EARNING OR LEARNING? HOW EXTENDING CLOSING TIME IN THE RETAIL SECTOR AFFECTS YOUTH EMPLOYMENT AND EDUCATION[1]

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Abstract
This paper estimates the causal impact of increased demand for low-skilled workers on youth employment, and short and long run education. We exploit quasi-experimental demand shifts for retail workers due to changes in allowed opening hours for retail stores across Norwegian municipalities. We find that relaxed restrictions on opening hours increased employment in the sector and permanently reduced educational attainment for affected high school students. The results suggest that policies or shocks that increase demand for low-skilled workers in the short term might have negative long-run effects in terms of reduced educational attainment.

JEL-codes: I20, I21, J24

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1. Introduction

Human capital is a key determinant of long-term economic growth and prosperity, as recently demonstrated by Gennaioli et al. (2013). Public debate and academic research in recent years have focused on the design of public policy to increase the quantity and quality of schooling and thus to increase human capital. While knowledge on the impact of public policies on the supply side of the education market is clearly important, the role of opportunity costs and returns to education in shaping individuals' demand for education and human capital acquisition have received relatively less attention. The effects of demand-side factors suggested by the human capital model of Becker (1964) can be difficult to measure but may be as important for human capital acquisition and long-run growth potential as traditional supply-side policies such as class size reduction and increased educational expenditure. The literature shows that local economic shocks have substantial effects on labor market outcomes, as well as other important economic variables, such as human capital investment, housing prices and public welfare expenditure. This paper aims to quantify the effect of changes in local labor market opportunities on human capital acquisition in school by exploring quasi-experimental variation in job opportunities originating from changes in the institutional environment facing firms in the retail industry.

Specifically, our quasi-experimental research strategy utilizes a national legislation change in Norway in 1985 that brought the regulation of opening hours in the retail industry to a more uniform level across the country. The new legislation generally increased the possibility for retail stores to expand opening hours in the evenings and on Saturdays by postponing the mandatory shop closing time to later in the day. We show that this move increased the availability of retail sector jobs and represents a particular demand-side change that plausibly alters the incentives to invest in schooling, thus affecting the distribution of human capital across geographical areas and individuals. This result occurs because retail jobs generally require little or no formal education and are therefore of particular relevance for young unskilled people, such as high school dropouts. This situation was also the case 30 years ago, when the new legislation was introduced, and is a reason why this particular intervention is relevant for understanding the effects of demand shocks on the distribution of human capital even today. The importance of the retail sector as an employer for young and less educated workers is evident around the world. For example, in the US, 1/5 of employed persons aged 16-24 are currently employed in the retail sector[2]. Over the last 30 years, several countries have removed or eased restrictions on opening hours in the retail sector on weekdays, Sundays and public holidays. In addition, this topic is recurring in other countries. Considering this international trend, the results presented in this paper on potential side effects on educational outcomes also have a clear policy relevance worldwide.

We contribute to the literature by first investigating the effect of the national legislation change on local youth employment in retail and on short- and long-run educational outcomes by exploiting rich census and administrative register data from Norway. While Lee (2013) finds that the removal of restrictions of Sunday shopping ("blue laws") at different points in time across US states lowered educational attainment and increased retail employment, we exploit the heterogeneity in opening hour regulations across municipalities in the pre-reform period (before 1985) as the identifying source of variation; hence, we circumvent potential endogeneity problems in studies using intertemporal variation in legislation across states. The legislation we study here affected opening hours on weekdays as opposed to Sundays, and we therefore analyze a more general intervention than in the previous literature on the removal of "blue laws". By focusing on the differential effect of the national policy change on different municipalities, we cannot evaluate nation-wide effects. Rather, we analyze how retail employment opportunities for young people and subsequent human capital investments differ between municipalities with large increases in potential opening hours relative to municipalities with little or no change.

We also contribute to the literature by investigating to what extent effects on educational outcomes differ across students with different family backgrounds and across gender. While we do not study intergenerational mobility directly in this paper, the results on effect heterogeneity can be informative: if the effects on human capital investments differ across students with different social backgrounds, it may, in turn, affect intergenerational mobility and contribute to changes in income inequality in the long run.

The paper is organized as follows. Section 2 reviews the literature, Section 3 presents the institutional setup and the data, and Section 4 provides the empirical strategy and estimation results for the impact of the deregulation on the employment of young workers in the retail

[2] Bureau of Labor Statistics (2016)

sector. The effect on high school graduation is explored in Section 5, followed by a heterogeneity analysis in Section 6. Section 7 provides evidence on the effect on completed years of education and earnings. Section 8 summarizes and concludes.

2. Literature review

Following the seminal paper by Blanchard and Katz (1992), several recent studies have analyzed the effects of different types of local economic shocks in several sectors, such as retail, natural resource industries, manufacturing, construction and housing, on labor market outcomes and related economic variables. Regarding the retail industry, Skuterud (2005) exploits temporal variation in decisions to remove restrictions to Sunday shopping ("blue laws") across provinces in order to identify the effects on employment in the Canadian retail industry and to find a positive retail employment effect. Lee (2013) uses a similar empirical strategy and finds that the removal of "blue laws" in US states reduced educational attainment and increased retail employment. Recent studies from Germany exploit the lifting of restrictions on opening hours on weekdays in some German states. Bossler and Oberfichtner (2016) and Paul (2016) find that deregulation increased total retail employment, particularly part-time work and the probability of working 1-16 hours a week. However, Senftleben-König (2014) find that full-time employment increased. Basker (2005) and Neumark et al. (2008) study the impact of the expansion of Wal-Mart in the US retail industry on retail employment. Humphreys and Marchand (2013) study the employment effects of the establishment of new casinos in Canada. None of these latter studies consider the potential side effects on education outcomes.

A separate but related stream of literature, such as Black et al. (2005a) and Marchand (2012), has analyzed the effects of booms and busts in natural resource markets. A few of these studies also consider potential side effects on human capital formation. Black et al. (2005b) exploit booms and busts in the coal industry in Appalachian US states and find that booms (busts) increase (decrease) high school dropout, while Emery et al. (2012) exploit the oil boom in Alberta, Canada and find that long-run completed years of education were unaffected, although high school dropout increased in the short run. Cascio and Narayan (2015) show that local variation in the introduction of new technology in oil and gas extraction ("fracking") within US states increased demand for low-skilled employees and increased high school dropout. In addition to studies of the incidence of shocks in natural resource industries, Charles et al. (2015) use across-city variations in housing booms in the US and find positive employment effects and a subsequent reduction in college enrollment. Additionally, Atkin (2016) finds a negative schooling effect of increased demand for unskilled labor in Mexico originating from international trade deregulation. In summary, there is some previous evidence that increased labor market opportunities decrease human capital accumulation, but much of the evidence is confined to North America.

3. Institutional background and data on opening hours

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 has been delegated to local authorities (municipalities)[3]. While the Closing Law imposed some general restrictions on activities on national holidays, Sundays and other Christian holidays, the municipalities were free to set their own shop closing regulations on other days. During the post-World War II period, local governments generally tended to pass more restrictive closing regulations. The resulting shortening of shop opening times was a concern for the government, as it forced a considerable share of the population to conduct their daily shopping within work hours.

As a response to this development, several official committees were appointed by the government to consider changes in the Closing Law. The majority of the members of the committees appointed in 1959 and 1970 proposed limiting the scope of local authorities' ability to restrict opening hours in retail firms. However, 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 changes in the law. A third committee was appointed in spring 1981 and delivered a report in April 1984, denoted NOU (1984). The committee recommended that local authorities should still have the authority to restrict opening hours, but not earlier than 8:00 pm on weekdays and 6:00 pm on Saturdays. By April 1985, the parliament passed the Opening Hours Act ("Åpningstidsloven"), implementing the committee's recommendations. Thus, beginning in 1985, local authorities were prevented from setting closing time restrictions in the retail sector earlier than 8:00 p.m. on workdays and earlier than 6:00 p.m. on Saturdays and days before official holidays[4].

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

[4] The regulation of opening hours on Sundays and specific national and religious holidays was unaffected by the 1985 reform.

As part of its work, the committee (NOU (1984)) collected data on local regulations on opening hours in retail stores and local government service production in each municipality beginning in 1982. These data include detailed information on closing rules for retail firms for ach day of the week. Thus, we can measure to what extent the Opening Hours Act of 1985 changed the legal environment in the municipalities. Below, we explain this dataset and demonstrate how it can be explored to estimate the impact of deregulation on retail employment, educational outcomes and earnings.

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. The data provide information on the 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.

A minority of municipalities had no regulations prior to the Opening Hours Act of 1985. With the current data, it is not possible to rule out the possibility that the passage of the new law led to a normative effect that induced additional regulations in these cases. In our main analysis, we include these municipalities in the control group, but we show in Table B2C in the Online Appendix that our results are not driven by this decision.

In our baseline specification, we define treatment variable T as the number of hours per week that retail shops could expand their opening hours due to the 1985 Opening Hours Act. For each day of the week, we calculate the difference between the maximum allowed opening hours in 1982 and the minimum allowed opening hours introduced by the 1985 Opening Hours Act. As a clarifying example, consider a municipality that, in 1982, allowed retail shops to be open until 7:00 p.m. Monday through Saturday. After the 1985 Opening Hours Act, it was no longer required for retail shops to close earlier than 8:00 p.m. on weekdays, effectively increasing potential opening hours by one hour each day from Monday to Friday. Because the municipality already allowed shops to be open until 7:00 p.m. on Saturdays, restrictions on opening hours on this day were unaffected. In total, retail shops in this municipality could therefore be open five more hours per week.

We wish to emphasize that even though we are unable to observe local opening hours restrictions after the deregulation in 1985, we are confident that all treated municipalities complied. First, the deregulation was heavily debated in Norway, ensuring that store owners, local politicians and the rest of the population were well aware of it. The intensity of the public debate is illustrated in a lively interview with former minister of Consumer Affairs and Administration Astrid Gjertsen and her secretary Erling Lae; see Hageman, Pharo and Lange (2004). Second, national laws trump local laws, so there was no room for local governments to challenge national laws. However, we are unable to explore whether some, if any, municipalities deregulated local opening hours between 1982 and 1985 in anticipation of the national law. Further, we cannot empirically observe whether the national law had any normative effects on municipalities that were more lenient than the national law prior to 1985. As such, all results must be interpreted as intent-to-treat-effects, as in most other comparable studies.

In addition to the data restrictions outlined above, we do not have data on the actual opening hours at the local level, and we are forced to rely on other aggregate survey data to demonstrate the effect on actual opening hours. The Norwegian Research Institute of Commerce published a report in 1985 on actual opening hours shortly prior to the deregulation, surveying approximately 30% of all retail stores (NORIC, 1985). Similar data were collected in 1990 by the Norwegian Competition Authority and reported in Lavik and Schjøll (2016). In Figure 1, we plot the share of surveyed retail stores' closing at various times of the day on weekdays and Saturdays. On both weekdays and Saturdays, there is a significant move towards longer opening hours over the period. Considering weekdays, nearly 80% of the surveyed stores closed at 5:00 p.m. in 1985. By 1990, this percentage had fallen to approximately 30%. At the same time, almost no stores were open until 8:00 p.m. in 1985, while nearly 40% were open until 8:00 p.m. in 1990. Because these data are survey data reported only on the national level, they are not suitable for our analysis. However, they indicate that the reform had a strong impact on actual opening hours at the store level.

The geographical distribution of the affected municipalities is presented in Figure 2. The figure shows the unaffected municipalities in white, and a darker shade indicates stronger treatment. There are 128 unaffected municipalities, 58 had restrictions that were so lenient that the reform had no effect. Opening hours in 326 municipalities were affected. Among these municipalities, the median expansion in permitted opening hours was 11 hours per week, with a mean of 10.14.[5]

4. Effects on youth employment in the retail sector

To help interpret the subsequent evidence on educational outcomes, we investigate the extent to which easing restrictions on shop opening hours affects youth employment opportunities. The impact of easing opening hours on retail sales and employment is not obvious. First, removing restrictions can, through increased sales, lead to a net increase in labor demand, satisfied by increased hours worked by existing employees, the hiring of new employees, or both. However, it is possible that deregulation changes only the timing of sales within the day or week, and therefore, the net effect on total sales and subsequent labor demand may be zero. The evidence in Jacobsen and Kooreman (2005) suggests that the liberalization of shopping hour regulations in the Netherlands in the late 1990s generally increased the time people spent shopping and thus indicates a positive effect on total sales and labor demand.

Second, removing opening hours restrictions may change the firm structure in the retail industry. Wenzel (2009) theoretically analyses how the deregulation of shopping hours affects competition in the retail industry. He shows that whether deregulation favors chain stores or independent retailers depends on the efficiency differences between the two types of retailers. While the results are theoretically unclear, we are aware of one contribution of the empirical effects that suggests that deregulation favors large shops: studying the effect deregulation of opening hours in Germany, Bossler and Oberfichtner (2016) find that shops increased employment by 0.1 worker per additional permitted opening hour per week and that this increase was driven by large shops. Given the ambiguous theoretical effects and limited previous empirical evidence, we now estimate the effects of the Norwegian deregulation on youth employment.

Empirical analysis

We use census employment data to estimate the effects on the growth of youth employment in the retail sector. The published census data are collected every ten years and include the number

[5] In the typical municipality, where opening hours increased by 11 hours per week, the maximum allowed closing time before the reform was 6:00 p.m. on weekdays, 7:00 p.m. on days with extended opening hours, and 4 p.m. on Saturdays.

of individuals employed in each municipality by sector and age group,[6] which allows us to explicitly trace youth employment growth in the retail sector across municipalities in 1970, 1980 and 1990. 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. In particular, students who were the minimum allowed working age (16) in 1985 were 21 in 1990. As we show later, the deregulation had the greatest impact on students finishing compulsory school and beginning high school in 1984 onwards, and we include them in our employment measure for 1990.

We report average youth employment shares in the retail sector for the years 1970-1990 in Table A1 in the Online Appendix. The sector employs approximately 13% of all workers aged 16-24 throughout the period, making it one of the main employment sectors for young workers. The employment share is comparable to the US, where the Bureau of Labor Statistics put the number at 20% (Bureau of Labor Statistics, 2017). The size of the sector suggests that labor demand variations in retail could have large effects on youth employment and thereby high school graduation rates.

Our identification strategy is straightforward: the number of opening hours that expanded in a municipality is used as the treatment variable, given the assumption that the opening hours restrictions, as measured in 1982, constitute a valid measure for the situation in 1980. The regression model is formally presented in Equation (1). 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, and 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, age composition and a dummy indicating whether the mayor represents a left-wing party. The coefficient of interest is β_2 and can be interpreted as the effect of lifting restrictions on opening hours by one hour per week on the growth of youth employment in the retail sector, controlling for time-varying municipality characteristics, including population size. A positive and significant β_2 suggests that deregulation increased youth employment growth.

[6] Only individuals who work a minimum of 100 hours within a given sector in the census year are considered to be 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 was conducted.

$$(1)\ \log(employment\ retail_{it}) = \beta_0 + \alpha_i + \beta_1 d1990 + \beta_2 T_i * d1990 + X\gamma + u_{it}$$

A possible threat to our identification strategy is that employment was trending upward in the treated municipalities for other reasons. Therefore, to strengthen the credibility of the results, we 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 as the post-treatment observation year. Because 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 growth in youth employment in the 1970 to 1980 period is predictive of which municipalities were affected by the reform, and thus, the parallel trend assumption would be violated.

Another potential source of bias in estimating Equation (1) is that there could be some unobserved characteristics that drive the general youth employment growth and that are possibly correlated with the probability that a municipality would loosen its restrictions in 1985. To address this concern, we re-estimate Equation (1) using the youth employment growth in the manufacturing industry sector as the dependent variable. While some spillover effects between sectors are possible, we expect the deregulation to have a very small effect on employment growth in sectors other than retail. All results are reported in Table 1A.[7]

As evident from Column (1), the interaction term is positive and significant at the 5% level. On the margin, for a municipality in which potential opening hours increased by one hour per week, the results predict an increase of 1.07% in youth employment in the retail sector from 1980 to 1990. As municipalities that were forced to lift restrictions expanded allowable opening hours by approximately 10 hours per week on average, the estimate implies that the 1985 Opening Hours Act caused a 10.7% growth in youth employment in the retail sector on average. The effect is relatively large and consistent with previous studies that find positive employment effects of deregulation of opening hours in retail firms; see Skuterud (2005) and Bossler and Oberfichtner (2016).

[7] In Online Appendix Table A2B, we report the results when we include only municipalities used in the analysis of educational outcomes below; these results show similar effects. Note that reported standard errors are not clustered, but robust, in these specifications because we observe each municipality only twice in each regression and include municipality fixed effects (Cameron and Miller, 2015).

Both the placebo specification and the effect on employment in the manufacturing industry yields non-significant effects. These results support the interpretation that the results in Column (1) are causal effects and are not biased by a trend[8].

To further explore the employment effects, we also estimate the models in Table 1A using only high-school-aged workers (16-19 years old). The results from this exercise are reported in Table 1B and show that there was no significant effect on the employment level for this age group in 1990. This finding is consistent with a hypothesis that there was a short-run employment effect for 16-19-year-olds. Combined with the evidence from Table 1A, this finding shows that those who were recruited into the retail sector due to the deregulation remained in the sector and are included in the 20-24 age group in 1990. There are other alternative explanations for the short-lived effect. A possible explanation is that the labor demand increase generated by increased opening hours was initially filled by school-aged workers. However, in the longer run, the deregulation may have generated changes in the firm structure in the retail industry, as well as the reorganization of staff and working hours within firms or the recruitment of workers from outside the local labor market, dampening the initial increase in the use of local school-aged workers. Unfortunately, further analysis of these issues is limited by the quality of the data. Nevertheless, the evidence seems to indicate that any employment effects on the high-school-aged population were relatively short lived.

Last, we also find an employment effect among older workers, which shows that aggregate employment in the sector rose and that younger workers did not merely replace older workers. These results are shown in the Online Appendix in Table A2D. An increase in total employment in the retail industry in the treated municipalities also indicates that stores in the treated municipalities responded to the Opening Hours Act by increasing their actual opening hours. After establishing a causal effect of the deregulation on youth employment, we now turn to the effects of the deregulation on human capital accumulation in school.

5. High school completion

Increased employment opportunities for young people in the retail sector generated by extended opening hours can affect students' educational choices through two main channels. First, the

[8] Full results corresponding to Table 1A are reported in Online Appendix Table A2A. The results are also robust to the inclusion of regional-time dummies, which will absorb any common trend within regions. These results are reported in Online Appendix Table A2C.

opportunity cost of education increases, leading individuals to reduce their schooling. Second, increased employment opportunities may have a positive income effect: as some parents increase their income levels and become less likely to be credit constrained, they may increase their children's schooling. In addition, expanded opening hours may have a direct effect on youth social incentives and behavior. Gruber and Hungerman (2008) show that the removal of "blue laws" in US states increased the incidence of risky behavior among young adults in terms of heavy drinking and drug use. Grönqvist and Niknami (2014) show that a policy experiment in Sweden that required alcohol retail stores to stay open on Saturdays in selected areas increased both alcohol consumption and criminal activity. The opportunity cost and social incentives channels may be particularly important if students are shortsighted and heavily discount the future, as the evidence in Oreopoulos (2007) indicates. The data available do not permit us to distinguish clearly between the different mechanisms, but it should be clear that the net effect of extending opening hours on educational investments is ambiguous in principle, even if employment opportunities in the retail sector increased[9].

The school system

In the period used in the empirical study, students in Norway begin mandatory schooling at the age of 7. Municipalities are tasked with funding and operating the public schools. A handful of students enroll in private mandatory schooling in the period. The curriculum is decided on the national level. Students are very rarely held back in mandatory schooling, and nearly all students graduate at the age of 16. Upon graduating from mandatory schooling, students can apply to an optional high school education, and approximately 95% of students currently choose to do so. High school is tracked into various academic and vocational tracks lasting 3 to 4 years. Public high schools are funded and operated by 19 county governments. Students who graduate from academic tracks are eligible to apply to college. Most vocational tracks consist of 2 years of schooling and 2 years of apprenticeship.

[9] Abramitzky and Lavy (2014) estimate a positive effect on education of a reform in Kibbutz wage-sharing arrangements in Israel that implied increased returns to education. They conclude that the income channel played a limited role and that the returns to education channel operated above and beyond the social incentives channel. Løken (2010) find no causal relationship between educational attainment and family income using regional variation in the expansion of the Norwegian oil and gas sector in the 1970s as an instrumental variable for family income. Neumark and Wascher (1995) show that minimum wages tend to decrease school enrollment in US states. Several studies from a number of countries and time periods report countercyclical patterns in high school enrollment and graduation that are consistent with the opportunity cost argument, see Duncan (1965), Rees and Mocan (1997), Goldin and Katz (1999), Clark (2011) and Reiling and Strøm (2015).

5.1 Empirical strategy and data

Empirical strategy

To estimate the impact of local deregulation on educational outcomes, we use the treatment variable defined in the data section. Because 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. Equation (2) shows the regression model representation of this strategy, where y_{icm} is the outcome variable for individual i in the cohort that finishes compulsory school in spring year c in municipality m. The outcome variables are further described below.

$$(2)\ y_{icm} = \sum_{t=1981}^{1987} \beta_t T_{icm} + X'_{icm}\delta + Z'_{cm}\gamma + \mu_m + C_c + u_{icm}$$

where X_{icm} is a vector of student-level controls and Z_{cm} is a vector of time-varying municipality-level controls, with δ and γ as the corresponding coefficient vectors. Both individual-level and time-varying municipality-level controls are detailed in the data section. μ_m and C_c are municipality and cohort fixed effects, respectively.[10] u_{icm} is the usual error term. β_{1982} through β_{1987} are the coefficients of interest and measure the effect of a one-hour increase in allowed opening hours per week from 1985 onward on students' completion of compulsory schooling in year t. A serious challenge with this quasi-experimental strategy is that selection into treatment is non-random. In our case, the treatment intensity will inherently depend on the severity of local restrictions prior to the 1985 reform. It is possible that dropout rates increase in the treated municipalities due to municipality-specific conditions that we cannot observe. Such a correlation would bias our estimates and create spurious results. We combat problems with non-random selection in two ways. First, the inclusion of municipality fixed effects effectively removes all time-invariant factors at the municipal level that might affect the outcome variables, and we effectively exploit within-municipality variation in opening hours and educational outcomes for students in cohorts before and after the deregulation. Second, we include linear regional trends[11]. If educational outcomes evolve smoothly across regions due to different labor market developments, this evolution will be captured by these trends. Note that

[10] Throughout the paper, cohort refers to the year students finish compulsory schooling, i.e., the spring of the year they turn 16.

[11] We use labor market regions rather than municipalities when constructing linear trends because we have only seven years of observations for each municipality, which means we would lose a great deal of variation by including trends on the municipal level.

we also include the lagged unemployment rate on the regional level among the control variables. Hence, we control for both smooth changes in regional business cycles and labor market conditions, such as wages, and short-run fluctuations in the unemployment rate.

Estimating Equation (2) will provide a separate estimated effect for each of the seven cohorts of students in the treated municipalities. Thus, we will have a total of six treatment coefficients relative to the reference cohort of 1981. 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 should be less affected than students in later cohorts. When estimating Equation (2), this situation will be reflected in smaller coefficients for the earlier cohorts. In fact, students in the first few cohorts might be completely unaffected by the changes in opening hours regulations in 1985 because 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 (2). Specifically, we test whether the treatment effect is zero and equal for the first three cohorts in the sample, i.e., whether $\beta_t=0$ for the 1982 and 1983 cohorts, with the 1981 cohort used as a reference category. This test is effectively equivalent to a test of the parallel trend assumption, or a placebo test. Given that these cohorts can be considered 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, the general formulation in Equation (2) also allows us to test the hypothesis that the treatment effect is the same for the last four cohorts, i.e., $\beta_t= \beta$ for the 1984-1987 cohorts. As it turns out below that neither of these restrictions can be rejected statistically, Equation (3) illustrates a more conventional differences-in-differences strategy with a single treatment coefficient β when the restrictions are imposed:

$$(3)\quad y_{icm} = \beta D_t T_{icm} + X'_{icm}\delta + Z'_{cm}\gamma + \mu_m + C_c + u_{icm}\,, \qquad D_t = \begin{cases} 1 \; if \; cohort > 1983 \\ 0 \; otherwise \end{cases}$$

While the model above formulates the empirical strategy using a continuous treatment variable, other specifications are discussed below.

Data: Individual outcome variables

Data on students' educational outcomes and adults' earnings and background are obtained as register data from Statistics Norway. These register data contain information on the years in which the students graduated from compulsory school and from high school, which is noncompulsory. Specifically, we use as our main outcome variable an indicator for graduation from high school five years after finishing lower secondary education, as is standard for studies using Norwegian data (see Reiling and Strøm (2015) and Falch et al. (2014 a, b)).

Data: Individual- and municipality-level control variables

We have administrative data on several student background characteristics, which we control for in our regressions: immigration status, gender and parental education.

We also have access to many municipality-level control variables, including demographic, economic and political variables, from the Norwegian Social Science Database and the database provided by Fiva et al. (2012). As municipal demographic controls, we include the share of young and elderly people in the municipality. The political affiliation of the mayor is included as well. As a demographic control, we include the share of young inhabitants in the municipality because it affects labor market conditions for potential drop-outs. One could argue that the share of young people is endogenous if young people are mobile between municipalities. However, the main results are very similar regardless of whether the share of young people is included, although the effects are more precisely estimated when this variable is included.

Because we consider 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 because it might be considered an outcome of the treatment; thus, it introduces a so-called bad controls problem (Angrist and Pischke, 2009). To reduce this problem, we use the lagged unemployment rate in the economic region instead[12]. Note that in our main sample, we exclude municipalities in which high school cohorts were smaller than 30 for at least one of the years in the empirical period to increase the comparability between the municipalities in the final sample. This approach is potentially important as very small municipalities could follow different trends throughout the period in the absence of the deregulation[13]. The final dataset used in our main specifications includes 293 municipalities.

It is possible that the presence and strictness of the opening hours regulations are correlated

[12] The regions denoted "economic regions" are defined by Statistics Norway and correspond to the NUTS 4 level in the EU's regional classification; they consist of municipalities that share common labor market and trade areas (see Statistics Norway (2000)). In total, there were 90 such regions, compared with a total of 454 municipalities, as of 1982.

[13] In Table B1B in the Online Appendix, we show that this restriction on the sample is not decisive for our results.

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 Online Appendix Table E1. Table 2 shows descriptive statistics for the municipalities included in the regression analysis, split by treatment status. In the third column, we report p-values for a t-test of equal means. It is worth noting that there are some differences between the municipalities that were affected by the reform and those that were not. Systematic differences of this kind could lead to biased estimates. However, most of these variables, e.g., the age distribution of the populations, change very slowly. Because we have data for seven different cohorts, the inclusion of municipality fixed effects, cohort effects and regional trends, in addition to the listed control variables, account for a large number of potential confounding factors to a great extent.

5.2 Baseline regression results for high school completion

We now present the results of regression equations corresponding to different variants of the regression model from Equation (2), with high school completion as the outcome. Standard errors are clustered at the municipality level[14].

To ensure that small municipalities do not drive the results, we exclude all municipalities that had fewer than 30 students of any cohort enrolling in high school[15]. Table 3A shows the main results. Column (1) shows results when we impose the restrictions of zero treatment effects for the 1981-1983 cohorts and equal treatment effects for the 1984-1987 cohorts (i.e., Equation (2)). In all specifications, we include cohort and municipality fixed effects, student characteristics, and time-varying municipality-level variables, including the lagged regional unemployment rate, with definitions and descriptive statistics, as shown in the data section above.

In Column (2), we extend the model to include labor market region-specific linear trends to

[14] To allow for spatially correlated error terms that are possibly due to spatially correlated treatment status, we report results when we re-estimate our baseline model using Conley standard errors, see Conley (1999), in the Online Appendix Table B1C. The table shows that standard errors are smaller than those when clustering on municipalities. This result suggests that spatial correlation in treatment status is a negligible challenge in our setting. See also Table 3B.

[15] This excludes a total of 156 municipalities and 34,832 students from the sample. In Table B1B in the Online 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 3A.

minimize threats to the parallel trends assumption[16]. The estimated treatment effects are negative and statistically significant in all specifications. According to Column (2), increasing allowed opening hours by one hour per week leads to a 0.15 percentage point reduction in the probability of graduating from high school within five years of compulsory school. The average treated municipality therefore observed a reduction in the graduation rate of 1.5 percentage points. This effect is substantial, 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 2.9%, which is almost twice the 1.6% (1.2 percentage point) effect implied by the estimates in Lee (2013)[17]. 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. One possibility is that the required number of hours spent on homework needed for an average student to graduate varies systematically between the school systems such that when Norwegian students take up work, they are more likely to spend an insufficient amount of time on homework compared to their American peers. However, this explanation is not supported by the OECD evidence that reports that 15-year-old US students spend approximately 1 hour more per week on homework than Norwegian students (OECD, 2014). Third, in the US, opening hours changed only for Sundays, while in our case, the reform affected opening hours on 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, which indicates that the net lifetime earnings loss experienced by high school dropouts is lower in Norway than in the US.

Columns (3) and (4) report results from models with a full set of cohorts by treatment interaction effects. At the bottom of each column, we report the p-values for the 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 effects on the 1982 and 1983 cohorts are jointly indistinguishable from zero. That is, if we cannot reject the null hypothesis, we are de facto

[16] In Table B2D in the Online Appendix, we report results when we allow the trends to be different before and after 1983, as suggested by a referee. However, because the post-treatment trends will absorb part of the treatment effect, the treatment interaction does not provide the full treatment effect.

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

unable to reject the parallel trend assumption for the 1982 and 1983 cohorts. Since the information required to obtain precise estimates of the separate cohort-specific treatment effects for the post-1983 cohorts are considerable, we also perform a formal test of the hypothesis that the effects are equal, which means that if we cannot reject the hypothesis of equal interaction coefficients for the post-1983 cohorts, the versions with a single treatment effect, as reported in Columns (1) and (2), represent a valid simplification of the more general version of the model and can be interpreted as an estimate of the average post-reform effect. Additionally, note that including the linear regional trends increases the p-value of both tests. There is therefore arguably 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 choice of a specification that uses only a single treatment interaction term. Detailed estimation results are reported in Table B1A in the Online Appendix.

To obtain a visual picture of the cohort-specific treatment effects, we also present the estimates in Column (4) graphically in Figure 3. Along the x-axis, we plot the treatment coefficients for each cohort with the size and confidence intervals of effects on the y-axis. The pattern from the figure is quite striking. We estimate precise zeroes for the 1982 and 1983 cohorts, with a clear shift in 1984. The following cohorts experience gradually smaller treatment effects, although as expected, the statistical precision of the estimated effects is low due to high estimated standard errors. The pattern suggests that the treatment had the strongest effect on the graduation probability of the cohort that could have completed one year of high school prior to treatment and that the effect was relatively short-lived and faded out for the most recent cohorts. This effect therefore mimics the employment effects for 16-19-year-olds reported in Table 1B, where we estimate no significant effect on the employment level for this age group in 1990. It therefore appears that the deregulation decreased the graduation probability and increased employment for students in the 1984 and 1985 cohorts the most and that these students remained in the retail sector until 1990, when we observe an increased employment rate in the sector for the 16-24 age group. A possible but somewhat speculative explanation is that retail firms adapted to the new regulations in the short run by recruiting a limited number of school-aged workers, while changes in the number of firms or a reorganization of staff inside existing retail firms occurred in the longer run. However, we are not able to distinguish between these explanations with the available data.

Our preferred definition of the treatment variable is the number of hours per week that permitted opening hours expanded. It is not obvious that this is the correct way to specify the variable for several reasons. First, retail firms might prefer to employ more experienced workers and may only employ young, unskilled workers once the supply of available older workers is depleted. Second, facing small increases in permitted opening hours, firms might limit new hires by increasing hours for current employees. Third, it is possible that students' drop-out decision is affected only if the increase in employment opportunities exceeds some threshold in terms of increased shop opening hours per week. Students living in municipalities that experience a very small increase in potential opening hours might therefore be unaffected. All these mechanisms suggest an underlying non-linear effect of increased opening hours. We therefore challenge our main specification with several alternative specifications and report the results in the Online Appendix. These specifications include a traditional difference-in-differences estimator with a treatment dummy for municipalities with expanded opening hours (Table B2A in the Online Appendix), as well as a specification in which we include separate treatment dummies for municipalities experiencing expansion above and below the median (Table B2B in Online Appendix). Estimations from both of these alternative specifications yield very similar results. We also challenge the definition of the estimation sample. Specifically, one might worry that the Opening Hours Act had a normative effect on non-regulating municipalities, causing an upward bias in our estimations. We therefore report results when re-estimating the baseline model while excluding these municipalities (Table B2C in the Online Appendix). These results are also very similar to the baseline results.

Finally, we also report results in which we challenge an implicit assumption in our model, namely, that students are unaffected by the treatment in neighboring municipalities. To do this, we re-estimate the models in Table 3A while controlling for the average treatment in the other municipalities in the same labor market region. The results are reported in Table 3B and show that students' graduation probability is unaffected by the treatment in neighboring municipalities. Considering that students in the sample are quite young, with a limited ability to commute or relocate to find work, this is an intuitive result.

An important question is the extent to which the behavioral response of the cohorts enrolled in high school in the mid-1980s is relevant for the current population and possible future changes in opening hours restrictions. Put another way, are the effects we identify contingent on the specifics of the culture, the economy and the educational system that prevailed in Norway in the 1980s? This question cannot be answered empirically with the data at hand. However, the evidence in Reiling and Strøm (2015) shows a stable positive relationship between high school completion and regional unemployment rates across cohorts enrolled in high school in the

1980s, 1990s and early 2000s. While not a definitive answer to the question of external validity, this finding suggests that the educational response identified here is relevant for changes in labor market opportunities currently experienced by the population. Further, many Western economies have recently undergone changes in opening hours regulations or are in the process of deregulating. Considering the concurrent development in this policy area, our results present auxiliary policy information. The fact that 30 years have passed since the quasi-experiment also benefit our analysis because it allows us to analyze both long-run and short-run effects.

6. Heterogeneity analysis

Concerning possible heterogeneous effects between individual students, we explore two dimensions: parental education and gender. On one hand, students with lower-educated parents are less likely to have an inherent motivation to invest in education and are thus more likely to be affected by increased opportunity cost in terms of more jobs available in the retail sector. On the other hand, if credit constraints are important, students whose parents have low education will be more likely to experience a positive income effect on educational investment. In addition to providing suggestive evidence regarding the potential importance of the opportunity cost argument, it is of interest to distinguish between students with different social backgrounds in the context of intergenerational mobility. While we do not study intergenerational mobility directly, if the educational response to the increase in retail job opportunities is strongest among students with low-educated parents, this response may reduce intergenerational mobility and contribute to income inequality in the long run.

The motivation for allowing heterogeneous effects by gender is partly because retail is viewed as a female-dominated industry and partly because male and female students have been shown to respond differently to policy interventions, as discussed in Pekkarinen (2012). In 1990, 66% of the retail workers aged 16-24 were female[18]. If the jobs created in the retail sector due to extended opening hours are more appealing to female students, the treatment effect could be stronger for females, reflecting gender differences in job preferences. The probability of graduating from high school is, in general, higher among girls than among boys; on average, 56.7% of girls and 51.3% of boys graduate high school. Previous Norwegian evidence in Falch and Strøm (2013) and Reiling and Strøm (2015) show that parental education and gender are

[18] Statistics Norway census for 1990. Employment data by gender, age, and profession are not available for 1980.

powerful predictors for a range of student performance measures, including high school graduation probability, and the baseline models reported above show that students with more highly educated parents and females are more likely to graduate from high school.

Parental education

We re-estimate our baseline model separately for students with parents with two different educational levels: students for whom at least one parent has an educational attainment of high school or lower and students for whom at least one parent has completed a bachelor's degree or higher. The results are reported in Table 4. The first column is the same as Column (2) in Table 3A and is included for reference. Starting with students whose parents are the least educated, in Column (2), it is evident that students with parents with relatively low levels of education are more strongly affected by the treatment, with an estimated interaction coefficient equal to -0.00163. For this group of students, increasing weekly opening hours by 10 hours reduced the probability of graduating from high school within five years by 1.6 percentage points. This effect is approximately 7% stronger than the average effect estimated in Table 3A. For students with better-educated parents, the effect is slightly smaller and is significant at only the 10% level. Thus, there appear to be some differences across students' different parental education: students whose parents have low education are both less likely to graduate high school in general and are slightly more likely to experience a reduction in graduation probability when potential opening hours in retail firms increased.

Gender

In Columns (4) and (5), we report the results from a model in which effects are allowed to differ by gender. The results reported in Columns (5) and (6) show that girls are slightly more affected by the treatment and suggests that increased job opportunities in the retail industry have a slightly larger negative effect on high school graduation probability for girls than for boys, which is consistent with the argument that employment in the retail industry is dominated by females. However, other explanations are possible. The heterogeneous effect could reflect that discount rates differ systematically between genders. However, another explanation could be that an increase in opening hours induces females to spend relatively more time than boys as customers of retail stores at the cost of reduced time spent on school-related activities. Although the data do not enable us to test between the different explanations, we consider the employment channel to be the more likely mechanism.

7. Longer-run outcomes

Education in the long run

Above, we used graduation from high school within five years of completing mandatory schooling as our education outcome. One could argue that this outcome measures the effect on educational choices in the short run and does not necessarily include information on students' long-run educational choices. A possibility is that increased job opportunities for unskilled workers in the retail sector change only the timing of educational investments. Looking at longer time horizons, students might re-evaluate their returns from schooling and make educational investments at a later point in time. To supplement our findings, we therefore use two longer-run measures of education as an outcome variable: completed years of education, as measured at age 40, and high school completion at age 26[19]. Building on the heterogeneity found in the previous section, we also estimate treatment effects when they can differ between students with different parental education and gender. Based on the results for high school graduation, we expect that the students with weaker family backgrounds are most affected by the treatment in terms of completed education.

The estimation results for years of education are reported in Table 5. Column (1) reports the average treatment effect on years of education, while Columns (2)-(3) allow treatment effects to vary by parental education, and Columns (4)-(5) show estimates when the treatment effect can differ between genders.

As shown in Column (1), the average effect on the number of years of education is negative, in line with the previous results. However, this average effect may hide 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 whose highest education is high school or less is -0.005. The effect on students with more highly educated parents is numerically similar but less precisely estimated and not significantly different from zero. A 10-hour increase in weekly opening hours implies a reduction in years of education of approximately 0.05 years. Although the estimated effects are not directly comparable, this finding is substantially lower than that obtained by Lee (2013). She finds that removing restrictions on Sunday shopping decreased the number of years of education by between 0.11 and 0.15 years. Regarding gender differences, we find that the estimated reduction in years of education for girls is twice that found for males

[19] Completed years of education is measured as in Falch et al. (2017), which contains more details on the definition of the variable.

and thus indicates that the gender differences found for high school graduation in Table 4 is reinforced in the long run.

While we find a negative effect on the years of education in the long run, this effect is not easily compared to the effect on the graduation rate. To estimate the long-run effects on education in a measure comparable to our main outcome, we turn to the effect on 10-year graduation rates. That is, we expand the time horizon for student graduation by 5 years, and we estimate the effect of the deregulation on the probability that students graduated by the time they were 26 years old. Nearly all students who ever graduate from high school have done so by the time they reach age 26. We expect the deregulation to have a smaller effect on the long-run high school graduation rate because some students are likely to leave school to work in retail only as a temporary option. The results are reported in Table 6. As expected, the effects are smaller than those when the outcome is graduation within 5 years: the coefficient in Column (1) is approximately 30% smaller than our baseline estimate, which suggests that many students who are induced to drop out of school to pursue work in retail return to their high school studies in a fairly timely manner. It therefore appears that the effect of the deregulation on high school graduation probability is twofold: Two-thirds of the students who dropped out of high school to pursue work never completed high school, and the remaining third postponed their high school graduation. The latter group is also probably less likely to pursue tertiary education due to this postponement. Consistent with the results for completed years of education, the gender difference found for high school graduation in the baseline model is reinforced when using the 10-year window.

Earnings

We have also analyzed the effect of the deregulation on earnings in adult life. Table 7 reports estimated earnings effects at age 39 [20]. While Lee (2013) finds that the repeal of "blue laws" in the US led to a reduction in earnings of approximately 1.2%, we find small negative effects, but they are not statistically significant[21]. However, dividing the point estimate in Column (1)

[20] The earnings measure used is pension-qualifying earnings reported in the tax registry, including labor earnings, sick benefits, unemployment benefits and parental leave payments. Our 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.

[21] To further explore if the earning effects follow the cohort pattern found for graduation probabilities, we report the results when we estimate a models with separate interaction terms for each cohort in Online Appendix Table D2. We also report results for models similar to those estimated in Table 7, where we exclude the 1986 and 1987 cohorts in Online Appendix Table D3, as suggested by a referee. We do not find any effect on earnings in these models, suggesting that the absence of effects reported in Table 7 is robust to alternative specifications.

of Table 7 by the point estimate in Column (1) of Table 5 gives a rate of return to education of 4.6%. Thus, our point estimates on the earnings effect are in line with previous findings of low returns to schooling in Norway and other Scandinavian countries (Trostel et al. (2002) estimate the return to be 4%). On one hand, these findings are consistent with the centralized wage bargaining system in Norway, with little room for individual wages to diverge from those specified in national contracts. On the other hand, the absence of a significant earnings effect is consistent with a broader interpretation of human capital, where individual productivity gains can be acquired by labor market experience and on-the-job training, as well as formal schooling. It is possible that the return to education relative to experience is small: high school students who drop out to pursue work in Norway might be sufficiently rewarded for their additional experience such that they do not lose out on significant earnings.

8. Conclusion

Changes in labor market conditions for unskilled youth can have long-term effects on educational acquisition because they change the opportunity cost of education. This paper quantifies the contribution of changes in labor market opportunities on formal human capital acquisition by exploring quasi-experimental variation in job opportunities originating from changes in the institutional environment of firms in the retail industry. Our quasi-experimental research strategy utilizes a national legislation change in Norway in 1985 that brought the regulation of opening hours in the retail industry to a more uniform level across the country. We show that when shops are freer to expand opening hours, they (1) increase the employment of young and low-skilled workers, which (2) causally reduces high school graduation rates and completed years of education. Interestingly, our results indicate that the effects on both the employment of 16-19-year-old workers and their graduation probability are short-lived. The results therefore suggest that firms responded by increasing their employment of high school students, who consequently dropped out of school, shortly after the deregulation. These students appear to have continued working in the retail sector over time.

We also analyze treatment effect heterogeneity across students with different individual characteristics and show that the negative effect on high school graduation is slightly greater among students with less educated parents and among females. The effect on completed years of education reveals that these gender differences are reinforced in the long run, with effects being small and statistically insignificant for males. This finding is consistent with the fact that retail is a female-dominated industry.

The effects of the specific reform analyzed here are sizeable in terms of both employment and educational outcomes. Although we analyze the effect of a change in a specific sector in Norway some thirty years ago, we argue that the estimates are informative for policy today. In the US, 20% of all employed individuals aged 16-24 are employed in the retail sector (Bureau of Labor Statistics, 2017). Additionally, the retail sector represents one of the few major sectors that still employ less educated workers. Lastly, similar deregulations of the retail sector are being implemented or discussed in several developed economies.

Although our results should be interpreted as reduced-form effects (intent-to-treat-effects), the combined effects on youth employment and educational attainment suggest that increased (decreased) job opportunities for young, unskilled workers can have negative (positive) effects on acquired education. Our findings can be compared with evidence in Leuven et al. (2008) and Falch et al. (2017) indicating that supply-side policies such as class size reductions have negligible effects on short-term outcomes (test scores) and long-term outcomes (educational attainment) in Norway. Taken literally, this finding suggests that education policy discussions should be relatively more concentrated on possible demand-side problems and student incentives than on traditional supply-side policies.

For earnings, the point estimates are negative but numerically small and statistically insignificant; this finding stands in contrast to the evidence in Lee (2013), who finds a substantial negative effect on subsequent adult earnings. A possible explanation for the difference in earnings effects is that wage compression in Norway and other Scandinavian countries – due to high unionization and centralized wage bargaining systems – substantially restricts the extent to which skills and formal education are rewarded in the labor market relative to work experience. In contrast, the absence of significant earnings effects is consistent with a broader interpretation of human capital, where individual productivity gains can be acquired by labor market experience and on-the-job training, as well as formal schooling.

Tables and figures

Figure 1. Frequency of actual store-level closing times nationally in 1985 and 1990. Source: Lavik and Schjøll (2016).

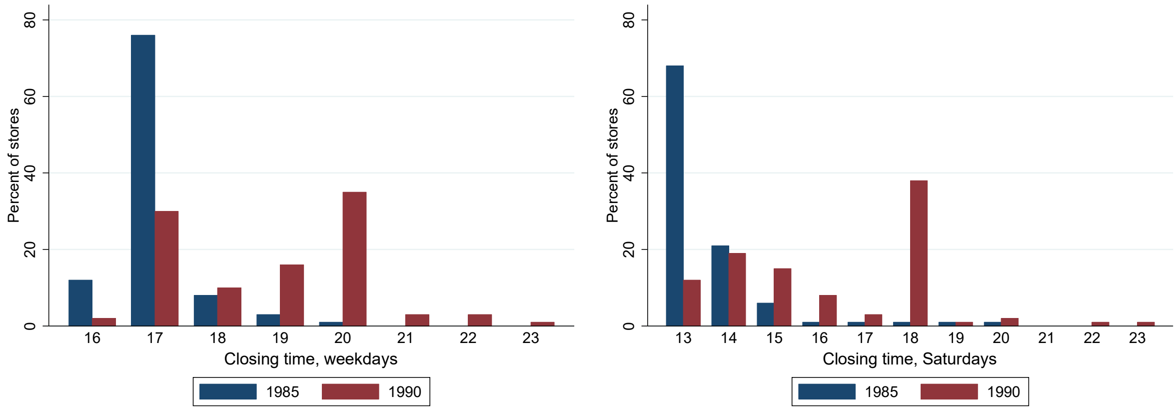

Figure 2. Frequency distribution of increases in allowed weekly opening hours. White indicates a lack of treatment, and darker colors indicate stronger treatment. Source: Norwegian Social Science Data Services.

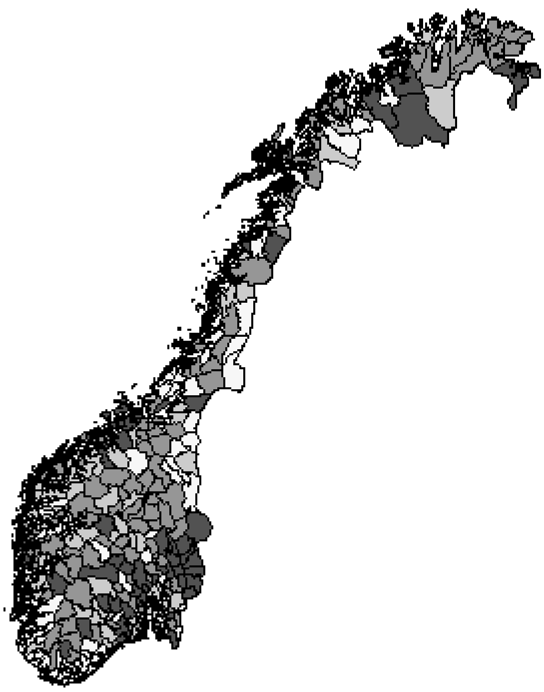

Figure 3. Graphical representation of treatment effects from Column (4) in Table 3A. See notes under Table 3A. Sources: Statistics Norway and The Norwegian Social Science Data Service.

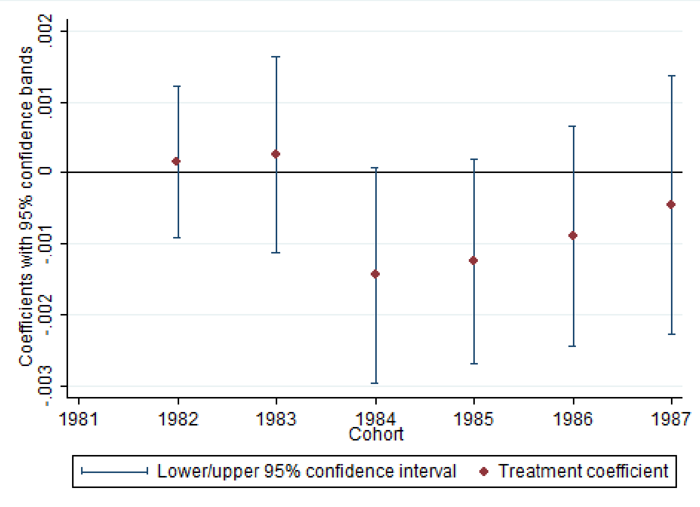

Table 1A. Estimated employment equations for individuals aged 16-24. Complete results are reported in Online Appendix Table A2A.

	(1)	(2)	(3)
	Log(Employment retail)	Log(Employment retail)	Log(Employment manufacturing industry)
	1980-1990	1970-1980	1980-1990
Year 1990	0.00915		0.189
	(0.133)		(0.149)
Hours treated x 1990	0.0107***		0.000397
	(0.00354)		(0.00513)
Year 1980		0.392***	
		(0.142)	
Hours treated x 1980		-0.00294	
		(0.00363)	
log(population)	0.765***	1.069***	-0.483
	(0.287)	(0.329)	(0.350)
Municipality fixed effects	Yes	Yes	Yes
Observations	594	594	544
R-squared	0.984	0.981	0.969
# Municipalities	297	297	272

Outcome variable is the log of the number of employed individuals aged 16-24 in the respective sectors. Regressions include demographic controls (age distribution and ln(population size)) and the political orientation of the mayor. Robust standard errors are given in parentheses. *** p<0.01, ** p<0.05, *** p<0.1

Table 1B. Estimated employment equations for individuals aged 16-19.

	(1) Log(Employment retail) Age 16-19 1980-1990	(2) Log(Employment retail) Age 16-19 1970-1980	(3) Log(Employment industry) Age 16-19 1980-1990
Year 1990	-0.234		0.115
	(0.175)		(0.152)
Hours treated x 1990	0.00696		0.00233
	(0.00486)		(0.00524)
Year 1980		0.443***	
		(0.170)	
Hours treated x 1980		0.000150	
		(0.00565)	
log(population)	0.597	1.509***	-0.467
	(0.372)	(0.483)	(0.355)
Observations	552	552	528
R-squared	0.968	0.955	0.967
# Municipalities	276	276	264

Outcome variable is the log of number of employed individuals aged 16-19 in the respective sectors. Regressions include demographic controls (age distribution and ln(population size)) and the political orientation of the mayor. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, *** p<0.1

Table 2. Descriptive statistics by the treatment status of the municipality.*

	Not treated	Treated	All	p-value on diff.
Graduation rate	55.25	53.84	54.16	0.14
Female	49.07	49.16	49.14	0.75
Share of population aged 16-20	8.22	8.20	8.21	0.88
Share of population aged 60+	18.44	19.94	19.60	0.02
First-generation immigrant	0.30	0.27	0.27	0.46
Second-generation immigrant	0.12	0.10	0.10	0.28
Parental education level				
Mandatory schooling	21.38	23.81	23.26	0.03
High school	59.32	58.55	58.72	0.28
Bachelor's or similar	15.05	13.60	13.92	0.01
Master's or more	4.25	4.05	4.09	0.63
Cohort size in municipality	181.18	195.34	192.15	0.68
Minimum cohort size in period	127.74	134.23	132.77	0.81
Population 1980	11,274.53	13,321.90	12,855.95	0.48
Lagged regional unemp in percent	1.03	1.21	1.17	0.00
Population density	110.75	80.32	86.83	0.37
Mayor left leaning	28.91	46.53	42.56	0.00
Number of municipalities	66	227	293	
Number of students	83,308	309,213	392,521	

*Treatment is defined as opening hours increasing because of the Opening Hours Act. p-values from t-tests on equal means. Source: Statistics Norway and Norwegian Social Science Data Services. Further details in Online Appendix Table E1.

Table 3A. High school graduation. Complete results reported in Online Appendix Table B1A.

	(1) With controls	(2) Regional trends	(3) With controls	(4) Regional trends
Hours treated x cohort > 1983	-0.000897**	-0.00153***		
	(0.000427)	(0.000472)		
Hours treated x cohort 1982			0.000236	0.000161
			(0.000516)	(0.000538)
Hours treated x cohort 1983			0.000430	0.000263
			(0.000632)	(0.000697)
Hours treated x cohort 1984			-0.00118*	-0.00144*
			(0.000706)	(0.000768)
Hours treated x cohort 1985			-0.000932	-0.00124*
			(0.000621)	(0.000731)
Hours treated x cohort 1986			-0.000532	-0.000892
			(0.000671)	(0.000782)
Hours treated x cohort 1987			-5.46e-05	-0.000453
			(0.000703)	(0.000921)
Observations	392,521	392,521	392,521	392,521
R-squared	0.101	0.101	0.101	0.101
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			0.793	0.929
p-value, F-test of same effect, cohorts 84-87			0.248	0.477
# Municipalities	293	293	293	293

Outcome variable is a dummy for completing upper secondary education within five years of completing lower secondary education. Municipality and cohort fixed effects, as well as individual-level controls (gender, immigration status, and parental education level) and municipality-level controls (dummy for the political affiliation of the mayor, the lagged unemployment rate, the shares of the population that are high school age and above 60), are included in all specifications. Standard errors clustered by municipality in parentheses. *** p<0.01, ** p<0.05, *** p<0.1.

Table 3B. High school graduation. Controlling for average treatment in other municipalities in the same labor market region.

	(1) With controls	(2) Regional trends	(3) With controls	(4) Regional trends
Hours treated, cohort > 1983	-0.000964*	-0.00134***		
	(0.000494)	(0.000468)		
Avg. hours treated neighbors, cohort > 1983	0.000172	-0.00128		
	(0.000712)	(0.000807)		
Hours treated x cohort 1982			-7.61e-05	-8.13e-05
			(0.000653)	(0.000711)
Hours treated x cohort 1983			-0.000142	-0.000190
			(0.000808)	(0.000891)
Hours treated x cohort 1984			-0.00151*	-0.00157
			(0.000859)	(0.00106)
Hours treated x cohort 1985			-0.00127	-0.00133
			(0.000781)	(0.00109)
Hours treated x cohort 1986			-0.000742	-0.000805
			(0.000822)	(0.00126)
Hours treated x cohort 1987			-0.000663	-0.000711
			(0.000857)	(0.00150)
Avg. hours treated neighboring municipalities x cohort 1982			0.000734	0.000628
			(0.000988)	(0.00148)
Avg. hours treated neighboring municipalities x cohort 1983			0.00135	0.00119
			(0.000910)	(0.00227)
Avg. hours treated neighboring municipalities x cohort 1984			0.000801	0.000492
			(0.001000)	(0.00324)
Avg. hours treated neighboring municipalities x cohort 1985			0.000821	0.000467
			(0.00106)	(0.00417)
Avg. hours treated neighboring municipalities x cohort 1986			0.000541	0.000136
			(0.00110)	(0.00518)
Avg. hours treated neighboring municipalities x cohort 1987			0.00146	0.00102
			(0.00108)	(0.00619)
Observations	392,521	392,521	392,521	392,521
R-squared	0.101	0.101	0.101	0.101
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			0.985	0.977
p-value, F-test of same effect, cohorts 84-87			0.563	0.735
# Municipalities	293	293	293	293

The outcome variable is a dummy for completing upper secondary education within five years of completing lower secondary education. The variable "Avg. hours treated neighbors" is the average hours treated for the other municipalities in the same labor market region. Municipality and cohort fixed effects, as well as individual-level controls (gender, immigration status, and parental education level) and municipality-level controls (dummy for the political affiliation of the mayor, the lagged unemployment rate, the shares of the population that are high school age and above 60), are included in all specifications. Standard errors clustered by municipality in parentheses. *** p<0.01, ** p<0.05, *** p<0.1.

Table 4. Heterogeneous effects by parental education (Columns (2)-(3)) and gender (Columns (4)-(5)). Complete results reported in Online Appendix Table C1.

	(1)	(2)	(3)	(4)	(5)
	Full sample	Parents have high school or less	Parents have more than high school	Girls	Boys
Hours treated x cohort > 1983	-0.00153***	-0.00163***	-0.00150*	-0.00181***	-0.00121*
	(0.000472)	(0.000534)	(0.000767)	(0.000585)	(0.000627)
p-value for t-test for equal coefficients		0.8841		0.4203	
Observations	392,521	305,253	87,268	192,973	199,548

The outcome variable is a dummy for completing upper secondary education within five years of completing lower secondary education. Municipality and cohort fixed effects, as well as individual-level controls (immigration) and municipality-level controls (dummy for political affiliation of the mayor, lagged unemployment rate, share of population of high school age and above 60), are included in all specifications. Regressions by parental education also include a gender dummy, and the model in Column (2) controls for the highest parental education being high school, while Column (3) controls for the highest parental education being a master's degree or similar. Regressions by gender include parental controls for parental education level. Standard errors clustered by municipality in parentheses. *** p<0.01, ** p<0.05, *** p<0.1

Table 5. Completed years of education. Complete results are reported in Online Appendix Table D1.

	(1) Full sample	(2) Parents have high school or less	(3) Parents have more than high school	(4) Girls	(5) Boys
Hours treated x cohort > 1983	-0.00479***	-0.00465**	-0.00572	-0.00606**	-0.00333
	(0.00181)	(0.00189)	(0.00397)	(0.00264)	(0.00241)
p-value for t-test for equal coefficients:		0.7979		0.4294	
Observations	392,313	305,079	87,234	192,874	199,439
R-squared	0.177	0.066	0.052	0.180	0.178
# Municipalities	293	293	293	293	293

The outcome variable is completed years of education at age 40. Municipality and cohort fixed effects, as well as individual-level controls (immigration) and municipality-level controls (dummy for the political affiliation of the mayor, the lagged unemployment rate, the shares of the population that are high school age and above 60), are included in all specifications. Regressions by parental education also include a gender dummy, and the model in Column (2) controls for the highest parental education being high school, while Column (3) controls for the highest parental education being a master's degree or similar. Regressions by gender include parental controls for parental education level. Standard errors clustered by municipality in parentheses. *** p<0.01, ** p<0.05, *** p<0.1

Table 6. Longer-run high school completion.

Long-run high school completion	(1) All	(2) Parents have high school or less	(3) Parents have more than high school	(4) Girls	(5) Boys
Hours treated, cohort > 1983	-0.00105**	-0.00111**	-0.00118*	-0.00158***	-0.000517
	(0.000430)	(0.000495)	(0.000673)	(0.000582)	(0.000514)
p-value for t-test for equal coefficients:		0.9230		0.1164	
Observations	392,521	305,253	87,268	192,973	199,548
R-squared	0.091	0.053	0.018	0.101	0.088
# Municipalities	293	293	293	293	293

The outcome variable is completing upper secondary education within ten years of completing lower secondary education. Municipality and cohort fixed effects, as well as individual-level controls (immigration) and municipality-level controls (dummy for political affiliation of the mayor, lagged unemployment rate, the shares of the population that are high school age and above 60), are included in all specifications. Regressions by parental education also include a gender dummy, and the model in Column (2) controls for the highest parental education being high school, while Column (3) controls for the highest parental education being a master's degree or similar. Regressions by gender include parental controls for parental education level. Standard errors clustered by municipality in parentheses. *** p<0.01, ** p<0.05, *** p<0.1

Table 7. Log(earnings) at age 39.

	(1) ln(earnings at 39)	(2) ln(earnings) at 39) Parents have high school or less	(3) ln(earnings at 39) Parents have more than high school	(4) ln(earnings) at 39) Girls	(5) ln(earnings) at 39) Boys
Hours treated x cohort > 1983	-0.000218	-4.70e-05	-0.000909	-0.000474	-1.94e-05
	(0.000602)	(0.000630)	(0.00120)	(0.000813)	(0.000714)
First-generation immigrant	-0.0812***	-0.0487	-0.162***	-0.0699*	-0.0879***
	(0.0236)	(0.0304)	(0.0401)	(0.0359)	(0.0244)
Second-generation immigrant	-0.0338	0.00944	-0.121	-0.0780	0.0105
	(0.0452)	(0.0343)	(0.0971)	(0.0609)	(0.0479)
Parents' highest education: more than high school	0.244***			0.253***	0.235***
	(0.00399)			(0.00643)	(0.00713)
Parents' highest education: High school or less	0.120***	0.120***		0.123***	0.116***
	(0.00343)	(0.00343)		(0.00471)	(0.00478)
Parents' highest education: Master's or similar	0.0822***		0.0822***	0.0788***	0.0846***
	(0.00684)		(0.00693)	(0.0116)	(0.00841)
Female	-0.420***	-0.424***	-0.406***		
	(0.00719)	(0.00802)	(0.00841)		
Share of pop. between 16 and 20	0.263	-0.364	2.782**	0.876	-0.275
	(0.510)	(0.592)	(1.241)	(0.756)	(0.684)
Share of pop. above 60	-0.366	-0.576	0.0230	-0.456	-0.301
	(0.403)	(0.451)	(0.868)	(0.570)	(0.489)
Leftist mayor	-0.00915	-0.00790	-0.0230	-0.00769	-0.0109
	(0.00692)	(0.00684)	(0.0154)	(0.00905)	(0.00975)
Regional unemp. last year	0.0414	0.190	-0.510	1.117	-0.857
	(0.643)	(0.709)	(1.521)	(1.038)	(0.786)
Observations	362,976	282,528	80,448	177,895	185,081
R-squared	0.134	0.128	0.107	0.060	0.055
# Municipalities	293	293	293	293	293

The outcome variable is the log of earnings at age 39 in inflation-adjusted Norwegian kroner. Municipality and cohort fixed effects, as well as individual-level controls (immigration status) and municipality-level controls (dummy for the political affiliation of the mayor, the lagged unemployment rate, and the shares of the population that are high school age and above 60), are included in all specifications. Regressions by parental education also include a gender dummy. Regressions by gender include parental controls for parental education level. Standard errors clustered by municipality in parentheses. *** p<0.01, ** p<0.05, *** p<0.1

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Supporting Information

Additional supporting estimations and information can be found in the Online Appendix at the journal's web page.