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Earning or Learning? The impact of relaxing shop opening hours restrictions on youth employment, education and earnings[1]

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Abstract

Many countries have recently removed or relaxed restrictions on shop opening hours. If deregulation increases job opportunities for unskilled young people it may affect incentives to make education investments. This paper studies the impact of deregulation of shop opening hours on youth employment, schooling decisions and subsequent earnings. We use a national reform in shop opening hour restrictions in Norway in 1985 to provide quasi-experimental evidence by exploiting that the bite of the reform varied considerably across municipalities. We find that increased potential opening hours substantially reduced the average probability to graduate from high school and especially so for the group of students with less educated parents. These students also experienced a reduction in completed years of education and some earnings reduction in adulthood. Combined with the finding that deregulation increased employment of 16-24 year old workers in the retail sector by 12% on average, the 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, earnings, employment, deregulation, opening hours

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Introduction

Over the last 30 years a number of countries have removed or relaxed restrictions on shop opening hours on weekdays, Sundays and other public holidays. While employment effects are theoretically unclear, evidence from both Europe and North America suggest that relaxing restrictions has increased labor demand in the retail industry, in particular for young and unskilled workers (e.g. Skuterud (2005), Bossler and Oberfichtner (2014)). A potential side effect is that the opportunity cost of time for young people changed and subsequently changed the allocation of time between studying and labor market participation, and other activities. Consistent with this line of reasoning, Lee (2013) finds that US states that deregulated Sunday shopping by removing "Blue laws" experienced decreased human capital investments in terms of reduced high school graduation and years of education and subsequent lower earnings. 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.

This paper contributes to the literature in three ways: First, we provide estimates of the effect of relaxing restrictions on shopping hours on employment of young workers in retail industry and individual human capital investments and subsequent earnings in Norway. Second, our identification strategy differs from previous contributions and is arguably relaying on a more exogenous variation. Third, we consider the effect of deregulating weekday opening hours on human capital choice rather than the more specific deregulation of Sunday opening hours. To obtain causal effects we explore a major national reform in the regulation of shop closing hours taking place in 1985. We exploit that prior to the reform each municipality could freely set its own shop closing regulations[2]. This discretion resulted in substantial variation in shop closing time across municipalities. In 1985, the parliament approved The Opening Hours Act ("Åpningstidsloven") which implied that municipalities could not restrict opening hours before 8 pm on weekdays and 6 pm on Saturdays. Exploring unique data on each municipality's shop closing regulations in the pre-reform period and the fact that the bite of the Opening Hours Act varied substantially between municipalities, we

[2] 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.

provide causal evidence of the effect of increased shop opening hours on high school graduation, years of education and subsequent earnings as well as the effect on youth employment in the retail industry.

The existing studies from US and Canada have used state by time variation in the removal of "Blue laws" in a 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 potential source of endogeneity[3]. Another contribution of the present paper is that we provide evidence on the impact of increased potential opening hours on weekdays and Saturdays, while the evidence from removing "Blue laws" only considers the impact of removing restrictions on Sunday shopping. In addition, we investigate to what extent effects differ between students with different levels of parental education.

Most existing studies of the effects of removing restrictions on shopping on employment, education and other outcomes use data from the US and Canada. It is not obvious that evidence from these countries can be generalized to countries with different educational and labor market institutions. In particular, the combination of smaller income differences, lower returns to education and more generous benefit levels in many European welfare states motivate separate investigation of the impact of deregulations in the retail market on employment and educational choices in such societies.

The paper is organized as follows: First a brief discussion of theoretical background and a review of the earlier literature are presented in section 2. Section 3 presents the institutional set up, data and the empirical strategy, while section 4 provides estimation results for the impact of the deregulation on employment of young workers in the retail sector. The effect on high school graduation is explored in section 5, which is followed by a series of

[3] 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).

robustness checks and heterogeneity analysis in section 6. Section 7 provides evidence on the effect on completed years of education and subsequent earnings. Section 8 summarizes and concludes.

2. Theoretical background and earlier literature.

The impact of deregulation of shop opening hour restrictions on sales and employment in the retail sector has been the subject of several studies. Removing restrictions can, through increased sales, lead to a net increase in labor 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) suggests 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 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 (2014) find that deregulation increased total employment and in particular the use of part-time employment.

Acknowledging that the the direct effects of opening hours regulations on total sales, prices and employment in the retail sector are important, this paper also investigates a possible side effect. Standard micro-economic models suggest that time allocation between different activities changes when the relative prices and availability of different activities changes. As removing opening hours restrictions increases 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 spend 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 acquisition. Thus, the net effect on human capital acquisition 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 opening hours restrictions 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 state by time variations in the repeal of blue laws is that these variations may coincide with changes in other determinants of student educational attainment. Although the results in Lee (2013) appears to be 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 the removal of Sunday shopping restrictions only, and it is not obvious that the results can be generalized to the impact of increased weekday shopping hours.

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 (2015) 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)[4]. 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 governments passed more restrictive closing regulations. The implied shortening of shop opening time was a concern for the government as it forced a considerable share of the population 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 appointed in1959 and 1970 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 from the 1959 and 1970 committees were not converted into law changes. A third committee was appointed in spring 1981 and delivered a report in April 1984, denoted NOU (1984). The committee recommended that local authorities still should have the mandate to restrict opening hours but not earlier than 8 pm on weekdays and 6 pm on Saturdays. While a Labor government appointed this committee, 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 Opening Hours Act was finally passed and made into law in April 1985. This law prevented local authorities from setting local closing time in retail firms earlier than 8 pm on workdays and earlier than 6 pm on

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

Saturdays and days before official holidays. Regulation of opening hours on Sundays and specific national and religious holidays were unaffected by the 1985 reform.

In 1982, as part of it's work, the committee (NOU (1984)) had collected data on local opening hours regulations in retail stores and local government service production in each municipality. These data include detailed information on closing rules for retail firms each day of the week. This allows us to measure to what extent The 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 deregulations on educational outcomes we take advantage of the variation in the change in opening hours between municipalities generated by the 1985 reform. For each municipality we calculate the number of hours per week retail stores could expand opening hours as the difference between local restrictions in 1982 and the requirements to minimum allowed opening hours imposed by The Opening Hours Act in 1985. We will refer to this measure as the number of hours municipalities are treated. Since we do not have data on 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 strategy where y_{it} is the outcome variable, (high school completion, completed years of education and adult earnings) for individual i in the cohort finishing compulsory school in spring year t. The outcome variables are 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 equal to the number of hours the municipality was treated. How this is calculated is explained in greater detail below. Students are linked to the opening hours regulations in their municipality of residence at age 16, 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 effect of a 1 hour increase in opening hours on the various outcomes for a student in cohort t. X_{it} is a vector of individual student characteristics and u_{it} is a random error term. Equation (1) will provide a separate estimated treatment effect for each of the 7 cohorts included in the data. Because The Opening Hours Act was passed and implemented in 1985 and students spend a limited time in high school, students in the earlier cohorts are less affected than students in later cohorts. This would be reflected in smaller coefficients for the earlier cohorts when estimating equation (1). In fact, students in the first few cohorts might be completely unaffected by the changes in opening hours regulations in 1985 as they have progressed further through high school, are older, and might already have dropped out by 1985. It is possible to test a zero treatment effect for the earlier cohorts using the regression framework in equation (1). In particular we test if the treatment effect is zero and equal for the first 3 cohorts in the sample, i.e. b_t=0 for the cohorts 1982-1983 with the 1981 cohort as reference category. This is effectively equivalent to a test of the parallel trend assumption, or as a placebo test. Given that these cohorts can be considered as untreated, we expect the change in opening hours generated by the Opening Hours Act in 1985 to have no effect on students in these cohorts. Further, we also test if the treatment effect is the same for the last 4 cohorts, i.e. b_t=b for the cohorts 1984-1987. As it turns out below that neither of these restrictions can be rejected statistically, equation (2) illustrates a more conventional differences in differences strategy with a single treatment coefficient b when the restrictions are imposed:

$$(2)\ y_{it} = aT_i + \sum_{t=1981}^{1987} d_t D_t + b_t C_t T_i + X_{it}c + u_{it}\ , \qquad C_t = \begin{cases} 1\ if\ cohort > 1983 \\ 0\ otherwise \end{cases}$$

Although the model above formulates the empirical strategy using a continuous treatment

variable, other specifications are possible. Several alternative specifications, including a traditional difference-in-difference approach with a pure treatment dummy are explored in the robustness checks.

Data: Opening hours regulation

Data on municipal opening hours regulations collected in NOU (1984) are available from the regional database provided by the Norwegian Social Science Data Services (NSD). The data provide information on allowed opening hours for each day of the week in each municipality as surveyed in 1982. At the time municipalities generally specified opening hours restrictions for weekdays and Saturdays separately. In addition most municipalities allowed extended opening hours for one of the weekdays, usually Thursdays. The severity of the local restrictions varied greatly both between municipalities on the same days, and within municipalities on different days. This implies a substantial variation in the effective change in allowed opening hours across municipalities induced by the 1985 reform. Broadly, municipalities were one of 4 types. The first group consists of municipalities that had restrictions on opening hours that were less strict than the new 1985 requirements on all days of the week. That is, municipalities where the latest allowed closing time was at least 8 pm on weekdays and 6 pm on Saturdays. For these municipalities the new law did not imply any changes in allowed opening hours. Other municipalities had regulations that were stricter than the national minimum allowed opening hours on some days of the week, but not on others. For example, a municipality could require all retail stores to close by 8 pm on weekdays, but 4 pm on Saturdays. The third group consists of municipalities whose regulations were stricter than the 1985 requirements on all days of the week. Finally, some municipalities had no formal restrictions on opening hours whatsoever. An overview of the number of municipalities with restrictions on different days of the week is provided in table 1.[5] In order to ease the exposition, we denote municipalities whose opening hours restrictions were eased by the 1985 Opening Hours Act as treated, whereas municipalities

[5] The number of municipalities included in the empirical analysis of educational outcomes and earnings below is lower than the number reported in Table 1 due to requirements on cohort sizes (Each cohort within each municipality must consist of at least 30 students for the municipality to be included in the sample used in the empirical analysis).

that already complied to the new law are denoted un-treated.

A total of 70 municipalities had no restrictions on any days of the week. With the current data it is not possible to rule out that The Opening Hours Act induced additional regulations on opening hours in these municipalities. For example, it could be the case that The Opening Hours Act had a normative power, making municipalities without prior restrictions on opening hours imposing new legislation. We are unable to test this hypothesis with the current data, but in section 6 we show that our results are not driven by including municipalities without opening hours restrictions in 1982.

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

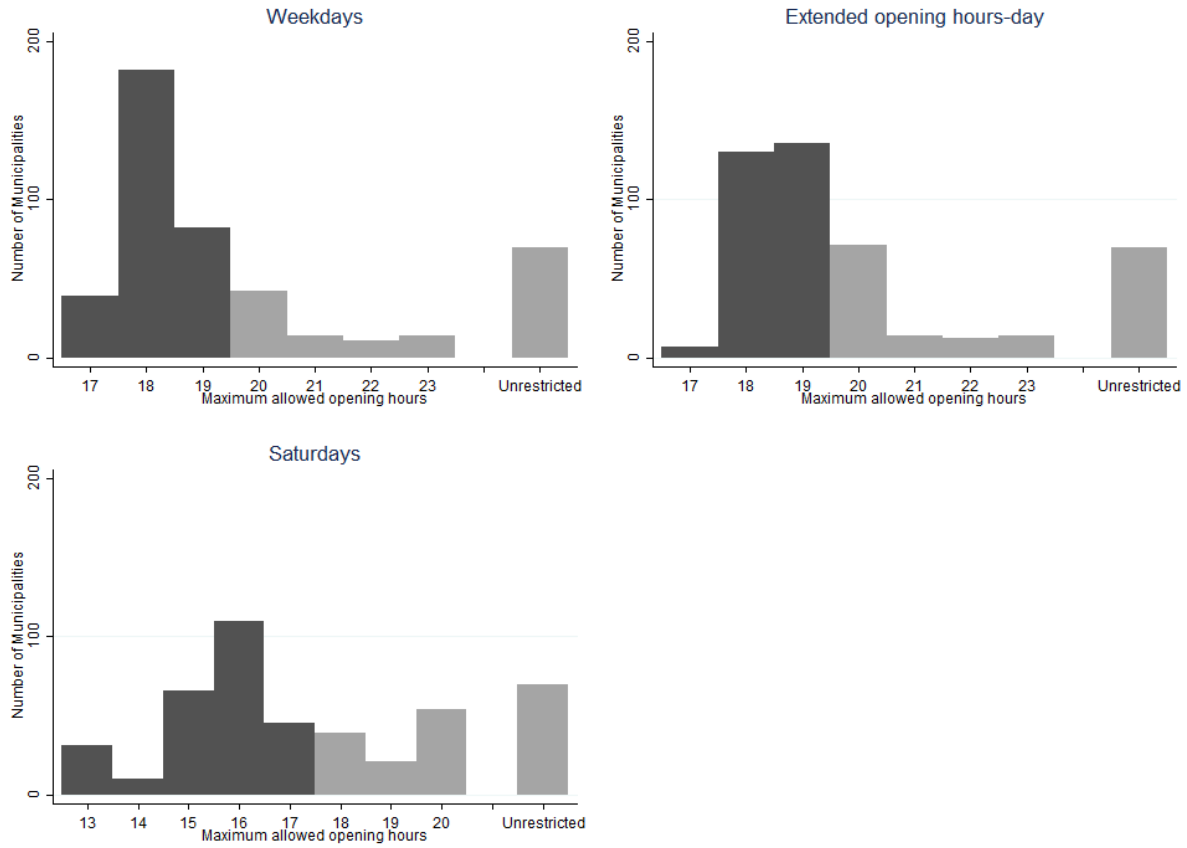
	No restrictions	Restrictions, not binding	Restrictions, binding	Total
Weekdays	70	81	303	454
Extended shopping hours day	70	111	273	454
Saturdays	78	114	262	454

To further illustrate the variation in opening hours regulations, Figure 1 shows the frequency of maximum allowed opening hours by day of the week as observed in 1982. There are 5 different degrees of treatment on Saturdays (closing 1 pm – 5 pm), and 3 different degrees for weekdays (closing 17 pm – 19 pm).

In our baseline specification we define the treatment variable T as the number of hours per week retail shops could expand opening hours as a result of The Opening Hours Act. For each day of the week we calculate the difference between maximum allowed opening hours in 1982 and the minimum allowed opening hours introduced by the Opening Hours Act in 1985. As a clarifying example consider a municipality that in 1982 allowed retail shops to be open until 7 pm Monday through Saturday. After The Opening Hours Act it was no longer possible to require retail shops to close earlier than 8 pm on weekdays, effectively increasing

opening by 1 hour Monday to Friday. Because the municipality already allowed shops to be open until 7 pm on Saturdays, restrictions on opening hours this day were unaffected. In total retail shops in this municipality could therefore be open 5 more hours per week.

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



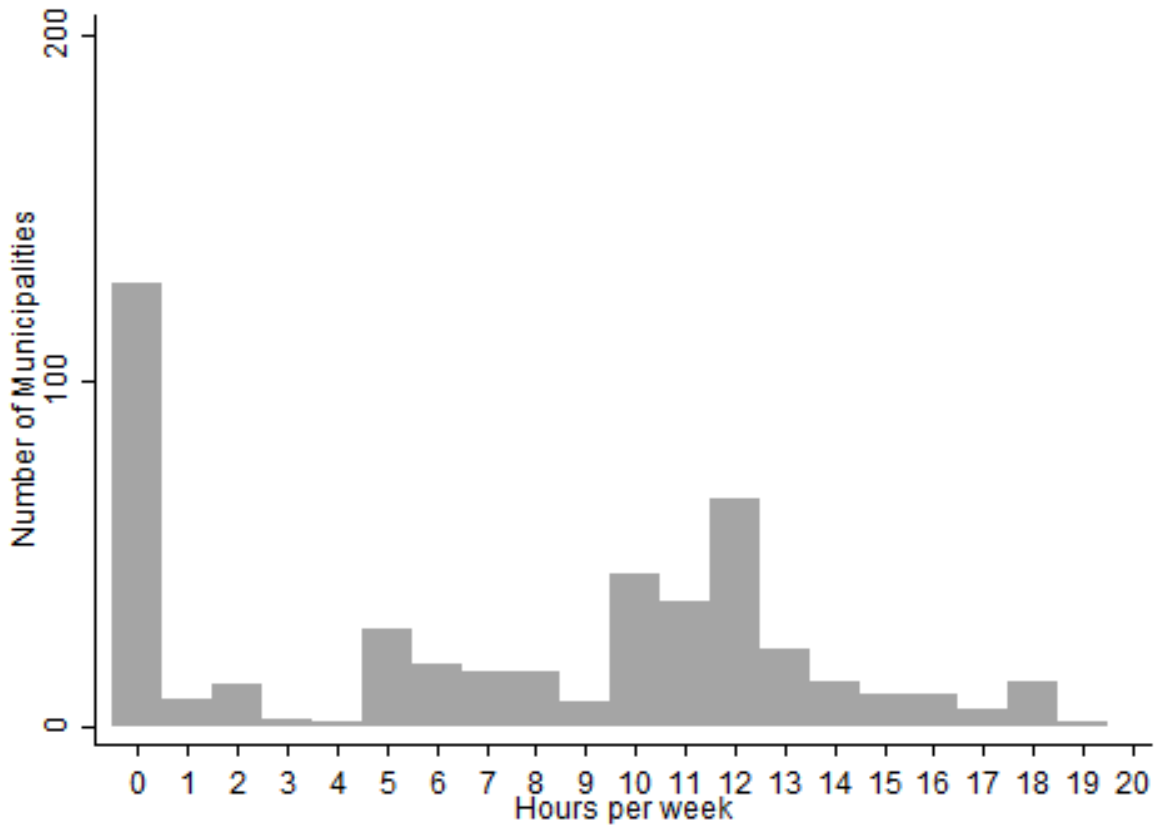
The distribution of our treatment variable is plotted in Figure 2. Of the 128 unaffected municipalities 58 had restrictions at least one day of the week, but so lenient that they were allowed under The Opening Hours Act. The remaining 70 had no restrictions any day of the week. 326 municipalities had restrictions at least one day of the week that were stricter than allowed under The Opening Hours Act. Among these municipalities the median expansion in

allowed opening hours was 11 hours per week, with a mean of 10.14.[6]

It is not obvious that a linear continuous relationship is the best way to specify treatment for our purposes for several reasons. First, retail firms might prefer to employ more experienced workers. Depending on the labor market, they might therefore chose not to employ young unskilled labor when the increase in allowed opening hours is relatively small as firms first deplete the supply of more experienced workers. Second, facing small increases in allowed opening hours, firms might be able to accommodate their increased need for labor by moving workers from part time to full time positions, instead of increasing total number of employees. Third, students might be less inclined to change behavior and ultimately drop out of high school if employment opportunities exist for a sufficient number of hours per week. Students living in municipalities where opening hours only marginally increased and consequently experienced only minor changes in job opportunities might therefore be unaffected. Unfortunately, the data used in the analysis of youth employment effects below do not allow for a thorough investigation of such nonlinearities. However, in the analysis of educational outcomes in sections 6 we investigate in more detail the possibility of alternative representations of the treatment variable and non-linear treatment effects.

[6] In the typical municipality where opening hours increased by 11 hours per week maximum allowed opening hours on weekdays was 6 pm, 7 pm on days with extended opening hours, and 4 pm on Saturdays.

Figure 2: Frequency distribution of increases in allowed weekly opening hours. Source: Norwegian Social Science Data Services.



Data: Individual 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 the year the students graduated from compulsory school as well as from high school, which is non-compulsory. Specifically, we use as one of our outcome variables an indicator for graduation 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 high school 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 (2015) and Falch et al. (2014 a, b).

Data: Individual control variables

Evidence from most countries show 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, 2015).

We have access to a number of municipality level control variables including demographic, economic and political variables from the Norwegian Social Science Database and the data base provided by Fiva et al. (2012). As municipal demographic controls we include the share of young people in the population and the share of elderly 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 high school completion rates it is natural to include labor market controls, specifically the unemployment rate at the time of leaving compulsory school. However, including the contemporaneous unemployment rate at the municipal level is problematic as it might be considered as an outcome of the treatment, and therefore introduce 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 an economic region consist of 4.8 municipalities.

It is possible that the presence and strictness of the opening hours' regulations are correlated with municipal characteristics. While municipal fixed effects account for time-constant municipal characteristics, we include controls for some time-varying municipal variables in the models. Definitions and sources for all variables are shown in the appendix. Table 2 shows descriptive statistics for the municipalities that are included in the regression analysis. We divide the municipalities into two main categories. Municipalities that

experienced an increase in allowed opening hours after 1985 are denoted treated and those without any change in allowed opening hours are denoted un-treated

Table 2. Descriptive statistics as measured in 1980/1981 by treatment status of municipality*

	Non-treated	Treated	All	p-value on diff.
Student characteristics				
Female	0.49	0.49	0.49	0.75
Both parents working	0.12	0.12	0.12	0.56
Exactly one parent working	0.29	0.29	0.29	0.72
Parents divorced	0.04	0.03	0.03	0.01
Parents married	0.49	0.42	0.43	0.00
First generation immigrant	0.00	0.00	0.00	0.46
Second generation immigrant	0.00	0.00	0.00	0.28
Parental education				
Mandatory schooling	0.03	0.03	0.03	0.16
High school	0.58	0.57	0.58	0.28
Shorter higher education	0.15	0.13	0.14	0.02
Longer higher education	0.04	0.04	0.04	0.65
Municipality characteristics				
Graduation rate	0.55	0.54	0.54	0.14
Share in the academic track	0.34	0.35	0.35	0.78
Share in the vocational track	0.49	0.45	0.46	0.00
Average cohort size in the period	181.18	195.34	192.15	0.68
Smallest cohort size in the period	127.74	134.23	132.77	0.81
Share of population 16-20 years	0.08	0.08	0.08	0.88
Share of population 60+ years	0.18	0.20	0.20	0.02
Population 1980	11,274.53	13,321.90	12,855.95	0.48
Lagged Regional Unemployment	0.01	0.01	0.01	0.00
Population Density	110.75	80.32	86.83	0.37
Mayor left leaning	0.29	0.47	0.43	0.00
Number of Municipalities	66	227	293	

The total number of municipalities differs from the number reported in table 1 because only the municipalities included in the analysis are used in table 2. Some municipalities are dropped due to particularly small cohorts (<30) in one or more years and some municipalities are dropped due to missing observations on some characteristics in 1980, 1981 or 1982. Treatment is defined as opening hours increasing as a consequence of The Opening Hours Act. Source: Statistics Norway and Norwegian Social Science Data Services.

4. Youth employment

The strategy to estimate the effect of deregulation on graduation rates and other subsequent individual outcomes outlined above is based on a reduced form framework. Consequently it is not clear how to interpret the results without exploring possible underlying mechanisms. In theory there could be a number of pathways for deregulation to impact graduation rates. The most intuitive channel is that expanded opening hours lead to

an increase in employment opportunities for young people, and hence increase the opportunity cost of schooling. However, other mechanisms unrelated to the labor market cannot be ruled out a priori. Explicitly, expanded opening hours could lead to a change in social patterns. Shops could act as social venues for students, effectively reducing the amount of time spent studying in the evenings. Moreover, expanded opening hours could lead to a change in other activities such as the consumption of drugs and alcohol as argued in Gruber and Hungerman (2008) and Lee(2013). Due to data limitations we are only able to investigate the plausibility of the labor market channel in this paper.

To motivate our empirical study of the effect on educational outcomes and the plausibility of the labor market channel, we first use census employment data to estimate the treatment effect on the growth of youth employment in the retail sector. The published census data is collected every ten years, and includes the number of individuals employed in each municipality by sector and age group.[7] This allows us to explicitly trace the youth employment growth in the retail sector in 1970, 1980 and 1990 across municipalities. The youngest age groups defined in the census are individuals aged 16-19 and 20-24. To allow for possible longer run youth employment effects we define youth employment in retail as the number of employed individuals in the age range 16 to 24.

The average relative size of the retail sector 1970, 1980 and 1990 is reported in table 3. Relative size is measured as the share of employed individuals aged 16-24 that are employed in the retail sector. As evident from the table, on average 12-14 % of all workers aged 16-24 are employed in the retail sector. This makes the retail sector one of the main employers of young people and suggests that the deregulation of opening hours restriction could substantially increase employment opportunities of 16-24 year olds.

[7] Only individuals who work a minimum of 100 hours within a given sector in the census year are counted as employed in the sector. Sectors are defined by Statistics Norway and are comparable to the International Standard Classification of Occupations (ISCO) at the time the census is conducted

Table 3. Share of working individuals aged 16-24 employed in the retail sector in different census years. The unit of measurement is municipalities.[8]

Year	Mean	sd	min	Max	# Municipalities
1970	0.124	0.044	0.036	0.236	297
1980	0.130	0.046	0.024	0.246	297
1990	0.141	0.058	0.028	0.361	297

Source: The Norwegian Social Science Data Service.

The goal of this section is to identify the causal effect of deregulating the opening hours restrictions on youth employment growth between 1980 and 1990 in the retail sector. Our identification strategy is relatively straightforward, using the number of hours a municipality is treated as the treatment variable under the assumption that the opening hours restrictions as measured in 1982 is a valid measure for the situation in 1980. The regression model is formally presented in equation (3). The dependent variable is the logarithm of the number of individuals aged 16 to 24 employed in the retail sector. α_i is a municipality fixed effect, d1990 is a dummy equal to 1 if the census year is 1990, T is the number of hours per week retail shops could extend opening hours under the 1985 Opening Hours Act. X is a vector of demographic controls that varies over time, including population size and age composition. The coefficient of interest is β_2 and can be interpreted as the effect of lifting restrictions on opening hours by 1 hour per week on the youth employment growth in the retail sector controlling for time varying municipality characteristics. A positive and significant β_2 would suggest that deregulation increased youth employment growth.

$$(3)\ \log(employment\ retail_{it}) = \beta_0 + \alpha_i + \beta_1 d1990 + \beta_2 T_i * d1990 + X\gamma + u_{it}$$

A possible threat to our strategy to identify employment effects is that employment was trending upward in the treated municipalities for other reasons. To strengthen the credibility of the results we therefore also report the results of a "placebo" regression in column 2. Here we re-estimate the model using 1970 as the pre-treatment observation year and 1980

[8] Due to budget cuts for Statistics Norway the 1990 census only included random subsamples for most municipalities. Combined with small population sizes this leads to missing values for employment in one or more sectors in many municipalities. Municipalities with missing observations for employment in the retail sector are excluded from the analysis. In total this is 157 municipalities.

as the post-treatment observation year. As the deregulation occurred in 1985 we expect to see no significant effect of the deregulation. Finding an effect in this specification would suggest that the youth employment growth in the period 1970 to 1980 is predictive for which municipalities were affected by the reform, and hence the parallel trend assumption would be violated.

Another potential source of bias in estimating equation (3) is that there could be some unobserved characteristics that drives the general youth employment growth and possibly correlated with the probability that a municipality had to deregulate restrictions in 1985. To address this concern we re-estimate equation (3) using the youth employment growth in the manufacturing industry sector. While some spillover effects between sectors are possible, we expect the deregulation to have a very small effect on the employment growth in other sectors than retail. All results are reported in table 4.[9]

[9] Results when only municipalities used in the analysis of educational outcomes below are included are reported in appendix table A1B and show similar effects

Table 4. Estimated employment equations. Complete results are reported in the appendix table A1.

	(1) Log(Employment retail)	(2) Log(Employment retail)	(3) Log(Employment manufacturing industry)
	1980-1990	1970-1980	1980-1990
Year 1990	0.0705		0.167
	(0.123)		(0.147)
Hours treated x 1990	0.0116***		-0.000750
	(0.00359)		(0.00514)
Year 1980		0.492***	
		(0.147)	
Hours treated x 1980		-0.00358	
		(0.00373)	
Municipality fixed effects	yes	Yes	Yes
Observations	594	594	544
R-squared	0.984	0.980	0.970
# Municipalities	297	297	272

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, *** p<0.1

As evident from column (1), the interaction term is positive and significant at the 5% level. On the margin a municipality where allowed opening hours increased by 1 hour per week saw an increase in youth employment in the retail sector from 1980 to 1990 equal to 1.16%. As municipalities that were forced to lift restrictions on average expanded allowed opening hours by about 10 hours per week, the estimate implies that the 1985 Opening Hours Act caused a 11.6% growth in youth employment in the retail sector on average. The effect is relatively large, and is consistent with the previous studies finding positive employment effects of deregulation of opening hours in retail firms, see Skuterud (2005) and Bossler and Oberfichtner (2014).

Turning to column (2) we see that the estimated effect using the placebo period is negative and statistically indistinguishable from 0. This strengthens the interpretation of the estimate in column (1) as providing causal evidence on the youth employment effect of the 1985 reform. Similarly, the estimated effect of the deregulation dummy is insignificant in column (3). This provides evidence that the employment growth effect found in the retail sector is caused by the sector specific deregulation in opening hours. This is reassuring, as a common

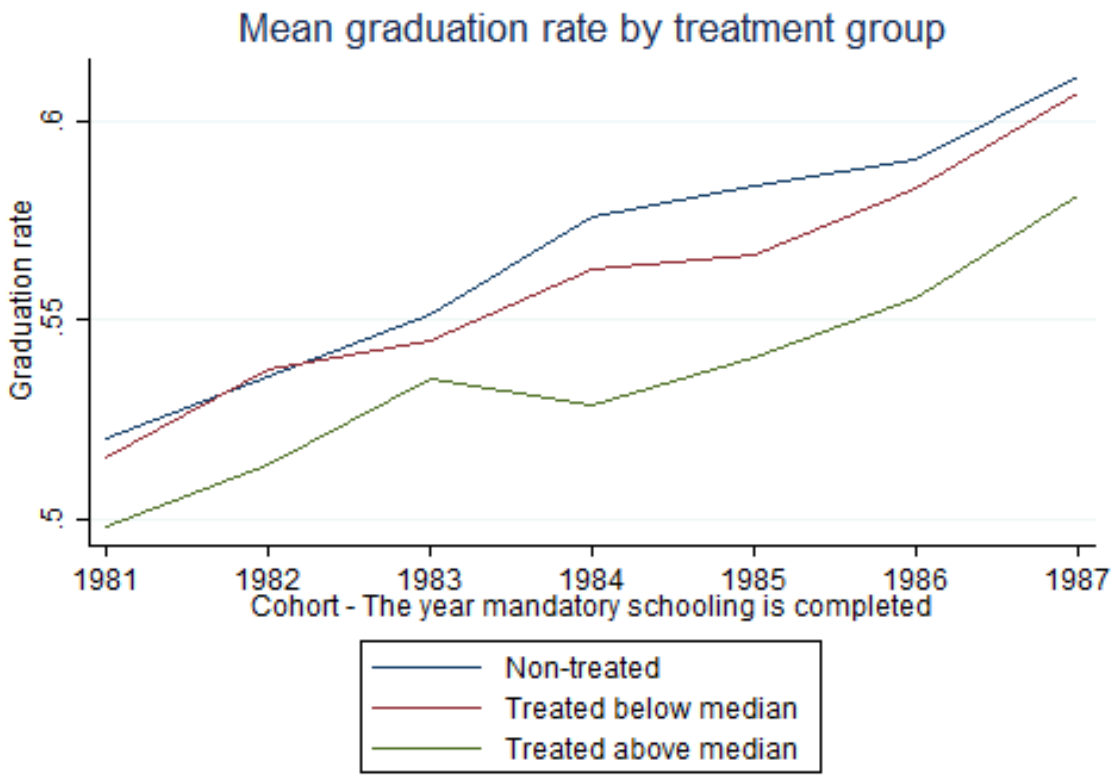
trend in the general youth employment growth in the deregulating municipalities can be ruled out as a likely explanation. Full results corresponding to table 4, and results when only municipalities used in the analysis of educational outcomes are included, are both reported in the appendix.

5. High school completion

5.1 Graphical evidence on graduation rates

Before turning to the main regression results for high school completion, we first present some simple graphical evidence. Figure 3 plots the average graduation rate for the cohorts 1981-1987 for municipalities that were affected by The Opening Hours Act to various degrees. Municipalities are divided into three groups: those where opening hours did not increase, those that were treated by less than the median number of hours per week, and those where that were treated more than median number of hours per week. First note that the graduation rate for the cohorts 1981-1983 follows a very similar trend regardless of treatment group. Comparing the untreated group to municipalities where the opening hours expanded by less than the median number of hours, we see that the in the pre-1984 cohorts students graduated at an almost identical rate. In the municipalities where opening hours increased the most the graduation rate is the lowest, but still follows a remarkably similar trend up until the 1984 cohort. From the 1984 cohort we see that graduation rates drop in the treated municipalities relative to the untreated. Additionally, the reduction in graduation rates are the greatest in the most affected municipalities. For the 1984 cohort the most affected municipalities also saw an absolute reduction in graduation rates of about .7%-points. Although this is purely a graphic representation of descriptive statistics, it tells a compelling story of how the substantial deregulation of opening hours is tightly connected with local graduation rates. The following sections presents a careful statistical analysis of the effects based on the strategy outlined in section 3 above.

Figure 3. Mean graduation rate by treatment group and cohort.



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5.2 Baseline regression results for high school completion

We now present results from regression equations corresponding to different variants of the regression model from equation (2) with high school completion as outcome. To exclude particularly small municipalities from driving the results, we exclude all municipalities that saw less than 30 students of any cohort enrolling in high school[10]. Table 5 shows the results using all students graduating from compulsory school in the cohorts 1981-1987 with the requirement imposed on minimum cohort size. Column (1) shows results when we impose the restrictions of zero treatment effects for the cohorts 1981-1983, and equal treatment

[10] This excludes a total of 156 municipalities and 34832 students from the sample. In table A2b in the appendix we provide estimation results when this qualification is not made. Both qualitatively and quantitatively the estimated effects are very similar to those reported in Table 5.

effects for the cohorts 1984-1987. In all specifications we include the student characteristics, time varying municipality level variables, including the lagged regional unemployment rate with definitions and descriptive statistics shown in the data section above.

To further increase comparability between the treated and non-treated municipalities column (2) includes linear regional time trends using the economic regions defined by Statistics Norway as region definition. In total there are 90 such regions compared to a total of 454 municipalities. Inclusion of linear regional trends accounts for possible unobserved smooth regional 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 (2), increasing allowed opening hours by 1 hour per week leads to a .17% percentage point reduction in the probability to graduate from high school within 5 years after compulsory school. The average treated municipality therefore saw a reduction in the graduation rate of 1.7%-points. This is a substantial effect as the average graduation rate in municipalities affected by the reform was 52.1% in the pre-treatment period. The percentage change in graduation rates evaluated at the average is 3.2%, which is twice the 1.6% (1.2%-points) effect implied by the estimates in Lee (2013)[11]. The reason for this difference is not easily explained, but several possibilities exist. First, the average high school graduation rate in Norway in the period covered by our analysis was low compared to the US (55% vs. 86%), and one possibility is that the effect of deregulation of opening hours in retail firms is higher for low initial graduation rates. Second, the educational systems differ. The number of study hours required for an average student to graduate could vary substantially between the school systems, but data does not allow for further investigation of this issue. Third, in the US, opening hours were changed only on Sundays, while in our case the reform also affected opening hours in weekdays. The effect of removing opening hours restrictions on Sundays is not necessarily the same as the effect of deregulating opening hours on weekdays. Fourth, wage inequality is much lower in Norway than in the US, with a lower wage return to education. This indicates that the net lifetime earnings loss experienced by high school dropouts is relatively lower in Norway than in the US.

[11] According to table 2 p. 290 in Lee (2013), average high school completion in her sample is 86%. No explicit numbers of average completion rates before repeal of blue laws are provided in the paper.

Columns (3)-(4) report results from models with a full set of cohort by treatment interaction effects. In the bottom of each column we report the p-values for two tests used to determine the validity of our specification in the two first columns. First, as a test of the parallel trend assumption, we test whether the treatment effect on the 1982 and 1983 cohorts are jointly indistinguishable from 0. That is, if we are unable to reject the null hypothesis, we are de facto unable to reject the parallel trend assumption for the cohorts 1982 and 1983. Second, we test if the treatment effects are equal for the cohorts leaving compulsory education after 1983. If we are unable to reject the hypothesis of equal interaction coefficients for the post 1983 cohorts, the versions with a single treatment effect for these cohorts as reported in column (1) and (2) is a valid simplification of the more general version of the model. In table 5 we first notice that the restriction of zero interaction effects for the cohorts before 1984 cannot be rejected at conventional significance levels in both specifications. Further, the restriction of equal coefficients in the post-treatment period (post 1983) is formally not rejected by the F-tests. Also note that including the linear regional trends increases the p-value of both tests. There is therefore arguable a stronger case for imposing the restrictions on treatment coefficients when trends are included. Taken together we interpret the results of these tests as supportive of our specification using only a single treatment interaction term.

Looking in more detail on the results in column (3)-(4), we find a negative interaction effect for all cohorts after 1983, although precisely estimated only for the 1984 and the 1985 cohort. The effect is strongest for the 1984 cohort and then it diminishes towards the end of the observation period. Also, even though all students in the cohorts up to, and including, the 1985 cohort were old enough to be legally employed when The Opening Hours Act was passed, yet we do not observe a treatment effect on students in the pre-1984 cohort. In a reduced form environment it is not straightforward to determine why we observe this exact pattern in the treatment effect, but we offer some tentative hypotheses. Note first that cohort here refers to the year students finish compulsory schooling. Students in the 1984 and 1985 cohorts had consequently spent a maximum of respectively 1 and 0 years in high school when the reform was enacted. Assuming that there is no return on high school education without graduation, this means that the students most affected by the reform are the students with the highest alternative costs to schooling.

The lack of any significant effect on the cohorts 1982-1983 could also be a consequence of the fact that we estimate intent to treat effects, i.e. the effect of changes in potential opening hours determined by the regulations since actual opening hours are not available in our data. A certain lag in the increase in shopping hours in the treated municipalities can therefore not be ruled out. Taking these two arguments into account, it is not very surprising that the treatment effect is significant at the 5% level for both the 1984 and 1985 cohorts, and not for earlier cohorts.

The fact that the treatment effect is smaller for the 1986 and 1987 cohorts could also be explained by national labor laws and the age of the students when the 1985 Opening Hours Act was implemented. By construction, students in the 1986 and 1987 cohorts are younger than the minimum required age for legal employment set at 16. If retail stores increased employment promptly when allowed opening hours increased, the labor market could be close to equilibrium when these younger students reach 16 and hence dampening the effect of the deregulation on these cohorts' schooling decisions.

Detailed estimation results are reported in the appendix, table A2. The coefficients for the control variables have expected signs and are in line with previous evidence from Norway. The probability of graduating is increasing in parental education and is higher for females than for males while the effect of immigration status is insignificant. Students' probability of graduating increases when their parents are married and when their parents have a stronger labor market connection. 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 increases the probability to graduate by approximately 1 percentage point. The estimated effect is in the same ballpark as the effect found in Reiling and Strøm (2015).

Table 5. Estimation results. High school graduation. Total sample of students. Complete results reported in the appendix, table A2.

Explanatory variables	(1) With Controls	(2) Regional Trends	(3) With Controls	(4) Regional Trends
Hours treated, Cohort > 1983	-0.000941**	-0.00169***		
	(0.000465)	(0.000516)		
Hours treated x Cohort 1982			7.71e-06	-8.01e-05
			(0.000598)	(0.000632)
Hours treated x Cohort 1983			0.000271	8.12e-05
			(0.000636)	(0.000719)
Hours treated x Cohort 1984			-0.00142*	-0.00173**
			(0.000724)	(0.000797)
Hours treated x Cohort 1985			-0.00139**	-0.00177**
			(0.000665)	(0.000818)
Hours treated x Cohort 1986			-0.000514	-0.000944
			(0.000710)	(0.000869)
Hours treated x Cohort 1987			-0.000192	-0.000652
			(0.000715)	(0.000982)
Constant	0.272***	22.40***	0.282***	20.99***
	(0.0845)	(2.595)	(0.0847)	(2.627)
Observations	323,600	323,600	323,600	323,600
R-squared	0.106	0.106	0.106	0.106
Individual level controls	Yes	Yes	Yes	Yes
Municipality level controls	Yes	Yes	Yes	Yes
Region trend	No	Yes	No	Yes
p-value, F-test of no effect, cohorts 82-83			0.883	0.964
p-value, F-test of same effect, cohorts 84-87			0.107	0.237
# Municipalities	293	293	293	293

Municipality and cohort fixed effects, as well as individual and municipality level controls are included in all specifications. Standard errors clustered on municipality in parentheses. *** p<0.01, ** p<0.05, *** p<0.1

6 Robustness checks and heterogeneity analysis

6.1 Robustness checks

High school graduation conditional on enrollment

So far the outcome variable has been the probability of high school graduation within a 5 year window for the students finishing compulsory school at the normal age. As an alternative outcome we also estimate the model on the subsample of students actually

enrolled into high school the same fall they finished compulsory school. If there is some omitted variable correlated with both locally given opening hours restrictions before The Opening Hours Act and the graduation choice of students the differences in differences estimator can be biased despite the control variables included. As both the choice to enroll and to graduate are schooling decisions and highly correlated they are likely to be correlated relatively similarly with unobserved omitted variables. Looking only at graduation probabilities for students who chose to enroll can be considered as a useful robustness check. Table 6 shows regression results for models similar to those reported in table 5 for this particular sample of students. It is important to note that our sample size drop by roughly 20%, reducing precision in our estimates. Looking first at the results when restrictions are imposed, in columns (1)-(2), we find a negative point estimate in both specifications, though they are significantly different from zero only when controlling for linear regional trends. The estimated effects in the latter specification is very similar to that reported in table 5.

Columns (3)-(4) 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 neither of these specifications, and can be interpreted as evidence supporting our identification strategy. Further, the restriction of equal coefficients in the post-treatment period (cohorts graduating from mandatory schooling after 1983) is not rejected by the F-tests.

The development of the coefficients over the period follows a pattern similar to that found using the total sample. However, the actual size of the interaction by cohort effects are not precisely estimated, at least partly attributable to the reduction in sample size. Considering that conditioning on high school enrollment status significantly reduces the sample size with only small effects on point estimates, we choose not to implement the requirement to enroll in high school as part of our main specification.

For the rest of the paper we will focus on specifications that include regional trends for three main reasons. First, viewing the test results from column (3) and (4) in table 5 and 6 together, imposing the restrictions on treatment effects is more reasonable when regional trends are included. Second, these trends account for possible unobserved smooth

development in regional labor market conditions potentially correlated with the treatment variable and hence including them offers more credible identification. Finally, they also control for potentially changing composition of students within municipalities. In general, we therefore consider specifications where trends are included to provide the most credible results.

Table 6. High school graduation conditional on enrollment. Complete results reported in the appendix, table A3.

Explanatory variables	(1) With Controls	(2) Regional Trends	(3) With Controls	(4) Regional Trends
Hours treated, Cohort > 1983	-0.000792	-0.00166***		
	(0.000547)	(0.000622)		
Hours treated x Cohort 1982			0.000565	0.000442
			(0.000703)	(0.000737)
Hours treated x Cohort 1983			0.00128	0.00101
			(0.000876)	(0.000991)
Hours treated x Cohort 1984			-0.000144	-0.000563
			(0.000843)	(0.000968)
Hours treated x Cohort 1985			-0.00100	-0.00153
			(0.000754)	(0.000949)
Hours treated x Cohort 1986			-0.000124	-0.000708
			(0.000739)	(0.00102)
Hours treated x Cohort 1987			0.000557	-6.35e-05
			(0.000869)	(0.00115)
Constant	0.408***	33.43***	0.417***	31.73***
	(0.0801)	(3.094)	(0.0811)	(3.197)
Observations	260,339	260,339	260,339	260,339
R-squared	0.080	0.081	0.080	0.081
Region trend	No	Yes	No	Yes
p-value, F-test of no effect, cohorts 82-83			0.345	0.595
p-value, F-test of same effect, cohorts 84-87			0.134	0.133
# Municipalities	293	293	293	293

Municipality and cohort fixed effects, as well as individual and municipality level controls are included in all specifications. Standard errors clustered on municipality in parentheses. *** p<0.01, ** p<0.05, *** p<0.1

Conventional difference-in-difference estimator.

As robustness check, we now present results from models using alternative representations of the treatment variable. So far the treatment variable has been defined as the number of hours each week opening hours expanded. A relevant alternative is to use a more conventional difference-in-difference strategy. That is, we define the municipalities that are in any way affected by the reform as treated, and all municipalities where the 1985 Opening Hours Act had no effect on regulations as untreated. As opposed to the linear estimates above, this specification has the advantage that it does not impose any functional form of the treatment effect. In our case there are several reasons why we would not expect the treatment effect to be linear. First, firms might prefer to employ older and more experienced workers rather than high school students. If so, small increases in allowed opening hours might not increase the number of job offerings for students. Second, firms might accommodate small increases in allowed opening hours by moving non-student workers from part time to full time. Third, even if the youth employment effect is linear, students might not choose to drop out of high school unless they are offered an amount of work above some threshold. In this case we could observe a linear employment effect, and a non-linear effect on graduation. Taking these factors into consideration we now report results when the treatment is defined as a dichotomous variable.

Including regional trends, column (2) reports an estimated 1.5% reduction in the graduation probability for students in the cohorts after 1983. Recall that the estimated linear effect evaluated at the average change in opening hours was only 0.2%-points higher (1.7%-points). The small discrepancy between these point estimates shows that even though the linear specification restricts the functional form, it captures the average effect very well. In the bottom part of columns (3) and (4) we again report the p-values of our tests on the restrictions imposed in column (1) and (2). Again we are unable to reject either of the two restrictions. Note that the estimated effect for the 1984 and 1985 cohorts are stronger here than in the analogous results from table 5. One explanation for this could be that there are some non-linarites in the treatment effect due to the factors discussed above. Non-linarites are also in line with the graphical evidence showing a decline in absolute graduation rates in

the most affected municipalities. In light of these findings we now turn to estimates where we estimate separate treatment coefficients for municipalities depending on the bite of the reform.

Table 7. High school graduation, dichotomous treatment variable. Total sample. Complete results are reported in the appendix, table A4.

Explanatory variables	(1) With Controls	(2) Region Trends	(3) With Controls	(4) Region Trends
Affected x Cohort > 1983	-0.0131*	-0.0152**		
	(0.00712)	(0.00666)		
Affected x Cohort 1982			-0.00325	-0.00280
			(0.00917)	(0.00958)
Affected x Cohort 1983			-0.00616	-0.00560
			(0.0101)	(0.0109)
Affected x Cohort 1984			-0.0206*	-0.0199*
			(0.0105)	(0.0112)
Affected x Cohort 1985			-0.0245**	-0.0232**
			(0.0103)	(0.0113)
Affected x Cohort 1986			-0.0129	-0.0110
			(0.0117)	(0.0115)
Affected x Cohort 1987			-0.00893	-0.00625
			(0.0107)	(0.0125)
Constant	0.282***	19.99***	0.282***	19.37***
	(0.0801)	(2.368)	(0.0822)	(2.301)
Observations	323,600	323,600	323,600	323,600
R-squared	0.106	0.106	0.106	0.106
Individual level controls	Yes	Yes	Yes	Yes
Municipality level controls	Yes	Yes	Yes	Yes
Region trends	No	Yes	No	Yes
# Municipalities	293	293	293	293
p-value of equality 1982-1983			0.832	0.876
p-value of equality 1984-1987			0.317	0.233

Municipality and cohort fixed effects included in all specifications. Standard errors clustered on municipality in parentheses. *** p<0.01, ** p<0.05, *** p<0.1

Alternative treatment indicators

As a further robustness check, we redefine our treatment variable in the previous section in order to estimate separate effects for students living in more and less affected municipalities. We do this by separating the affected municipalities in two groups; those where allowed opening hours expanded by more than the median, and those where they expanded by less than the median. We then have two treatment groups, while the control

group consists of all unaffected municipalities. Table 8 reports the results. As evident from columns (3) and (4) the effects are strongest for the 1984 and 1985 cohorts regardless of treatment intensity. This is in line with our baseline estimations. However, the treatment effects are only significant for the most affected students. While smaller expansions in opening hours are still associated with some reduction in graduation rates, estimates are lower than in table 7, and mostly insignificant. For the most affected students the estimated effect are very large. Although we would expect to see a larger effect for the students living in the most affected municipalities even if the treatment effect was linear, the observed difference is too large to be explained by a purely linear relationship. The reasons for relatively large differences in the treatment effects are not easily identified are outside the scope of this paper, but could include one or more of the factors discussed above.

As indicated by the F-tests in column (3) and (4), the restrictions imposing zero treatment effect for the pre-1984-cohorts and equal treatment effects for the 1984-1987 cohorts are not rejected. Columns (1) and (2) report results when these restrictions are imposed. Finding that the effect of deregulation increases with the treatment intensity strengthens our belief in the causality of the results. If the effect was similar regardless of treatment intensity, or even reversed, would suggest that other omitted factors could cause a spurious relationship.

While the results in table 8 suggests that the treatment is non-linear we will proceed by using the more parsimonious linear treatment as our baseline, which will allow us to evaluate a general treatment effect on the entire sample.

Table 8. High school graduation, multiple treatment levels, total sample. Complete results are reported in the appendix, table A6.

Explanatory variables	(1) With Controls	(2) Region Trends	(3) With Controls	(4) Region Trends
Below median x Cohort > 1983	-0.00876	-0.00954		
	(0.00753)	(0.00685)		
Above median x Cohort > 1983	-0.0206***	-0.0314***		
	(0.00793)	(0.00796)		
Below median x Cohort 1982			0.00196	0.00239
			(0.0103)	(0.0106)
Below median x Cohort 1983			-0.00797	-0.00723
			(0.0116)	(0.0121)
Below median x Cohort 1984			-0.0123	-0.0113
			(0.0116)	(0.0117)
Below median x Cohort 1985			-0.0188*	-0.0173
			(0.0112)	(0.0118)
Below median x Cohort 1986			-0.00744	-0.00545
			(0.0128)	(0.0118)
Below median x Cohort 1987			-0.00573	-0.00321
			(0.0117)	(0.0129)
Above median x Cohort 1982			-0.0120	-0.0136
			(0.00979)	(0.0103)
Above median x Cohort 1983			-0.00368	-0.00759
			(0.0101)	(0.0112)
Above median x Cohort 1984			-0.0347***	-0.0407***
			(0.0112)	(0.0124)
Above median x Cohort 1985			-0.0344***	-0.0417***
			(0.0111)	(0.0126)
Above median x Cohort 1986			-0.0222*	-0.0308**
			(0.0122)	(0.0135)
Above median x Cohort 1987			-0.0142	-0.0237
			(0.0117)	(0.0146)
Constant	0.269***	26.48***	0.276***	24.73***
	(0.0833)	(3.142)	(0.0833)	(3.310)
Observations	323,600	323,600	323,600	323,600
R-squared	0.106	0.106	0.106	0.106
Individual controls	Yes	Yes	Yes	Yes
Region trends	No	Yes	No	Yes
# Municipalities	293	293	293	293
p-value, F-test of no effect centile 0-50, cohorts 82-83			0.587	0.604
p-value, F-test of same effect for centile 0-50, cohorts 84-87			0.531	0.444
p-value, F-test of no effect centile 50-100, cohorts 82-83			0.444	0.413
p-value, F-test of same effect for centile 50-100, cohorts 84-87			0.158	0.315

Municipality and cohort fixed effects, as well as individual and municipality controls are included in all specifications. Standard errors clustered on municipality in parentheses. *** p<0.01, ** p<0.05, *** p<0.1

Excluding municipalities without regulations from the control group

When data on opening hours regulations were collected in 1982 a minority of the municipalities reported that they had no regulations on opening hours. 50 of these municipalities were very sparsely populated at the time, and are dropped from our sample due to the restrictions on cohort size. In our baseline estimations we include the remaining 20 in our control group as The Opening Hours Act did not directly affect regulations in this group. However, as we do not observe actual restrictions on opening hours after 1985 we cannot rule out any normative effect of The Opening Hours Act. Some anecdotal evidence suggests at least a few of these municipalities chose to implement restrictions on opening hours as a results of the new laws. If this effect is relevant, the restrictions imposed by the 1985 reform could lead to lower employment, and hence higher observed graduation rates in these municipalities. In order to test whether these municipalities are driving our results we discard them from our sample and re-run our baseline model. Results are reported in table 9. The coefficients of interest appears to be slightly larger than in our baseline results, providing some evidence for normative effects. The sample is slightly smaller leading to a somewhat lower degree of precision. We conclude that any normative effects are fairly small in our data, and continue to include municipalities without restrictions on opening hours in the rest of the paper.

Table 9. High school graduation. Excluding municipalities without regulations in 1982. Full results reported in appendix, table A7.

Explanatory variables	(1) With Controls	(2) Regional Trends	(3) With Controls	(4) Regional Trends
Hours treated, Cohort > 1983	-0.000875*	-0.00172***		
	(0.000475)	(0.000538)		
Hours treated x Cohort 1982			0.000161	8.08e-05
			(0.000612)	(0.000653)
Hours treated x Cohort 1983			0.000429	0.000263
			(0.000650)	(0.000745)
Hours treated x Cohort 1984			-0.00140*	-0.00168**
			(0.000737)	(0.000824)
Hours treated x Cohort 1985			-0.00119*	-0.00152*
			(0.000677)	(0.000858)
Hours treated x Cohort 1986			-0.000253	-0.000597
			(0.000727)	(0.000913)
Hours treated x Cohort 1987			2.05e-06	-0.000315
			(0.000730)	(0.00104)
Constant	0.260***	22.80***	0.273***	20.90***
	(0.0839)	(2.607)	(0.0841)	(2.660)
controls	yes	yes	yes	yes
Regional trends	no	yes	no	yes
Observations	316,024	316,024	316,024	316,024
R-squared	0.106	0.106	0.106	0.106
Region trend	No	Yes	No	Yes
p-value, F-test of no effect, cohorts 82-83			0.800	0.933
p-value, F-test of same effect, cohorts 84-87			0.0766	0.145
# Municipalities	273	273	273	273

Municipality and cohort fixed effects, as well as individual and municipality controls are included in all specifications. Standard errors clustered on municipality in parentheses. *** p<0.01, ** p<0.05, *** p<0.1

6.2 Heterogeneous effects

Above we have established that extending potential shopping hours decreased high school graduation probability using a series of different specifications. We next investigate possible heterogeneous effects between students in two dimensions: parental education, and gender. Parental education is one of the most powerful predictors for a range of student performance measures including graduation probability, see Falch and Strøm (2013), and Reiling and Strøm (2015). In line with previous research, the baseline models reported above

show that females and students with higher educated parents are more likely to graduate from high school.

To investigate if the effect of extending potential opening hours vary with socio-economic background, we re-estimate our baseline model including interaction terms between the treatment indicator and the indicators of parental education level. The reference category is students whose parents have only completed compulsory schooling. The results are reported in column (1) in table 10. As can be seen from the results, students in the reference category are strongly affected by the treatment with an estimated interaction coefficient equal to -0.00224. For this group of students, increasing weekly opening hours by 10 hours reduced the probability to graduate high school within 5 years by 2.2%-points. This effect is more than 30% stronger than the average effect estimated in table 5. For students with the highest educated parents the interaction coefficient is 0.00185, and implies that this group of students was almost unaffected by the treatment. Thus, students whose parents have low education are both less likely to graduate high school in general, and more likely to experience a reduction in graduation probability when potential opening hours in retail firms increased.

In column 2 we report the results from a model where treatment effects are allowed to differ between genders. The probability to graduate from high school is in general higher for girls than for boys and according to table 1 on average 57% of girls and 54% of boys graduate high school. Combined with evidence from many countries, including Norway, that girls outperform boys in terms of academic achievement, one could expect boys to be more responsive to treatment than girls. Alternatively, the jobs created in the retail sector could be gender biased in the sense that they are relatively more appealing to female students, inducing a gender biased treatment effect in the opposite direction. The results reported in column 2 find a small negative, but insignificant gender interaction effect and suggests that the treatment effect on high school graduation are not significantly different between boys and girls.

Table 10. Heterogeneous effects by parental education (column (1)) and gender (column (2)). Complete results reported in appendix, table A8.

	(1) Graduation Background interactions	(2) Graduation Gender interaction
Hours treated x Cohort > 1983	-0.00224***	-0.00167***
	(0.000543)	(0.000556)
Hours treated Cohort > 1983 x Parents completed high school	0.000521	
	(0.000390)	
Hours treated Cohort > 1983 x Parents completed short high. educ.	0.000988	
	(0.000620)	
Hours treated Cohort > 1983 x Parents completed long high. educ.	0.00185**	
	(0.000774)	
Hours treated Cohort > 1983 x Female		-3.87e-05
		(0.000432)
Constant	22.73***	22.39***
	(2.593)	(2.596)
Regional trends	yes	yes
Observations	323,600	323,600
R-squared	0.106	0.106
# Municipalities	293	293

Municipality and cohort fixed effects, as well as individual and municipality controls are included in all specifications. Standard errors clustered on municipality in parentheses. *** p<0.01, ** p<0.05, *** p<0.1

7 Longer run outcomes: Years of schooling and earnings

Above, we used graduation from high school within five years after completing mandatory schooling as our education outcome. One could argue that this outcome measures the effect on educational choices in the short run, not necessarily carrying information on the long run educational choices of the students. Looking at longer time horizons students might re-evaluate their returns from schooling and make further education investments at a later point in time. To supplement our findings we therefore use a long run measure of education as an outcome variable: completed years of education as measured at age 40. Building on the heterogeneity found in the previous section we also estimate treatment effects allowed to differ between students with different parental education. Based on the results for high school graduation, we expect that the students with weaker family backgrounds are most affected by treatment in terms of completed education.

Another relevant long run measure is earnings. The positive relationship between earnings and education is well established although the estimated returns to education varies between time periods and countries, with returns typically much smaller in Scandinavian countries than in the UK and US, see Trostel et al (2002). Lee (2013) find that repeal of "blue laws" in US states led to a significant 1.2% reduction in annual wages in adulthood. Estimating the earnings effect of treatment allows us to gauge the potential net income foregone by the students not graduating high school within the 5 year window due to the lifting of opening hours regulations. Moreover we can investigate whether this income effect differ by students parental background. Theoretically, the effect on earnings is not obvious. The education effect suggest a negative impact on earnings. However, at least for some students, working while in school may actually increase future labor market performance by learning productivity enhancing skills and lower the costs of finding an efficient job match.[12]

The earnings measure used is pension-qualifying earnings reported in the tax registry, including labor earnings, sick benefits, unemployment benefits and parental leave payments. Data on earnings are available until the year 2010, which makes it possible to track earnings until the youngest cohort in the sample is aged 39. In our analysis we could therefore use either or both earnings measured at the same age or in the same year as the outcome variable. Havnes and Mogstad (2014) point out that these two measures relies on different parallel trend assumptions. When earnings are measured in the same year the parallel trend assumption would not hold if the students in the treated municipalities would have experienced an earnings profile distinct from that of the students in the non-treated municipalities in the absence of a reform. When earnings are measured at the same age on the other hand, the parallel trend assumption would not hold if the treated and non-treated municipalities had dissimilar labor market developments.[13] Being unable to directly test the parallel trend assumption in either case we include both measures in our analysis.

[12] An extensive literature exists using US data to estimate the effect of work while in school on academic achievement and labor market performance, but results are not conclusive. While Ruhm (1997) find positive effects on earnings later in life, Stinebrickner and Stinebrickner (2003) report negative effects on academic achievement in college and Rothstein (2007) find small or zero effects on high school GPA. Parent (2006), using Canadian data look at the impact of work while in highs school on later labor market performance and find small or zero effects.

[13] See Havnes and Mogstad (2014) for a more detailed discussion

The estimation results for years of education and earnings are reported in table 11. Column (1) and (2) reports the treatment effects on years of education where column (2) allows treatment effects to vary with parental education.

Table 11 Estimation results. Completed years of education and log(earnings). Complete results are reported in table A9.

Explanatory variables	(1) Years of education	(2) Years of education	(3) log(Wage aged 30)	(4) log(Wage aged 30)	(5) log(Wage aged 39)	(6) log(Wage 2008-10)
Hours treated x cohort > 1983	-0.00250	-0.00948***	-0.000697	-0.00207**	-0.000618	-3.72e-05
	(0.00172)	(0.00246)	(0.000795)	(0.000978)	(0.000690)	(0.000690)
Hours treated x cohort > 1983 x Par.ed. = high school		0.00675***		0.00149**	0.00122**	0.000832*
		(0.00159)		(0.000584)	(0.000528)	(0.000487)
Hours treated x cohort > 1983 x Par.ed. = short high. educ.		0.00649**		0.00215***	0.000503	-9.64e-05
		(0.00286)		(0.000745)	(0.000710)	(0.000626)
Hours treated x cohort > 1983 x Par.ed. = long high. educ.		0.0111**		0.00340***	0.000952	0.000854
		(0.00427)		(0.00130)	(0.000924)	(0.000948)
Constant	12.09***	37.23***	-8.600**	-7.957**	39.37***	22.82***
	(0.328)	(9.809)	(3.728)	(3.789)	(2.711)	(2.582)
Region trends	yes	yes	yes	yes	yes	yes
Observations	323,600	323,600	294,536	294,536	294,536	294,536
R-squared	0.179	0.179	0.116	0.116	0.142	0.117
# Municipalities	293	293	293	293	293	293

Municipality and cohort fixed effects, as well as individual and municipality controls are included in all specifications. Standard errors clustered on municipality in parentheses. *** p<0.01, ** p<0.05, *** p<0.1

As can be seen from column (1), the average effect on the number of years of education is negative, but numerically small and not significantly different from zero. However, this average effect may hide important heterogeneity as suggested by the high school graduation results above. Moving on to column (2) we see that the treatment effect for students with parents with only mandatory schooling is -0.001 and significantly different from zero. A 10 hours increase in weekly opening hours implies a reduction in years of education of about 0.1 years. Since Lee (2013) does not provide any evidence on heterogeneous effects by parental background it is difficult to compare her estimated effects with that obtained here. However, we notice that our estimated effect on years of education for the group with low

educated parents is close to the range of estimated average effects reported in that paper (-0.1 compared to -0.15).

If students with parents with higher education have more inherent motivation for education in the first place it may explain the absence of treatment effects on high school graduation and completed years of education for that group of students. Similarly the rather strong negative treatment effect for students from families with low educated parents is consistent with the view that changes in job market opportunities is most important for those students most likely to be on the margin of early school leaving and choosing the labor market alternative.

The remainder of Table 11 reports results for different adult earnings measures. In column (3) and (4) earnings are measured at the age of 30. Here we see the same pattern as for years of education. While the average treatment effect reported in column (3) is negative, but small and statistically insignificant, allowing for heterogeneous treatment effects in column (4), reveal that a 10 hours increase in allowed opening hours reduce earnings around 2% at age 30 for students with less educated parents. This can be compared to Lee (2013) finding an average 1% reduction in annual earnings following repeal of blue laws in the US. Thus, while the average effect is ignorable in our case, the numerical effect for the group of students with low educated parents is roughly twice the average effect reported by Lee. On the other hand the point estimates in column (4) in table 11 implies that a 10 hours increase in weekly opening hours lead to a 1.3% *gain* in earnings for students with highly educated parents. Viewing this result in combination with the absence of effect on high school graduation and years of education for this group of students it may suggest that the effect of job opportunities and working while studying on later labor market performance is highly dependent on family background and parents education. Unfortunately, our data and reduced form approach does not allow for more extensive investigation of the link between early labor market experience when attending school and later earnings.

Moving on to earnings measured at later life stages, column (5) reports the estimation results when earnings are measured at age 39. The pattern is roughly similar to the one found in column (3), but effects are closer to zero and less precisely estimated. The absence of a significant treatment effect for this earnings measure could have several explanations.

One important factor is the high degree of earnings equality in the Nordic countries, including Norway, which decreases the variation in observed earnings directly. Another potential explanation concerns the importance of high school graduation relative to labor market experience in the determination of earnings. A plausible interpretation of the results in this paper is that students living in a treated municipality choose to work in retail and thereby increase their labor market experience at the cost of reduced education relative to the students living in a non-treated municipality. Consequently, if the importance of high school graduation relative to labor market experience decrease over the life cycle we would expect to see a smaller effect on earnings for all individuals when aged 39 than 30.

The last column in table 11 reports the results when the earnings measure used is average earnings in the year 2009-2010. Again the results are too imprecise to reach significance at conventional levels, although the pattern is similar to the one in columns (4) and (5). Overall the effect on earnings seem to weaken over time, possibly because the increased labor market experience of students living in treated municipalities largely compensates for a reduced number of years of education.

Note that both when the outcome is wages measured at age 39 and when we use the average wage in the years 2008-2010 we find a positive and significant effect on students with high school educated parents. This is somewhat surprising given that we do not find any significant effect for the rest of the students, however, it could be a result of the changing composition of high school graduation among students. When fewer students graduate high school, the competition for jobs among individuals with only high school education could diminish, leading to an increase in wages.

8. Conclusion

Previous evidence has shown that deregulation of shop opening hours in developed countries has increased employment in the retail industry. As the retail industry to a large extent uses low skilled and young employees, the opportunity cost of education likely falls as shop opening hours increase. A national reform in Norway in 1985 affected potential shop opening hours differently across geographical areas. This paper uses this reform to provide quasi-experimental evidence on the effect of deregulation of opening hours on employment of young workers in the retail industry. We further provide evidence on the effect of this

deregulation on education outcomes and later earnings using register data on high school graduation, completed years of education and earnings for cohorts finishing compulsory education in 1981-1987.

Our evidence suggest that the increase in potential opening hours on average increased employment of workers aged 16-24 years in retail industry by 12%. Further, it induced a decrease in high school graduation rates. The quantitative effect on is sizeable. Our main results suggest that students in areas increasing potential shop opening hours had on average 1.6%-points lower high school graduation probability than comparable students in other areas. The results are qualitatively similar to that found by Lee (2013) using the removal of "Blue laws" in US as a natural experiment. Analysis of treatment effect heterogeneity show that most of the average negative effect on high school graduation can be attributed to students with low educated parents, while students with high educated parents are mainly unaffected. We find no difference in the treatment effect by gender. Further, investigation of the effect on completed years of education also reveal the same strong heterogeneity pattern with average effects being small and statistically insignificant for these outcomes. For earnings we find a statistically significant negative effect at age 30 for students with low educated parents, but this effect seems more or less to have disappeared by the time individuals reach the age of 39.

Although our results are to be interpreted as reduced form effects (Intent to treat effects) the effects on youth employment, educational attainment and earnings combined suggest that increased job opportunities for young and unskilled workers can have negative effects on acquired education and earnings for students from families with low educated parents.

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Appendix

Table A1. Estimated employment equations, full results corresponding to table 4.

VARIABLES	(1) Log(Employment retail) Age 16-24 1980-1990	(2) Log(Employment retail) Age 16-24 1970-1980	(3) Log(Employment industry) Age 16-24 1980-1990	(4) Log(Employment industry) Age 16-24 1970-1980
Year 1990	0.0705		0.167	
	(0.123)		(0.147)	
Hours treated x 1990	0.0116***		-0.000750	
	(0.00359)		(0.00514)	
Year 1980		0.492***		0.0563
		(0.147)		(0.157)
Hours treated x 1980		-0.00358		-0.00381
		(0.00373)		(0.00605)
Population	3.72e-05***	1.21e-05**	-6.54e-05***	2.29e-05
	(1.24e-05)	(6.05e-06)	(1.81e-05)	(1.63e-05)
ln(Share aged 0-6)	0.367	0.706**	0.572**	-0.117
	(0.230)	(0.295)	(0.239)	(0.315)
ln(Share aged 7-15)	0.843***	1.004***	1.218***	-0.328
	(0.209)	(0.373)	(0.274)	(0.458)
ln(Share aged 16-20)	0.747***	0.422*	0.881***	0.302
	(0.214)	(0.233)	(0.295)	(0.389)
ln(Share aged 21-25)	0.610***	0.787***	0.206	1.399***
	(0.212)	(0.255)	(0.215)	(0.301)
ln(Share aged 26-30)	0.190	0.213	0.300	-0.463*
	(0.207)	(0.223)	(0.277)	(0.267)
ln(Share aged 31-35)	-0.406*	0.358	0.0919	0.530*
	(0.228)	(0.235)	(0.282)	(0.280)
ln(Share aged 36-40)	0.413*	0.494**	-0.333	0.180
	(0.238)	(0.250)	(0.259)	(0.303)
ln(Share aged 41-45)	-0.246	0.168	0.229	0.0907
	(0.224)	(0.236)	(0.254)	(0.308)
ln(Share aged 46-50)	0.411*	0.842***	-0.0865	-0.0221
	(0.225)	(0.203)	(0.274)	(0.315)
ln(Share aged 51-55)	0.0589	0.209	0.478**	-0.436*
	(0.204)	(0.231)	(0.214)	(0.242)
ln(Share aged 56-60)	0.298	0.306	0.117	-0.0548
	(0.190)	(0.228)	(0.222)	(0.296)
Leftist mayor	-0.0690*	-0.00603	0.0373	-0.0439
	(0.0380)	(0.0425)	(0.0493)	(0.0636)
Constant	11.62***	17.51***	13.09***	6.814
	(2.800)	(4.491)	(3.206)	(5.395)
Observations	594	594	544	544
R-squared	0.984	0.980	0.970	0.966
# Municipalities	297	297	272	272

Municipality fixed effects included in all regression. Robust standard errors in parentheses, *** p<0.001, ** p<0.05, * p<0.1

Table A1B. Estimated employment equations, using only municipalities included in the analysis of educational outcomes.

	(1) log(Employment retail)	(2) log(Employment retail)	(3) log(Employment industry)	(4) log(Employment industry)
Explanatory variables	1980-1990	1970-1980	1980-1990	1970-1980
Year 1990	0.334**		0.00529	
	(0.136)		(0.200)	
Hours treated x 1990	0.00774**		-0.00665	
	(0.00376)		(0.00664)	
Year 1980		0.473***		-0.0407
		(0.130)		(0.173)
Hours treated x 1980		0.00352		-0.00417
		(0.00392)		(0.00627)
Population	1.94e-05	1.14e-05**	-5.21e-05***	2.13e-05
	(1.18e-05)	(4.92e-06)	(1.98e-05)	(1.90e-05)
ln(Share aged 0-6)	0.811***	0.355	-0.292	-0.238
	(0.262)	(0.304)	(0.393)	(0.421)
ln(Share aged 7-15)	1.224***	0.592	0.949**	-0.308
	(0.268)	(0.406)	(0.415)	(0.643)
ln(Share aged 16-20)	0.943***	0.130	1.081***	0.644*
	(0.243)	(0.245)	(0.382)	(0.370)
ln(Share aged 21-25)	0.578**	0.479**	0.148	1.320***
	(0.236)	(0.218)	(0.282)	(0.467)
ln(Share aged 26-30)	-0.0622	0.244	0.663*	0.548*
	(0.246)	(0.240)	(0.378)	(0.310)
ln(Share aged 31-35)	-0.587*	0.296	0.380	0.579
	(0.301)	(0.251)	(0.408)	(0.438)
ln(Share aged 36-40)	0.0903	0.417	-0.145	0.428
	(0.256)	(0.255)	(0.405)	(0.307)
ln(Share aged 41-45)	-0.0997	0.229	0.251	0.678
	(0.257)	(0.251)	(0.341)	(0.426)
ln(Share aged 46-50)	-0.0859	1.094***	0.0981	-0.109
	(0.242)	(0.264)	(0.370)	(0.355)
ln(Share aged 51-55)	0.163	0.000736	0.241	-0.0806
	(0.179)	(0.232)	(0.274)	(0.352)
ln(Share aged 56-60)	0.403*	0.143	0.355	0.327
	(0.233)	(0.212)	(0.292)	(0.370)
Leftist mayor	-0.0505	0.00363	0.0580	-0.167**
	(0.0429)	(0.0455)	(0.0535)	(0.0761)
Constant	12.02***	14.48***	14.75***	15.22**
	(2.945)	(3.828)	(4.697)	(7.105)
Observations	302	302	256	256
# Municipalities	151	151	128	128
R-squared	0.991	0.991	0.976	0.976

Municipality fixed effects included in all regression. Robust standard errors in parentheses, *** p<0.001, ** p<0.05, * p<0.1

Table A2. Estimation results. High school graduation. Students graduating from mandatory schooling at the normal age. Complete results corresponding to table 5.

Explanatory variables	(1) With Controls	(2) Regional Trends	(3) With Controls	(4) Regional Trends
Hours treated, Cohort > 1983	-0.000941**	-0.00169***		
	(0.000465)	(0.000516)		
Hours treated x Cohort 1982			7.71e-06	-8.01e-05
			(0.000598)	(0.000632)
Hours treated x Cohort 1983			0.000271	8.12e-05
			(0.000636)	(0.000719)
Hours treated x Cohort 1984			-0.00142*	-0.00173**
			(0.000724)	(0.000797)
Hours treated x Cohort 1985			-0.00139**	-0.00177**
			(0.000665)	(0.000818)
Hours treated x Cohort 1986			-0.000514	-0.000944
			(0.000710)	(0.000869)
Hours treated x Cohort 1987			-0.000192	-0.000652
			(0.000715)	(0.000982)
Min. 1 parent compl. h.s.	0.191***	0.191***	0.191***	0.191***
	(0.00314)	(0.00314)	(0.00314)	(0.00314)
Min. 1 parent compl. short high. edu.	0.387***	0.387***	0.387***	0.387***
	(0.00481)	(0.00482)	(0.00482)	(0.00482)
Min. 1 parent compl. long high. edu.	0.468***	0.468***	0.468***	0.468***
	(0.00630)	(0.00629)	(0.00630)	(0.00629)
Female	0.0456***	0.0454***	0.0456***	0.0455***
	(0.00317)	(0.00317)	(0.00317)	(0.00317)
Share of pop. between 16 and 20	0.929*	0.328	0.882*	0.385
	(0.510)	(0.476)	(0.505)	(0.477)
Share of pop. above 60	-0.414	-0.748*	-0.456	-0.724*
	(0.362)	(0.426)	(0.361)	(0.425)
Leftist Mayor	0.00703	0.0125**	0.00706	0.0129**
	(0.00578)	(0.00586)	(0.00577)	(0.00591)
First generation immigrant	-0.0160	-0.0145	-0.0160	-0.0145
	(0.0272)	(0.0271)	(0.0272)	(0.0271)
Second generation immigrant	0.0144	0.0151	0.0144	0.0151
	(0.0223)	(0.0223)	(0.0223)	(0.0223)
Regional unemp. previous year	0.938	1.179*	1.084*	1.386**
	(0.610)	(0.621)	(0.623)	(0.639)
Parents married	0.0727***	0.0728***	0.0727***	0.0728***
	(0.00274)	(0.00274)	(0.00273)	(0.00274)
Parents divorced	-0.232***	-0.232***	-0.232***	-0.232***
	(0.00468)	(0.00469)	(0.00469)	(0.00469)
Exactly 1 parent working	0.00500**	0.00496**	0.00498**	0.00495**
	(0.00228)	(0.00229)	(0.00228)	(0.00228)
Both parents working	0.0424***	0.0421***	0.0424***	0.0421***
	(0.00332)	(0.00331)	(0.00332)	(0.00331)
Constant	0.272***	22.40***	0.282***	20.99***
	(0.0845)	(2.595)	(0.0847)	(2.627)
Observations	323,600	323,600	323,600	323,600
R-squared	0.106	0.106	0.106	0.106
Region trend	No	Yes	No	Yes
p-value, F-test of no effect, cohorts 82-83			0.883	0.964
p-value, F-test of same effect, cohorts 84-87			0.107	0.237
# Municipalities	293	293	293	293

Municipality and cohort fixed effects, as well as individual and municipality level controls are included in all specifications. Standard errors clustered on municipality in parentheses. *** p<0.01, ** p<0.05, *** p<0.1

Table A2b. Estimation results. High school graduation. Students graduating from mandatory schooling at the normal age. No requirement on cohort sizes.

Explanatory variables	(1) With Controls	(2) Regional Trends	(3) With Controls	(4) Regional Trends
Hours treated, cohort > 1983	-0.00102**	-0.00142***		
	(0.000467)	(0.000474)		
Hours treated x Cohort 1982			-0.000193	-0.000197
			(0.000582)	(0.000606)
Hours treated x Cohort 1983			0.000237	0.000199
			(0.000610)	(0.000675)
Hours treated x Cohort 1984			-0.00156**	-0.00161**
			(0.000690)	(0.000731)
Hours treated x Cohort 1985			-0.00133**	-0.00138*
			(0.000653)	(0.000746)
Hours treated x Cohort 1986			-0.000774	-0.000831
			(0.000688)	(0.000770)
Hours treated x Cohort 1987			-0.000436	-0.000474
			(0.000713)	(0.000863)
Min. 1 parent compl. h.s.	0.188***	0.187***	0.188***	0.187***
	(0.00293)	(0.00293)	(0.00293)	(0.00293)
Min. 1 parent compl. short high. edu.	0.385***	0.385***	0.385***	0.385***
	(0.00445)	(0.00447)	(0.00446)	(0.00447)
Min. 1 parent compl. long high. edu.	0.466***	0.466***	0.466***	0.466***
	(0.00603)	(0.00602)	(0.00603)	(0.00602)
Female	0.0489***	0.0489***	0.0490***	0.0489***
	(0.00308)	(0.00308)	(0.00308)	(0.00308)
Share of pop. between 16 and 20	0.442	-0.0758	0.406	-0.0588
	(0.470)	(0.399)	(0.466)	(0.399)
Share of pop. above 60	-0.480	-0.890**	-0.506	-0.871**
	(0.351)	(0.355)	(0.350)	(0.354)
Leftist mayor	0.00619	0.00811	0.00623	0.00836
	(0.00549)	(0.00551)	(0.00549)	(0.00555)
First generation immigrant	-0.0180	-0.0160	-0.0180	-0.0161
	(0.0265)	(0.0262)	(0.0265)	(0.0262)
Second generation immigrant	0.0103	0.0110	0.0103	0.0110
	(0.0226)	(0.0226)	(0.0226)	(0.0226)
Regional unemp. previous year	1.069*	1.186**	1.165*	1.344**
	(0.610)	(0.578)	(0.623)	(0.591)
Parents married	0.0701***	0.0701***	0.0701***	0.0701***
	(0.00277)	(0.00278)	(0.00277)	(0.00277)
Parents divorced	-0.228***	-0.228***	-0.228***	-0.228***
	(0.00467)	(0.00467)	(0.00467)	(0.00468)
Exactly 1 parent working	0.00550**	0.00547**	0.00548**	0.00546**
	(0.00216)	(0.00216)	(0.00216)	(0.00216)
Both parents working	0.0430***	0.0427***	0.0430***	0.0427***
	(0.00313)	(0.00311)	(0.00313)	(0.00311)
Constant	0.321***	21.32***	0.328***	19.93***
	(0.0831)	(2.312)	(0.0831)	(2.334)
Observations	353,036	353,036	353,036	353,036
R-squared	0.107	0.108	0.107	0.108
Region trend	No	Yes	No	Yes
p-value, F-test of no effect, cohorts 82-83			0.751	0.787
p-value, F-test of same effect, cohorts 84-87			0.230	0.291
# Municipalities	449	449	449	449

Municipality and cohort fixed effects, as well as individual and municipality level controls are included in all specifications. Standard errors clustered on municipality in parentheses. *** p<0.01, ** p<0.05, *** p<0.1

Table A3. High school graduation. Students who enrolled in high school the same year they completed mandatory schooling. Complete results corresponding to table 6.

Explanatory variables	(1) With Controls	(2) Regional Trends	(3) With Controls	(4) Regional Trends
Hours treated, Cohort > 1983	-0.000792	-0.00166***		
	(0.000547)	(0.000622)		
Hours treated x Cohort 1982			0.000565	0.000442
			(0.000703)	(0.000737)
Hours treated x Cohort 1983			0.00128	0.00101
			(0.000876)	(0.000991)
Hours treated x Cohort 1984			-0.000144	-0.000563
			(0.000843)	(0.000968)
Hours treated x Cohort 1985			-0.00100	-0.00153
			(0.000754)	(0.000949)
Hours treated x Cohort 1986			-0.000124	-0.000708
			(0.000739)	(0.00102)
Hours treated x Cohort 1987			0.000557	-6.35e-05
			(0.000869)	(0.00115)
Min. 1 parent compl. h.s.	0.165***	0.164***	0.165***	0.164***
	(0.00317)	(0.00317)	(0.00317)	(0.00318)
Min. 1 parent compl. short high. edu.	0.323***	0.323***	0.323***	0.323***
	(0.00543)	(0.00544)	(0.00543)	(0.00545)
Min. 1 parent compl. long high. edu.	0.389***	0.389***	0.389***	0.389***
	(0.00574)	(0.00576)	(0.00575)	(0.00578)
Female	0.0534***	0.0533***	0.0534***	0.0533***
	(0.00336)	(0.00336)	(0.00336)	(0.00336)
First generation immigrant	-0.0135	-0.0127	-0.0136	-0.0128
	(0.0209)	(0.0209)	(0.0210)	(0.0209)
Second generation immigrant	0.0140	0.0147	0.0140	0.0146
	(0.0245)	(0.0244)	(0.0245)	(0.0244)
Share of pop. between 16 and 20	0.440	0.0322	0.392	0.0963
	(0.513)	(0.510)	(0.514)	(0.516)
Share of pop. above 60	-0.266	-0.665	-0.298	-0.620
	(0.333)	(0.469)	(0.335)	(0.467)
Leftist mayor	0.0117	0.0131*	0.0118	0.0135*
	(0.00790)	(0.00760)	(0.00789)	(0.00763)
Regional unemp. previous year	0.914	1.360*	1.034	1.524**
	(0.708)	(0.745)	(0.744)	(0.769)
Parents married	0.0568***	0.0568***	0.0568***	0.0568***
	(0.00248)	(0.00248)	(0.00248)	(0.00248)
Parents divorced	-0.204***	-0.204***	-0.204***	-0.204***
	(0.00580)	(0.00581)	(0.00580)	(0.00581)
Exactly 1 parent working	-0.00196	-0.00200	-0.00198	-0.00202
	(0.00225)	(0.00225)	(0.00224)	(0.00225)
Both parents working	0.0259***	0.0257***	0.0259***	0.0257***
	(0.00333)	(0.00332)	(0.00333)	(0.00332)
Constant	0.408***	33.43***	0.417***	31.73***
	(0.0801)	(3.094)	(0.0811)	(3.197)
Observations	260,339	260,339	260,339	260,339
R-squared	0.080	0.081	0.080	0.081
Region trend	No	Yes	No	Yes
p-value, F-test of no effect, cohorts 82-83			0.345	0.595
p-value, F-test of same effect, cohorts 84-87			0.134	0.133
# Municipalities	293	293	293	293

Municipality and cohort fixed effects, as well as individual and municipality level controls are included in all specifications. Standard errors clustered on municipality in parentheses. *** p<0.01, ** p<0.05, *** p<0.1

Table A4. Estimation results. High school graduation. Dichotomous treatment variables. Total sample of students. Complete results corresponding to table 7.

Explanatory variables	(1) With Controls	(2) Region Trends	(3) With Controls	(4) Region Trends
Affected x Cohort 1982	-0.00325	-0.00280		
	(0.00917)	(0.00958)		
Affected x Cohort 1983	-0.00616	-0.00560		
	(0.0101)	(0.0109)		
Affected x Cohort 1984	-0.0206*	-0.0199*		
	(0.0105)	(0.0112)		
Affected x Cohort 1985	-0.0245**	-0.0232**		
	(0.0103)	(0.0113)		
Affected x Cohort 1986	-0.0129	-0.0110		
	(0.0117)	(0.0115)		
Affected x Cohort 1987	-0.00893	-0.00625		
	(0.0107)	(0.0125)		
First generation immigrant	-0.0160	-0.0147	-0.0159	-0.0147
	(0.0272)	(0.0271)	(0.0271)	(0.0271)
Second generation immigrant	0.0144	0.0151	0.0145	0.0151
	(0.0223)	(0.0222)	(0.0223)	(0.0222)
Min. 1 parent compl. h.s.	0.191***	0.191***	0.191***	0.191***
	(0.00314)	(0.00314)	(0.00315)	(0.00314)
Min. 1 parent compl. short high. edu.	0.387***	0.387***	0.387***	0.387***
	(0.00481)	(0.00482)	(0.00481)	(0.00482)
Min. 1 parent compl. long high. edu.	0.468***	0.468***	0.468***	0.468***
	(0.00629)	(0.00629)	(0.00630)	(0.00629)
Female	0.0455***	0.0454***	0.0456***	0.0454***
	(0.00317)	(0.00317)	(0.00317)	(0.00317)
Share of pop. between 16 and 20	0.874*	0.469	0.893*	0.494
	(0.486)	(0.463)	(0.487)	(0.467)
Share of pop. above 60	-0.447	-0.689	-0.449	-0.739*
	(0.353)	(0.419)	(0.345)	(0.423)
Leftist mayor	0.00707	0.0131**	0.00706	0.0130**
	(0.00589)	(0.00586)	(0.00588)	(0.00585)
Regional unemp. previous year	0.896	1.163*	0.795	1.014*
	(0.611)	(0.615)	(0.607)	(0.608)
Parents married	0.0727***	0.0728***	0.0727***	0.0728***
	(0.00274)	(0.00274)	(0.00274)	(0.00274)
Parents divorced	-0.232***	-0.232***	-0.232***	-0.232***
	(0.00469)	(0.00468)	(0.00469)	(0.00469)
Exactly 1 parent working	0.00498**	0.00495**	0.00500**	0.00496**
	(0.00228)	(0.00228)	(0.00228)	(0.00228)
Both parents working	0.0424***	0.0421***	0.0424***	0.0421***
	(0.00332)	(0.00331)	(0.00332)	(0.00331)
Affected x Cohort > 1983			-0.0131*	-0.0152**
			(0.00712)	(0.00666)
Constant	0.282***	19.37***	0.282***	19.99***
	(0.0822)	(2.301)	(0.0801)	(2.368)
Observations	323,600	323,600	323,600	323,600
R-squared	0.106	0.106	0.106	0.106
Region trends	No	Yes	No	Yes
# Municipalities	293	293	293	293
p-value, F-test of no effect, cohorts 82-83	0.832	0.876		
p-value, F-test of same effect, cohorts 84-87	0.317	0.233		

Municipality and cohort fixed effects included in all specifications. Standard errors clustered on municipality in parentheses. *** p<0.01, ** p<0.05, *** p<0.1

Table A5. High school graduation, multiple treatment levels, total sample. Complete results corresponding to table 7.

VARIABLES	(1) With Controls	(2) Region Trends	(3) With Controls	(4) Region Trends
Affected x Cohort > 1983	-0.0131*	-0.0152**		
	(0.00712)	(0.00666)		
Affected x Cohort 1982			-0.00325	-0.00280
			(0.00917)	(0.00958)
Affected x Cohort 1983			-0.00616	-0.00560
			(0.0101)	(0.0109)
Affected x Cohort 1984			-0.0206*	-0.0199*
			(0.0105)	(0.0112)
Affected x Cohort 1985			-0.0245**	-0.0232**
			(0.0103)	(0.0113)
Affected x Cohort 1986			-0.0129	-0.0110
			(0.0117)	(0.0115)
Affected x Cohort 1987			-0.00893	-0.00625
			(0.0107)	(0.0125)
First generation immigrant	-0.0159	-0.0147	-0.0160	-0.0147
	(0.0271)	(0.0271)	(0.0272)	(0.0271)
Second generation immigrant	0.0145	0.0151	0.0144	0.0151
	(0.0223)	(0.0222)	(0.0223)	(0.0222)
Min. 1 parent compl. h.s.	0.191***	0.191***	0.191***	0.191***
	(0.00315)	(0.00314)	(0.00314)	(0.00314)
Min. 1 parent compl. short high. edu.	0.387***	0.387***	0.387***	0.387***
	(0.00481)	(0.00482)	(0.00481)	(0.00482)
Min. 1 parent compl. long high. edu.	0.468***	0.468***	0.468***	0.468***
	(0.00630)	(0.00629)	(0.00629)	(0.00629)
Female	0.0456***	0.0454***	0.0455***	0.0454***
	(0.00317)	(0.00317)	(0.00317)	(0.00317)
Share of pop. between 16 and 20	0.893*	0.494	0.874*	0.469
	(0.487)	(0.467)	(0.486)	(0.463)
Share of pop. above 60	-0.449	-0.739*	-0.447	-0.689
	(0.345)	(0.423)	(0.353)	(0.419)
Leftist mayor	0.00706	0.0130**	0.00707	0.0131**
	(0.00588)	(0.00585)	(0.00589)	(0.00586)
Regional unemp. previous year	0.795	1.014*	0.896	1.163*
	(0.607)	(0.608)	(0.611)	(0.615)
Parents married	0.0727***	0.0728***	0.0727***	0.0728***
	(0.00274)	(0.00274)	(0.00274)	(0.00274)
Parents divorced	-0.232***	-0.232***	-0.232***	-0.232***
	(0.00469)	(0.00469)	(0.00469)	(0.00468)
Exactly 1 parent working	0.00500**	0.00496**	0.00498**	0.00495**
	(0.00228)	(0.00228)	(0.00228)	(0.00228)
Both parents working	0.0424***	0.0421***	0.0424***	0.0421***
	(0.00332)	(0.00331)	(0.00332)	(0.00331)
Constant	0.282***	19.99***	0.282***	19.37***
	(0.0801)	(2.368)	(0.0822)	(2.301)
Observations	323,600	323,600	323,600	323,600
R-squared	0.106	0.106	0.106	0.106
Region trends	No	Yes	No	Yes
# Municipalities	293	293	293	293
p-value, F-test of no effect, cohorts 82-83			0.832	0.876
p-value, F-test of same effect, cohorts 84-87			0.317	0.233

Municipality and cohort fixed effects included in all specifications. Standard errors clustered on municipality in parentheses. *** p<0.01, ** p<0.05, *** p<0.1

Table A6. High school graduation, multiple treatment levels, total sample. Complete results corresponding to table 8.

Explanatory variables	(1) With Controls	(2) Region Trends	(3) With Controls	(4) Region Trends
Below median x Cohort > 1983	-0.00876	-0.00954		
	(0.00753)	(0.00685)		
Above median x Cohort > 1983	-0.0206***	-0.0314***		
	(0.00793)	(0.00796)		
Below median x Cohort 1982			0.00196	0.00239
			(0.0103)	(0.0106)
Below median x Cohort 1983			-0.00797	-0.00723
			(0.0116)	(0.0121)
Below median x Cohort 1984			-0.0123	-0.0113
			(0.0116)	(0.0117)
Below median x Cohort 1985			-0.0188*	-0.0173
			(0.0112)	(0.0118)
Below median x Cohort 1986			-0.00744	-0.00545
			(0.0128)	(0.0118)
Below median x Cohort 1987			-0.00573	-0.00321
			(0.0117)	(0.0129)
Above median x Cohort 1982			-0.0120	-0.0136
			(0.00979)	(0.0103)
Above median x Cohort 1983			-0.00368	-0.00759
			(0.0101)	(0.0112)
Above median x Cohort 1984			-0.0347***	-0.0407***
			(0.0112)	(0.0124)
Above median x Cohort 1985			-0.0344***	-0.0417***
			(0.0111)	(0.0126)
Above median x Cohort 1986			-0.0222*	-0.0308**
			(0.0122)	(0.0135)
Above median x Cohort 1987			-0.0142	-0.0237
			(0.0117)	(0.0146)
First generation immigrant	-0.0160	-0.0145	-0.0160	-0.0145
	(0.0272)	(0.0271)	(0.0272)	(0.0271)
Second generation immigrant	0.0145	0.0152	0.0144	0.0150
	(0.0223)	(0.0223)	(0.0224)	(0.0223)
Min. 1 parent compl. h.s.	0.191***	0.191***	0.191***	0.191***
	(0.00315)	(0.00315)	(0.00315)	(0.00314)
Min. 1 parent compl. short high. edu.	0.387***	0.387***	0.387***	0.387***
	(0.00482)	(0.00482)	(0.00482)	(0.00483)
Min. 1 parent compl. long high. edu.	0.468***	0.468***	0.468***	0.468***
	(0.00630)	(0.00629)	(0.00629)	(0.00628)
Female	0.0456***	0.0454***	0.0456***	0.0454***
	(0.00317)	(0.00317)	(0.00317)	(0.00317)
Share of pop. between 16 and 20	0.934*	0.340	0.896*	0.354
	(0.501)	(0.471)	(0.494)	(0.466)
Share of pop. above 60	-0.396	-0.831**	-0.428	-0.784*
	(0.356)	(0.422)	(0.355)	(0.420)
Leftist mayor	0.00675	0.0115**	0.00678	0.0118**
	(0.00563)	(0.00576)	(0.00563)	(0.00582)
Regional unemp. previous year	0.891	1.171*	1.045*	1.356**
	(0.594)	(0.625)	(0.599)	(0.632)
Parents married	0.0727***	0.0727***	0.0727***	0.0727***
	(0.00274)	(0.00274)	(0.00273)	(0.00274)
Parents divorced	-0.232***	-0.232***	-0.232***	-0.232***
	(0.00468)	(0.00468)	(0.00468)	(0.00468)
Exactly 1 parent working	0.00500**	0.00495**	0.00498**	0.00495**

	(0.00228)	(0.00228)	(0.00228)	(0.00228)
Both parents working	0.0423***	0.0421***	0.0424***	0.0421***
	(0.00332)	(0.00331)	(0.00332)	(0.00331)
Constant	0.269***	26.48***	0.276***	24.73***
	(0.0833)	(3.142)	(0.0833)	(3.310)
Observations	323,600	323,600	323,600	323,600
R-squared	0.106	0.106	0.106	0.106
Region trends	No	Yes	No	Yes
# Municipalities	293	293	293	293
p-value, F-test of no effect centile 0-50, cohorts 82-83			0.587	0.604
p-value, F-test of same effect for centile 0-50, cohorts 84-87			0.531	0.444
p-value, F-test of no effect centile 50-100, cohorts 82-83			0.444	0.413
p-value, F-test of same effect for centile 50-100, cohorts 84-87			0.158	0.315

Municipality and cohort fixed effects included in all specifications. Standard errors clustered on municipality in parentheses. *** p<0.01, ** p<0.05, *** p<0.1

Table A7. High school graduation, multiple treatment levels, total sample. Complete results corresponding to table 9.

Explanatory variables	(1) With Controls	(2) Regional Trends	(3) With Controls	(4) Regional Trends
Hours treated, cohort > 1983	-0.000875*	-0.00172***		
	(0.000475)	(0.000538)		
Hours treated x Cohort 1982			0.000161	8.08e-05
			(0.000612)	(0.000653)
Hours treated x Cohort 1983			0.000429	0.000263
			(0.000650)	(0.000745)
Hours treated x Cohort 1984			-0.00140*	-0.00168**
			(0.000737)	(0.000824)
Hours treated x Cohort 1985			-0.00119*	-0.00152*
			(0.000677)	(0.000858)
Hours treated x Cohort 1986			-0.000253	-0.000597
			(0.000727)	(0.000913)
Hours treated x Cohort 1987			2.05e-06	-0.000315
			(0.000730)	(0.00104)
Min. 1 parent compl. h.s.	0.191***	0.191***	0.191***	0.191***
	(0.00320)	(0.00320)	(0.00320)	(0.00320)
Min. 1 parent compl. short high. edu.	0.388***	0.388***	0.388***	0.388***
	(0.00493)	(0.00493)	(0.00493)	(0.00494)
Min. 1 parent compl. long high. edu.	0.469***	0.469***	0.469***	0.469***
	(0.00639)	(0.00638)	(0.00639)	(0.00638)
Female	0.0452***	0.0451***	0.0452***	0.0451***
	(0.00322)	(0.00322)	(0.00322)	(0.00322)
First generation immigrant	-0.0158	-0.0143	-0.0157	-0.0144
	(0.0276)	(0.0276)	(0.0276)	(0.0276)
Second generation immigrant	0.0135	0.0142	0.0135	0.0142
	(0.0226)	(0.0226)	(0.0226)	(0.0226)
Share of pop. between 16 and 20	1.012*	0.506	0.956*	0.589
	(0.514)	(0.480)	(0.510)	(0.476)
Share of pop. above 60	-0.381	-0.698	-0.431	-0.663
	(0.366)	(0.445)	(0.365)	(0.443)
Leftist Mayor	0.00766	0.0138**	0.00770	0.0142**
	(0.00595)	(0.00610)	(0.00594)	(0.00615)
Regional unemp. previous year	0.960	1.381**	1.103*	1.601**
	(0.602)	(0.621)	(0.614)	(0.637)
Parents married	0.0727***	0.0728***	0.0727***	0.0727***
	(0.00279)	(0.00279)	(0.00279)	(0.00279)
Parents divorced	-0.232***	-0.232***	-0.232***	-0.232***
	(0.00475)	(0.00475)	(0.00475)	(0.00475)
Exactly 1 parent working	0.00461**	0.00458**	0.00459**	0.00456**
	(0.00231)	(0.00232)	(0.00231)	(0.00231)
Both parents working	0.0422***	0.0419***	0.0422***	0.0419***
	(0.00339)	(0.00338)	(0.00339)	(0.00337)
Constant	0.260***	22.80***	0.273***	20.90***
	(0.0839)	(2.607)	(0.0841)	(2.660)
Observations	316,024	316,024	316,024	316,024
R-squared	0.106	0.106	0.106	0.106
Individual level controls	Yes	Yes	Yes	Yes
Municipality level controls	Yes	Yes	Yes	Yes
Region trend	No	Yes	No	Yes
p-value, F-test of no effect, cohorts 82-83			0.800	0.933
p-value, F-test of same effect, cohorts 84-87			0.0766	0.145
# Municipalities	273	273	273	273

Municipality and cohort fixed effects included in all specifications. Standard errors clustered on municipality in parentheses. *** p<0.01, ** p<0.05, *** p<0.1

Table A8. High school graduation, . Excluding municipalities without regulations in 1982. Complete results corresponding to table 10.

Explanatory variables	(1) With Controls	(2) Regional Trends	(3) With Controls	(4) Regional Trends
Hours treated x Cohort 1982	0.000161	8.08e-05		
	(0.000612)	(0.000653)		
Hours treated x Cohort 1983	0.000429	0.000263		
	(0.000650)	(0.000745)		
Hours treated x Cohort 1984	-0.00140*	-0.00168**		
	(0.000737)	(0.000824)		
Hours treated x Cohort 1985	-0.00119*	-0.00152*		
	(0.000677)	(0.000858)		
Hours treated x Cohort 1986	-0.000253	-0.000597		
	(0.000727)	(0.000913)		
Hours treated x Cohort 1987	2.05e-06	-0.000315		
	(0.000730)	(0.00104)		
Min. 1 parent compl. h.s.	0.191***	0.191***	0.191***	0.191***
	(0.00320)	(0.00320)	(0.00320)	(0.00320)
Min. 1 parent compl. short high. edu.	0.388***	0.388***	0.388***	0.388***
	(0.00493)	(0.00494)	(0.00493)	(0.00493)
Min. 1 parent compl. long high. edu.	0.469***	0.469***	0.469***	0.469***
	(0.00639)	(0.00638)	(0.00639)	(0.00638)
Female	0.0452***	0.0451***	0.0452***	0.0451***
	(0.00322)	(0.00322)	(0.00322)	(0.00322)
First generation immigrant	-0.0157	-0.0144	-0.0158	-0.0143
	(0.0276)	(0.0276)	(0.0276)	(0.0276)
Second generation immigrant	0.0135	0.0142	0.0135	0.0142
	(0.0226)	(0.0226)	(0.0226)	(0.0226)
Share of pop. between 16 and 20	0.956*	0.589	1.012*	0.506
	(0.510)	(0.476)	(0.514)	(0.480)
Share of pop. above 60	-0.431	-0.663	-0.381	-0.698
	(0.365)	(0.443)	(0.366)	(0.445)
Leftist Mayor	0.00770	0.0142**	0.00766	0.0138**
	(0.00594)	(0.00615)	(0.00595)	(0.00610)
Regional unemp. previous year	1.103*	1.601**	0.960	1.381**
	(0.614)	(0.637)	(0.602)	(0.621)
Parents married	0.0727***	0.0727***	0.0727***	0.0728***
	(0.00279)	(0.00279)	(0.00279)	(0.00279)
Parents divorced	-0.232***	-0.232***	-0.232***	-0.232***
	(0.00475)	(0.00475)	(0.00475)	(0.00475)
Exactly 1 parent working	0.00459**	0.00456**	0.00461**	0.00458**
	(0.00231)	(0.00231)	(0.00231)	(0.00232)
Both parents working	0.0422***	0.0419***	0.0422***	0.0419***
	(0.00339)	(0.00337)	(0.00339)	(0.00338)
Hours treated, cohort > 1983			-0.000875*	-0.00172***
			(0.000475)	(0.000538)
Constant	0.273***	20.90***	0.260***	22.80***
	(0.0841)	(2.660)	(0.0839)	(2.607)
Observations	316,024	316,024	316,024	316,024
R-squared	0.106	0.106	0.106	0.106
Region trend	No	Yes	No	Yes
p-value, F-test of no effect, cohorts 82-83	0.800	0.933		
p-value, F-test of same effect, cohorts 84-87	0.0766	0.145		
# Municipalities	273	273	273	273

Municipality and cohort fixed effects included in all specifications. Standard errors clustered on municipality in parentheses. *** p<0.01, ** p<0.05, *** p<0.1

Table A9. High school graduation. Excluding municipalities without regulations in 1982. Complete results corresponding to table 11.

Explanatory variables	(1) With Controls	(2) Regional Trends	(3) With Controls	(4) Regional Trends
Hours treated, Cohort > 1983	-0.000875*	-0.00172***		
	(0.000475)	(0.000538)		
Hours treated x Cohort 1982			0.000161	8.08e-05
			(0.000612)	(0.000653)
Hours treated x Cohort 1983			0.000429	0.000263
			(0.000650)	(0.000745)
Hours treated x Cohort 1984			-0.00140*	-0.00168**
			(0.000737)	(0.000824)
Hours treated x Cohort 1985			-0.00119*	-0.00152*
			(0.000677)	(0.000858)
Hours treated x Cohort 1986			-0.000253	-0.000597
			(0.000727)	(0.000913)
Hours treated x Cohort 1987			2.05e-06	-0.000315
			(0.000730)	(0.00104)
Min. 1 parent compl. h.s.	0.191***	0.191***	0.191***	0.191***
	(0.00320)	(0.00320)	(0.00320)	(0.00320)
Min. 1 parent compl. short high. edu.	0.388***	0.388***	0.388***	0.388***
	(0.00493)	(0.00493)	(0.00493)	(0.00494)
Min. 1 parent compl. long high. edu.	0.469***	0.469***	0.469***	0.469***
	(0.00639)	(0.00638)	(0.00639)	(0.00638)
Female	0.0452***	0.0451***	0.0452***	0.0451***
	(0.00322)	(0.00322)	(0.00322)	(0.00322)
First generation immigrant	-0.0158	-0.0143	-0.0157	-0.0144
	(0.0276)	(0.0276)	(0.0276)	(0.0276)
Second generation immigrant	0.0135	0.0142	0.0135	0.0142
	(0.0226)	(0.0226)	(0.0226)	(0.0226)
Share of pop. between 16 and 20	1.012*	0.506	0.956*	0.589
	(0.514)	(0.480)	(0.510)	(0.476)
Share of pop. above 60	-0.381	-0.698	-0.431	-0.663
	(0.366)	(0.445)	(0.365)	(0.443)
Leftist Mayor	0.00766	0.0138**	0.00770	0.0142**
	(0.00595)	(0.00610)	(0.00594)	(0.00615)
Regional unemp. previous year	0.960	1.381**	1.103*	1.601**
	(0.602)	(0.621)	(0.614)	(0.637)
Parents married	0.0727***	0.0728***	0.0727***	0.0727***
	(0.00279)	(0.00279)	(0.00279)	(0.00279)
Parents divorced	-0.232***	-0.232***	-0.232***	-0.232***
	(0.00475)	(0.00475)	(0.00475)	(0.00475)
Exactly 1 parent working	0.00461**	0.00458**	0.00459**	0.00456**
	(0.00231)	(0.00232)	(0.00231)	(0.00231)
Both parents working	0.0422***	0.0419***	0.0422***	0.0419***
	(0.00339)	(0.00338)	(0.00339)	(0.00337)
Constant	0.260***	22.80***	0.273***	20.90***
	(0.0839)	(2.607)	(0.0841)	(2.660)
Observations	316,024	316,024	316,024	316,024
R-squared	0.106	0.106	0.106	0.106
Region trend	No	Yes	No	Yes
p-value, F-test of no effect, cohorts 82-83			0.800	0.933
p-value, F-test of same effect, cohorts 84-87			0.0766	0.145
# Municipalities	273	273	273	273

Municipality and cohort fixed effects included in all specifications. Standard errors clustered on municipality in parentheses. *** p<0.01, ** p<0.05, *** p<0.1

Table A8. Heterogeneous effects by parental education (column (1)) and gender (column (2)). Complete results corresponding to table 10.

Explanatory variables	(1) Background interactions	(2) Gender interaction
Hours treated x Cohort > 1983	-0.00224***	-0.00167***
	(0.000543)	(0.000556)
Hours treated cohort 1983 x Parents completed high school	0.000521	
	(0.000390)	
Hours treated cohort 1983 x Parents completed short high. educ.	0.000988	
	(0.000620)	
Hours treated cohort 1983 x Parents completed long high. educ.	0.00185**	
	(0.000774)	
Hours treated cohort 1983 x Female		-3.87e-05
		(0.000432)
First generation immigant	-0.0143	-0.0145
	(0.0270)	(0.0271)
Second generation immigant	0.0149	0.0151
	(0.0223)	(0.0223)
Min. 1 parent compl. h.s.	0.188***	0.191***
	(0.00354)	(0.00314)
Min. 1 parent compl. short high. edu.	0.382***	0.387***
	(0.00533)	(0.00482)
Min. 1 parent compl. long high. edu.	0.459***	0.468***
	(0.00690)	(0.00629)
Female	0.0455***	0.0456***
	(0.00317)	(0.00437)
Share of pop. between 16 and 20	0.340	0.328
	(0.477)	(0.476)
Share of pop. above 60	-0.764*	-0.748*
	(0.426)	(0.426)
Leftist Mayor	0.0127**	0.0125**
	(0.00584)	(0.00586)
Regional unemp. previous year	1.191*	1.179*
	(0.622)	(0.621)
Parents married	0.0728***	0.0728***
	(0.00274)	(0.00274)
Parents divorced	-0.232***	-0.232***
	(0.00469)	(0.00469)
Exactly 1 parent working	0.00492**	0.00496**
	(0.00229)	(0.00229)
Both parents working	0.0419***	0.0421***
	(0.00333)	(0.00331)
Constant	22.73***	22.39***
	(2.593)	(2.596)
Observations	323,600	323,600
R-squared	0.106	0.106
# Municipalities	293	293

Municipality and cohort fixed effects as well as regional trends included in both specifications. Standard errors clustered on municipality in parentheses. *** p<0.01, ** p<0.05, *** p<0.1

Table A9. Estimation results. Completed years of education and log(earnings). Complete results are corresponding to table 11.

Explanatory variables	(1) Years of education	(2) Years of education Background interactions	(3) log(Wage aged 30)	(4) log(Wage aged 30) Background interactions	(5) log(Wage aged 39) Background interactions	(6) log(Wage 2008-10) Background interactions
Hours treated x Cohort > 1983	-0.00250	-0.00948***	-0.000697	-0.00207**	-0.000618	-3.72e-05
	(0.00172)	(0.00246)	(0.000795)	(0.000978)	(0.000690)	(0.000690)
Hours treated x Cohort > 1983 x Par.ed. = high school		0.00675***		0.00149**	0.00122**	0.000832*
		(0.00159)		(0.000584)	(0.000528)	(0.000487)
Hours treated x Cohort > 1983 x Par.ed. = short high. educ.		0.00649**		0.00215***	0.000503	-9.64e-05
		(0.00286)		(0.000745)	(0.000710)	(0.000626)
Hours treated x Cohort > 1983 x Par.ed. = long high. educ.		0.0111**		0.00340***	0.000952	0.000854
		(0.00427)		(0.00130)	(0.000924)	(0.000948)
First generation immigrant	0.0269	0.0340	-0.0539	-0.0535	-0.0347	-0.0416*
	(0.184)	(0.183)	(0.0362)	(0.0361)	(0.0228)	(0.0234)
Second generation immigrant	0.0730	0.0735	-0.0101	-0.0104	-0.00745	0.0190
	(0.146)	(0.147)	(0.0420)	(0.0420)	(0.0446)	(0.0351)
Min. 1 parent compl. h.s.	0.986***	0.953***	0.0925***	0.0853***	0.0855***	0.0905***
	(0.0138)	(0.0154)	(0.00403)	(0.00528)	(0.00412)	(0.00427)
Min. 1 parent compl. short high. edu.	2.437***	2.405***	0.182***	0.172***	0.202***	0.210***
	(0.0201)	(0.0212)	(0.00557)	(0.00700)	(0.00514)	(0.00510)
Min. 1 parent compl. long high. edu.	3.485***	3.431***	0.213***	0.196***	0.276***	0.288***
	(0.0304)	(0.0397)	(0.0166)	(0.0197)	(0.00840)	(0.00761)
Female	0.140***	0.139***	-0.497***	-0.497***	-0.393***	-0.362***
	(0.0150)	(0.0150)	(0.0102)	(0.0102)	(0.00680)	(0.00649)
Share of pop. between 16 and 20	0.997	0.550	0.106	0.125	-0.0637	0.285
	(2.047)	(1.930)	(0.653)	(0.653)	(0.536)	(0.491)
Share of pop. above 60	-1.841	-2.908**	-0.277	-0.306	-0.493	-0.0794
	(1.383)	(1.471)	(0.537)	(0.536)	(0.413)	(0.374)
Leftist mayor	0.0353*	0.0599***	0.00469	0.00501	-0.00243	0.00633
	(0.0205)	(0.0212)	(0.00930)	(0.00927)	(0.00672)	(0.00653)
Regional unemp. previous year	1.834	2.127	1.084	1.111	0.157	0.729
	(2.344)	(2.405)	(0.884)	(0.882)	(0.665)	(0.657)
Parents married	0.268***	0.268***	0.0328***	0.0328***	0.0724***	0.0947***
	(0.0160)	(0.0160)	(0.00762)	(0.00763)	(0.00504)	(0.00513)
Parents divorced	-1.136***	-1.137***	-0.240***	-0.240***	-0.116***	-0.140***
	(0.0256)	(0.0257)	(0.0118)	(0.0118)	(0.00766)	(0.00753)
Exactly 1 parent working	0.0110	0.0107	0.0119***	0.0118***	0.0195***	0.0245***
	(0.0107)	(0.0107)	(0.00306)	(0.00305)	(0.00283)	(0.00253)
Both parents working	0.195***	0.193***	0.0536***	0.0531***	0.0577***	0.0635***
	(0.0168)	(0.0167)	(0.00489)	(0.00490)	(0.00410)	(0.00377)
Constant	12.09***	37.23***	-8.600**	-7.957**	39.37***	22.82***
	(0.328)	(9.809)	(3.728)	(3.789)	(2.711)	(2.582)
Observations	323,600	323,600	294,536	294,536	294,536	294,536
R-squared	0.179	0.179	0.116	0.116	0.142	0.117
# Municipalities	293	293	293	293	293	293

Municipality and cohort fixed effects as well as regional trends included in both specifications. Standard errors clustered on municipality in parentheses. *** p<0.01, ** p<0.05, *** p<0.1

Table A10. Data definitions and sources.

Variable	Description	Source
High school graduation	Graduated from high school within five years after graduating from compulsory school	Register data from Statistics Norway
First gen. immigrant	=1 if first generation immigrant	Register data from Statistics Norway
Second gen. immigrant	=1 if second generation immigrant	Register data from Statistics Norway
Female	=1 if female	Register data from Statistics Norway
High school graduation conditional on enrolment	Graduated from high school conditional on being enrolled in high school the fall after graduating from mandatory schooling	Register data from Statistics Norway
Parents completed high school	=1 if highest parental education is high school	Register data from Statistics Norway
Parents completed short higher education	=1 if highest parental education is equal to bachelors degree or equivalent	Register data from Statistics Norway
Parents completed long higher education	=1 if highest parental education is equal to masters degree or PhD	Register data from Statistics Norway
Age younger than 20	Share of population in municipality younger than 20 years old	Data from Fiva et al (2011)
Age older than 60	Share of population in municipality older than 60 years old	Data from Fiva et al. (2011)
Population size		Data from Fiva et. al. (2011)
Leftist Mayor	=1 if mayor is socialist	Data from Fiva et al (2011)
Regional unemployment (t-1)	Regional unemployment rate previous year	Data from Norwegian Social Data Services
One parent working	=1 if exactly one parent is employed	Register data from Statistics Norway
Both parents working	=1 if exactly both parents are employed	Register data from Statistics Norway
Earnings	All pensionable income as registered by the tax authorities.	Register data from statistics Norway
Years of education		Register data from Statistics Norway
Opening Hours 1982	Maximum allowed opening hours given by municipal law in 1982	NOU 1984
Sectorial employment	Number of individuals employed by sector and age group. Employed defined as 100 or more working hours in the last year.	Norwegian Social Sciences Data Services