriptive statistics of the children and the household for these groups. Table 2.B.4 in the Appendix shows the placebo test of the unaffected periods for Hungary and Slovakia (Panel A) and France as well as Switzerland (Panel B) separately. It suggests that the parallel trends

Table 2.7: Unaffected periods

Panel A: Hungary and Croatia

	Effect	Standard error	P-value	Number of observations
Frequency of sodas	0.10	0.08	0.23	19,069
Body Mass Index (BMI)	-0.06	0.15	0.67	17,949

Panel B: France and Spain

	Effect	Standard error	P-value	Number of observations
Frequency of sodas	0.09	0.08	0.26	24,919
Body Mass Index (BMI)	0.01	0.15	0.96	21,826

Note: Standard errors are estimated by bootstrap.

assumption holds for both Panels, except for the outcome variable BMI in Panel A. Finally, Table 2.8 reports the results for robustness test. Even if the control group changes we find a highly statistically significant positive effect on the frequency of consumed sodas in Hungary. In France, the results on the soda consumption and the BMI have a negative sign, yet they are insignificant as in the main results in Table 2.6. Figure 2.B.3 and 2.B.4 in the Appendix report a parallel GDP growth of each country pair prior to the measured effect in 2014. Therefore, GDP growth can be excluded as a driver for the increase in soda consumption in Hungary.

Table 2.8: Robustness test

Panel A: Hungary and Slovakia

	Effect	Standard error	P-value	Number of observations
Frequency of sodas	0.48	0.09	0.00	15,425
Body Mass Index (BMI)	-0.13	0.15	0.38	14,059

Panel B: France and Switzerland

	Effect	Standard error	P-value	Number of observations
Frequency of sodas	-0.04	0.07	0.52	22,986
Body Mass Index (BMI)	-0.13	0.11	0.24	20,258

Note: Standard errors are estimated by bootstrap.

2.7 Conclusion

This paper examines the effect of two different health policies on sugar-sweetened soda consumption behaviour and body mass index (BMI) of school-aged children in Europe. Hungary has implemented a Public Health Product Tax (PHPT) on several unhealthy products, including sugar-sweetened sodas, in 2011, while France only taxes sodas, containing sugar and artificial sweetener, since 2012. Methodologically, I apply a Difference-in-Differences (DiD) approach to evaluate this natural experiment and use neighbouring countries without such a soda tax as a control group. I analyse the effect in Hungary and France separately, because of the different policy designs. Since the frequency of soda consumption is measured by a categorical scale, I use a semi-parametric method to estimate the effect in a flexible way. To the best of my knowledge, this is the first paper analysing the impact of two different soda taxes on the consumption and health of school-aged children.

The results suggest that the PHPT had a statistically significant effect ($p < 0.01$) on soda consumption of school-aged children in Hungary, yet the sign is unexpectedly positive. An explanation for this counter-intuitive result might be the substitution behaviour among children, as the price of other unhealthy products, such as energy drinks or syrups, are taxed even higher. In France, the soda consumption of school-aged children is not affected by the soda tax. This result is in line with the analyses of Capacci et al. (2019) and Kurz and König (2021), who use soda sales data at a more aggregated level and find a very small, but not robust effect on the quantity of purchased sodas. Capacci et al. (2019) explain this finding by the very low tax level of 7.16 Eurocents per litre.

Moreover, I analyse the effect on children's BMI and find neither in France nor in Hungary a statistically significant effect. This finding is consistent with Powell et al. (2009) who analyses the effect of soda tax on BMI among adolescents. Regarding the sensitivity analysis, I run a placebo test with two unaffected periods. The results suggest an insignificant effect, which supports the parallel trend assumption. The results are robust to an alternative control group.

Consequently, policy makers should think carefully about the design and the tax rate before implementing a soda tax. The availability of data about children's quantity of soda consumption would help to estimate the effect of the soda tax in a more precise way.

Appendices

A Acknowledgment

HBSC is an international study carried out in collaboration with WHO/EURO. The International Coordinator of the 2005/06 survey was Prof. Candace Currie and the Data Bank Manager was Prof. Oddrun Samdal.

B Appendix

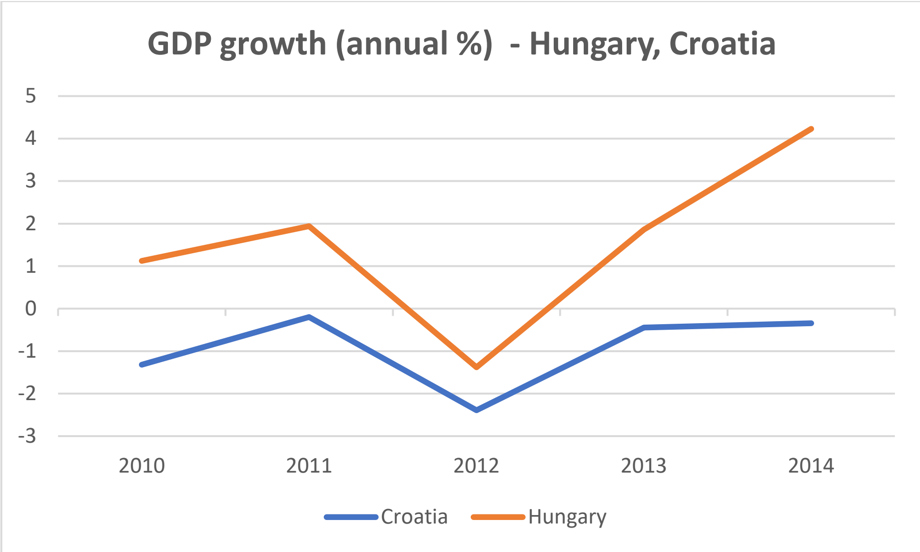


Figure 2.B.1: GDP growth in Hungary and Croatia

Source: The World Bank[16]

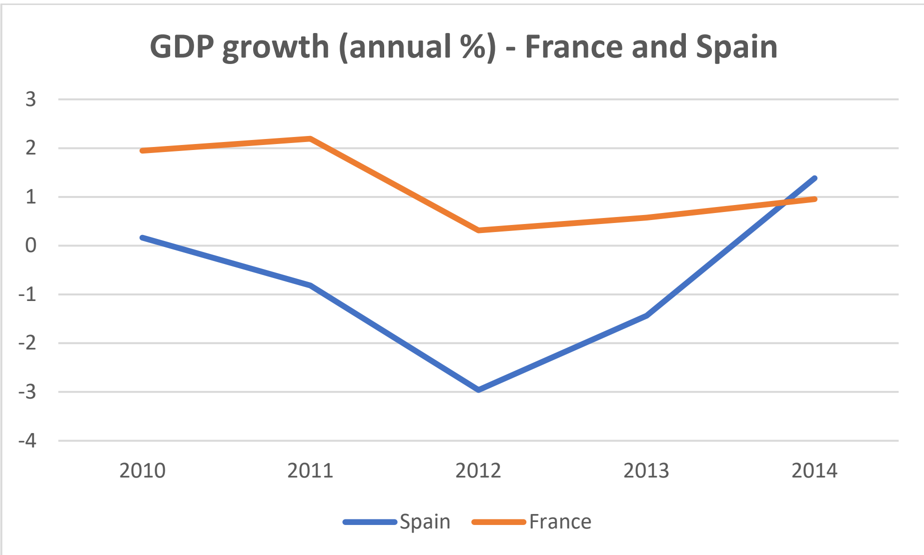


Figure 2.B.2: GDP growth in France and Spain

Source: The World Bank[17]

[16] https://data.worldbank.org/indicator/NY.GDP.MKTP.KD.ZG?end=2014&locations=HU-HR&start=2010, last accessed: 10.11.2021.

[17] https://data.worldbank.org/indicator/NY.GDP.MKTP.KD.ZG?end=2014&locations=FR-ES&start=2010, last accessed: 10.11.2021.

Table 2.B.1: Results with school-year specific clusters

Panel A: Hungary and Croatia

	Effect	Standard error	P-value	Number of observations
Frequency of sodas	0.35	0.10	0.01	18,712
Body Mass Index (BMI)	0.12	0.15	0.42	17,553

Panel B: France and Spain

	Effect	Standard error	P-value	Number of observations
Frequency of sodas	-0.08	0.09	0.36	20,951
Body Mass Index (BMI)	-0.07	0.15	0.67	18,723

Note: Bootstrapped and clustered standard errors.

Table 2.B.2: Descriptive statistics: Hungary (treated) and Slovakia (non-treated)

	Treated		Non-treated			
	mean	std.dev	mean	std.dev	mean difference	p-value
	Time					
year	2011.74	1.98	2011.53	1.94	0.21	0.00
	Control variables					
Female	0.52	0.50	0.53	0.50	-0.01	0.37
Age	13.59	1.65	13.98	1.35	-0.39	0.00
TV consumption on a weekday	2.04	1.65	2.64	1.77	-0.60	0.00
Mother living at main home (Dummy)	0.95	0.23	0.96	0.19	-0.02	0.00
Father living at main home (Dummy)	0.74	0.44	0.88	0.33	-0.14	0.00
Number of family cars	1.04	0.71	1.24	0.66	-0.20	0.00
Own bedroom (Dummy)	0.73	0.44	0.58	0.49	0.15	0.00
Number of computers per family	1.83	0.87	1.95	0.87	-0.12	0.00
Family well-off	2.40	0.83	2.07	0.82	0.33	0.00
	Outcome variables					
Frequency of sodas	4.03	1.98	4.36	1.89	-0.33	0.00
Body mass index (BMI)	19.54	3.49	19.54	3.07	0.00	0.99
	7,544		6,419			

Source: Health behaviour of school-aged children (HBSC)

Table 2.B.3: Descriptive statistics: France (treated) and Switzerland (non-treated)

	Treated		Non-treated			
	mean	std.dev	mean	std.dev	mean difference	p-value
	Time					
year	2011.97	2.00	2011.93	2.00	0.04	0.15
	Control variables					
Female	0.51	0.50	0.50	0.50	0.01	0.16
Age	13.54	1.65	13.58	1.57	-0.04	0.08
TV consumption on a weekday	2.09	1.77	1.50	1.35	0.59	0.00
Mother living at main home (Dummy)	0.92	0.27	0.97	0.18	-0.05	0.00
Father living at main home (Dummy)	0.75	0.44	0.81	0.39	-0.06	0.00
Number of family cars	1.69	0.53	1.46	0.60	0.24	0.00
Own bedroom (Dummy)	0.85	0.36	0.87	0.33	-0.03	0.00
Number of computers per family	2.30	0.80	2.41	0.76	-0.11	0.00
Family well-off	2.28	0.85	2.50	0.84	-0.22	0.00
	Outcome variables					
Frequency of sodas	3.97	1.87	4.11	1.85	-0.14	0.00
Body mass index (BMI)	18.97	3.18	19.01	3.04	-0.04	0.38
	8,821		11,352			

Source: Health behaviour of school-aged children (HBSC)

Table 2.B.4: Unaffected periods

Panel A: Hungary and Slovakia

	Effect	Standard error	P-value	Number of observations
Frequency of sodas	0.06	0.09	0.51	16,012
Body Mass Index (BMI)	-0.33	0.14	0.02	14,866

Panel B: France and Switzerland

	Effect	Standard error	P-value	Number of observations
Frequency of sodas	-0.01	0.06	0.84	22,845
Body Mass Index (BMI)	-0.13	0.09	0.15	20,646

Note: Standard errors are estimated by bootstrap.

Table 2.B.5: Unaffected periods with school-year specific clusters

Panel B: France and Switzerland

	Effect	Standard error	P-value	Number of observations
Frequency of sodas	-0.01	0.07	0.86	22,845
Body Mass Index (BMI)	-0.13	0.11	0.21	20,646

Note: Bootstrapped and clustered standard errors.

Table 2.B.6: Results with school-year specific clusters

Panel A: Hungary and Slovakia

	Effect	Standard error	P-value	Number of observations
Frequency of sodas	0.48	0.11	0.00	15,425
Body Mass Index (BMI)	-0.13	0.18	0.46	14,059

Panel B: France and Switzerland

	Effect	Standard error	P-value	Number of observations
Frequency of sodas	-0.04	0.07	0.57	22,986
Body Mass Index (BMI)	-0.13	0.11	0.24	20,258

Note: Bootstrapped and clustered standard errors.

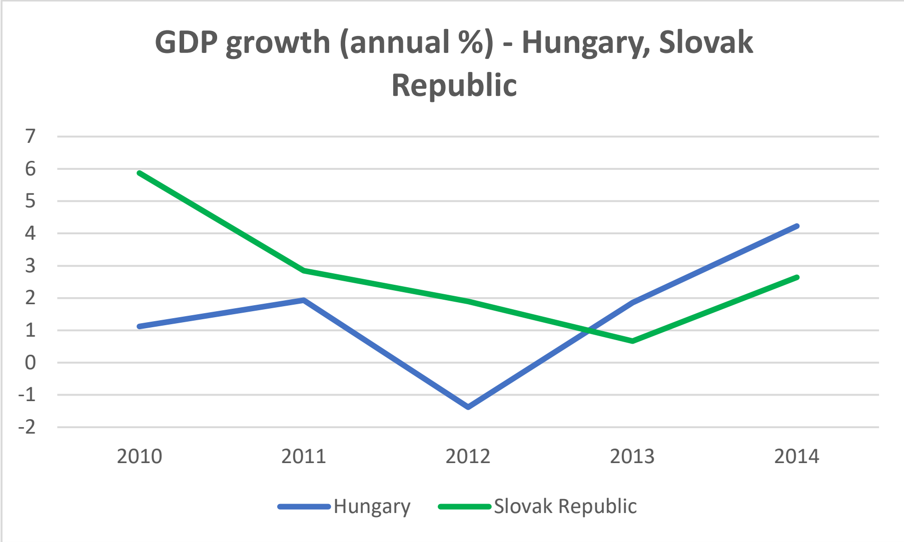


Figure 2.B.3: Parallel GDP growth in Hungary and Slovakia

Source: The World Bank[18]

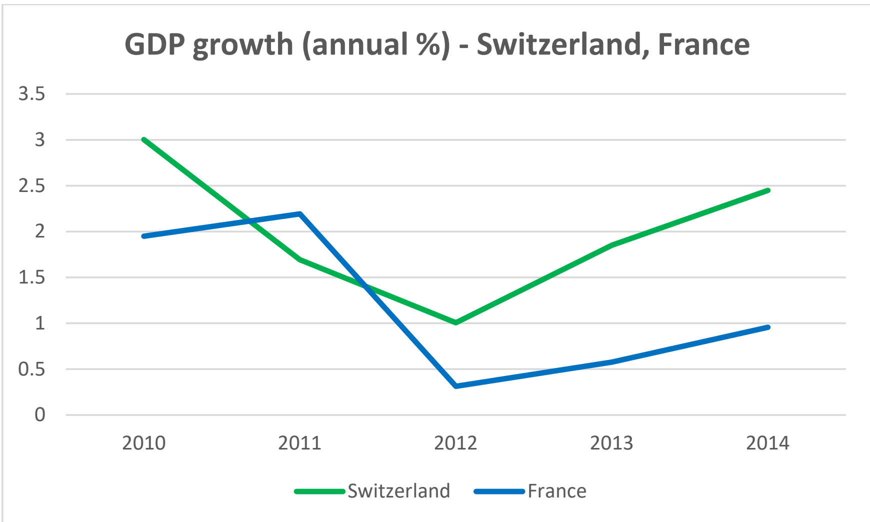


Figure 2.B.4: Parallel GDP growth in France and Switzerland

Source: The World Bank[19]

[18] https://data.worldbank.org/indicator/NY.GDP.MKTP.KD.ZG?end=2014&locations=HU-SK&start=2010, last accessed: 29.03.2021.

[19] https://data.worldbank.org/indicator/NY.GDP.MKTP.KD.ZG?end=2014&locations=HU-SK&start=2010, last accessed: 29.03.2021.

3 From homemakers to breadwinners? How mandatory kindergarten affects maternal labour market attachment*†

with Martin Huber

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

*We thank Laura Ravazzini for her helpful suggestions on a prior version of this paper. We have benefited from comments by seminar participants in Fribourg, the Ski & Labour Seminar in Engelberg-Titlis, the 9th ifo Dresden Workshop on Labour Economics and Social Policy, the 10th International Conference of Panel Data Users in Switzerland in Lausanne, the Swiss Society of Economics and Statistics Annual Meeting in Geneva, the EEA in Manchester, the 3rd International BFH Conference on Discrimination in the Labor Market in Bern, the Verein für Socialpolitik - Annual Conference 2019, the 1st Workshop of the Swiss Network on Public Economics, and the Lunch-time Meeting at the University of Innsbruck 2021. Addresses for correspondence: Martin Huber, University of Fribourg, Bd. de Pérolles 90, 1700 Fribourg, Switzerland; selina.gangl@unifr.ch, martin.huber@unifr.ch. Declaration of conflicts of interest (all two authors): none. Corresponding author: Martin Huber.
† A previous version of the paper was published as a working paper on EconStor: ”From housewives to employees? How mandatory kindergarten affects mothers’ labour supply in Switzerland”

3.1 Introduction

The labour 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%.[20] 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%.[21] Albeit this figure is above the OECD average, it is largely due to part-time (63%), rather than full-time employment (15%).[22]

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 in the long term (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 increases 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' labour 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

[20] Reported by OECD. https://www.oecd.org/els/family/database.htm, retrieved 2021-08-24.
[21] Reported by OECD. https://www.oecd.org/els/family/database.htm, retrieved 2021-08-24.
[22] Reported by OECD. https://www.oecd.org/els/family/database.htm, retrieved 2021-08-24.

and the birthday cut-off dates across cantons. We exploit these two cantonal differences in our 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. These 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 have probably finished 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 as 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' labour market behaviour: We find that the maternal labour force participation 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 that the school entry of the child raises 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,[23] we address younger children in the preschool age. Furthermore, we contribute to the literature analysing countries with a rather high maternal labour force participation rate.

The remainder of this paper is organised 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 from the empirical analysis.

3.2 Institutional background: Kindergarten reform

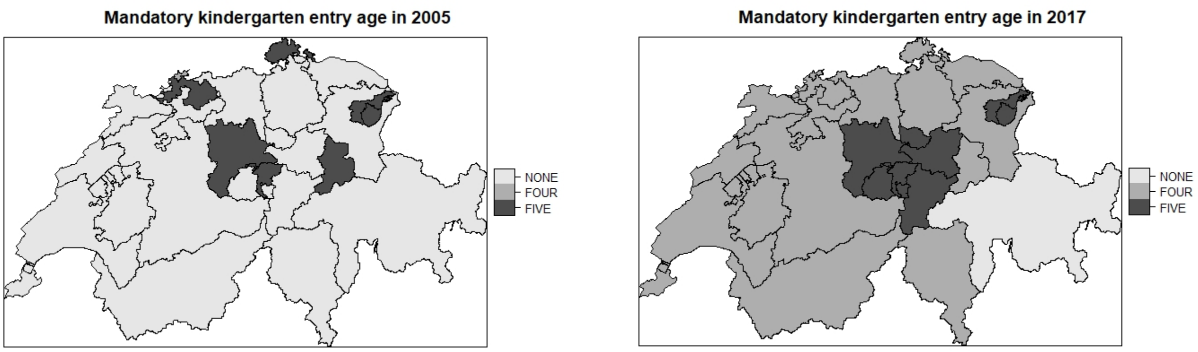
This section provides an overview of the initial situation and the education policy reform in Switzerland. Switzerland regulates education policy at the cantonal level, consequently, the quality and quantity of education varies from canton to canton. For example, the school curriculum, entrance age, 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, 15 cantons entered into the concordat, while seven cantons rejected it and four cantons are still indecisive.[24] The main goal of the concordat is to harmonise the mandatory school education

[23] Goux and Maurin (2010) analyse two-year-old children.
[24] as of 2019

among the cantons. This agreement also includes an age decrease 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 3.1 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, 17 of 26 cantons had implemented an obligation to attend kindergarten for four-year-olds.[25] Eight cantons have implemented mandatory kindergarten for five-year-olds.[26] In the canton Grisons, children are generally not obliged to attend kindergarten. Not directly observable from Figure 3.1 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 3.1: Expansion of the kindergarten reform



Source: The map of Switzerland and its subdivisions is downloaded from the GADM homepage,[27] 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 Classification of Education (ISCED) (Eurydice, 2016/17). In the German- and Italian-speaking parts

[25] Aargau, Basel-Land, Basel-Stadt, Bern, Fribourg, Geneva, Glarus, Jura, Neuchâtel, St Gallen, Schaffhausen, Solothurn, Thurgau, Ticino, Vaud, Valais, and Zurich.

[26] Appenzell Outer-Rhodes, Appenzell Inner-Rhodes, Lucerne, Nidwalden, Obwalden, Schwyz, Uri, and Zug.

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). As in school, there is a curriculum for kindergarten, which is 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.[28] 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 exists at least since 2007. We can distinguish between the implementation behaviour of four groups: Cantons that have implemented mandatory kindergarten (Table 3.1) have either implemented the mandatory offer simultaneously or prior to the reform.[29] Cantons which have a voluntary kindergarten (Table 3.2) have either implemented the mandatory offer or not.

The kindergarten attendance 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 implemented the obligation. The aim of the kindergarten policy is twofold: Children's develop-

[28] EDK Kantonsumfrage 2016/17

[29] 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 3.1: Mandatory kindergarten & 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 3.2: Voluntary kindergarten & Mandatory offer

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

ment 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.

3.3 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[30]. This chapter presents the hypothesis and the possible channels.

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

[30] four-years-olds

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).[31] 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, labour 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 increases 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[32] 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 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,

[31] Exception Ticino: kindergarten offer for 3-year-old children.
[32] The number of cantons varies over time

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 hypothesise 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.

3.4 Data

3.4.1 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 these 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[33]. Subsequently, we generated the variable "In labour 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 labour 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[34] in the OASI data and keep 289,631 observations. We exclude observations, which live in cantons without a mandatory kindergarten for four-years-old. Since two sources[35] reported two consecutive days as birthday cut-off dates in Basel-Stadt, we took

[33] excluding agriculture

[34] for example due to adjustment entries

[35] 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.

the later cut-off date and excluded observations on the previous day from our analysis[36]. Finally, 201,993 observations remain in our evaluation dataset.

Figure 3.2 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).

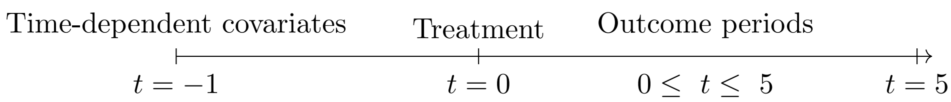


Figure 3.2: Timeline of measured variables

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 3.3 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 stem from dependent employment. Fathers labour 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%).

Table 3.4 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

[36] to run a Donut-RDD for this canton

Table 3.3: Descriptive statistics

	Sample mean	Coefficient	Standard error	P-value	Relative number of missings in %
Mother's characteristics					
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
Nationality					
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
Age					
Age	35.62	0.01	0.14	0.92	6.70
Partnership					
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
Mother's labour market characteristics					
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 labour 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
Father's labour market characteristics					
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 labour 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
Cantonal characteristics					
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

Valais (Upper Valais and Central/Lower Valais), we analyse these regions separately for the canton Valais. In most cases, our tests do not reject the null hypothesis that the cantonal dummies are balanced around the birthday cut-off, with the two exceptions of Basel-Land and Ticino.[37]

Table 3.5 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 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

[37]Ticino is the only canton implementing mandatory kindergarten offer for 3-year-old children.

Table 3.4: 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

to a total average annual gross income of 35,008 CHF.

Table 3.5: 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 labour 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

3.4.2 Data for the Difference-in-Differences Approach

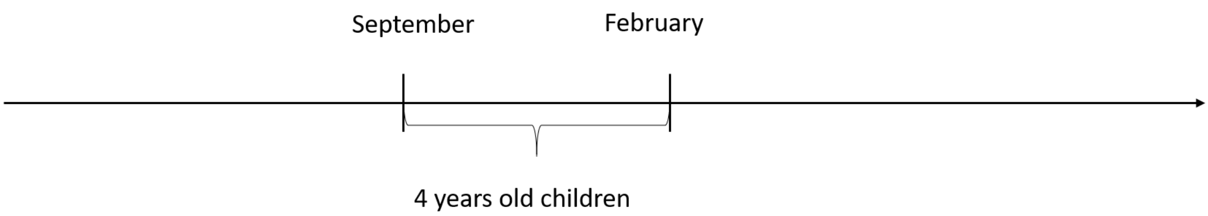
For the second evaluation strategy of the paper, we link data from different sources. Firstly, we use survey data from the Swiss Household Panel (SHP). This panel starts in 1999 and has drawn households by NUTS II regions. The sample is representative with respect to social groups across Switzerland. 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 these 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 "Cenral 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 3.3.

Figure 3.3: Definition of the age span



Since we use this time span, we exclude rather older children from our sample. Table 3.6 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 3.6: 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-treatment 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 3.7 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-treatment 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 3.7 gives an overview of the descriptive statistics of the outcome

Table 3.7: SHP: Descriptive statistics

	Treated		Non-treated			
	Mean	Std.dev	Mean	Std.dev	Mean difference	p-value
	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
Mothers' characteristics						
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
Cantonal characteristics						
Unemploment 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)

variables. Mothers in the treatment group are more likely to take part in the labour force, have a 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.

3.5 Econometric approach

3.5.1 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 an Regression Discontinuity Design (RDD) for evaluating the effect of mandatory kindergarten on mothers' labour 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 McCrary 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 McCrary Test extrapolates in areas away from the threshold, which leads to inconsistency if the running variable is discrete (Frandsen, 2017).

Table 3.8 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 non-linearity 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 3.8 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 3.8: 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 (3.1) presents the identification result, where γ 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 ($r = 0$).

$$\gamma = E[Y_1 - Y_0 | R = r] \tag{3.1}$$

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:

$$Y_i \; = \; \alpha \; + \beta_0 D_i \; + \; \beta_1 R_i \; + \; \beta_2 R_i * D_i \; + \; \beta_3 Z_i + \epsilon_i \tag{3.2}$$

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. β_0 is the average treatment effect of the mandatory kindergarten on mother's labour market behaviour at the threshold and β_1 and β_2 give the average effect of the running variable on the outcome. Z_i is a vector of additional covariates for individual i.[38] 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".

3.5.2 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 squares (OLS) to estimate the results.

[38] We include all covariates listed in Table 3.3 and 3.4.

Equation (3.3) shows the estimated model in our empirical analysis. The subscript i denotes the individual, s the canton, and t the time. $\mathrm{Y_{ist}}$ is a vector of the following binary dependent variables: "Out of labour force", "Unemployed", "Employed", "Part-time employment","Full-time employed", "Income from dependent work."

$$Y_{ist} = \alpha + \gamma_s + \lambda_t + \delta D_{st} + \beta X'_{ist} + \epsilon_{ist} \tag{3.3}$$

The right hand side of equation (3.3) presents the independent variables. We include canton (γ_s) and time fixed effects (λ_t), the former controls for time-invariant differences between the cantons, the latter for differences across years which are common to all cantons. $\mathrm{D_{st}}$ 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 δ gives the effect we are interested in, the average treatment effect on the treated (ATT). ϵ_{ist} corresponds to time-varying unobservables. We include a vector of time-variant as well as time-invariant covariates ($\mathrm{X_{ist}}$) 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 Tables 3.15 and 3.16 in Section 3.6.2.

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".[39] 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.

3.6 Results

3.6.1 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 3.9 reports the ITT estimates at the threshold for pooled outcome periods when including covariates and corresponding missing dummies. It

[39] 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.

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 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.[40]

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 (Tables 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 3.4 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 depicts the plots without covariates and shows similar results in

[40] R-package "sgof", command "BH", where alpha = 0.1

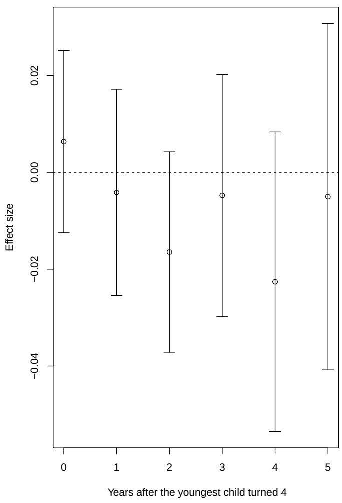
Table 3.9: RDD: Empirical results with covariates

	Sample mean	Coefficient	Standard error	P-value	Bandwidth	Observations	Observations within bandwidth
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

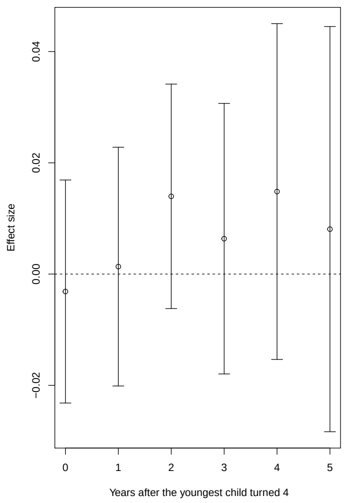
Sources: STATPOP (2010 - 2017) and OASI (2010 - 2017); calculations are done by ourselves

terms of significance, point estimates, and precision. 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 3.10 represents the results for mothers, who have an income lower than the median, including not working mothers, whereas Table 3.11 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 3.10 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 3.11 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 children.

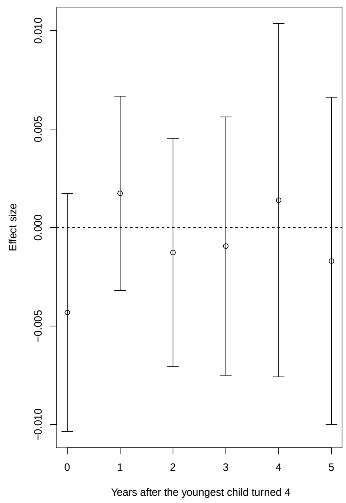
Secondly, we split the sample size in younger (age < 38, Table 3.12) and older mothers (age > 37, Table 3.13) and find that the implementation of the mandatory kindergarten has rather an effect on older mothers than younger ones. The B-H procedure supports the statistical significance at the 10% level for all four effects in Table 3.13. 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.



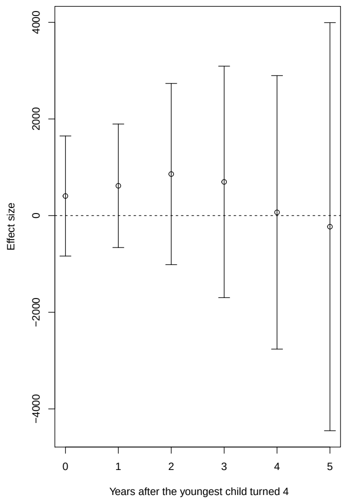
(a) Out of labour force (binary)



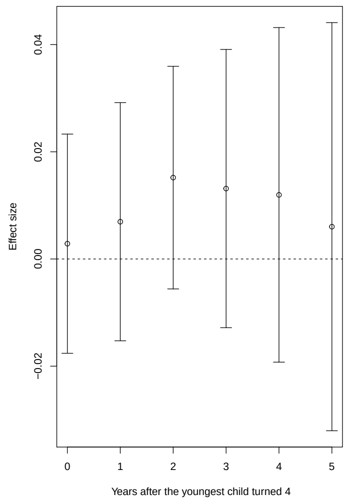
(b) Employed (binary)



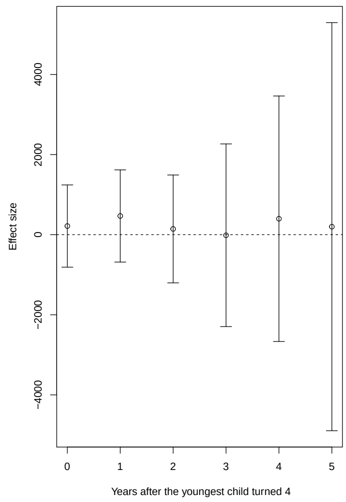
(c) Unemployed (binary)



(d) Total annual work income



(e) Income from dependent employment (binary)



(f) Income from dependent employment

Figure 3.4: RDD: Effects over years with covariates

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

Table 3.10: RDD: Heterogenous effects: Annual work income $< 15{,}706$ CHF

	Sample mean	Coefficient	Standard error	P-value	Bandwidth	Observations	Observations within bandwidth
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 3.11: RDD: Heterogenous effects: Annual work income $\geq 15{,}706$ CHF

	Sample mean	Coefficient	Standard error	P-value	Bandwidth	Observations	Observations within bandwidth
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 3.12: RDD: Heterogenous effects: Age < 38

	Sample mean	Coefficient	Standard error	P-value	Bandwidth	Observations	Observations within bandwidth
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 3.13: RDD: Heterogenous effects: Age > 37

	Sample mean	Coefficient	Standard error	P-value	Bandwidth	Observations	Observations within bandwidth
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

3.6.2 Findings from a Difference-in-Differences Approach

Subsequently, we report the results of the complementing DiD analysis. Table 3.14 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 3.7 in Section 3.4.2, 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.[41] 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 3.14: DiD: Empirical results

	socio-economic X			all X			lasso-picked X			
	effect	se	pval	effect	se	pval	effect	se	pval	obs
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 work 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

[41] 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 trend assumption we conduct two placebo tests. In Table 3.15, 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 years 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 3.16, 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 3.15: 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 3.17, 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 is

Table 3.16: 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			
	est	se	pval	est	se	pval	est	se	pval	obs
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)

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

Table 3.17: DiD: Empirical results with changed control group (only eventually treated)

	socio-economic X			all X			lasso-picked X			
	est	se	pval	est	se	pval	est	se	pval	obs
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 significant in the lasso-picked specification. 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 sample 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.

3.7 Conclusion

This paper analyses the impact of mandatory kindergarten for 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 moderate 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.[42]
	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	15.07.
	2017	31.07.
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.[43]
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).

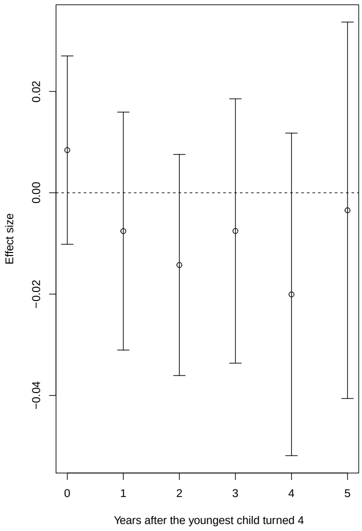
[42] Municipalities can decide whether they use the 31.7. as cut-off date from 2008 an or use this stepwise approach: `https://www.tg.ch/news/news-detailseite.html/485/news/2817/newsarchive/1`, last downloaded 2019/10/07.

[43] Transition period of 6 years.

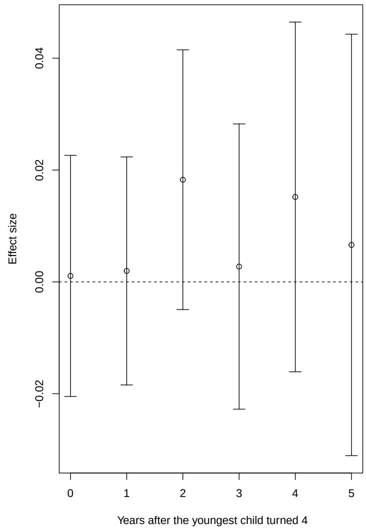
Table 3.B.1.2: RDD: Empirical results without covariates

	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

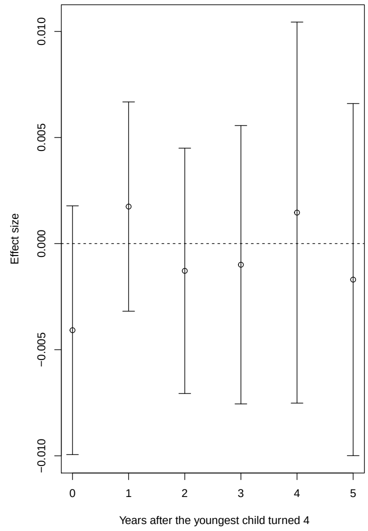
Sources: STATPOP (2010 - 2017) and OASI (2010 - 2017); calculations are done by ourselves



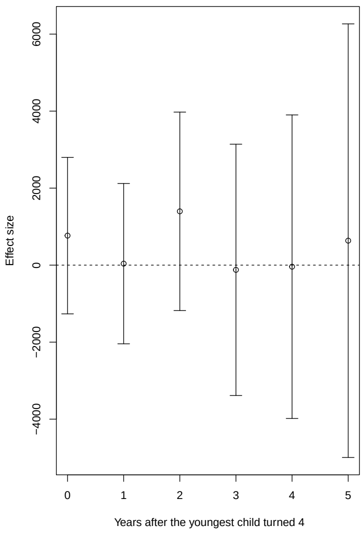
(a) Out of labour force (binary)



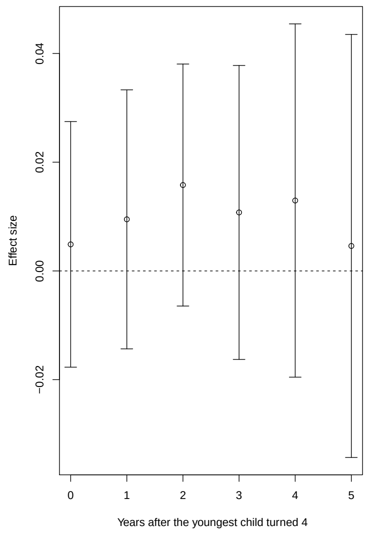
(b) Employed (binary)



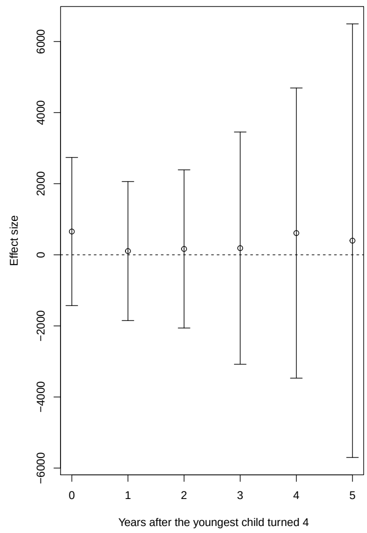
(c) Unemployed (binary)



(d) Total annual work income (in CHF)



(e) Income dependent employment (binary)



(f) Income dependent employment (in CHF)

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
Mothers' characteristics	*Mothers' characteristics*	*Mothers' characteristics*
age	age	age
age^2	age^2	
education	education	education
citizenship	citizenship	citizenship
civil status	civil status	
political attitude	political attitude	
political attitude^2	and political attitude^2	
self-reported health	self-reported health	self-reported health
Household characteristics	*Household characteristics*	*Household characteristics*
number of children in the household	number of children in the household	number of children in the household
	Cantonal characteristics	*Cantonal characteristics*
	unemployment rate	unemployment rate
	unemployment rate^2	
	mandatory kindergarten for five-year-old children	mandatory kindergarten for five-year-old children
	canton is member of Harmos	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			
	effect	se	pval	effect	se	pval	effect	se	pval	obs
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			
	est	se	pval	est	se	pval	obs
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 labour 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 *"Currently, in your main job, do you work parttime or 100% ?"*
Full-time employed	Dummy working in full-time, constructed from pw39==2 *"Currently, in your main job, do you work parttime or 100% ?"*

4 How residence permits affect the labor market attachment of foreign workers: Evidence from a migration lottery in Liechtenstein*†

with Berno Büchel and Martin Huber

Abstract

We analyze the impact of obtaining a residence permit on foreign workers' labor market and residential attachment. To overcome the usually severe selection issues, we exploit a unique migration lottery that randomly assigns access to residence permits for workers with an employment contract in Liechtenstein, which is situated centrally in Europe. Using an instrumental variable approach, our results suggest that lottery compliers raise their employment probability in Liechtenstein by on average 24 percentage points across outcome periods (2008 to 2018) as a result of receiving a permit. Relatedly, their activity level and employment duration in Liechtenstein increase by on average 20 percentage points and 1.15 years, respectively, over the outcome window. These substantial and statistically significant effects are predominantly driven by individuals not (yet) working in Liechtenstein prior to the lottery rather than by previous cross-border commuters, but even for the latter group, positive employment effects emerge in the longer run. Indeed, we find both the labor market and residential effects to be persistent even several years after the lottery with no sign of fading out. These results suggest that granting resident permits to foreign workers can be effective to foster labor supply, despite the alternative of commuting cross-border from adjacent regions.

Keywords: International migration, cross-border commuting, natural experiment, lottery

JEL classification: F22, J61.

*We are grateful to the Government of Liechtenstein for the permission to realize this research project and to the Immigration and Passport Office as well as the Office of Statistics of Liechtenstein for their valuable support concerning data provision and processing. We thank Andreas Brunhart, Cem Özgüzel, Mariola Pytlikova, Christoph Sajons, Michael Siegenthaler, and Andreas Steinmayr for their many helpful suggestions on prior versions of this paper. We have benefited from comments by seminar participants at the 2nd Workshop of the Swiss Network on Public Economics (SNoPE) (virtual), the Swiss Society of Economics and Statistics Annual Meeting (virtual), the European Society for Population Economics (ESPE) (virtual), the Competence Centre on Microeconomic Evaluation (COMPIE) (virtual), the CEMIR Junior Economist Workshop on Migration Research 2021 in Munich, the EALE Annual Conference (virtual), the Annual Conference of the Verein für Socialpolitik (virtual), and the Innsbruck - Munich Applied Micro Workshop (Obergurgl).

† A previous version of the paper was published as a CESifo working paper (Number No. 9390), a media report on this paper will soon appear on the internet platform "Ökonomenstimme."

4.1 Introduction

Many labor markets rely on both locally-born and foreign-born workers. For instance, the share of foreign-born persons in the U.S. labor market corresponds to 17.4% in 2019, amounting to 28.4 million people.[44] World-wide, the number of migrant workers is estimated to be 164 million in 2017, with 23% in North America, 32% in Europe, and 13% Asia.[45] While migration from low to higher income countries receives a lot of attention in public and scientific discussions, an immense amount of labor mobility is realized between rather developed nations, likely competing for skilled workers. For example, 54 million people migrated from one developed country to another one in 2013 (Martin, 2013). Attracting and retaining qualified foreign workers may be key for economic growth of a given country. To address the high demand for such talent, there are various policy tools, ranging from admitting cross-border commuters or seasonal workers to granting temporary or permanent residence, or even citizenship. As granting more rights to foreign workers may attract more and better talent, but may also be (perceived as) more costly and contentious, a crucial question for a policy-maker is how far one should go in this direction (e.g. Hainmueller and Hopkins, 2014; Dustmann and Görlach, 2016).

Specifically, we ask *how important is residence in a country for a worker's integration into its labor market?* Residence is not always a necessary condition for labor supply because in many countries and regions, foreign workers could also commute cross-border. In France, for instance, about 438k workers commute to another country every day for work.[46] Nevertheless, residence permits might be more successful in fostering foreign labor market attachment relative to the alternative of cross-border commuting, in particular if they come with benefits valued by foreign workers like a more favorable income taxation and/or reduced commuting times. The challenge in answering our research question is that the comparison between foreign workers with and without residence permit is generally plagued by selection bias. Driven for instance by a host country's formal eligibility criteria for residence as well as individual factors determining the

[44] Reported by U.S. Bureau of Labor Statistics, https://www.bls.gov/news.release/forbrn.nr0.htm/labor-force-characteristics-of-foreign-born-workers-summary, retrieved 2021-02-11.

[45] Reported by International Labour Organization, https://migrationdataportal.org/themes/labour-migration, retrieved 2021-02-11.

[46] Reported by EUROSTAT, https://ec.europa.eu/eurostat/statistics-explained/index.php?title=Archive:Statistics_-on_commuting_patterns_at_regional_level&oldid=463740, retrieved 2021-02-11.

inclination to reside in another country, the two groups of foreign workers are typically different to an extent that makes it prohibitively hard to isolate the effect of the residence permit from other factors. To identify the causal effect of interest we would ideally assign residence permits at random, while cross-border commuting remains a viable option.

In this paper, we assess the causal effect of obtaining a residence permit on the labor market and residential attachment of foreign workers based on an annual migration lottery that is unique in Europe. In Liechtenstein, a wealthy microstate that is situated between Austria and Switzerland, two lottery draws for residence permits are held every year. The lottery randomly assigns access to residence permits among applicants from the European Economic Area (EEA) who hold an employment contract with a company in Liechtenstein. The European Economic Area (EEA) is a free trade agreement among all member states of the European Union (EU), plus the three countries, Norway, Iceland, and Liechtenstein.[47] We are the first to link European migration lottery data with administrative data on individual labor market records. We exploit the random assignment as instrument for the reception of a residence permit that is conditional on actually moving to Liechtenstein. This allows us to assess the local average treatment effect (LATE) of moving among compliers. As there are two draws for each lottery, compliers are winners of the pre-draw of the lottery who also participate and win in the second draw and actually move to Liechtenstein, making up 36% of our sample. We consider the assignment in the pre-draw of the *first* lottery participation, as it is endogenous whether interested candidates participate multiple times in such a lottery. We apply a flexible instrumental variable (IV) estimator based on inverse probability weighting (IPW), in which we reweigh the outcomes by the inverse of the conditional instrument probability given the lottery year, the so-called instrument propensity score. This enables us to control for the fact that the share of lottery winners changes over the years as a function of lottery applicants, which is important in order to avoid confounding, e.g. due to business cycle effects.

We find that receiving a residence permit statistically significantly increases the employment probability of compliers by 24 percentage points on average across our outcome periods 2008

[47]The EU member states are Austria, Belgium, Bulgaria, Croatia, Cyprus, Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Ireland, Italy, Latvia, Lithuania, Luxembourg, Malta, Netherlands, Poland, Portugal, Romania, Slovakia, Slovenia, Spain, and Sweden.

to 2018. Likewise, it increases the activity level by 20 percentage points, and the employment duration in Liechtenstein by 1.15 years. Lottery losers also hold an employment contract with a company in Liechtenstein, but do not remain as long in its labor market or even chose not to enter it. These substantial labor attachment effects remain robust to the inclusion of further covariates in the IPW estimator like gender, age, and nationality. Moreover, we assess the effect of receiving the residence permit on residence in Liechtenstein two and more years after the lottery.[48] The residence probability and the duration increase significantly by 71 percentage points and 3.44 years, respectively, on average (across outcome periods), with little differences between previous cross-border commuters and non-commuters. We also consider the labor market and residential effects separately by year after the lottery and find them to be persistent with no sign of fading out. That is, even in the tenth year after the first lottery participation, the impact on the employment and residence probability is comparable to the respective average effect across all periods and statistically significant at the 5% level, albeit confidence intervals are large due to the smaller sample size. We argue that tax advantages and more convenient commutes are likely the main motivations to move to Liechtenstein, as confirmed by a survey among foreign workers in Liechtenstein, see Marxer et al. (2016).

When assessing the heterogeneity of the employment effect across previous cross-border commuters and non-commuters who have not worked in Liechtenstein yet when participating in the lottery, we find the LATE to be stronger and statistically significant in the latter group. Therefore, resident permits appear to incentivize potential new labor market entrants to immediately start working in Liechtenstein, which very importantly contributes to the overall employment effect. Nevertheless, the permits also seem to keep previously commuting workers in the labor market in the longer run, while hardly affecting their employment decisions in the short run. Therefore, the LATE on employment strongly differs in initial outcome periods between previous commuters and non-commuters, but becomes much more similar in later periods, with the caveat that confidence intervals are generally large in those periods.

Very broadly speaking, our paper fits into the literature on labor migration, see for instance Fasani et al. (2020). More specifically, it is among a relatively scarce number of studies that

[48] Lottery losers may participate in future lotteries and then receive a residence permit.

exploit migration lotteries to convincingly assess the causal effect of residence permits on labor market behavior or related outcomes. Gibson et al. (2011), for instance, investigate a lottery in the Pacific island state of Tonga for residence permits in New Zealand and study welfare effects on household members of families left behind. Gibson et al. (2017) use that same lottery and find positive income effects among migrants themselves. Clemens et al. (2012) analyze a lottery in a specific multinational firm that allocated U.S. visas to Indian software workers and conclude that migration to the U.S. entails a sixfold increase in wages. Mergo (2016) considers the U.S. Diversity Visa lottery for Ethopians and finds that their migration to the U.S. increases welfare (in particular consumer expenditure) of the families left behind in Ethiopia. Mobarak et al. (2020) examine a visa lottery for low-skilled workers from Bangladesh intending to work in the palm-oil industry in Malaysia; and find that migration leads not only to a substantial income rise among migrants, but also to an increase in the household consumption of the family left behind.

While the previously mentioned studies consider migration from a less developed to a more developed country, a rather unique feature of our lottery study is that it concerns member states of the European Economic Area (EEA). Therefore, our paper contributes to the literature by considering labor migration between rather developed and wealthy countries. This is important, because a large amount of labor mobility is realized between rather developed nations, which are likely competing for qualified workers. A second important distinction is that we focus on the labor force attachment of individuals that could resume or start working in Liechtenstein even without living there, i.e. by means of cross-border commuting, in particular from nearby Austria or Switzerland, which is actually the most common form of labor in Liechtenstein.[49] Comparable scenarios with opportunities to commute cross-border exist in the border regions of the United States with Canada and Mexico, respectively, and at the outside borders of the EU, e.g. between Poland and the Ukraine. Depending on the legislation, cross-border commuting is regulated more or less restrictively across these countries, see, e.g. Orraca Romano (2015), Francis (2019), and Strzelecki et al. (2021).

[49] Indeed, slightly more than 50% of the employees in Liechtenstein commute cross-border. Among the 29k foreigners who work in Liechtenstein, about three thirds (76.9%) commute cross-border (Amt für Statistik Fürstentum Liechtenstein (AS), 2018a).

Our paper therefore sheds light on whether residence permits (and the associated amenities like tax reductions) incentivize foreigners to remain in the labor market, which appears an important piece of information for policy makers in a competitive open economy with a high demand for foreign labor, as it is the case in Liechtenstein.[50]

From a policy perspective, it appears interesting to compare the migration lottery with alternative labor migration policies. The economic relevance of such regulations with respect to cross-border commuters is for instance studied by Beerli et al. (2021). The authors find for Switzerland that reducing restrictions for cross-border workers increased the size and productivity of skill-intensive sectors (in particular those with previous skill shortages) such that even the wages of highly educated natives rose, despite the hike in foreign employment. Naguib (2019) focuses rather on heterogeneous effects and finds that travel time to the Swiss border increases the wage mobility of middle-aged workers, but decreases it for less-educated workers. Our paper appears to be the first that investigates the effects of a migration lottery relative to cross-border commuting. Well-known labor migration policies include the EU's "Blue Card" and the American "Green Card" system. The former provides non-permanent residence to candidates with university degree. Similarly, the latter targets highly-skilled workers, but even provides permanent residence. The migration lottery in Liechtenstein is somewhat in between these two by offering residence permits that last for five years, can be renewed, but are contingent on the reason of working in Liechtenstein. In contrast to the Green Card and Blue Card systems, the migration lottery in Liechtenstein does not impose requirements in terms of education or work experience. However, it does require an employment contract with an employer from Liechtenstein, which means that the participants have skills and experience that match the demand.[51] Complementary to this lottery, Liechtenstein's government also regularly grants a similar num-

[50]See Huber and Nowotny (2013) for an empirical study on which personal characteristics drive the willingness to commute and migrate across borders in regions of the Czech Republic, Hungary, and Slovakia that are situated close to Austria. The intention to commute or migrate is for instance found to be significantly negatively associated with age and being a female, and significantly positively associated with being single or feeling deprived when comparing the own social status to peers. Also higher education has a positive correlation, which is, however, not significant at the 5% level. Therefore, personal factors most likely play a role for the question which type of workers respond to specific incentives like cross-border work permits or residence permits, even though it needs to be pointed out that the findings in Huber and Nowotny (2013) do not necessarily directly carry over to the context of Liechtenstein.

[51]Among the biggest employers in Liechtenstein are ThyssenKrupp-Presta, Hilti, Ivoclar Vivadent, Hilcona, LGT, Ospelt, OC Oerlicon, Liechtensteinische Landesbank, and the VP Bank.

ber of residence permits to employees from the EEA and Switzerland at their discretion, see Marxer (2012). This tool is said to be used for filling key positions in local key employers and hence addresses workers with exceptionally high qualifications.[52] The benefits of this targeted labor migration tool are likely higher than those of the random lottery, but we cannot identify its effects due to the non-random assignment. Finally, granting a long-term or permanent perspective, e.g. through citizenship, might be beneficial not only to attract and keep talent, but also to foster the employees' investment in specific human resources (Dustmann and Görlach, 2016) and to foster integration (Hainmueller et al., 2017).

The remainder of the paper is structured as follows. Section 4.2 provides information about Liechtenstein and its migration lottery. Section 4.3 introduces our data and provides descriptive statistics. Section 4.4 discusses the empirical strategy. Section 4.5 presents and interprets the results. Finally, Section 4.6 concludes.

4.2 Institutional background

This section gives a very brief overview of the economy, the labor market, and the migration lottery of Liechtenstein.[53] As illustrated in Figure 4.A.2.1 in the Appendix, Liechtenstein is a micro-state situated in Central Europe, between Switzerland in the West and Austria in the East. The official language is German. Liechtenstein's population amounts to almost 40k inhabitants, while its labor force is of roughly the same size – in fact, slightly exceeding the population. Liechtenstein is a small open economy. Since 1923 it has a customs union with Switzerland and since 1995 it is member of the European Economic Area (EEA), which includes all European Union (EU) states plus Norway and Iceland, but not Switzerland. Hence, Liechtenstein has close economic ties with both Switzerland and the European Union. Exports of goods and services, excluding trade with and via Switzerland, account for 55% of its GDP. Its most important industries are mechanical engineering and the provision of financial and insurance services, which account for 16.2% and 13.3% of the GDP, respectively (Amt für Statistik Fürstentum Liechtenstein (AS), 2018c). Liechtenstein is among the wealthiest countries in the world with a

[52]For EEA citizens who do not belong to the working population and can finance their livelihood from their own resources, there is another lottery that is conducted at the same time as the lottery we study.

[53]More details about the institutional background are provided in Appendix A.1.

nominal GDP per employed person of about 200k USD. The official currency in Liechtenstein is the Swiss Franc (CHF), which had an average exchange rate of 1.04 USD/CHF in the last decade.

The labor market in Liechtenstein is characterized by a low unemployment rate and a large demand for foreign labor. The strong economic growth in recent decades in combination with the small size of the country fueled an ongoing employment expansion. Table 4.1 documents the increase in the number of employees from 1980 onwards and also distinguishes between employees residing in Liechtenstein and cross-border commuters, which have grown even faster than the total labor force. Since 2010 there are more cross-border commuters in the work force than employees residing in Liechtenstein. Most employees work in the service sector (61.9%) followed by the industrial sector (37.4%).

Wages in Liechtenstein are relatively high when compared to other Western European countries, which is most likely an important pull factor for attracting foreign labor. The median gross wage per month is about 7k USD, which is similar to the level of neighboring Switzerland, and substantially higher than in most EU countries, including neighboring Austria. The gross median income of cross-border commuters (CHF 6'723) is similar to that of residents (CHF 6'612) (Amt für Statistik Fürstentum Liechtenstein (AS), 2018b). Cross-border commuters are predominantly male (64.4%), working in the tertiary sector (55%), living in Switzerland or Austria (96%), and holders of a citizenship of a member country of the EEA (62.2%) (AS, 2018b).

Table 4.1: Number of employees in Liechtenstein

	Employees in Liechtenstein		
Year	Residing in Liechtenstein	Cross-border commuters	Total
1980	11,543	3,297	14,840
1990	13,020	6,885	19,905
2000	15,605	11,192	26,797
2010	16,764	17,570	34,334
2017	17,362	21,299	38,661
2018	17,597	22,038	39,635

Source: Amt für Statistik Fürstentum Liechtenstein (AS) (2020); "residing in Liechtenstein" is self-calculated

One important reason for the high share of cross-border commuters among the labor force is regulated access to residence permits in Liechtenstein. Despite being a member of the EEA, Liechtenstein is permitted to restrict residence of EEA citizens in Liechtenstein. However, by the EEA treaty, Liechtenstein is required to issue at least 56 residence permits for the purpose of employment every year, half of which must be assigned by a lottery.[54]

Holding at least one lottery per year is required by law (see Law on the Free Movement of Persons for EEA and Swiss nationals (2009), section 39, 1). Usually, two lotteries take place per year, one in spring and one in fall (Ausländer- und Passamt, 2020). Each lottery consists of two stages, namely the pre-draw and the final draw (Landesverwaltung Fürstentum Liechtenstein, 2009, section 37, 2). Participants must win in both parts of the lottery to receive a residence permit, which is valid up to five years (Aufenthaltsbewilligung B) an can be extended. Family reunification for spouses, children and parents (if they receive financial support from the lottery participant) is possible at any time.[55]

Requirements for participation include holding an EEA citizenship and paying the participation fees in time. In the final draw, participants must also provide an employment contract of more than one year with a minimum activity level of 80% or, else, an authorized permanent cross-border business activity in case of self-employment (Ausländer- und Passamt, 2019b). After winning both the pre-draw and the final draw, the lottery participant is required to relocate within six months to Liechtenstein, otherwise the residence permit expires (Landesverwaltung Fürstentum Liechtenstein, 2009, section 37, 2). For this reason, our treatment is defined based on residing in Liechtenstein in the year after the lottery, as obtaining the permit is tied to actually moving there. The drawing of winners is done blindly by hand. This procedure is monitored by at least one judge.

Lottery losers of either stage may participate again in subsequent lotteries, while multiple applications to the very same lottery are not allowed (Landesverwaltung Fürstentum Liechtenstein, 2009, section 38, 1) c)). As the decision to repeatedly take part in the lottery is most likely endogenous to the first lottery outcome, our main evaluation strategy relies on the first lottery

[54]Despite the restrictive rules for immigration to Liechtenstein there is an inflow of 17.0 (net inflow of 4.3) immigrants per 1,000 inhabitants (Amt für Statistik Fürstentum Liechtenstein (AS), 2019b). The dominant formal purpose for immigration to Liechtenstein is family reunification. The fraction of foreigners among the residents in Liechtenstein is 34% (Amt für Statistik Fürstentum Liechtenstein (AS), 2020).

[55]https://www.llv.li/inhalt/117535/amtsstellen/fur-angehorige-eines-ewr-und-ch-staatsangehorigen

participation of an individual in our data window. Furthermore, as participation in the final draw is conditional on succeeding in the pre-draw, we base our instrumental variable approach on the pre-draw alone.

The incentives to participate in the lottery are arguably related with the costs and benefits of residing in Liechtenstein. For most lottery participants, the relevant alternative is to reside in a neighboring country and commute cross-border.[56] In a survey of cross-country commuters to Liechtenstein Marxer et al. (2016, p. 57) ask about the reasons to move to Liechtenstein, given the presumption that the respondents would move there in the future. The top answer is "taxes and duties" (86% of respondents), which is even ticked more often than "proximity to the workplace" (80% of respondents), while all other categories are ticked by less than 22% of respondents.[57] Indeed, taxes are substantially lower in Liechtenstein than in Switzerland such that the net disposable income of given gross incomes and household types is about 10 percentage points higher in Liechtenstein.[58] In Austria taxes are even substantially higher than in Switzerland such that the net disposable income there is likely even lower, despite the lower costs of living. Accordingly, Marxer et al. (2016) find that 31% of the cross-border commuters living in Switzerland and 75% of those living in Austria are not satisfied with their tax system; while there is a particularly low willingness to move to Liechtenstein for those (comparably few) cross-border commuters who pay taxes in Liechtenstein (these are employees who reside in Austria and work in Liechtenstein's public sector).

The second motivation to participate in the lottery, living closer to the workplace, appears obvious, but must be put into perspective: most cross-border commuters have quite short commutes. 59% of them travel less than 30 minutes to work and only 6% more than 1 hour (Marxer

[56]For other participants it can be the case that they will stop working in Liechtenstein or not start working in Liechtenstein, despite holding an employment contract.

[57]Those who are not interested in moving to Liechtenstein are typically Austrian or Swiss citizens who live in their home country.

[58]For example, if a single adult with a relatively low gross income (CHF 60k p.a.) and no wealth moved from Sevelen (Canton St. Gallen, Switzerland) to neighboring Vaduz (Liechtenstein), her net disposable income would increase from 38% of her market income to 49% (Brunhart and Buechel, 2016, their Table I). Similarly, if a family consisting of two children and two adults with a gross income of CHF 90k and identical wealth moved from Maienfeld (Canton Grisons, Switzerland) to nearby Balzers (Liechtenstein) their net disposable income would increase from 45% of their market income to 54% (Brunhart and Buechel, 2016, their Table I). The calculation of net disposable income includes the costs and subsidies of housing, child care, and health insurance, while the quality of such services and other amenities appears comparable between Liechtenstein and the two neighboring countries.

et al., 2016, p. 36). Furthermore, Liechtenstein's accession to Schengen in 2008 led to the abolition of systematic border controls in 2011,[59] which might lead to a shorter commuting time for cross-border commuters from Austria.[60] Still, residing in Liechtenstein can lead to more convenient commutes, be it because of even shorter commuting times, fewer bus or train changes when using public transport, or different means of transport (e.g. biking instead of driving). Given these advantages of residing in Liechtenstein over commuting to Liechtenstein, we note that financial incentives are probably the most important factor for participating in the lottery, while distance to the workplace also matters. Moreover, there can be stronger integration into the local society and additional amenities which need not be fully anticipated by those willing to migrate.

4.3 Data

This section provides a description of our data set and the key variables along with descriptive statistics. Our data base was created by linking records from the migration lottery with employment statistics in Liechtenstein. The lottery records cover all lottery participants from 2003 to 2019. In particular, they include information on when and how often an individual applied to the migration lottery. This enables us to define the instrument based on whether an applicant has won the pre-draw or not in the first lottery participation. In addition, the data contain personal characteristics such as the year of birth, nationality, and gender, which are asked in the application form for the lottery.

The employment statistics cover the years 2005 to 2018. Every employer in Liechtenstein is obliged to report new employment entries and exits on a monthly base. At the end of each year, companies receive a list of their reported employees for proofreading and are obliged to resubmit a corrected version (Amt für Statistik Fürstentum Liechtenstein (AS), 2019a). The employment statistics contain variables characterizing the labor market behavior of the applicants. This includes information on whether an individual has worked in Liechtenstein in the year prior

[59] https://ec.europa.eu/home-affairs/sites/default/files/e-library/docs/schengen_brochure/schengen_brochure_dr3111126_de.pdf

[60] We capture a potential effect of the abolition of systematic border controls in 2011 by controlling for year dummies in our estimation (see Section 4.5).

to lottery participation and whether she or he has started or continued dependent or self-employment in the years after lottery participation. For each year, also the activity level in percent is reported, as well as the country of residence. Finally, several personal characteristics are observed that are also available in the migration lottery records, namely the year of birth, nationality, and gender. Whenever there are differences in these variables across the two data sources, we prioritize the employment statistics which we suspect to be of higher quality, as they are repeatedly provided and checked. In contrast, the lottery records only contain information that was originally handwritten in the application form. Linking both data sets is based on a unique personal identifier and the created data base is fully anonymized.

In total, the migration lottery data contain 9,906 observations from 2003 to 2019. While each lottery draw is random, the possibility to repeatedly participate in case of losing might induce a selection problem, as more persistent applicants who participate more than once in the lottery likely differ in terms of their characteristics from the initial pool of applicants. We overcome this concern by exclusively considering the first lottery participation in our data window, which reduces the sample to 5,091 observations. Hence, we compare individuals who won when first participating in the lottery with those who lost, but might have participated again and won in a later lottery. This strategy yields conservative effects in the sense that they likely provide a lower bound to those of a hypothetical comparison of winning vs. losing and being prevented from any further lottery participation. Since the employment statistics are only available from 2005 onwards, we restrict the sample of first lottery participants to the years 2006 or later, in order to observe the labor market state of each applicant in the year prior to the lottery. This will be important for our analysis of effect heterogeneity across previous cross-border commuters and newly attracted workers. Another sample restriction comes from the fact that the last period in which outcomes are observed in the employment statistics is 2018. This requires us to consider 2016 as last lottery year, because outcomes are measured at the earliest 2 years after the lottery, as it will become clear from the discussion further below. Figure 4.1 shows the annual number of the first lottery participants from 2006 to 2016, separately for the spring and fall lotteries. Moreover, Figure 4.1 indicates that this number varies across years, which is also true for the number of all lottery participants, with a peak during the financial crisis in 2008.[61] Thus, the odds of winning change over time, as the amount of lottery-assigned permits is not deterministic in the number of (first) applications. This implies that the lottery year is a likely confounder of our instrument variable assignment, as the year is likely associated with labor market outcomes through the business cycle. We therefore control for lottery year dummies in our IV approach and include the additional control variables age, nationality, and gender in a robustness check. In sum, our evaluation data set contains 3,145 participants, out of which 350 win the pre-draw in their first participation.

Table 4.2 reports the proportion of winners and losers for their first-time lottery participation starting in the year 2007.[62] In total 2,834 participants take part in the first draw of whom 2,513 lose the first draw and 321 win the first draw. 76% of the pre-draw winners participate in the final draw. The remaining 23% consist of individuals who do not participate in the final draw because their plans have changed or they did not have an employment contract in Liechtenstein and could not secure one within a deadline of less than three months, or because they are not allowed to participate in the final round.[63] Of the pre-draw winners that do participate in the final draw, 151 individuals – that is 62% of the participants in the final draw, and 47% of the winners of the pre-draw – win the second draw and are compliers if they indeed move to Liechtenstein. Any other winners of the pre-draw lottery are non-compliers. In particular, 38% of the second-draw participants do not win and hence do not obtain a residence permit.

Figure 4.2 provides a time line for the measurement of the key variables in our analysis, with t denoting a specific year. The instrument, namely the lottery assignment which we henceforth denote by Z (with $Z = 1$ for winning and $Z = 0$ for losing), is measured in the year of the first lottery participation, which is our baseline period ($t = 0$). The treatment (denoted by D), namely whether someone has moved to Liechtenstein ($D = 1$), which is conditional on the

[61] The high number of lottery applications during the financial crisis may partly be driven by more applications from individuals previously not working in Liechtenstein, aiming to escape the crisis-induced deteriorating labor market conditions in their home country, as suggested by descriptive statistics in Table 4.A.2.3 below. Furthermore, the higher number might partly be caused by cross-border commuters suspecting a larger chance of losing employment when being a commuter rather than a resident, in line with findings in Kuptsch (2012) that migrants face disproportionately higher risks of job loss in case of economic woes.

[62] Since we had no data available for the winners in the second draw in 2006.

[63] The latter group includes participants whose participation form for the pre-draw was submitted incomplete or late, as they were nevertheless included in the pre-draw such that they had the opportunity to appeal against the decision that their submission was invalid.

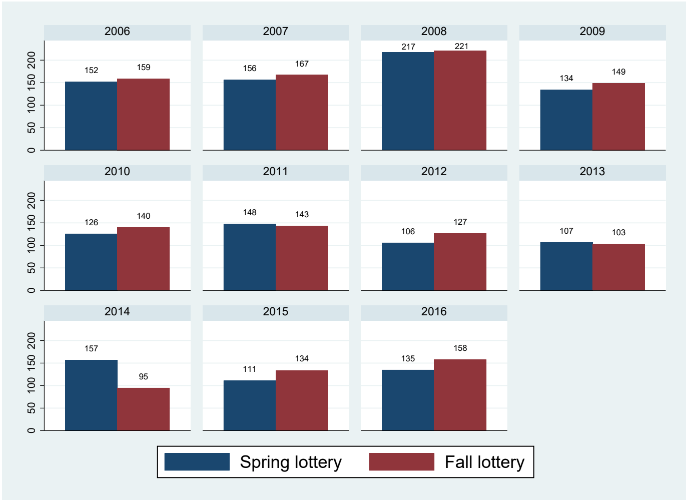


Figure 4.1: Annual number of first lottery participation

Table 4.2: The number and proportion of winners and losers for first-time lottery participants.

	Observations	Proportion of participants first draw	Proportion of winners first draw	Proportion of participants second draw
Participants first draw	2,834	100%		
Losers first draw	2,513	89%		
Winners first draw	321	11%	100%	
Non-participants in second draw	75	3%	23%	
Participants second draw	245	9%	76%	100%
Losers second draw	94	3%	29%	38%
Winners second draw	151	5%	47%	62%

Note: First lottery participation from 2007 onwards, because we had no data about the second draw winners for 2006.

possession of a residence permit, or not ($D = 0$), is measured one year later ($t = 1$). The outcome periods start two years after the lottery (t $\geq$ 2) and continue until the end of the data window for the respective observation, at most up to 12 years after the lottery for someone participating in 2006 with the final outcome being observed in 2018. All in all, our evaluation sample includes 20,009 outcome observations. Personal characteristics (e.g. nationality) are generally measured in the year prior to the first lottery participation ($t = -1$), even though those variables that are independent from or deterministic in time (gender and age) may also be obtained from different periods. In some cases there are differences between the personal characteristics in the migration lottery records (stemming from the application form) and the employment statistics (regularly provided by the employer). As the data quality of the employment statistics appears to be higher than that of the lottery records, priority is given to the former when measuring these characteristics. We henceforth denote the control variables by X, which either only contain period dummies for the lottery years in the main specification, or also additional characteristics in our robustness checks.

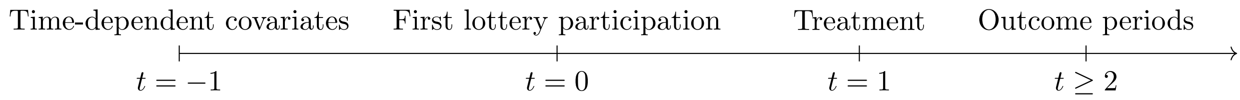


Figure 4.2: Timeline of measured variables

To check for violations of the random assignment of residence permits through the lottery, Table 4.3 reports descriptive statistics on personal characteristics separately for winners ($Z = 1$) and losers ($Z = 0$) of the first lottery in our evaluation sample. For either group, the mean and the standard deviation (std.dev) of the variables are reported as well as the mean differences across groups along with t-values and p-values. Among those with non-missing information in the respective personal characteristics, age, gender, and the various dummy variables for nationality do not differ importantly or statistically significantly (at any conventional level of significance) across groups, thus pointing to a fair lottery. We also see from the table that the majority of the lottery participants is male, of either Austrian or German nationality, and on average 37 to 38 years old.[64]

In contrast to the observed characteristics, the probability of missing information in nationality and age is statistically significantly different across winners and losers, albeit very small in absolute terms (amounting to only 1 percentage point). This difference is, however, most likely caused by an imbalance in missingness across our two data sources rather than a failure of the lottery. To see this, note that for any individual already working in Liechtenstein prior to the lottery participation, we have access to information from the employment statistics, in which case there is no missing information. For those not working in Liechtenstein prior to the lottery, we need to rely on the variables from the migration lottery, in which missings do

[64] In this context, we note that Swiss nationals are not allowed to participate in the lottery. The reason that their share amounts to 1% in our data is most likely due to holding a second citizenship from the EEA but reporting the Swiss nationality in the employment statistics.

occur. Albeit some of the missing information can be filled based on the employment statistics in later (i.e. treatment or outcome) periods in particular for determining the age in the year of the lottery, this is dependent on entering the labor market in Liechtenstein at some point in time. As lottery losers enter the labor market less frequently than lottery winners, their share of missing covariates is endogenously higher even under a satisfaction of randomized assignment. This issue does not affect our main results since we do not drop any observations with missing covariate information, in order to avoid jeopardizing randomization through an endogenously selected subsample.

Table 4.3: Descriptive statistics of covariates: First participation from 2006 to 2016

	$Z = 1$		$Z = 0$					
	mean	std.dev	mean	std.dev	mean difference	t-value	p-value	observations
Female	0.29	0.45	0.30	0.46	-0.01	-0.51	0.61	3,145
Nationality								
Missing Dummy	0.00	0.00	0.02	0.13	-0.02	-6.76	0.00	3,145
Austria	0.38	0.49	0.37	0.48	0.01	0.40	0.69	3,100
Germany	0.39	0.49	0.42	0.49	-0.02	-0.88	0.38	3,100
Italy	0.06	0.24	0.07	0.26	-0.01	-0.88	0.38	3,100
Switzerland	0.01	0.09	0.01	0.08	0.00	0.53	0.59	3,100
Others	0.16	0.37	0.14	0.34	0.02	1.11	0.27	3,100
Age								
Missing Dummy	0.01	0.09	0.02	0.15	-0.01	-2.52	0.01	3,145
Age	37.25	9.25	37.49	9.62	-0.24	-0.46	0.65	3,078
First lottery participation								
Dummy 2006	0.09	0.29	0.10	0.30	-0.01	-0.31	0.76	3,145
Dummy 2007	0.09	0.29	0.10	0.30	-0.01	-0.57	0.57	3,145
Dummy 2008	0.09	0.29	0.14	0.35	-0.05	-2.97	0.00	3,145
Dummy 2009	0.10	0.30	0.09	0.28	0.01	0.84	0.40	3,145
Dummy 2010	0.06	0.24	0.09	0.28	-0.02	-1.74	0.08	3,145
Dummy 2011	0.11	0.31	0.09	0.29	0.01	0.85	0.39	3,145
Dummy 2012	0.09	0.28	0.07	0.26	0.02	1.02	0.31	3,145
Dummy 2013	0.08	0.28	0.06	0.25	0.02	1.17	0.24	3,145
Dummy 2014	0.10	0.30	0.08	0.27	0.03	1.50	0.13	3,145
Dummy 2015	0.09	0.28	0.08	0.27	0.01	0.75	0.45	3,145
Dummy 2016	0.08	0.28	0.09	0.29	-0.01	-0.73	0.46	3,145
Number of observations	350		2,795					

Sources: Lottery data (2005 - 2016) and employment statistics (2005 - 2016); calculations are our own.

Table 4.3 also reports the year dummies for the first lottery participation across instrument values, providing information about variation in the ratio of pre-draw winners and losers across different years. For year 2008, the mean difference in dummies is statistically significant at the 1% level, owing to the large number of lottery applicants in that year (see Figure 4.1), a likely consequence of the financial crisis of 2007-2008. Such potential business cycle-related confounding motivates controlling for period dummies in our IV approach.

Table 4.A.2.1 in the Appendix reports the statistics on the outcomes of the first lottery in our evaluation sample over the each outcome period and pooled over the time. For each outcome, the mean and the standard deviation (std.dev) are reported. We see that the probability of residing in Liechtenstein is rather stable over time and amounts to an average probability of 15% among lottery participants. The probability of being employed and the activity level decrease over time. We note that the activity level is defined to be zero for those who do not work in Liechtenstein, as an alternative definition that discards these observations by conditioning on employment in Liechtenstein would introduce Heckman (1976)-type sample selection bias. On average, lottery participants are employed with a probability of 44% and work at a 40% level. Contrarily, the years employed and residing in Liechtenstein increase over time. On average, a lottery participant resides in Liechtenstein for half a year and is employed there for two years.

4.4 Econometric approach

In this section, we discuss our instrumental variable (IV) approach for evaluating local average treatment effect (LATE), see Imbens and Angrist (1994) and Angrist et al. (1996), among lottery compliers, i.e. among those who are induced to move to Liechtenstein in the year after the lottery by winning. Following Abadie (2003), we assume that our lottery Z is a valid and relevant instrument conditional on covariates X, which either include the lottery year dummies (main specification) or both the year dummies and additional personal characteristics (robustness check). To formally state the identifying assumptions, we make use of the potential outcome notation, see for instance Rubin (1974). We denote by $Y(z,d)$ the potential outcome (e.g. hypothetical employment) under specific instrument and treatment states $z, d \in \{1,0\}$, and by $D(z)$ the potential treatment state as a function of the instrument assignment.

Conditional IV validity, as formally stated in equation (4.1), consists of two parts: The first part (i) implies that the lottery assignment is as good as random given X and thus not associated with other factors affecting the treatment and/or the outcome. For reasons discussed in Section 4.3, this appears plausible conditional on the year dummies. The second part (ii) states that the lottery assignment must not have a direct effect on the outcome other through the treatment, such that the IV exclusion restriction holds. This assumption is satisfied if winning or losing the lottery does not directly affect the employment decision conditional on the moving decision. Hence, the assumption excludes, for instance, that winning or losing the lottery induces sufficiently strong feelings of appreciation or disappointment that would make the participant change her labor market status.

$$\begin{aligned} &(i) \quad Z \perp (D(1), D(0), Y(1,1), Y(1,0), Y(0,1), Y(0,0))|X \\ &(ii) \quad \Pr(Y(1,d) = Y(0,d) = Y(d)|X) = 1 \quad \text{for } d \in \{1,0\} \end{aligned} \tag{4.1}$$

Equation (4.2) formalizes the conditional monotonicity assumption, which rules out the existence of so-called defiers, i.e. of individuals that would move to Liechtenstein in the year after the lottery if losing it, but would not move if winning. Since lottery losers are generally not allowed to move to Liechtenstein,[65] this assumption holds by design in our context.

$$\Pr(D(0) > D(1)|X) = 0 \tag{4.2}$$

Equation (4.3) is a common support assumption, implying that for any value of X, both lottery winners and losers do exist. Indeed we find in our data that winners and losers appear in any lottery year and as well across age groups, nationalities and gender.

$$0 < \Pr(Z = 1|X) < 1 \tag{4.3}$$

Finally, equation (4.4) states that the instrument is relevant in the sense that it affects the treatment decision conditional on X. As discussed in Section 4.5 below, winning the pre-draw does indeed importantly and statistically significantly affect the decision to move to Liechtenstein given the control variables.

$$\Pr(D = 1|Z = 1, X) - \Pr(D = 1|Z = 0, X) \neq 0 \tag{4.4}$$

Under these assumptions, the LATE is nonparametrically identified, see Frölich (2007), for instance by reweighing observations based on the inverse of the conditional instrument probability $\Pr(Z = 1|X)$, known as the instrument propensity score. Equation (4.5) presents the

[65] Exceptions are that someone gets married or has a common child with a resident of Liechtenstein.

identification result based on such an inverse probability weighting (IPW) approach as suggested in Frölich (2007) and Tan (2006). It is worth noting that the numerator provides the intention-to-treat effect (ITT) or reduced form effect of the lottery assignment Z on the outcome Y, which is a weighted average of the LATE on compliers and a zero effect of Z among non-compliers (whose treatment does not react to the instrument). The denominator consists of the first-stage effect, i.e. the impact of the lottery assignment Z on the decision to reside in Liechtenstein one year after the first lottery participation D.

$$LATE = \frac{E[Y \cdot Z/\Pr(Z=1|X) - Y \cdot (1-Z)/(1-\Pr(Z=1|X))]}{E[D \cdot Z/\Pr(Z=1|X) - D \cdot (1-Z)/(1-\Pr(Z=1|X))]} \tag{4.5}$$

For the estimation of (4.5), we use the 'lateweight' command of the 'causalweight' package (Bodory and Huber, 2018) for the statistical software R, with 1999 bootstrap replications for computing the standard error and the default trimming rule of dropping observations with propensity scores smaller than 0.05 or larger than 0.95 to ensure common support in the sample. The instrument propensity score $\Pr(Z=1|X)$ is estimated by means of a probit specification. However, we point out that our estimator is fully nonparametric when controlling for lottery period dummies only, which amounts to a fully saturated model. Our estimator is semiparametric when additionally controlling for further covariates and in particular age, whose inclusion in the linear index of the probit model imposes parametric assumptions on $\Pr(Z=1|X)$ (but in contrast to two-stage least squares neither on the treatment, nor on the outcome model).

4.5 Results

This section provides the empirical results. First, the average LATE estimates when pooling all outcome periods; second, the outcome period-specific LATE estimates; and third, an analysis of effect heterogeneity.[66] Pooling the outcome periods provides a weighted average of effects over different complier groups and outcome periods, in which compliers who first participate in the lottery in an earlier period obtain a larger weight due to having a longer outcome window than compliers participating in a later period. Furthermore, earlier outcome periods obtain a larger

[66] We also briefly discuss the results when considering the second and third (rather than the first) lottery participation as instrument and present the results of these further analyses in Appendix A.3.

weight than later ones, as earlier outcome periods (e.g. two years after first lottery participation, $t = 2$) are also observed for first lottery participants in later periods, while the observability of later outcome periods (e.g. ten years after first lottery participation, $t = 10$) is conditional on a relatively early participation in the lottery. While pooling and its implied weighting of observations might be considered as hampering the interpretability of the results, our outcome period-specific results presented further below suggest that the LATEs on the binary employment and residence decision as well as the activity level (in %) are quite persistent across different choices of t. Given that the effects are quite stable across periods, pooling yields a concise and informative LATE and at the same time entails a higher statistical power (or a smaller standard error) than outcome period-specific estimations that rely on a relatively small subsample of the data.

Table 4.4 reports the LATE estimates for pooled outcome periods. The upper panel reports the effects along with bootstrap-based standard error and p-values obtained from t-tests. As an individual might be observed in multiple outcome periods, we cluster observations on the personal identifier by using the cluster or block bootstrap (which resamples individuals with all related observations in any outcome period rather than single observations) when computing standard errors. We find that having moved to Liechtenstein one year after the first lottery participation increases the probability of residing in Liechtenstein by 71 percentage points and the probability of being employed in Liechtenstein by 24 percentage points among compliers when averaging over all outcome periods. Similarly, the effect on the activity level, which is measured in percent and by definition zero if not working in Liechtenstein, amounts to almost 20 percentage points. Furthermore, the duration of residing and being employed in Liechtenstein increases by 3.44 and 1.15 years on average, respectively, across the outcome periods, which start with $t = 2$ and are restricted by the time window of the data set. These important labor market and residential effects are highly statistically significant, as p-values are close to zero.[67] The intermediate panel reports the first-stage effect of the instrument on the treatment, which implies that 36% of the participants in the sample are compliers. The group of non-compliers largely

[67] Since testing multiple hypotheses can lead to detecting more statistically significant results than actually exist (the so-called false-positive rate (Benjamini and Hochberg, 1995)), we apply the Benjamini-Hochberg (B-H) procedure. We use the command "BH" from the R-package "sgof" and set alpha equal to 0.05. We find that all five statistically significant effects in Table 4.4 remain significant.

consists of participants who won the pre-draw of the lottery but not the final draw. A back-of-the-envelope calculation suggests that this is the case for 43% of the non-compliers, while 35% did not or could not participate in the final draw and only 13% are winners of the final draw who do not move to Liechtenstein. The p-value of the first-stage is close to zero and the instrument is therefore strongly associated with the treatment, thus supporting the relevance assumption postulated in (4.4). For completeness, the lower panel of Table 4.4 reports the intention-to-treat effect (ITT) of the instrument on the outcome, which is smaller than the corresponding LATE due to the presence of non-compliers for whom the effect is zero by definition (if defiers do not exist). In many policy evaluations, the ITT may actually appear more policy-relevant than the LATE because the number of compliers can typically not be controlled by the policy maker. In our context, however, the government sets the number of winners of both draws of the lottery and has hence control over the fraction of compliers. Hence, the LATE seems to be the more relevant effect in our study. All effects are highly statistically significant. As no extremely high (>0.95) or extremely low (<0.05) probabilities of winning the lottery occur in any year of first lottery participation, no observation was trimmed such that the estimates are based on all 20,009 pooled observations, as indicated at the bottom of Table 4.4.

Table 4.4: Empirical results based on first participation and year dummies

	Outcomes				
	Residing (binary)	Employed (binary)	Activity level (%)	Years residing	Years employed
LATE					
Effect	0.71	0.24	19.72	3.44	1.15
Standard error	0.05	0.08	7.43	0.27	0.39
P-value	0.00	0.00	0.01	0.00	0.00
First-stage					
Effect			0.36		
Standard error			0.03		
P-value			0.00		
ITT					
Effect	0.25	0.09	7.06	1.23	0.41
Standard error	0.03	0.03	2.92	0.15	0.15
P-value	0.00	0.00	0.02	0.00	0.01
Observations			20,009		
Trimmed			0		

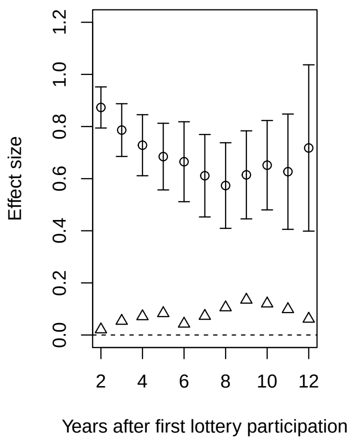
Note: Standard errors are estimated by cluster bootstrapping.

Table 4.A.3.1 in Appendix A.3 provides the results for pooled outcome periods when including age, gender, nationality, and missing dummies for these variables as covariates in addition

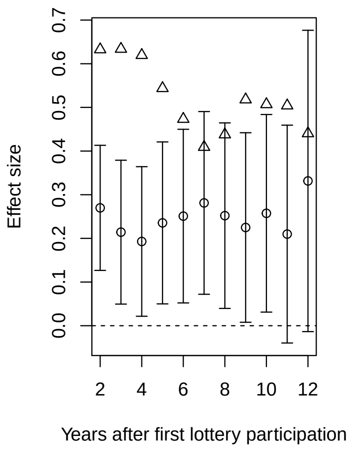
to the lottery period dummies. The effect estimates are rather similar and again highly statistically significant. Furthermore, Tables 4.A.3.2 to 4.A.3.5 in Appendix A.3 report the estimates for pooled outcome periods when considering the second and third lottery participation (rather than the first one) as instrument, respectively, when either using the lottery period dummies alone or additionally the personal characteristics as control variables. Also in these cases, the findings are all qualitatively similar to our main results.

In a next step, we investigate the effects in specific outcome periods defined relative to the year of the first lottery participation. Figure 4.3 displays the estimates for the various outcomes from period $t = 2$ (i.e. two years after the lottery) up to period $t = 12$. The dots represent the period-specific LATEs and the bands correspond to the pointwise 95% confidence intervals based on the standard bootstrap. The triangles depict the estimated mean potential outcome among compliers under non-treatment, see for instance Huber (2019) for a discussion of its computation. The triangles hence provide the relevant counter-factual of not moving to Liechtenstein. Against this backdrop we can judge the relative importance of the LATEs. The effects on the binary residence and employment dummies as well as the activity level are positive throughout all periods and statistically significant at the 5% level in most cases. However, many of the effects are imprecisely estimated in particular in later outcome periods with a limited number of observations, which results in wider confidence intervals. Nevertheless, the positive point estimates appear to be quite persistent with no sign of fading out at the end of the data window. Relatedly, the LATEs on the durations of residing or being employed in Liechtenstein from $t = 2$ on monotonically increase as we consider later outcome periods and due to the persistence of the residence and labor market decisions over time, they appear to roughly follow a linear path. Panel (f) of Figure 4.3 provides the number of observations per outcome period as well as the number of trimmed observations, which is equal to zero just as for the pooled estimations. We also inspected the plots when controlling for age, gender, nationality, and missing dummies in addition to the lottery period dummies and obtained similar results.

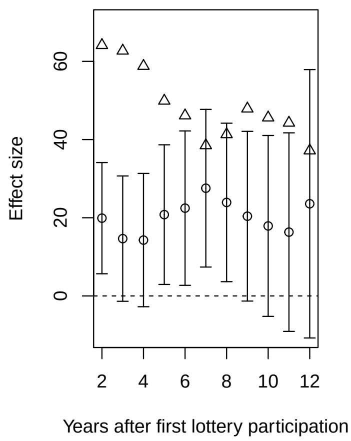
Approximately half of the observations in our evaluation data consist of participants that had already worked as cross-border commuters one year prior to their first lottery, the other half of potentially new foreign workers was not employed in and thus, not commuting to Liechtenstein



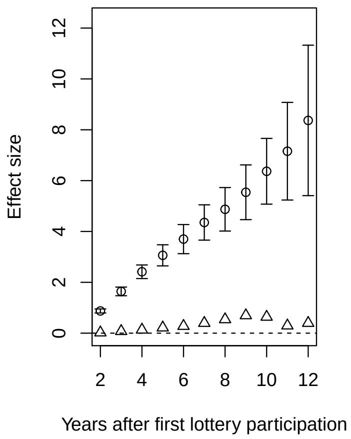
(a) Residing (binary)



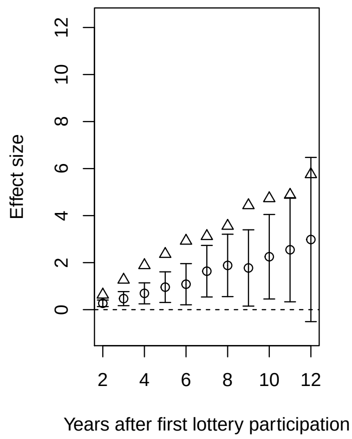
(b) Employed (binary)



(c) Activity level (%)



(d) Years residing



(e) Years employed

	Observations	trimmed
t=2	3,145	0
t=3	2,852	0
t=4	2,607	0
t=5	2,355	0
t=6	2,145	0
t=7	1,912	0
t=8	1,621	0
t=9	1,355	0
t=10	1,072	0
t=11	634	0
t=12	311	0

(f) Number of observations

Figure 4.3: Effects over years

Note: Dots represent LATEs, bands correspond to 95% confidence intervals, triangles depict the counterfactuals (i.e., mean potential outcomes among compliers under non-treatment)

in the previous year. Tables 4.A.2.2 and 4.A.2.3 in the Appendix report descriptive statistics about personal characteristics separately for cross-border commuters and non-commuters in our evaluation sample. We see from the tables that the majority of applicants is male, on average 36 to 38 years old, and of either Austrian or German nationality. However, the share of the nationalities differs in both groups: German is the most frequent nationality among the non-commuters (on average 47%), whereas the Austrian nationality dominates among the cross-border commuters (on average 44%).[68]

In a next step, we check whether cross-border commuters who apply for the lottery are in terms of their personal characteristics similar to or different from cross-border commuters to Liechtenstein in general. For this reason, we compare the average age, gender, and nationality of the cross-border commuters in our sample with those respective average values in the administrative statistics (Amt für Statistik Fürstentum Liechtenstein (AS), 2018a, 2019a). Male (71% versus 74%) and younger (37 years versus 41 years) occur more frequently in our lottery data. While Austrian and Germans cross-border commuters are the most frequent applicants in our data, they are still underrepresented, as 55% of the cross-border commuters with EEA nationality are Austrian and 24% German. Intuitively, Austrians and Germans who can commute from their home country are less likely to apply for the lottery than cross-border commuters with other nationalities. Additionally, we use the cross-border survey to draw conclusions about what the average educational level might be in our evaluation data, in which information on education is not available. Marxer et al. (2016) report that cross-border commuters are on average highly educated, as 57.5% of them hold a degree from a higher education institution. Based on these findings, we suspect that cross-border commuters applying for the migration lottery have an average education that is likely considerably higher than that of the general population in Austria, Switzerland, and Liechtenstein.

From a policy perspective, it appears interesting whether treatment effects are heterogeneous across cross-border commuters and non-commuters, i.e. if residence permits are rather effective for attracting new or keeping existing foreign workers that have already decided to enter Liechtenstein's labor market at an earlier point in time. If permits were more effective among one rather than the other group, policy makers might want to consider to adapt the targeting of immigration policies accordingly. For this reason, Tables 4.5 and 4.6 report the results with pooled outcome periods separately for applicants working (cross-border commuters) and not working (non-commuters) in Liechtenstein one year prior to the lottery. In both subsam-

[68] As mentioned in Section 4.3, nationality is missing for some non-commuters, due to missing information in the lottery data that could not be compensated by information in the employment statistics.

Table 4.5: Effects among non-commuters

	Outcomes				
	Residing (binary)	Employed (binary)	Activity level (%)	Years residing	Years employed
LATE					
Effect	0.71	0.34	29.78	3.31	1.56
Standard error	0.13	0.15	13.83	0.60	0.79
P-value	0.00	0.02	0.03	0.00	0.05
First-stage					
Effect	0.28				
Standard error	0.05				
P-value	0.00				
ITT					
Effect	0.20	0.10	8.35	0.93	0.44
Standard error	0.04	0.04	4.13	0.21	0.23
P-value	0.00	0.03	0.04	0.00	0.06
Observations	10,081				
Trimmed	0				

Note: Standard errors are estimated by cluster bootstrapping.

ples, the residence permit has a similarly positive and highly significant effect on the compliers' probability to reside in Liechtenstein (71 vs. 69 percentage points) and their residence duration (3.31 vs. 3.46 years).[69] In contrast, we find heterogeneous effects for the LATEs on the labor market outcomes: The effects for previous cross-border commuters are positive but statistically insignificant, whereas the impacts are considerably larger and significant at conventional levels for people not (yet) working in Liechtenstein. For the latter group, we find that a residence permit leads to an increase in the employment probability of 34 percentage points, in the activity level of almost 30 percentage points, and in the employment duration of 1.56 years in the outcome periods.[70] We therefore conclude that the policy is more effective in raising labor supply among individuals previously not working in Liechtenstein than among cross-border commuters, while effects on residential choices are similar among both groups.

To further investigate the heterogeneous effects on the labor market attachment of (a) non-commuters and (b) cross-border commuters, Figure 4.4 plots the period-specific LATEs on the employment probabilities for the respective group over time by means of dots. In analogy to Figure 4.3, the graphs also include the mean potential complier outcomes under non-treatment by period as triangles. It is worth noting that we omit period $t = 12$ for the non-commuters

[69]These effects remain statistically significant after running the B-H procedure for multiple hypothesis testing with an alpha of 0.5.

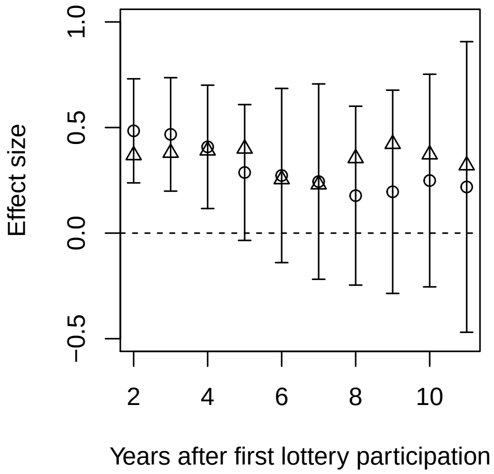
[70]These effects remain statistically significant after running the B-H procedure for multiple hypothesis testing with an alpha of 0.5.

Table 4.6: Effects among cross-border commuters

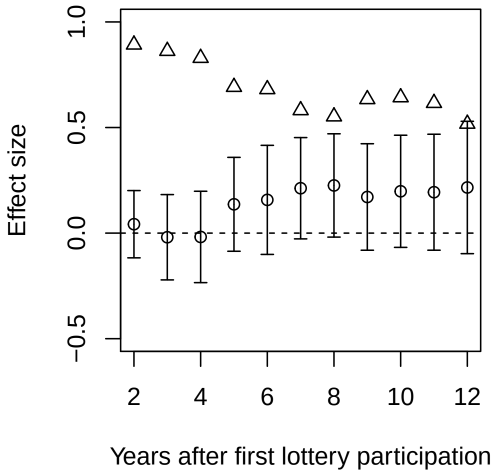
	Outcomes				
	Residing (binary)	Employed (binary)	Activity level (%)	Years residing	Years employed
LATE					
Effect	0.69	0.11	6.75	3.46	0.56
Standard error	0.06	0.09	8.83	0.28	0.42
P-value	0.00	0.21	0.44	0.00	0.18
First-stage					
Effect	0.42				
Standard error	0.04				
P-value	0.00				
ITT					
Effect	0.29	0.05	2.86	1.47	0.24
Standard error	0.04	0.04	3.75	0.20	0.18
P-value	0.00	0.23	0.45	0.00	0.20
Observations	9,928				
Trimmed	0				

Note: Standard errors are estimated by cluster bootstrapping.

due to a very small number of observations, which entails confidence intervals that are too wide to present them in a meaningful way in the figure. We find for the non-commuters in the periods right after the first lottery participation positive and statistically significant effects at the 1% level for the periods 2 to 4 and at the 10% level in period 5. The initially very large effects of an almost 50 percentage points increase in the employment probability decrease over time, but remain quite sizeable at around 20 percentage points even in later periods, even though the confidence intervals are admittedly large in those periods. This suggests that the residence permit triggers a very strong immediate labor supply response which somewhat levels off over time. For the commuters, exactly the opposite pattern arises. The employment effects are initially very close to zero and therefore suggest that for individuals already working in Liechtenstein, the resident permit hardly affects their labor supply in the short run. The effects, however, increase over time to stabilize at roughly 20 percentage points and are even statistically significant at the 10% level in periods 7 and 8. Taken at face value, this implies that resident permits are very effective for keeping previously commuting workers in the labor market in the longer run. The LATE estimates in the later periods are in fact rather similar to those of the non-commuters. Therefore, the striking differences in the average LATEs over all periods between the commuting and non-commuting groups are mainly driven by the differential effects in initial periods.



(a) Non-commuters



(b) Commuters

Figure 4.4: Effects over years on employment (binary) separately for cross-border commuters and non-commuters.

Note: Dots represent LATEs, bands correspond to 95% confidence intervals, triangles depict the counterfactuals (i.e., mean potential outcomes among compliers under non-treatment

4.6 Conclusion

In this paper, we analyzed the effect of a residence permit on the labor supply and residential decisions of foreign workers by an instrumental variable approach exploiting a migration lottery in Liechtenstein. Our results pointed to substantial effects on the labor market and residential attachment of compliers, whose migration decision complies with the permit assignment in their first lottery. We also found the labor market effects to be more strongly driven by individuals previously not working in Liechtenstein than by previous cross-border commuters. In particular, resident permits appear to incentivize potential new labor market entrants to actually start working in Liechtenstein, which very importantly contributes to the overall effect. However, the permits also seem to keep previously commuting workers in the labor market in the longer run, while hardly affecting their employment decisions in the short run. Overall, our results pointed to stronger effects at the extensive margin of labor supply due to additional employment contracts rather than the intensive margin due to an increase in the hours worked (even though it is not feasible to strictly disentangle the two). In future research following up on our findings, one would ideally link the migration lottery data with tax or other administrative data to examine the effect of resident permits on the income (at least) of current cross-border commuters, the tax revenues in Liechtenstein, or social and political integration. This would, however, be conditional on an expansion of access to anonymized administrative data for research purposes by the local authorities.

Appendices

A Appendix

A.1 Detailed institutional background

This section provides information about the conditions of participation in the lottery and the draw in more detail. Lottery participants must hold an EEA citizenship and transfer the required application documents as well as the participation fees prior to a specific deadline (Landesverwaltung Fürstentum Liechtenstein, 2009, section 38). The amount of the fee varies between the pre-draw (100 CHF) and the final draw (500 CHF) (Ausländer- und Passamt, 2020). Persons with an entry ban, posing a threat to public safety, or providing false statements are already excluded from the first draw of the lottery (Landesverwaltung Fürstentum Liechtenstein, 2009, section 38, 3).

In the final draw, participants must be of full age and must not hold a permanent residence permit (Ausländer- und Passamt, 2019b). Importantly, they must also provide an employment contract of more than one year with a minimum activity level of 80% or an authorized permanent cross-border business activity in case of self-employment (Ausländer- und Passamt, 2019b). After winning both the pre-draw and the final draw, the lottery participant must relocate to Liechtenstein within six months, otherwise the residence permit expires (Landesverwaltung Fürstentum Liechtenstein, 2009, section 37, 2). For this reason, our treatment is defined based on residing in Liechtenstein in the year after the lottery, as obtaining the permit is tied to actually moving there. The drawing procedure can be described as follows. All submitted applications (see Figure 4.A.1.1) are put into a box and even include participants not fulfilling all conditions (to give them the chance to appeal against a later denial of a residence permit due to a violation of the conditions). In the presence of a national judge and media representatives, the winners are blindly drawn from the box and the person who draws announces the total number of winners as well as their nationality (see Figure 4.A.1.2). Lottery losers may participate again in subsequent lotteries, while multiple applications to the very same lottery are not allowed (Landesverwaltung

Fürstentum Liechtenstein, 2009, section 38, 1) c)).

Bitte hier Teilnahmecoupon abtrennen!

Antragsteller/in **Pflichtfelder sind mit * gekennzeichnet.**

Nachname *		Vorname *	
Geburtsdatum *	Geschlecht * ☐ weiblich	☐ männlich	Staatsangehörigkeit *
Strasse, Hausnummer *		Postleitzahl, Ort *	
Wohnland *			

Bewerbergruppen *
Bitte kreuzen Sie nur **eine** Bewerbergruppe an.

☐ Erwerbstätige (Kennzahl:103.431.00.07)

☐ Nicht Erwerbstätige (Kennzahl: 103.431.00.09)

Ich bestätige die Richtigkeit der Angaben und die Einzahlung der Gebühr von CHF 100.-

Unterschrift des Bewerbers/der Bewerberin *

(Bei der Unterschrift durch eine andere Person ist die Kopie einer Vollmacht erforderlich.)

Beachten Sie bitte die zweite Seite!

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Figure 4.A.1.1: Participation voucher (of 2019)

Source: Ausländer- und Passamt (2019a)

Figure 4.A.1.2: Final draw (of spring lottery 2016)

Source: Michael Zanghellini, Liechtensteiner Volksblatt

A.2 Additional information

Figure 4.A.2.1: Map of Liechtenstein

Source: Liechtenstein Marketing

Table 4.A.2.1: Descriptive statistics of outcomes: First participation from 2006 to 2016

	Residing (binary)		Employed (binary)		Activity level (%))		Years residing		Years employed		observations
	mean	std.dev	mean	std.dev	mean	std.dev	mean	std.dev	mean	std.dev	
t = 2	0.13	0.34	0.55	0.50	51.94	48.75	0.13	0.34	0.55	0.50	3,145
t = 3	0.15	0.35	0.51	0.50	47.32	48.63	0.28	0.67	1.06	0.95	2,852
t = 4	0.15	0.36	0.47	0.50	43.19	48.18	0.43	0.99	1.52	1.39	2,607
t = 5	0.15	0.36	0.43	0.50	39.92	47.63	0.57	1.30	1.93	1.82	2,355
t = 6	0.14	0.35	0.40	0.49	36.53	46.79	0.70	1.60	2.30	2.23	2,145
t = 7	0.15	0.35	0.38	0.49	34.66	46.24	0.85	1.93	2.68	2.66	1,912
t = 8	0.14	0.35	0.36	0.48	32.20	45.48	0.98	2.21	2.99	3.06	1,621
t = 9	0.14	0.35	0.35	0.48	31.07	45.09	1.12	2.52	3.36	3.48	1,355
t = 10	0.14	0.35	0.34	0.47	30.06	44.51	1.23	2.80	3.69	3.86	1,072
t = 11	0.15	0.36	0.32	0.47	28.33	43.61	1.55	3.26	4.00	4.26	634
t = 12	0.18	0.38	0.34	0.48	29.57	43.93	2.02	3.83	4.58	4.75	311
Pooled	0.15	0.35	0.44	0.50	40.14	47.71	0.64	1.74	2.03	2.54	20,009

Sources: Employment statistics (2006 - 2018); calculations are our own.

Table 4.A.2.2: Descriptive statistics for cross-border commuters: First participation from 2006 to 2016

	$Z = 1$		$Z = 0$					
	mean	std.dev	mean	std.dev	mean difference	t-value	p-value	observations
Female	0.28	0.45	0.29	0.45	-0.01	-0.25	0.81	1,615
Nationality								
Austria	0.45	0.50	0.44	0.50	0.01	0.24	0.81	1,615
Germany	0.36	0.48	0.36	0.48	0.00	0.13	0.90	1,615
Italy	0.05	0.21	0.08	0.26	-0.03	-1.70	0.09	1,615
Switzerland	0.02	0.12	0.01	0.07	0.01	1.08	0.28	1,615
Others	0.12	0.33	0.12	0.32	0.00	0.19	0.85	1,615
Age	37.40	9.11	36.35	8.99	1.05	1.50	0.13	1,615
First lottery participation								
Dummy 2006	0.08	0.28	0.09	0.28	-0.00	-0.17	0.87	1,615
Dummy 2007	0.08	0.27	0.09	0.29	-0.02	-0.79	0.43	1,615
Dummy 2008	0.10	0.30	0.13	0.34	-0.04	-1.50	0.13	1,615
Dummy 2009	0.10	0.31	0.09	0.28	0.02	0.74	0.46	1,615
Dummy 2010	0.06	0.24	0.09	0.29	-0.03	-1.43	0.15	1,615
Dummy 2011	0.09	0.29	0.09	0.29	0.00	0.02	0.98	1,615
Dummy 2012	0.08	0.28	0.08	0.27	0.01	0.30	0.77	1,615
Dummy 2013	0.09	0.29	0.08	0.27	0.01	0.59	0.56	1,615
Dummy 2014	0.13	0.34	0.09	0.29	0.04	1.52	0.13	1,615
Dummy 2015	0.08	0.28	0.07	0.26	0.01	0.43	0.67	1,615
Dummy 2016	0.09	0.29	0.10	0.29	-0.00	-0.11	0.92	1,615
Number of observations	193		1,422					

Sources: Lottery data (2005 - 2016) and employment statistics (2005 - 2016).

Table 4.A.2.3: Descriptive statistics for non-commuters: First participation from 2006 to 2016

	$Z = 1$		$Z = 0$					
	mean	std.dev	mean	std.dev	mean difference	t-value	p-value	observations
Female	0.29	0.46	0.31	0.46	-0.02	-0.43	0.67	1,530
Nationality								
Missing Dummy	0.00	0.00	0.03	0.18	-0.03	-6.82	0.00	1,530
Austria	0.29	0.45	0.29	0.45	0.00	0.03	0.97	1,485
Germany	0.43	0.50	0.48	0.50	-0.05	-1.21	0.23	1,485
Italy	0.08	0.27	0.07	0.25	0.01	0.35	0.72	1,485
Switzerland	0.00	0.00	0.01	0.08	-0.01	-2.83	0.00	1,485
Others	0.20	0.40	0.16	0.36	0.05	1.42	0.16	1,485
Age								
Missing Dummy	0.02	0.14	0.05	0.21	-0.03	-2.22	0.03	1,530
Age	37.06	9.46	38.74	10.12	-1.67	-2.06	0.04	1,463
First lottery participation								
Dummy 2006	0.11	0.31	0.11	0.32	-0.00	-0.17	0.86	1,530
Dummy 2007	0.11	0.32	0.11	0.32	0.00	0.04	0.97	1,530
Dummy 2008	0.09	0.29	0.16	0.36	-0.07	-2.71	0.01	1,530
Dummy 2009	0.10	0.30	0.09	0.29	0.01	0.45	0.65	1,530
Dummy 2010	0.06	0.24	0.09	0.28	-0.02	-1.02	0.31	1,530
Dummy 2011	0.12	0.33	0.09	0.28	0.03	1.18	0.24	1,530
Dummy 2012	0.10	0.29	0.07	0.25	0.03	1.13	0.26	1,530
Dummy 2013	0.07	0.26	0.05	0.22	0.02	1.00	0.32	1,530
Dummy 2014	0.07	0.26	0.06	0.24	0.01	0.31	0.76	1,530
Dummy 2015	0.10	0.29	0.08	0.27	0.02	0.65	0.51	1,530
Dummy 2016	0.07	0.26	0.09	0.29	-0.02	-1.06	0.29	1,530
Number of observations	157		1,373					

Sources: Lottery data (2005 - 2016) and employment statistics (2005 - 2016).

A.3 Further analyses and robustness checks

Table 4.A.3.1: Empirical results based on first participation and further covariates

	Outcomes				
	Residing (binary)	Employed (binary)	Activity level (%)	Years residing	Years employed
LATE					
Effect	0.70	0.21	16.70	3.43	1.02
Standard error	0.06	0.08	7.54	0.27	0.39
P-value	0.00	0.01	0.03	0.00	0.01
First-stage					
Effect	0.35				
Standard error	0.03				
P-value	0.00				
ITT					
Effect	0.25	0.07	5.88	1.21	0.36
Standard error	0.03	0.03	2.86	0.15	0.15
P-value	0.00	0.01	0.04	0.00	0.02
Observations	20,009				
Trimmed	392				

Note: Standard errors are estimated by cluster bootstrapping.
Only observations whose first lottery participation was after 2005 are included.

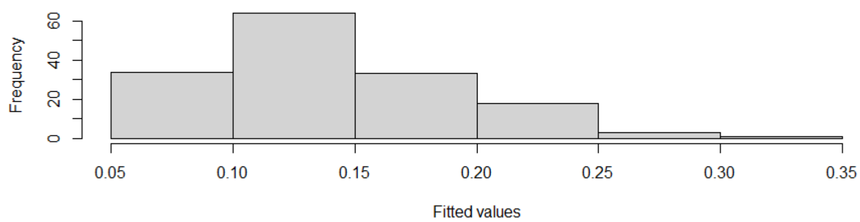

Figure 4.A.3.1: Second participation; Propensity score; Assignment=1

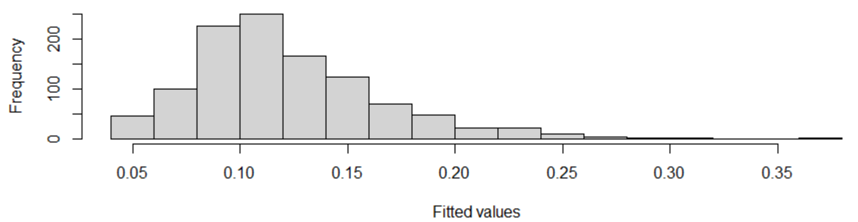

Figure 4.A.3.2: Second participation; Propensity score; Assignment=0

Table 4.A.3.2: Empirical results based on second participation and year dummies

	Outcomes				
	Residing (binary)	Employed (binary)	Activity level (%)	Years residing	Years employed
LATE					
Effect	0.78	0.31	27.39	3.24	1.29
Standard error	0.12	0.15	13.91	0.53	0.72
P-value	0.00	0.04	0.05	0.00	0.07
First-stage					
Effect			0.40		
Standard error	0.06	0.07	0.07	0.07	0.07
P-value			0.00		
ITT					
Effect	0.31	0.12	10.84	1.28	0.51
Standard error	0.06	0.06	5.13	0.29	0.26
P-value	0.00	0.03	0.03	0.00	0.05
Observations			6,771		
Trimmed			1,251		

Note: Standard errors are estimated by cluster bootstrapping.
Only observations whose first lottery participation was after 2005 are included.

Table 4.A.3.3: Empirical results based on second participation and further covariates

	Outcomes				
	Residing (binary)	Employed (binary)	Activity level (%)	Years residing	Years employed
LATE					
Effect	0.70	0.25	26.16	2.81	0.84
Standard error	0.11	0.14	13.58	0.47	0.66
P-value	0.00	0.07	0.05	0.00	0.20
First-stage					
Effect			0.38		
Standard error			0.06		
P-value			0.00		
ITT					
Effect	0.26	0.09	9.82	1.06	0.32
Standard error	0.06	0.05	5.02	0.25	0.25
P-value	0.00	0.08	0.05	0.00	0.20
Observations			6,771		
Trimmed			1,727		

Note: Standard errors are estimated by cluster bootstrapping
Only observations whose first lottery participation was after 2005 are included.

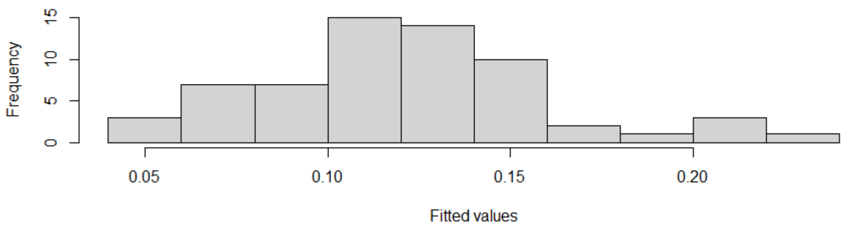

Figure 4.A.3.3: Third participation; Propensity score; Assignment=1

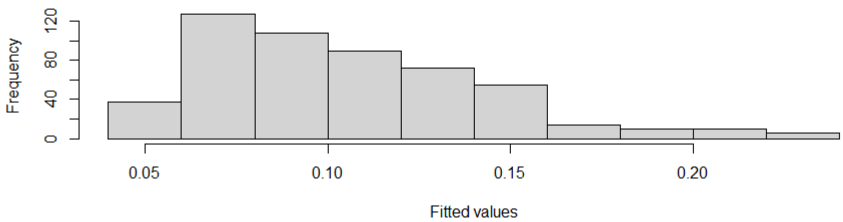

Figure 4.A.3.4: Third participation; Propensity score; Assignment=0

Table 4.A.3.4: Empirical results based on third participation and year dummies

	Outcomes				
	Residing (binary)	Employed (binary)	Activity level (%)	Years residing	Years employed
LATE					
Effect	0.77	0.22	15.19	2.99	0.88
Standard error	0.17	0.18	16.16	0.89	0.82
P-value	0.00	0.22	0.35	0.00	0.28
First-stage					
Effect			0.61		
Standard error			0.10		
P-value			0.00		
ITT					
Effect	0.47	0.13	9.21	1.81	0.53
Standard error	0.10	0.09	8.26	0.53	0.39
P-value	0.00	0.16	0.27	0.00	0.17
Observations			3,369		
Trimmed			1,088		

Note: Standard errors are estimated by cluster bootstrapping.
Only observations whose first lottery participation was after 2005 are included.

Table 4.A.3.5: Empirical results based on third participation and further covariates

	Outcomes				
	Residing (binary)	Employed (binary)	Activity level (%)	Years residing	Years employed
LATE					
Effect	0.80	0.24	13.87	3.58	0.98
Standard error	0.18	0.20	18.68	0.94	0.98
P-value	0.00	0.22	0.46	0.00	0.32
First-stage					
Effect			0.50		
Standard error	0.10	0.11	0.10	0.11	0.11
P-value			0.00		
ITT					
Effect	0.39	0.12	6.87	1.77	0.49
Standard error	0.09	0.10	8.94	0.50	0.45
P-value	0.00	0.22	0.44	0.00	0.27
Observations			3,369		
Trimmed			1,193		

Note: Standard errors are estimated by cluster bootstrapping.
Only observations whose first lottery participation was after 2005 are included.

Table 4.A.3.6: Descriptives: Non-cross-country commuter (one year prior to the first lottery participation)

	Assigned		Non-assigned					
	mean	std.dev	mean	std.dev	mean difference	t-value	p-value	number of observations
Female	0.28	0.45	0.31	0.46	-0.03	-0.79	0.43	1,986
Nationality								
Missing Dummy	0.00	0.07	0.03	0.17	-0.03	-3.95	0.00	1,986
Austria	0.29	0.46	0.28	0.45	0.01	0.44	0.66	1,931
Germany	0.45	0.50	0.49	0.50	-0.04	-1.01	0.31	1,931
Italy	0.06	0.25	0.07	0.25	-0.00	-0.22	0.82	1,931
Switzerland	0.00	0.00	0.00	0.07	-0.00	-2.83	0.00	1,931
Others	0.19	0.40	0.16	0.37	0.03	1.07	0.28	1,931
Age								
Missing Dummy	0.02	0.16	0.05	0.21	-0.02	-1.85	0.06	1,986
Age	37.53	9.54	38.96	10.35	-1.43	-1.97	0.05	1,897
First lottery participation								
Dummy 2006	0.08	0.28	0.09	0.28	-0.00	-0.13	0.90	1,986
Dummy 2007	0.09	0.29	0.09	0.28	0.00	0.08	0.94	1,986
Dummy 2008	0.07	0.25	0.12	0.33	-0.05	-2.62	0.01	1,986
Dummy 2009	0.08	0.27	0.07	0.25	0.01	0.48	0.63	1,986
Dummy 2010	0.05	0.22	0.07	0.25	-0.02	-0.98	0.33	1,986
Dummy 2011	0.09	0.29	0.07	0.25	0.03	1.19	0.23	1,986
Dummy 2012	0.07	0.26	0.05	0.22	0.02	1.15	0.25	1,986
Dummy 2013	0.05	0.23	0.04	0.19	0.02	1.01	0.31	1,986
Dummy 2014	0.05	0.23	0.05	0.22	0.01	0.34	0.74	1,986
Dummy 2015	0.07	0.26	0.06	0.24	0.01	0.68	0.50	1,986
Dummy 2016	0.05	0.23	0.07	0.26	-0.02	-1.01	0.31	1,986
Dummy 2017	0.06	0.24	0.07	0.25	-0.01	-0.47	0.64	1,986
Dummy 2018	0.08	0.28	0.08	0.27	0.01	0.30	0.76	1,986
Dummy 2019	0.08	0.27	0.08	0.28	-0.01	-0.27	0.79	1,986
	202		1,784					

Sources: Lottery data (2005 - 2019) and employment statistics (2005 - 2018); calculations are our own.

Table 4.A.3.7: Descriptives: Cross-country commuter (one year prior to the first lottery participation)

	Assigned		Non-assigned					
	mean	std.dev	mean	std.dev	mean difference	t-value	p-value	number of observations
Female	0.28	0.45	0.28	0.45	-0.01	-0.20	0.84	2,157
Nationality								
Missing Dummy	0.00	0.00	0.00	0.00	0.00			2,157
Austria	0.41	0.49	0.41	0.49	-0.00	-0.02	0.98	2,157
Germany	0.36	0.48	0.36	0.48	-0.00	-0.01	0.99	2,157
Italy	0.07	0.26	0.08	0.27	-0.01	-0.46	0.64	2,157
Switzerland	0.01	0.11	0.01	0.09	0.00	0.33	0.74	2,157
Others	0.15	0.35	0.14	0.35	0.01	0.28	0.78	2,157
Age								
Missing Dummy	0.00	0.00	0.00	0.00	0.00			2,157
Age	37.49	9.29	36.36	9.00	1.14	1.87	0.06	2,157
First lottery participation								
Dummy 2006	0.06	0.24	0.07	0.25	-0.00	-0.31	0.76	2,157
Dummy 2007	0.06	0.23	0.07	0.26	-0.01	-0.94	0.35	2,157
Dummy 2008	0.07	0.26	0.10	0.30	-0.03	-1.68	0.09	2,157
Dummy 2009	0.08	0.26	0.07	0.25	0.01	0.59	0.56	2,157
Dummy 2010	0.05	0.21	0.07	0.25	-0.02	-1.57	0.12	2,157
Dummy 2011	0.07	0.25	0.07	0.25	-0.00	-0.13	0.90	2,157
Dummy 2012	0.06	0.24	0.06	0.23	0.00	0.16	0.87	2,157
Dummy 2013	0.07	0.25	0.06	0.24	0.01	0.45	0.65	2,157
Dummy 2014	0.09	0.29	0.07	0.25	0.03	1.37	0.17	2,157
Dummy 2015	0.06	0.24	0.06	0.23	0.00	0.30	0.77	2,157
Dummy 2016	0.07	0.25	0.07	0.26	-0.00	-0.26	0.80	2,157
Dummy 2017	0.10	0.30	0.08	0.27	0.02	0.82	0.41	2,157
Dummy 2018	0.08	0.27	0.08	0.28	-0.00	-0.20	0.84	2,157
Dummy 2019	0.10	0.30	0.08	0.28	0.01	0.73	0.46	2,157
	266		1,891					

Sources: Lottery data (2005 - 2019) and employment statistics (2005 - 2018); calculations are our own.

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