Schooling or shopping? The impact of shop opening hours deregulation on education and earnings

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Abstract

Recent years have witnessed a strong move towards lifting shop opening regulations in many developed countries. Most evidence suggests that this deregulation has increased employment in the retail industry and subsequently job opportunities for unskilled and young people. This paper first studies the indirect impact of deregulation of shop opening hours on youth schooling decisions, through the changed opportunity cost of human capital acquisition. The analysis is then extended to considering longer run outcomes. To provide causal evidence, we use a national reform in shop opening hour regulation in Norway in 1985 and exploit that the bite of the reform varied substantially across municipalities. We find that increased opening hours substantially reduced the probability to graduate from high school within a five year window. This evidence is consistent with the view that opportunity cost of study time is an important determinant of human capital investments.

JEL-codes: I21, J24

Keywords: high school graduation, deregulation, opening hours

Introduction

Over the last 30 years a number of countries have removed regulations of shop opening hours generally and restrictions on shopping opportunities on Sundays and other public holidays in particular. Evidence from both Europe and North America suggest that deregulation has increased labor demand in the retail industry and in particular increased job opportunities for unskilled workers and youth (e.g. Skuterud (2005), Bossler and Oberficher (2013)). A potential side effect of this deregulation is that the opportunity cost of time for young people changed, subsequently the allocation of time between studying and labor market participation, and other activities changed. Following this line of reasoning, Gruber and Hungerman (2008) investigate the impact of Sunday shopping deregulation on a number of outcomes and find that it led to a fall in religious attendance and a rise in drinking and drug use. Most interestingly, Lee (2013) finds that US states that liberalized restrictions on Sunday shopping by removing "Blue laws" experienced decreased human capital investments in terms of high school graduation and years of education and subsequent lower earnings.

This paper contributes to this literature by providing estimates of the effect of deregulation on human capital investments and subsequent earnings using unique data and a law change that took place in Norway in 1985. We exploit that prior to the law change each municipality could set its own regulations[1]. This discretion resulted in substantial variation in shop closing laws across municipalities. In 1985, the parliament approved a new Opening Hours Act ("Åpningstidsloven") saying that municipalities could not restrict opening hours before 8 p.m. on weekdays and 6 p.m. on Saturdays. Exploring unique data on each municipal's regulations in the pre-reform period (1982) and the fact that the bite of the Opening Hours Act varied substantially between municipalities, we provide causal evidence of the effect of deregulation on educational outcomes and subsequent earnings.

The existing studies from US and Canada have used state by time variation in the removal of

[1] Both before and after the law change there were national restrictions on shop opening hours on Sundays, and religious- and national holidays. Because these regulations were nationwide and unchanged in the period, they do not affect our identification strategy.

"Blue laws" and differences in differences strategy to estimate the impact of shopping deregulation on several outcomes, including human capital investment. One possible problem with the use of state by time-variation in law changes to infer the impact of deregulation is that the timing of legal changes may be endogenously determined and possibly correlated with other factors affecting the outcomes. By using a national reform and exploiting that the bite of the reform varies geographically, this paper circumvents this source of endogeneity[2]. Another contribution of the present paper is that we provide evidence of the impact of increased allowed opening hours on weekdays and Saturdays, while the evidence from removing "Blue laws" only considers the impact of removing restrictions on Sunday shopping.

The paper is organized as follows: Section 2 provides a brief discussion of theory background and reviews the earlier literature. Section 3 presents the institutional set up, data and the empirical strategy. Section 4 provides empirical results while Section 5 concludes.

2. Theoretical background and earlier literature.

The impact of removing shop opening hour regulations on performance in the retail sector has been the subject of several studies. A main concern has been the effect on sales and employment in the retail sector. Deregulation can, through increased sales, lead to a net increase in employment demand, satisfied by increased hours worked by existing employees, hiring of new employees, or both. However, it is possible that deregulation only changes the timing of sales within the day or week and so the net effect on total sales and subsequent labor demand may be zero. The evidence in Jacobsen and Kooreman (2005) suggest that liberalization of shopping hour regulations in the Netherlands in late 1990's generally increased the time people spent shopping. Recent empirical studies also generally find positive labor demand effects. Using information from provincial by year repeal of "blue

[2] The approach is similar in spirit to the use of geographical differences in the bite of a national minimum wage to identify the effect of minimum wages on employment as first introduced in Card (1992) and subsequently used by others in Stewart (2002) and Draca et al. (2011).

laws" in Canada, Skuterud (2005) finds a substantial positive effect on employment in retail firms. Exploiting the 2006 lifting of restriction on business hours in German states in a differences in differences framework, Bossler and Oberfichtner (2013) find that deregulation increases total employment and in particular the use of part-time employment.

While the direct effects of opening hours regulations on total sales, prices and employment in the retail sector are important, standard micro-economic models suggest that time allocation between different activities changes when the relative price and availability of different activities changes. As liberalizing opening hours regulations increase employment and availability of low skilled jobs especially on evenings and weekends, it may affect student time allocation and investment in human capital. In particular, when the opportunity cost of study time increases, a student may allocate less time to school work and more time to other activities with potential reduction of acquired human capital as a result. This may be particularly important if students are shortsighted and heavily discounts the future as recent evidence in Oreopoulos (2007) suggests.

The standard opportunity cost argument predicts decreased human capital investments, but there might be offsetting effects if part time work while in school increases productivity in schoolwork. Further, increased employment opportunities in the retail sector might reduce the probability that families are credit constrained in the education market and lead to increased human capital aquisition. Thus, the net effect on human capital aquisition from deregulation of shop closing hours is in principle ambiguous. To our knowledge, Lee (2013) is the only study providing direct empirical evidence on the effect. She explores the different timing of repeal of Sunday shopping (Blue laws) in US states to estimate the impact of deregulation of shopping hours on educational attainment. Consistent with the opportunity cost argument, she finds that repeal of Sunday shopping reduced the probability of high school graduation by a significant 1.2-1.7 percentage points and years of education by 0.11-0.15 years. Further, the reduction in educational attainment translated into a 1.2 percent reduction in adult earnings.

A possible concern with the use of time-variations in the repeal of shopping time regulations is that these variations may coincide with changes in other determinants of student educational attainment. Although the results in Lee (2013) are robust to a series of

specification checks, empirical analysis using different identification strategies and from other countries and institutional settings seems warranted. Further, her study provides evidence on deregulation of Sunday shopping only, and it is not obvious that the results can be generalized to the impact of weekday shopping hours deregulation. Gruber and Hungerman (2008) investigate the impact of Sunday shopping deregulation on a number of other outcomes and find that it led to a fall in religious attendance and a rise in drinking and drug use.

The findings in Lee (2013) are consistent with the broader literature demonstrating that student opportunity costs and returns to schooling are important determinants of educational attainment as predicted by the seminal work of Becker (1964). Black et al (2005) find that changed outside opportunities for unskilled workers generated by the boom (bust) in the American coal industry led to significant decrease (increase) in high school enrollment. Clark (2011) finds a positive effect of regional unemployment on high school enrolment in England and Wales, while Reiling and Strøm (2013) find a similar countercyclical pattern in high school completion in Norway. Atkin (2012) finds that local expansion of the exporting manufacturing sector in Mexico following trade reform led to an increase in school dropout through the implied demand increase for unskilled labor and increased opportunity cost of schooling. Using reforms in Kibbutz wage sharing arrangements in Israel as a natural experiment, Abramitzky and Lavy (2011) find that increased returns to education causally increase investment in schooling.

3. Institutional background, empirical strategy and data.

Regulation of shopping hours in Norway

Dating back to the Closing law of 1913 ("Lukkeloven av 1913"), the regulation of shop opening hours in Norway was delegated to local authorities (municipalities)[3]. While the closing law imposed some general restrictions on activities on national holidays, Sundays and other Christian holidays, the municipalities were free to set their own shop closing regulations. During the post WW2-period, there was a general tendency that the local

[3] The description here builds on NOU (1984).

governments passed more restrictive closing regulations. The implied shortening of shop opening time was a concern for the government as it forced a lot of people to make their daily shopping within their work hours. Accordingly, several official committees were appointed by the government to consider changes in the closing law. The majority of the members in the committees of 1959 and 1972 proposed to limit the scope for local authorities to restrict opening hours in retail firms. But partly due to strong opposition from interest groups, mainly from trade unions and organizations of retail firms, and partly due to political opposition, the proposals were not converted into law changes. A third committee was appointed in spring 1981, delivered it's proposal in April 1984, denoted NOU (1984) and recommended that local authorities should not be able to restrict opening hours before 8 p.m. on weekdays and 6 p.m. on Saturdays and days before holidays. While this committee was appointed by a Labour government, a Center-right government had come to power in fall 1981. This government in early 1985 proposed a new law in line with the recommendations made by the committee. After some debate in the parliament the new Opening Hours Act was finally passed and made into law in April 1985. This law made it impossible for local authorities to set local closing time in retail firms earlier than 8 p.m. on workdays and earlier than 6 p.m. on Saturdays and days before official holidays. There was still a national law against shops being open Sundays and specific national and religious holidays.

As part of its work, the committee (NOU (1984)) collected data on local opening hours regulations in retail stores and local government service production in each municipality as of 1982. These data include detailed information on closing rules in retail firms and allows us to measure to what extent the new Opening hours act from 1985 changed the legal environment in the municipalities. Below, we explain this data set and demonstrate how it can be explored to estimate the impact of deregulation on educational outcomes and earnings.

Empirical strategy

To estimate the impact of local regulations we use a differences in differences strategy by comparing the change in educational outcomes between cohorts in municipalities

experiencing a substantial liberalization of opening hours regulation induced by the 1985 reform (treated municipality) with the corresponding change in outcomes in municipalities unaffected by the 1985 reform (non-treated municipality). Since we do not have access to actual opening hours in retail firms, the effect estimated by this procedure should be interpreted as intent to treat effects (ITT). Equation (1) shows the regression model representation of this differences in differences strategy where y is the outcome variable (high school completion) for the cohort finishing compulsory school in spring year t. Subscript i denotes the individual student. The outcome variable is further described below.

$$(1)\quad y_{it} = aT_i + \sum_{t=1981}^{1988} d_t D_t + \sum_{t=1981}^{1988} b_t D_t T_i + X_{it}c + u_{it}$$

T_i is our treatment indicator taking the value one if the individual is living in a treated municipality at age 16, while D_t is a cohort indicator where cohort is defined as the year the student finished compulsory school. The coefficients of interest are b_t measuring the difference in outcome between the treatment and control group for cohort t. X_{it} is a vector of individual student characteristics and X and u_{it} is a random error term. The traditional differences in differences estimator restricts b_t=b for the post-treatment cohorts and b_t=0 for pretreatment cohorts. The latter restriction implies a parallell trend assumption, and the general formulation in equation (1) allows a test of this null hypothesis that can be interpreted as a placebo test, i.e. whether the reform had an impact on outcomes in treated municipalities before it was actually implemented.

Although the model above formulates the empirical strategy in a standard differences-in-difference framework, the variation in the bite of the 1985 reform across municipalities implied variation in the treatment intensities. Among the municipalities affected, some experienced very mild restrictions on opening hours, relative to the 1985 floor while in other municipalities the change was substantial. In the next section we describe how we translate this variation into a familiar differences in differences framework.

Data: Opening hours regulation

The data is available from the regional database provided by the Norwegian Social Science

Data Services (NSD). The data provides information on allowed opening hours for each day of the week within each municipality as of 1982. Because some municipalities had restrictions on opening hours that were no stricter than the national floor introduced in 1985 their restrictions were not binding in the sense that the upper limit of allowed opening hours were not increased by the introduction of the 1985 floor. That is, the new national law did not affect a municipality that already allowed shops to be open to 8 p.m. or later on weekdays and 6 p.m. or later on Saturdays prior to 1985. Note that in addition to a substantial spatial difference in opening hours between municipalities, there was also a significant variation in regulations of weekday and Saturday opening hours within municipalities in the pre-1985 period. In addition a weekly shopping day with extended opening hours was common practice. Finally, some municipalities did not have any form of regulations on opening hours by law. This leaves us without any information on the practice and we therefore choose to exclude these municipalities from the analysis.

Table 1 illustrates the varying restrictions on opening hours given by municipalities prior to the reform.

Table 1: Number of municipalities with binding restrictions on shop opening hours relative to 1985 national floor.

	No restrictions	Restriction, not binding	Restriction, binding	Total
Weekdays	54	72	274	400
Weekly Extended Shopping Hours Day	54	220	126	400
Saturdays	61	100	239	400

The number of municipalities included in the analysis is reduced due to requirements to cohort sizes (Each cohort within each municipality must be at least 30 students).

To further illustrate the variation in opening hour regulations, Figure 1 shows the frequency of the varying regulations. There are 5 different degrees of treatment on Saturdays (closing 1 pm – 5 pm), and three different degrees for weekdays (closing 17 pm – 19 pm).

In defining the treatment variable on the basis of the opening hours restrictions as observed

in 1982 and the law passed in 1985 we aim to create a parsimonious model that does not impose too many restrictions on the relationship between the treatment and the outcome. Estimating a model with continuous treatment effects will impose a linear relationship between the treatment and the outcome. However, it is not at all obvious that a linear relationship is the best representation of the data generating process. Rather one would expect the marginal treatment effect to be increasing in the bite of the reform. Consequently we choose to formulate the treatment as a dichotomous variable.

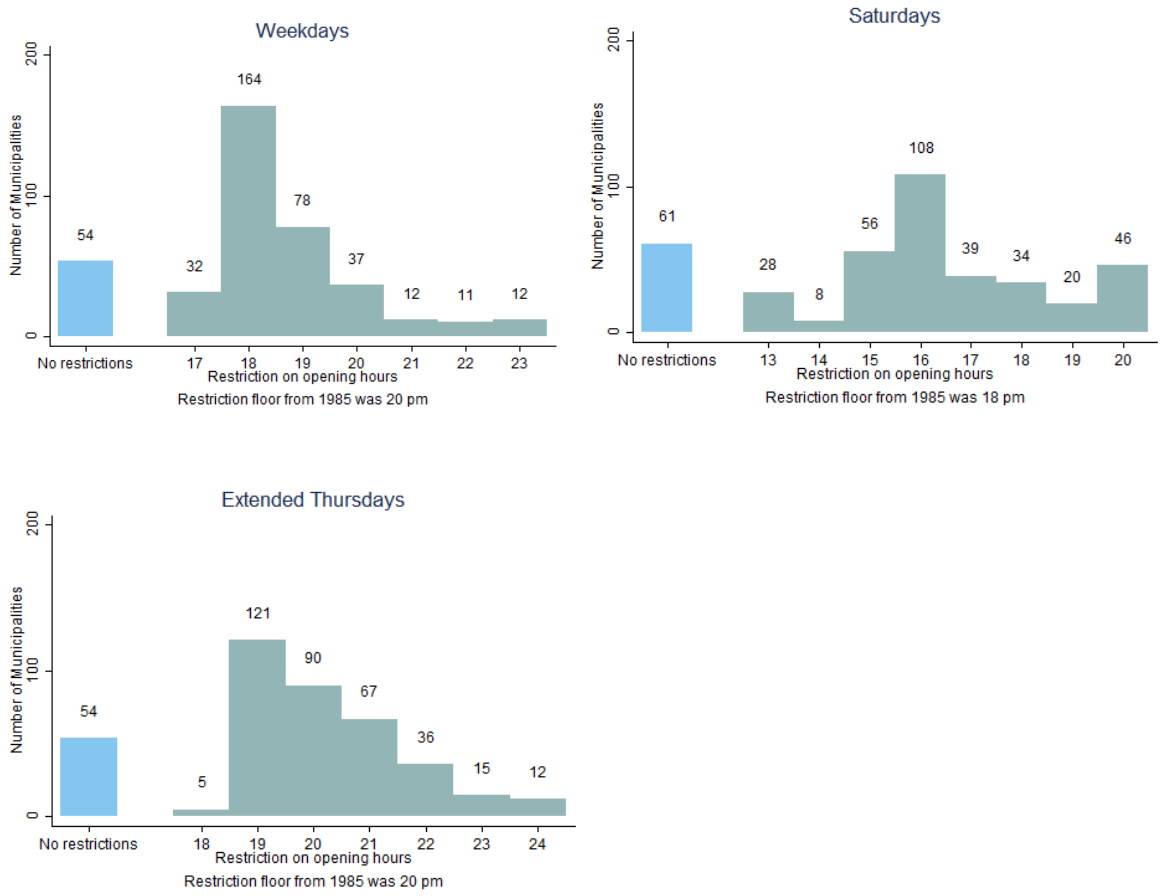

Figure 1: Opening hours restrictions in 1982 by frequency. All municipalities included.

A simple and attractive way of defining the treatment variable is to use a statistical observation of the distribution of restrictions prior to the 1985 Opening Hours Act. As such we use the median treatment intensity of the treated municipalities. That is, first looking

only at the treated municipalities, we obtain the median of total hours treated per week. Then we define those above the median as treated, and the rest as non-treated. The median is 9 hours and is marked with a red vertical bar in figure 2. All municipalities which had restrictions that were milder than the national requirement are kept as non-treated. Figure 2 also shows the geographical distribution of the municipalities assigned to each group. In the empirical analysis below, we also present results using a cut-off at 11 hours.

The rationale for this strategy is composed of two arguments. First, small changes in the opening hours are less likely to affect employment in the sector as part time workers easily can increase their working hours by small amounts. Second, it merely imposes the restriction that the treatment effect is increasing in the underlying treatment measure, i.e. the hours treated, and not any restrictions on the form of the relationship.

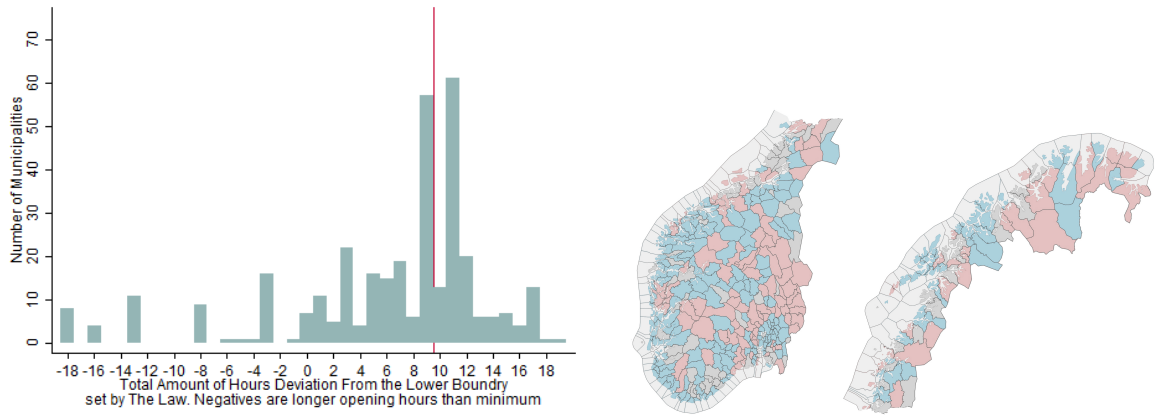

Figure 2: Left hand: Distribution of treated hours. Right hand: Red - Treated more than 9 hours per week, Blue – Treated 9 or less hours per week, Grey – Dropped. The dropped municipalities either miss information on one or more controls or have one or more cohorts consisting of less than 30 individuals. Source: Norwegian Social Science Data Services and Statistics Norway.

Data: Outcome variables

Data on student educational outcomes, adult earnings and background is obtained from register data in Statistics Norway. These register data contains information on when students graduated from compulsory school as well as from high school, which is non-compulsory. Specifically, we use as one of our outcome variable whether a student

graduated from high school five years after finishing lower secondary education. The reason we use this exact window is that Statistics Norway and the government use this definition when presenting official statistics on national completion rates. Therefore it can be considered a standard measure of high school graduation in the Norwegian context. This measure is also used in other papers using Norwegian data, see Reiling and Strøm (2014) and Falch et al. (2014).

Data: Control variables

Evidence from most countries shows a strong and robust link between family background and other individual characteristics and educational outcomes. In order to control for the effect of such characteristics we use the available information from the register dataset on immigration status, parental education and other family related characteristic. In line with previous research on international and Norwegian data we expect the educational level of parents to have a positive effect on the individual graduation rate (e.g. Falch and Strøm, 2011 and Reiling and Strøm, 2014).

Through the Norwegian Social Science Database we have access to a number of municipality level controls including demographic, economic and political variables. As municipal demographic controls we include the share of young people in the population, the share of old, and the share of inhabitants living in urban areas in the municipality. The political affiliation of the mayor is included as an indicator of the political orientation of the local authority. A larger share of young people in the total population could reflect a relatively higher labor supply within this age group resulting in a weaker effect of exogenous changes in the job opportunities. As we are considering the effect of labor market conditions on completion rates it is natural to include labor market controls, specifically the unemployment rate. Including the contemporaneous unemployment rate at the municipal level is problematic as it might be considered as an outcome of the treatment, and therefore constitute a so-called bad controls problem (Angrist and Pischke, 2009). To reduce this problem we instead use the lagged unemployment rate in the economic region. The 90 economic regions are defined by Statistics Norway and constructed based on commuting statistics, on average they contain 4.8 municipalities. From the descriptive statistics

presented in table 1 it is apparent that the presence and strictness of the opening hours' regulations are correlated with population size and urbanity, we therefore include these variables in certain specifications of our regressions. All variables are defined and their descriptive statistics shown in the appendix. Table 2 shows descriptive statistics for the municipalities that are included in the regression analysis according to the treatment definition previously discussed.

Table 1: **Descriptive statistics*.**

Treated median hours	Treated 9 hours or less	Treated more than 9 hours	All
Graduation Rate	0.50	0.47	0.49
Share in Academic Track	0.34	0.33	0.34
Share in Vocational Track	0.41	0.43	0.42
Share Female	0.49	0.50	0.49
Share between 16 and 20	0.08	0.08	0.08
Share older than 60	0.18	0.20	0.19
Both Parents Employed	0.40	0.40	0.40
Only Father Employed	0.13	0.15	0.14
Only Mother Employed	0.15	0.15	0.15
Parents Divorced	0.06	0.06	0.06
Parents Married	0.32	0.33	0.32
1st Generation Immigrant	0.00	0.00	0.00
2nd Generation Immigrant	0.00	0.00	0.00
Cohort size in Municipality	214.36	182.93	203.42
Average Minimum cohort size in period	156.07	128.80	146.58
Population 1990	12,450.42	10,773.12	11,872.99
Population 1980	14,550.81	12,449.04	13,827.25
Lagged Regional Unemployment	0.01	0.01	0.01
Population Density	80.05	41.53	66.84
Mayor Left	0.24	0.49	0.33
Mayor Right	0.74	0.49	0.65
Municipal expenditures per. Inhabitant:			
Childcare	69.74	63.70	67.66
Education	1,055.12	1,150.07	1,087.81
Eldercare	439.83	488.63	456.63
Health	279.35	294.13	284.44
Culture	161.15	166.23	162.90
Transport	57.58	75.96	63.91
Admin	209.74	248.70	223.05
Other	815.42	941.35	858.77
Number of Municipalities	161	86	247

- The number of municipalities differs from the number reported in table 1 because only the municipalities included in the analysis are used here. Some municipalities are dropped due to particularly small cohorts (<30). Based on observables in 1981, 1980 when 1981 not available. Treatment is defined as opening hour laws increasing by more than 9 hours per week. Source: Statistics Norway and Norwegian Social Science Data Services.

4. Empirical results

4.1 Graphical evidence on graduation rates

Before turning to the regression results we will now consider some simple graphical evidence regarding graduation rates. First we consider the development of graduation rates

in the municipalities treated at more than (treated) and less than (nontreated) the median number of hours. Figure 3 shows this development for the cohorts graduating from mandatory schooling from 1981 to 1987. It is evident from the figure that while the graduation rate is lower for all cohorts in the treated group, it declines for the 1984 cohort with roughly a percentage point relative to the year before. This is the only cohort in the two treatment groups experiencing a decline in the graduation rate. For the rest of the period the trend is very similar regardless of treatment.

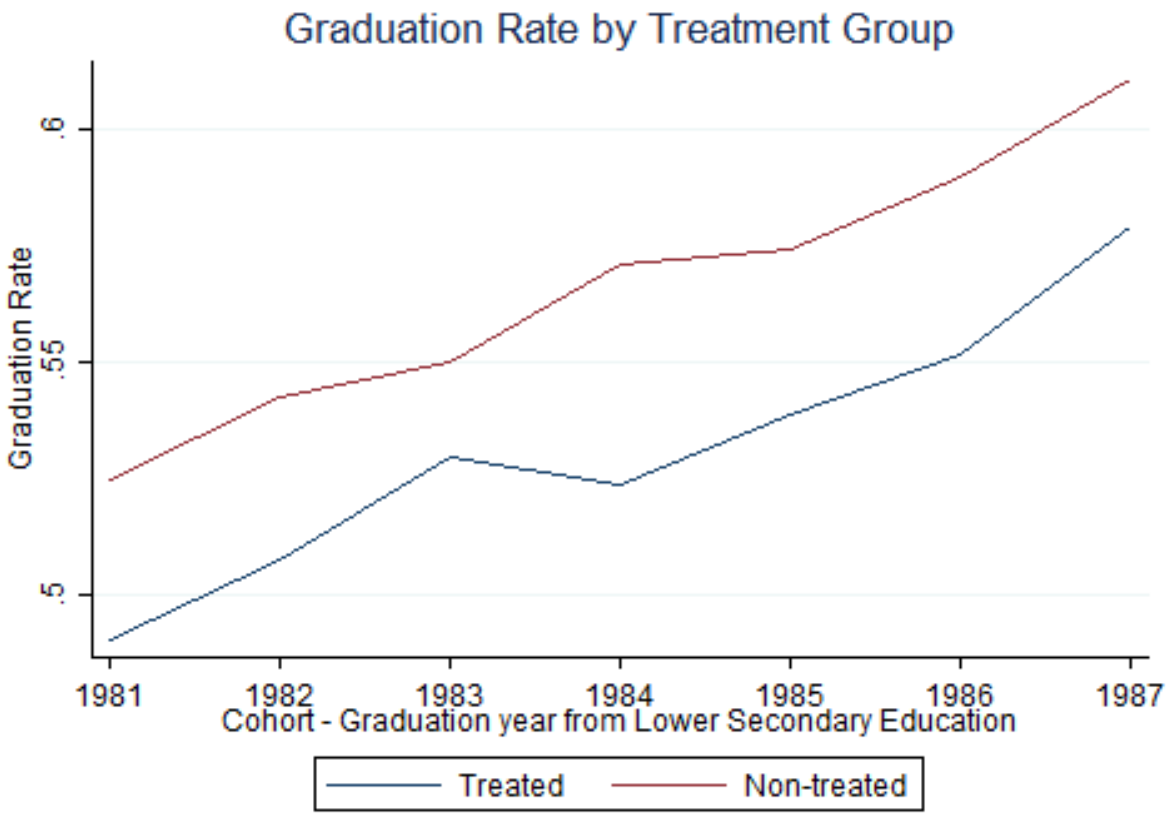

Figure 3: Graduation rates in the treated and non-treated groups, cohorts 1981-1987. Source: Statistics Norway.

Synthetic Matching

A more sophisticated graphical analysis can be conducted using synthetic matching. Invalid control groups represent a constant threat when attempting to identify a causal effect in a

natural experiment. One way of handling this problem is to construct a control group with similar observed characteristics in the pretreatment period. We will now present graphical results inspired by the synesthetic matching method as applied by Abadie, Diamond and Hainmueller (2010) and Abadie et. al. (2012). In order to create a synthetic match we need to define a pre-treatment period in which to match observables. To construct the synthetic control unit we match all predictor variables on the municipality level from 1981 to 1983; The pretreatment outcome (the average graduation rate in the municipality), is matched in the years 1981 and 1982. We then compare the post-treatment graduation rate for different cohorts in the treated municipalities with the graduation rate for the same cohorts in this estimated counterfactual (the synthetic control group). Comparing the average treated unit to an estimated counterfactual is a useful exercise because we are then able to match the treated municipalities to a control group with similar observed characteristics in the pre-reform period.

The results from this procedure are presented in Figure 4 and are very illustrating. Prior to the 1984 cohort the graduation rate in the treatment and synthetic control groups follow the exact same pattern, but then deviates sharply and remain dispersed. The estimated difference using this simple procedure is similar to the effect we estimate in section 3.2 below when restricting the regression model coefficients to be equal in the pretreatment and treatment periods, respectively. The pattern is also evident, when we reduce the sample to only include the students who enrolled in high school immediately after graduating from mandatory schooling. The most pronounced dissimilarity is that the difference between the groups for the treated cohorts is less stable.

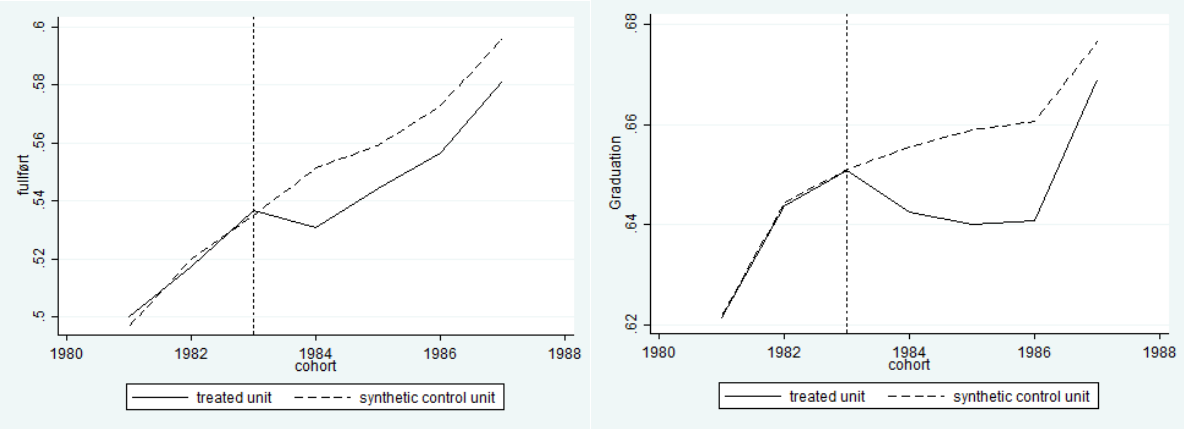

Figure 4: The figure on the left hand side shows the development of the graduation rate in the average treated municipality compared with an artificial control municipality created using information from the non-treated municipalities. The figure on the right hand side shows the result when only looking at students that enrolled in high school in the fall the same year they graduated from mandatory schooling.

4.2 Regression results

We now present results from regression equations corresponding to different variants of model (1). Table 3 shows the model results based on the total sample of students in each cohort gradiating from compulsory school 1981-1987. In the first column no controls are included except for municipality and cohort fixed effects. In all specifications we exclude all municipalities that have less than 30 students enrolled from a single cohort at any point. This is to exclude particularly small municipalities from driving the results. Column (1) shows results from the traditional DID regressions with treated cohorts defined as those finishing compulsory school after 1983, augmented by control variables and linear regional time trends in columns (2) and (3), respectively. The controls include individual characteristics and time varying municipality variables as well as the lagged regional unemployment rate and accounts for compositional differences between the cohorts.

To further increase comparability between the treated and non-treated municipalities column (3) includes linear regional time trends using the economic regions defined by Statistics Norway as region definition. In all there are 90 such regions compared to a total of 456 municipalities. Inclusion of linear economic region trends accounts for possible unobserved smooth changes in the labor market opportunities of potential dropouts from high school. The estimated treatment effects are negative and statistically significant in all

specifications. According to column (3), treatment leads to a 2.2 percentage point reduction in the probability to graduate from high school within 5 years after compulsory school. This is a substantial effect as the average completion rate in the treated municipalities prior to treatment was 47.0%.

Columns (4)-(6) report results from models with a full set of cohort by treatment interaction effects. We first notice that the restriction of zero interaction effects for the cohorts before 1984 cannot be rejected at conventional significance levels in all three specifications. This can be interpreted as evidence supporting the parallel trends assumption. Further, the restriction of equal coefficients in the post-treatment period (post 1983) is formally not rejected by the F-tests.

Looking at the more detailed result, we find a negative interaction effect for all cohorts after 1983, although precisely estimated only for the 1984 cohort. The 1984 cohort had spent a maximum of one year in high school when the reform was enacted; as the cohort year is the year students finish compulsory school.

Detailed estimation results are reported in Appendix table A1. The coefficients for the control variables have expected signs and are in line with previous Norwegian evidence. The probability of graduating is increasing in parental education and is higher for females than for males while the effect of immigration status and labor market status of the parents are insignificant. We also find that the probability of graduation is increasing in the lagged regional unemployment rate. The coefficient estimate suggests that one percentage point increase in regional unemployment increase the probability to graduate by approximately 1 percentage point which is in the same ballpark as the effect found in Reiling and Strøm (2013).

Table 3. Estimation results. High school graduation. Total sample of students

Explanatory variables	(1) Graduation	(2) Graduation	(3) Graduation	(4) Graduation	(5) Graduation	(6) Graduation
Treated - post 1983	-0.0121*	-0.0135**	-0.0223***			
	(0.00624)	(0.00541)	(0.00649)			
Treated - Cohort 1982				-0.00147	-0.00106	-0.00168
				(0.00706)	(0.00749)	(0.00768)
Treated - Cohort 1983				0.0102	0.00941	0.00754
				(0.00844)	(0.00811)	(0.00833)
Treated - Cohort 1984				-0.0167*	-0.0204**	-0.0230***
				(0.00934)	(0.00819)	(0.00880)
Treated - Cohort 1985				-0.00581	-0.0105	-0.0136
				(0.00953)	(0.00839)	(0.00929)
Treated - Cohort 1986				-0.00929	-0.0112	-0.0148
				(0.00906)	(0.00810)	(0.00980)
Treated - Cohort 1987				-0.00471	-0.000425	-0.00435
				(0.0101)	(0.00915)	(0.0111)
Observations	353,226	322,138	322,138	353,226	322,138	322,138
R-squared	0.017	0.097	0.097	0.017	0.097	0.097
Individual level controls	no	yes	yes	no	yes	yes
Municipality level controls	no	yes	yes	no	yes	yes
Cohort FE	yes	yes	yes	yes	yes	yes
Municipality FE	yes	yes	yes	yes	yes	yes
Region time trend	no	no	yes	no	no	yes
Clustering level	Municpality	Municpality	Municpality	Municpality	Municpality	Municipality
# Municipalities	247	247	247	247	247	247
p-value, F-test of zero interaction effects, 1982-1983 cohorts				0.130	0.171	0.229
p-value, F-test of equality 1984-1987				0.406	0.107	0.191

Robust standard errors clustered at indicated level in parentheses, *** p<0.01, ** p<0.05, * p<0.1

High school graduation conditional on enrollment

So far the outcome variable has been the probability of high school graduation within a five year window for the total sample of students finishing compulsory school. As a robustness check we also estimate the model on the subsample of students actually enrolled into high school the same fall they finished compulsory school. Table 4 shows regression results for models similar to those reported in Table 3 for this particular sample of students. Looking first at the basic differences in differences results in columns (1)-(3) we find a very similar pattern as for the total sample. The treatment effect is significantly negative and becomes both numerically stronger and more precisely estimated when controls and linear regional trends are included in the models. According to the result in column (3), the treatment group has a 2.8 lower probability of graduating from high school 5 years after enrolment than the non-treated group.

Columns (4)-(6) report results from models with a full set of cohort by treatment interaction effects. Again, the p-values for F-tests of the restriction of zero interaction effects for the cohorts before 1984 cannot be rejected at conventional significance levels in all these specifications and can be interpreted as evidence supporting our identification strategy. Further, the restriction of equal coefficients in the post-treatment period (post 1983) is formally not rejected by the F-tests.

The development of the coefficients over the period follows a pattern broadly similar to that provided by the results from the total sample. but that the actual size of the interaction by cohort effects differs somewhat. Considering that we analyze an intent-to- treat variable we are not able to verify to what extent shops actually extended their opening hours. Consequently a certain lag in the increase in shopping hours in the treated municipalities cannot be ruled out. On these grounds it is not very surprising that the treatment effect is significant at the 5% level only for the 1986 cohort.

Table 4. High school graduation conditional on enrollment

Explanatory variables	(1) Graduation	(2) Graduation	(3) Graduation	(4) Graduation	(5) Graduation	(6) Graduation
Treated - post 1983	-0.0134*	-0.0148**	-0.0284***			
	(0.00680)	(0.00620)	(0.00741)			
Treated - Cohort 1982				0.00297	0.00317	-0.000260
				(0.00882)	(0.00934)	(0.00956)
Treated - Cohort 1983				0.0187	0.0167	0.00939
				(0.0125)	(0.0126)	(0.0131)
Treated - Cohort 1984				-0.00686	-0.0115	-0.0223**
				(0.0109)	(0.0104)	(0.0113)
Treated - Cohort 1985				-0.00650	-0.00882	-0.0227*
				(0.0113)	(0.0106)	(0.0117)
Treated - Cohort 1986				-0.00919	-0.0118	-0.0283**
				(0.00949)	(0.00910)	(0.0120)
Treated - Cohort 1987				-0.00121	0.000234	-0.0192
				(0.0118)	(0.0109)	(0.0130)
Observations	283,208	258,277	258,277	283,208	258,277	258,277
R^2	0.013	0.073	0.074	0.013	0.073	0.074
Individual level controls	no	yes	yes	no	yes	yes
Municipality level controls	no	yes	yes	no	yes	yes
Cohort FE	yes	yes	yes	yes	yes	yes
Municipality FE	yes	yes	yes	yes	yes	yes
Regional time trend	no	no	yes	no	no	yes
Clustering level	Municipality	Municpality	Municipality	Municipality	Municipality	Municipality
# municipalities	247	247	247	247	247	247
p-value, F-test of zero interaction effects, 1982-1983 cohorts				0.296	0.390	0.406
p-value, F-test of equality 1984-1987				0.854	0.596	0.722

Robust standard errors clustered at indicated level in parentheses, *** p<0.01, ** p<0.05, * p<0.10

Different treatment cutoffs

To further investigate robustness, we present results from models using different definitions of the treatment groups. So far the treatment status has been defined at having a change in legal opening hours by 9 or more hours per week. Columns (1) and (4) in Table 5 reproduces the baseline results for the 9 hours cut off for the total sample and conditional on enrolment samples respectively. Columns (2)-(3) and (5)-(6) shows treatment effects using 7 and 11 hours per week as cut-offs for the two samples. Results for the alternative cutoff definitions are quite similar as for the 9 hours case. This strengthen our belief that the treatment effects are robust across different definitions of the treatment-control groups.

Table 5. High school graduation, total sample and conditional on enrollment, respectively

	(1)	(2)	(3)	(4)	(5)	(6)
	Total sample	Total sample	Total sample	Conditional on enrollment	Conditional on enrollment	Conditional on enrolment
VARIABLES						
Treated 9 - post 1983	-0.0223***			-0.0284		
	(0.00649)			(0.00741)		
Treated 7 - post 1983		-0.0191***			-0.0194***	
		(0.00601)			(0.00686)	
Treated 11 - post 1983			-0.0207**			-0.0215**
			(0.00822)			(0.00949)
Observations	322,138	322,138	322,138	258,277	258,277	258,277
R^2	0.097	0.097	0.097	0.074	0.074	0.074
Individual level controls	yes	yes	yes	yes	Yes	yes
Municipality level controls	yes	yes	yes	yes	Yes	yes
Cohort FE	yes	yes	yes	yes	Yes	yes
Municipality FE	yes	yes	yes	yes	Yes	yes
Regional time trend	Yes	yes	yes	yes	Yes	yes
Clustering	Municipality	Municipality	Municipality	Municipality	Municipality	Municipality

Robust standard errors clusterd at indicated level in parenthesis. ***p<0.01, **p<0.05, *p<0.1

5. Long run outcomes (to be included)

Higher Education outcomes and years of education

Earnings

6. Conclusion

Previous evidence has shown that the past and current deregulation of shop opening hours in developed countries has increased employment in the retail industry. As an industry largely using low skilled and young employees, the opportunity cost of education likely falls as shop opening hours increase. Exploring the fact that a national reform in Norway in 1985 had different impact of shop opening hours across geographical areas as a natural experiment, we find that an extension of the opening hours in the retail industry induced a decrease in the graduation rates in upper secondary education. The quantitative effect is sizeable. Our main results suggest that students in areas experiencing a strong potential increase in shop opening hours had 1.6-2%-points lower high school graduation rates than comparable students in other areas. These results are relatively similar to that found by Lee (2013) using the removal of "Blue laws" in US as a natural experiment. Although our results are to be interpreted as reduced form effects (Intent to treat effects) the results are also consistent with evidence that increased outside job opportunities have negative effects on graduation rates for high school students.

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Appendix: A short description of the synthetic matching procedure.

This section draws heavily on Abadie et. al. (2012). First we collapse our observations into a panel data set with municipalities and cohorts. We then construct our treated unit by finding the average of the observed characteristics in the treated municipalities for the cohorts 1981-1987. The averages are then used as the observed values for the new treated municipality. Let this unit be called *q* and be observation number *j* = 1. The units j[2, *J* + 1] are the non treated municipalities, the donor pool. Our comparison unit used in Figure 4 in the main text is created from this donor pool. We divide our observations into two periods, the pre-treatment period T^- [1981,1983], and the treatment period T^+[1984,1985], with $T^- + T^+ = T$. Defining X_1 as the *(k x 1)* vector of controls (those included in our regression models) and the outcome variable (graduation rates) in the pre-treatment period for the unit *q*. These are the variables we want to match to the synthetic control unit, the predictors. Let X_0 be the *(k x J)* matrix containing the same variables in the T^- period for the *J* untreated municipalities. The synthetic control unit is constructed using a (*J* x 1) weighting vector $W^* = (w_2, ..., w_{j+1})$, with $w_2 + ... + w_{j+1} = 1$ such that it minimizes:

$$\sum_{m=1}^{k} v_m (X_{1m} - X_{0m} W)^2$$

In which v_m is the weighting of the *m*-th predictor according to its relative importance within the group of predictors as a whole. We choose equal weighting in our specification so it is effectively a vector of 1's. Once we have created a synthetic control unit to match the treated unit *t* in the pre-treatment period we can estimate a theoretical counterfactual development of the outcome had it not been treated. In order to do this let *Graduation* be the graduation rate for cohort *t*[1981,1987]. Then define $Graduation_1$ as the (T^+ x 1) vector containing the post-treatment graduation rate for each cohort for the treated unit *q*, and Y_0 is the (T^+ x J) matrix containing the post treatment graduation rate for each cohort in municipality *j* + 1. Subsequently the average treatment effect will be estimated as the difference between the observed average graduation rate in the treated municipalities and the synthetic unit:

$$Y - \sum_{j=1}^{J+1} w_j^* Y_{jt}$$

Table A1. Data definitions and sources.

Variable	Description	Source
Graduation	Graduated from high school within five years after graduating from mandatory schooling	Statistics Norway
1st gen. immigrant	=1 if first generation immigrant	Statistics Norway
2nd gen. immigrant	=1 if second generation immigrant	Statistics Norway
Female	=1 if female	Statistics Norway
Enrolled	=1 if enrolled in high school the fall after graduating from mandatory schooling	Statistics Norway
Parents completed high school	=1 if highest parental education is high school	Statistics Norway
Parents completed short higher edu.	=1 if highest parental education is between 1 and 4 years of higher education	Statistics Norway
Parents completed long higher edu.	=1 if highest parental education is more than 4 years of higher education	Statistics Norway
Age younger than 20	Share of population in municipality younger than 20 years old	Statistics Norway
Age older than 60	Share of population in municipality older than 60 years old	Statistics Norway
Leftist Mayor	=1 if mayor is socialist	Statistics Norway
Regional Unemp. T minus 1	Regional unemployment previous year	Statistics Norway
One parent working	=1 if exactly one parent is employed	Statistics Norway
Both parents working	=1 if exactly both parents are employed	Statistics Norway
Opening Hours 1982	Maximum allowed opening hours given by municipal law in 1982	NOU 1984

Table A1. Graduation, total sample of students. Complete results corresponding to table 3.

VARIABLES	(1) Graduation No controls	(2) Graduation With Controls	(3) Graduation Region Time Trend	(4) Graduation No Controls	(5) Graduation With Controls	(6) Graduation Region Time Trend
Treated - post 1983	-0.0121*	-0.0135**	-0.0223***			
	(0.00624)	(0.00541)	(0.00649)			
1st gen. immigrant		0.000158	0.00198		0.000327	0.00200
		(0.0170)	(0.0169)		(0.0170)	(0.0169)
2nd gen. immigrant		-0.00631	-0.00560		-0.00628	-0.00561
		(0.0264)	(0.0265)		(0.0265)	(0.0265)
Parents completed high school		0.196***	0.196***		0.196***	0.196***
		(0.00331)	(0.00331)		(0.00331)	(0.00330)
Parents completed short higher edu.		0.401***	0.401***		0.401***	0.401***
		(0.00470)	(0.00472)		(0.00471)	(0.00472)
Parents completed long higher edu.		0.482***	0.482***		0.482***	0.482***
		(0.00679)	(0.00678)		(0.00679)	(0.00678)
Female		0.0459***	0.0458***		0.0459***	0.0458***
		(0.00345)	(0.00346)		(0.00346)	(0.00346)
Share 16-20 years		1.240**	0.506		1.201**	0.587
		(0.561)	(0.477)		(0.551)	(0.475)
Share above 60 years		-0.575	-0.885*		-0.634	-0.850*
		(0.421)	(0.450)		(0.407)	(0.453)
Leftist Mayor		0.00531	0.00828		0.00531	0.00946
		(0.00595)	(0.00583)		(0.00595)	(0.00598)
Regional Unemployment, t-1		1.107*	1.361**		1.179*	1.477**
		(0.611)	(0.638)		(0.618)	(0.652)
Parents Married		-0.00129	-0.00138		-0.00130	-0.00138
		(0.00191)	(0.00191)		(0.00191)	(0.00191)
Parents Divorced		0.00583	0.00581		0.00588	0.00585
		(0.00547)	(0.00545)		(0.00546)	(0.00544)
One parent working		0.000491	0.000425		0.000485	0.000417
		(0.00208)	(0.00207)		(0.00208)	(0.00207)
Both parents working		0.000195	0.000246		0.000196	0.000244
		(0.00190)	(0.00189)		(0.00190)	(0.00189)
Treated - Cohort 1982				-0.00147	-0.00106	-0.00168
				(0.00706)	(0.00749)	(0.00768)
Treated - Cohort 1983				0.0102	0.00941	0.00754
				(0.00844)	(0.00811)	(0.00833)
Treated - Cohort 1984				-0.0167*	-0.0204**	-0.0230***
				(0.00934)	(0.00819)	(0.00880)
Treated - Cohort 1985				-0.00581	-0.0105	-0.0136
				(0.00953)	(0.00839)	(0.00929)
Treated - Cohort 1986				-0.00929	-0.0112	-0.0148
				(0.00906)	(0.00810)	(0.00980)
Treated - Cohort 1987				-0.00471	-0.000425	-0.00435
				(0.0101)	(0.00915)	(0.0111)
Constant	0.512***	0.303***	17.83***	0.512***	0.316***	14.24***

	(0.00344)	(0.101)	(3.240)	(0.00342)	(0.0977)	(3.505)
Observations	353,226	322,138	322,138	353,226	322,138	322,138
R-squared	0.017	0.097	0.097	0.017	0.097	0.097
Individual level controls	no	yes	yes	no	yes	yes
Municipality level controls	no	yes	yes	no	yes	yes
Cohort FE	yes	yes	yes	yes	yes	yes
Municipality FE	yes	yes	yes	yes	yes	yes
Econ. region time trend	no	no	yes	no	no	yes
Clustering	Municpality	Municpality	Municpality	Municpality	Municpality	Municpality
n. Municipalities	247	247	247	247	247	247
Min. Cohort-Municipality Size	30	30	30	30	30	30
p-value of equality 1982-1983				0.300	0.348	0.463
p-value of equality 1984-1987				0.406	0.107	0.191
Robust standard errors in parentheses						
*** p<0.01, ** p<0.05, * p<0.1						

Table A2. Graduation, conditional on enrolment. Complete results corresponding to table 4.

VARIABLES	(1) Graduation No controls Enrolled	(2) Graduation Full Controls Enrolled	(3) Graduation Region Time Trend Enrolled	(4) Graduation No Controls Enrolled	(5) Graduation Full Controls Enrolled	(6) Graduation Region Time Trend Enrolled
Treated - post 1983	-0.0134*	-0.0148**	-0.0284***			
	(0.00680)	(0.00620)	(0.00741)			
1st gen. immigrant		0.00364	0.00467		0.00382	0.00472
		(0.0137)	(0.0137)		(0.0137)	(0.0137)
2nd gen. immigrant		-0.0136	-0.0131		-0.0135	-0.0130
		(0.0286)	(0.0286)		(0.0287)	(0.0286)
Parents completed high school		0.166***	0.166***		0.166***	0.166***
		(0.00329)	(0.00329)		(0.00329)	(0.00329)
Parents completed short higher edu.		0.331***	0.331***		0.331***	0.331***
		(0.00570)	(0.00572)		(0.00571)	(0.00573)
Parents completed long higher edu.		0.395***	0.395***		0.395***	0.395***
		(0.00681)	(0.00683)		(0.00682)	(0.00684)
Female		0.0511***	0.0510***		0.0511***	0.0510***
		(0.00380)	(0.00380)		(0.00379)	(0.00380)
Age younger than 20		1.000*	0.489		0.962*	0.522
		(0.542)	(0.536)		(0.539)	(0.538)
Age older than 60		-0.421	-0.895*		-0.453	-0.855*
		(0.342)	(0.528)		(0.345)	(0.517)
Leftist Mayor		0.00923	0.00733		0.00928	0.00773
		(0.00808)	(0.00794)		(0.00807)	(0.00795)
Regional Unemploym,t-1		0.467	0.929		0.477	0.909
		(0.674)	(0.771)		(0.689)	(0.756)
Parents Married		-0.00214	-0.00215		-0.00215	-0.00216
		(0.00222)	(0.00222)		(0.00222)	(0.00222)
Parents Divorced		0.00833	0.00831		0.00835	0.00832
		(0.00606)	(0.00605)		(0.00606)	(0.00605)
One parent working		-0.000333	-0.000415		-0.000333	-0.000410
		(0.00231)	(0.00230)		(0.00230)	(0.00230)
Both parents working		-0.000203	-0.000215		-0.000196	-0.000207
		(0.00201)	(0.00201)		(0.00201)	(0.00201)
Treated - Cohort 1982				0.00297	0.00317	-0.000260
				(0.00882)	(0.00934)	(0.00956)
Treated - Cohort 1983				0.0187	0.0167	0.00939
				(0.0125)	(0.0126)	(0.0131)
Treated - Cohort 1984				-0.00686	-0.0115	-0.0223**
				(0.0109)	(0.0104)	(0.0113)
Treated - Cohort 1985				-0.00650	-0.00882	-0.0227*
				(0.0113)	(0.0106)	(0.0117)
Treated - Cohort 1986				-0.00919	-0.0118	-0.0283**
				(0.00949)	(0.00910)	(0.0120)

Treated - Cohort 1987				-0.00121	0.000234	-0.0192
				(0.0118)	(0.0109)	(0.0130)
Constant	0.625***	0.424***	24.38***	0.625***	0.433***	22.93***
	(0.00417)	(0.0859)	(3.977)	(0.00405)	(0.0864)	(4.296)
Observations	283,208	258,277	258,277	283,208	258,277	258,277
R-squared	0.013	0.073	0.074	0.013	0.073	0.074
Individual level controls	no	yes	yes	no	yes	yes
Municipality level controls	no	yes	yes	no	yes	yes
Cohort FE	yes	yes	yes	yes	yes	yes
Municipality FE	yes	yes	yes	yes	yes	yes
Econ. region time trend	no	no	yes	no	no	yes
Clustering	Municpality	Municpality	Municpality	Municpality	Municpality	Municpality
n. Municipalities	247	247	247	247	247	247
Min. Cohort-Municipality Size	30	30	30	30	30	30
p-value 1982-1983				0.296	0.390	
p-value 1984-1987				0.854	0.596	