Discussion paper

The effect of school consolidation on student achievement

BY
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The effect of school consolidation on student achievement[*]

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Abstract

Many countries have seen a substantial increase in the average school size over the past decades, and a corresponding reduction in the number of schools. It has been widely argued that both students and local communities have suffered from these consolidations. Despite their vast extent and controversy, the literature is scarce with evidence of how consolidation affects performance of exposed students. This study explores how consolidation of 76 rural lower secondary schools in Norway affects students' educational performance, using rich register data. I find no indication that school consolidation reduces educational achievement. Nonetheless, the analysis suggests a negative association between school closure and grade point average, but this effect appears to be driven by lenient grading in small schools.

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1 Introduction

Few political issues involve more emotions and local engagement than the closure of a neighborhood school. Consolidations have large impact on the environment in which affected students undergo their daily schooling. Evidence from various countries suggests vast school consolidation over the past decades, and increasingly so in recent years.[1] The extent of school closures emphasizes the importance of the issue. To the degree that consolidation affects achievement, it has important implications for the accumulation of human capital.

Opponents to consolidation argue that closures may impair students' learning. There are various mechanisms through which consolidation may cause a permanent reduction in the quality of education. Consolidation involves an increase in school size. The possible sources of a negative scale effect include a likely reduction in the teacher-to-student ratio, which may limit the scope to adapt teaching and supervision. Closure often concerns schools with a low number of students, and a single class at each grade level. Consolidation is thus likely to increase affected students' number of classmates. To the extent that small classes provide a better learning environment, consolidations may weaken school quality. For students at a closing school, consolidation usually implies a longer travel. This may cause exhaustion, and reduce the time available for school work at home. According to *The National Society for Neighbourhood Schools* (LUFS, 2008), children pay the price for school closures: "Centralization will result in longer working hours, reduced learning outcomes, weaker understanding of the local community, decreased health, less leisure time and less cooperation between parents and school."

School mergers involve a permanent change in the learning environment. Some consequences of closure may be transient, however, and specific to students who are transferred from one school to another at closure. The potential sources of such disadvantage include the adaption to new teachers, peers, curricula and routines. Although schools' coordination problems are temporary, the consequences of disruption may be long-term for exposed students.

Although there are possible disadvantages associated with consolidation of schools, scale effects may also improve the quality of education. Larger units have a larger teacher staff, and hence hold more diverse teacher competency. This allows for more efficient matching of teachers,

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[1]In Norway, the number of primary and lower secondary schools has decreased by twelve percent during the past ten years, while the average school size increased by thirteen percent. Statistics Norway (2013), Pupils in primary and lower secondary school, http://www.ssb.no/en/utdanning/ statistikker/utgrs/aar/ 2013-12-13?fane=tabell&sort=nummer&tabell=152677

students and subjects. It is also possible that larger units are able to attract better teachers. In addition to peer and scale effects, there might be a positive resource effect associated with consolidation. OECD's Economic Survey for Norway (OECD, 2008) recommends a reduction in the number of schools, to free resources for quality enhancement. According to the OECD, small school size emerges as one of the principal factors explaining relative educational inefficiency in Norway.

While the effect of school closure is widely debated in public, the academic literature is scarce with evidence of how consolidation affects student performance. The limited existing evidence explores institutional settings, demographies and data that differ vastly from this study. This paper aims to reveal the net effect of school consolidation on student achievement, by exploring 76 closures from 1989 to 2009. Explored schools are located in rural areas, and provide lower secondary education.[2] In order to address the potential bias caused by selected migration, I employ family specific fixed effects. The analysis benefits from extensive data on lower secondary education. In combination with rich Norwegian register data, this enables observation of a more differentiated exposure to closure than the previous literature. Most previous studies focus on students who are transferred from one school to another at closure. This study also incorporates the effect of closure for students who would be enrolled at the closing school in absence of the consolidation, but instead undergo their entire schooling at the replacing school. This is arguably the effect that is most important in the long run. My analysis also addresses effects of consolidation for students at the receiving schools, i.e., the schools to which students at closing schools are transferred. In addition, detailed register data enables analysis of how consolidation affects both short and long term educational outcomes.

My findings suggest that potential positive and negative effects of consolidation offset each other. The results leave no reason to believe that school consolidation is either detrimental or beneficial for affected students. However, the analysis reveals that school closure involves a reduction in the grade point average (GPA). Estimation of the relative grading practice in small schools suggests that the negative effect of consolidation on GPA is driven by lenient grading in small schools. This is consistent with previous evidence of classwork grading in Galloway,

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[2]There are two reasons for the focus on rural districts. First, consolidations are most common in rural areas. Second, most of the controversy regarding closures concerns rural schools. Rural schools play an important part in the local community and labor market, and are typically located far from alternative options.

Kirkebøen and Rønning (2011).[3] A decline in the GPA may be perceived as a reduction in the underlying performance, and hence contribute to local resistance to consolidation.

The remainder of this paper is organized as follows. Section 2 provides a review of relevant literature. The applied data are described in Section 3, whereas Section 4 presents the empirical approach. Results are discussed in Section 5, while Section 6 offers some concluding remarks.

2   Literature

Although school closure is a controversial and long-debated issue, there is little research on the consequences for affected students. Liu, Zhang, Luo, Rozelle and Loyalka (2010) evaluate the impact of primary school mergers in rural China. Using a survey designed to examine the effect of closure, they conclude that the mergers do not impact students' academic performance. Nevertheless, students' grade level at exposure seems to matter. While students in the fourth grade responds positively to the mergers in both Chinese and Mathematics, the performance of second graders fall.

Engberg, Gill, Zamarro and Zimmer (2012) explore the effect of consolidation in an urban area where low performing schools are targeted for closure. Although the analysis suggests that transferred students experience a negative effect on test scores and attendance, this effect is reduced by transition to a higher-performing school. These results are largely consistent with Brummet (2012), who explores a sample of urban and rural primary schools in Michigan. Brummet finds that school closure does not persistently harm the performance of exposed students. Displaced students may benefit from the transition to a higher performing school, but these gains are partly offset by negative effects at receiving schools. Beuchert, Humlum, Nielsen and Smith (2016) observe individual test scores over time, an are hence able to explore the short-term effect of consolidation. Beuchert et al. evaluate the effect of local school reforms that involved vast consolidation of Danish primary schools. They estimate a negative effect of consolidation, in the order of 2.5 percent of a standard deviation.

Several other strands of research are closely related to school consolidation, including the literature on school size. Kenny and Schmidt (1994) explore the trade-off between cost savings

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[3]Møen and Tjelta (2010) observe a similar tendency in higher education; study programs with many students have a stricter grading than small programs.

and a diverse population's demand for choice in public schooling. They conclude that the decline in the number of school districts in the US has been driven by both demographic and institutional change. Berry and West (2008) explore the effect of school and district size on student outcomes. They find that students from states with smaller schools tend to pursue longer educations, and earn higher returns to education.

Hanushek's (1986) literature review summarizes the early evidence of the relationship between resources and achievement in education. The review concludes that there is no causal association between extra resources and student performance. Later contributions challenge this conclusion. One strand of research explores the effect of class size. Angrist and Lavy (1999) and Krueger (1999) made early and important contributions to this literature. While Angrist and Levy exploited class size rules in Israel, Krueger (1999) conducted a field experiment where students were randomly assigned to classes of different sizes. Despite the different approaches, both Angrist and Lavy (1999) and Krueger (1999) find a negative association between class size and student performance. Bonesrønning (2003) and Leuven, Oosterbeek and Rønning (2008) exploit class size rules to study the effect of consolidation in Norwegian data. Although Bonesrønning finds a negative effect from increases in class size, the magnitude of the coefficient is small, and only statistically significant for subgroups of students. Leuven et al. (2008) do not find any significant evidence of a class size effect.

Research on the effect of class size typically considers outcomes that are measured during schooling, or shortly after. A study by Fredriksson, Öckert and Oosterbeek (2012) investigates the long-term effects of class size in primary schools. Fredriksson et al. reveal a strong and persistent benefit from low class sizes.

The past literature on class size does not necessarily reflect effects that are relevant for my analysis, as class sizes in this study are substantially lower than in previous research. The cited studies exploit maximum class size rules of about 30 students, while the median cohort size of closing schools in my study is 5. The receiving schools are also relatively small in my sample. One could easily imagine that the effect of class size on student performance is non-linear, and even positive at parts of the class size distribution.

Leuven and Rønning (2016) explore how experience from a mixed grade classroom affects performance. Mixed grade classrooms are a common practical adaption to low school size.

Many of the consolidations explored in this paper are thus likely to involve change from a mixed classroom to one with separate grade levels. Leuven and Rønning conclude that students benefit from mixed grade classrooms. The positive effect from sharing the classroom with older students exceeds the observed disadvantage from being mixed with younger peers.

The trade-off between scale and competition is a common perspective in the school size literature. Consolidation involves lower school density and hence less choice and competition. De Haan, Leuven and Oosterbeek (2016) exploit a Dutch reform that reduced the number of primary schools. They find that the increase in the minimum required school size has a small positive effect on student achievement, i.e., that the consolidations do not harm school quality. De Haan et al. conclude that the disadvantage of less competition is offset by a positive scale effect. Brasington (1997, 1999, 2003) is also concerned with the trade-off between school size and competition. In Brasington (1997), numerous covariates are utilized to explore how school size and district size affect graduation rates. Brasington (1999) explores area characteristics that are associated with school mergers, while Brasington (2003) assesses how city size influences the decision to consolidate schools.

Although the competition for students may encourage increased quality of education in many institutional settings, it is probably less so in Norwegian compulsory education. Different Norwegian primary or lower secondary schools can hardly be seen as substitutes, both due to the institutional and the geographical setting. Almost all schools are public, and students are assigned to a school based on their home address. It is also useful to recall that schools included in this analysis are located in rural areas, and in most cases relatively far from alternative options. Even if the institutional setting was different, there would hence be little scope for competition in this part of the sector. One may argue that the setting explored in this analysis does not stimulate the potential benefits of school competition. Everything else equal, the institutional setting may therefore imply a higher gain from larger units than in settings with more school choice.

3 Data

I use data from *The Norwegian Directorate for Education and Training.* The data provide information about which lower secondary school individuals graduate from. This information

is available for graduates from 1975 through 2010. Potential closures are observed when the appearance of a school unit ends from one year to the other. Candidates to actual closures are then manually cross-checked against newspaper archives and official school registers.[4]

The focus on lower secondary schools is partly due to availability of data, and partly because the students are approaching a level where subject specific teaching may be as important as the teachers' general educational skills. Lower secondary education has the additional advantage that it only takes three years to finish. The relatively short duration increases the variation in exposure within families, which is useful in the empirical analysis.

Data on lower secondary schools include a grade point average for students graduating later than 2001. The GPA is calculated as an average of eleven subject grades. Students are awarded a grade for classwork in each subject. They are also drawn to do a written exam in either English, Mathematics or Norwegian, and an oral exam in one subject. Subject grades are calculated as the average of the classroom grade and exam grade(s).[5] Grades are awarded on a scale from one to six, where six is highest. The GPA offers a credible measure of actual performance, and it determines admittance to upper secondary school.

The grade point average is not the only outcome from lower secondary education that is explored in this analysis. Written exam results are also assessed in separate regressions. Exam grades are available for cohorts graduating between 2002 and 2009. Both the GPA and exam grades are observed at completion of lower secondary school, and hence reflect short-term effects of the education at lower secondary school. The data also enable analysis of the persistence of potential effects. Most students start their upper secondary school education shortly after graduation from lower secondary school. Upper secondary school track is hence chosen shortly after graduation from lower secondary school, but it is not observed until the completion of upper secondary school. Another explored outcome, the probability of completing upper secondary school, is of course also observed at the completion of upper secondary school. This measure has proven to be a good indicator of labor market outcomes, see e.g., Falch and Nyhus (2011).

Students' highest level of education is the most long-term outcome that is explored in this analysis. This measure involves a trade-off between sample size and the degree of completion.

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[4] "Grunnskolens Informasjonssystem"

[5] Students receive a total of 13 grades by the subject teachers, but the average of oral and written evaluation is weighted as one grade in English and Norwegian. There are two forms of the Norwegian language, however, and both account for a separate grade in the GPA. Students who are drawn to do a written exam in Norwegian conduct two exams, one in each form of the language.

In the analysis, education is observed at age 26. There is hence an eleven year lag between graduation from lower secondary school and observed education. Completed education is thus observed for students graduating from lower secondary school between 1975 and 1999.[6] The *level* of education relates closely to the *years* of education. I have chosen to focus on the level of education for two reasons. First, and most important, the level of education is observed for a larger sample. Second, levels offer a better reflection of the differentiated scale of educational opportunities. In contrast to years of education, increases have a meaningful interpretation across the scale, as each increase is associated with education at a higher level. The level of education is defined by gradual increases from one to a maximum of eight. The different levels are defined as follows:

Level 1: Primary school

Level 2: Lower secondary school

Level 3: Upper secondary school, 2 years (Typically vocational track)

Level 4: Upper secondary school, 3 years (Typically academic track)

Level 5: College/ university, two years

Level 6: College/ university, equivalent to bachelor degree

Level 7: College/ university, equivalent to master degree

Level 8: PhD

In addition to various educational data, the empirical approach rests on the opportunity to observe kinship. The register data include extensive information about family and student characteristics. As this study aims to explore schools in rural areas, it is useful to assess some summary statistics of demographics. Table 1 offers insight into the municipalities in which the closing schools are located. The table also holds unaffected municipalities, for comparison. As expected, affected schools are located in relatively small municipalities. With regard to earnings and education, there are no signs of divergence between affected and unaffected municipalities. This may seem surprising, as both the level of education and earnings are expected to be higher in urban areas. Note, however, that education and earnings occur through average levels for each municipality. Municipalities are not weighted by population. The relatively few urban

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[6]The sample also holds five students who graduated from lower secondary school in year 2000.

Table 1: Municipalities, summary statistics

	Mean	Std.dev.	Median	Min	Max	1986-2010
Population, affected municipalities	5,458	(9,893)	3,110	670	59,796	13.7%
Population, unaffected municipalities	8,673	(27,412)	3,300	154	455,087	18.7%
Avg. years of education, affected municipalities	11.0	(0.5)	11.0	10.1	12.1	18.3%
Avg. years of education, unaffected municipalities	11.1	(0.4)	11.1	9.4	12.7	15.6%
Avg. earnings, affected municipalities	345.9	(48.9)	345.6	222.1	460.1	69.9%
Avg. earnings, unaffected municipalities	343.9	(59.0)	343.4	143.6	961.7	68.4%

Some municipalities have changed and merged over the period of our study. This table refers to municipalities as defined in 2010. Population refers to people of at least 16 years of age. Average years of education is based on individuals of at least 30 years of age, to capture completed education. Earnings are calculated at age 33. This particular age is chosen in order to minimize lifetime earnings bias, in accordance with Bhuller, Mogstad and Salvanes (2017).

municipalities are hence balanced out by the numerous small ones.

Summary statistics of the municipalities that are observed with a relevant consolidation yields insight into regional characteristics. The empirical analysis is not conducted at municipality level, however, but at school and family level. There are two types of exposures for affected schools, referred to as "closing" and "receiving" schools, repectively. Whereas some schools close, others receive students from the closing schools. Both exposures may potentially change the performance of affected students. A thorough review of the different exposures follows in the presentation of the empirical strategy. In Table 2, I present some summary statistics for affected schools. Closing schools have a median cohort size of only five students in their last year before closure.[7] The median size of receiving schools is almost six times larger, even before the absorption of additional students. For comparison, the corresponding numbers for all providers of lower secondary education are included in the bottom row. The table shows that affected schools in this study serve small districts, in areas with a low population density. The number of families affected by each closure is therefore relatively low, which implies a surprisingly small sample size, given the number of explored consolidations.

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[7]The small school size at the year of closure could partly be due to a gradual closure of the unit, but even five years prior to the closure, the median cohort size is only six.

Table 2: Cohort size, affected schools

| | No. of schools | Cohort size | | | | |
		Mean	Std. dev	Median	Min	Max
Closing schools	76	8.5	(9.8)	5	1	54
Receiving schools	55	33.1	(24.2)	28	1	102
All lower sec. schools	1,239	51.3	(46.1)	38	1	212

Cohort size is measured the year before consolidation. The number of students in the receiving school is hence exclusive of students from the closing school. Students from one closing school may be transferred to more than one receiving school. In the table, "receiving" refers to the school that receives the highest number of students. "All lower secondary schools" refers to all schools that graduate students from lower secondary education in 2010. Some of these offer both elementary and lower secondary education. The total number of schools may be somewhat higher, as inclusion is conditional on observation of at least one graduate in 2010. The relatively low number of receiving schools reflects the tendency that some receiving schools replace more than one closing school.

4 Empirical strategy

In order to explore the effects of consolidation, students graduating prior to a closure are compared to those exposed to school closure. As consolidations typically follow from demographic trends, it is necessary to account for the possibility that migrating families are not randomly selected. Whereas consolidation follows from demographic trends, closures may also cause demographic change. This simultaneity does not only reflect an empirical challenge related to estimation of the effect of closure. It also addresses some of the externalities associated with school closures. The sources for potential selected migration are twofold. First, the long-term urbanization may change the composition of academic skills in an area over time. If migrators are positively selected, observed reduction in performance may not be caused by the consolidation. Second, school closure may cause migration per se. It is possible that families' response to (expected) consolidation reflects their attitude towards education, and correlate with academic skills.

In the event that affected areas are subject to selected migration, the observed performance could change regardless of the consolidation. It would hence be difficult to separate the effect of closure from the counterfactual trend. In order to address this potential bias, I employ family specific fixed effects. The family fixed effects ensure that estimates do not suffer from a changing sample of families over time. Estimation within families hence controls for many of

the unobserved characteristics that could potentially bias the results.[8]

The detailed data allow for differentiation of exposures to consolidation. Direct exposure applies to students who would attend the closing school in the absence of its closure. By the construction of data, and by the empirical strategy, this requires that the student has an older sibling who attended the closed school prior to its closure. Indirect exposure applies to students who would attend the receiving school regardless of the consolidation. Effects for students at receiving schools are identified from families who are observed with graduates at a receiving school both prior to and after the consolidation. Whereas location and facilities remain unchanged, this exposure entails an increase in class size, and a potential disturbance to the social and academic environment. The isolation of one set of mechanisms offers a useful reference for directly affected students.

I distinguish between "full" and "partial" exposure. Fully exposed students graduate at least three years after the consolidation. They hence undergo their entire lower secondary education at the receiving school, regardless of which school they would have attended in the absence of a closure. Partly exposed students graduate less than three years after the consolidation. If they were assigned to the closing school, they were hence transferred to the receiving school during lower secondary school. Although the effects of consolidation may be sensitive to the extent of exposure, the direction of a possible difference is ambiguous. Partial exposure might limit potential effects of consolidation. On the other hand, a transition during lower secondary school might involve additional challenges. Potential difficulties can arise due to the adaption to new teachers, peers, curricula and routines. It is possible that the decision to consolidate schools is associated with additional resources at transition. This could offset potential negative effects of exposure through the transition.

The data hold families that are observed with graduates from a closing or receiving lower secondary school both before and after the consolidation, i.e., families who contribute to the identification of main effects. I use the following specification to estimate the effect of closure by family fixed effects.

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[8]As main effects are estimated within families, single-child families do not contribute to their identification. Unreported analysis shows the relative ability of single-child families, and how their exclusion may affect results and external validity. Students without siblings perform significantly lower than students from families with two or three children, and somewhat better than families with more than three children. The exclusion of families with one child will cause a minor overestimation of the true average performance. With regard to estimated main effects, the exclusion of single-child families may affect coefficients if such families have different mobility response to (an expected) consolidation.

$$PERFORMANCE_{ifc} = \beta_0$$
$$+\beta_1 CLOSING * PARTLY\ EXPOSED_{if} + \beta_2 CLOSING * FULLY\ EXPOSED_{if}$$
$$+\beta_3 RECEIVING * PARTLY\ EXPOSED_{if} + \beta_4 RECEIVING * FULLY\ EXPOSED_{if}$$
$$+\beta_5 f(BIRTH\ ORDER_i) + \beta_6 f(COHORT_c) + \beta_7 FEMALE + FAMILY_f + \epsilon_{ifc}$$

where i, f and c denote individuals, families and cohorts, respectively. The included components are defined as follows:

$PERFORMANCE$: Five different measures of educational achievement, as presented in Section 3.

$CLOSING$: This dummy variable refers to families observed with at least one graduate from a school that is later to be closed.

$RECEIVING$: This dummy variable is one for families with at least one graduate from the receiving school prior to the relevant closure.

$PARTLY\ EXPOSED$: This dummy variable is one for students who would graduate from a closing or receiving school in absence of consolidation, less than three years after consolidation.

$EXPOSED$: This dummy variable is one for students who would attend a closing or receiving school in the absence of consolidation, three or more years after the respective consolidation.

$BIRTH\ ORDER$: Birth order effects are addressed by separate dummies for the second and third sibling, in addition to a joint dummy for siblings of fourth or higher order. Previous research, e.g., Black, Devereux and Salvanes (2005), reveals strong birth order effects.

$COHORT$: Regressions include a second order polynomial on birth cohort, to account for a possible common trend in achievement (e.g., due to an increasing level of education).

$FEMALE$: Control for gender differences is captured by a dummy with value one for female students.

The causal effects of consolidation are extracted from β_1, β_2, β_3 and β_4. Most previous studies have focused on students who experience a closure during schooling, i.e., the treatment captured by β_1. My analysis holds two additional dimensions of treatment. First, I observe students at receiving schools. Second, I observe students who would attend the closed school in the absence of a closure, but undergo their entire schooling at the replacing school.

5   Empirical analysis and results

Table 3 illustrates how the explored measures of educational performance vary with exposure to closure. Obviously, these measures cannot be given a causal interpretation, but it is interesting to note that full attendance to a closing school is associated with low relative performance at virtually all outcomes.

Table 3: Summary statistics: Outcomes by treatment status

	Lower sec., GPA	Lower sec., anonym. exam	Prob. of upper sec. completion	Prob. of acad. upper sec. track	Level of educ. by age 26
Not affected by closure	3.87 (.85)	3.44 (1.05)	.631 (.483)	.372 (.483)	4.03 (1.53)
Closing * Partly exposed	4.03 (.70)	3.45 (.96)	.671 (.471)	.333 (.471)	3.53 (1.49)
Closing * Fully exposed	3.55 (.88)	3.21 (1.06)	.622 (.485)	.342 (.475)	3.37 (1.47)
Receiving * Partly exp.	3.89 (.81)	3.36 (1.07)	.659 (.474)	.339 (.473)	3.49 (1.55)
Receiving * Fully exp.	3.91 (.81)	3.27 (1.04)	.681 (.466)	.355 (.479)	3.84 (1.53)

Standard errors in parentheses. By construction of data, the unaffected students graduate prior to the analyzed consolidations. Differences across treatments might therefore be confused with time trends.

OLS results from estimated equations are presented in Table 4. It is difficult to draw a consistent pattern out of these results, and most coefficients are insignificant. Consolidation appears to be associated with reduced GPA for students at both closing and receiving schools. Students who are fully exposed to a closing school perform significantly worse than students without exposure to the consolidation. This holds for both of the short term outcomes, i.e., both GPA and results at anonymously graded exams.

In the lower panel of Table 4, family fixed effects are introduced to control for a possible bias caused by demographic trends. Coefficients are mainly small and insignificant, and the main tendency is the lack of a consistent and significant effect on medium and long term outcomes. As for the short-term effect of consolidation, the significant negative coefficients in the first column suggest that consolidations reduce affected students' results in lower secondary school. For fully affected students, school closure is associated with a GPA reduction of .344 relative to unaffected students. This corresponds to a reduction of one grade in almost one third of the subjects. Partly exposed students, i.e., students who are transferred from one school to another at closure, also suffer from the consolidation. The effect associated with partial exposure is somewhat lower than that associated with full exposure.

Table 4: Results, OLS and FE

Panel A: OLS

	(1) Lower sec., GPA	(2) Lower sec., anonym. exam	(3) Prob. of upper sec. completion	(4) Prob. of acad. upper sec. track	(5) Level of educ. by age 26
CLOSING * PARTLY EXP.	0.0197	0.0671	0.00825	0.0104	0.0785
	(0.0642)	(0.0695)	(0.0252)	(0.0245)	(0.112)
CLOSING * FULLY EXP.	-0.177***	-0.172***	-0.0214	0.0360	-0.0529
	(0.0549)	(0.0664)	(0.0240)	(0.0224)	(0.108)
RECEIVING * PARTLY EXP.	-0.148***	-0.0141	0.00103	0.0205	0.0788
	(0.0510)	(0.0544)	(0.0183)	(0.0175)	(0.0768)
RECEIVING * FULLY EXP.	-0.131**	-0.0938	0.0214	0.0527***	0.140*
	(0.0567)	(0.0600)	(0.0191)	(0.0181)	(0.0839)

Panel B: Family fixed effects

	(1) Lower sec., GPA	(2) Lower sec., anonym. exam	(3) Prob. of upper sec. completion	(4) Prob. of acad. upper sec. track	(5) Level of educ. by age 26
CLOSING * PARTLY EXP.	-0.294***	-0.225**	-0.0228	-0.0420	-0.0296
	(0.0809)	(0.105)	(0.0286)	(0.0284)	(0.115)
CLOSING * FULLY EXP.	-0.344***	-0.00620	-0.0162	-0.00130	-0.0725
	(0.0750)	(0.120)	(0.0289)	(0.0280)	(0.113)
RECEIVING * PARTLY EXP.	-0.122**	-0.00583	-0.0278	-0.0297	-0.0447
	(0.0586)	(0.0663)	(0.0210)	(0.0208)	(0.0825)
RECEIVING * FULLY EXP.	-0.178**	-0.0506	0.000463	-0.0163	-0.0496
	(0.0815)	(0.104)	(0.0252)	(0.0246)	(0.102)
Observations	2,513	3,223	8,995	9,438	6,057
Obs., closing * partial exp.	201	275	428	442	231
Obs., closing * full exposure	569	487	640	728	327
Obs., receiving * part. exp.	498	720	991	1,046	539
Obs., receiving * full exposure	621	551	1,142	1,291	557

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. In OLS regressions, standard errors are clustered on family. Birth order dummies and a second order polynomial on cohort are included in the regressions, but not reported. (5) refers to the one-digit education code from NUS2000. The number of observations is the same for the two empirical approaches.

Whereas coefficients remain negative in column (2), most are insignificant. The only exposure with a significant effect apply to students with partial exposure to a closing school. In other words, transfer across schools at closure has a negative effect on performance at externally evaluated exams.

Consolidation typically involves a transfer to a larger school. To the extent that smaller schools set better grades, families may wrongly conclude that the transition to a larger school reduces the underlying performance. This is a possible interpretation of regressions (1) and (2) in Panel B of Table 4. All exposures to consolidation involves a significant reduction in the GPA. As for anonymously graded exams, a reduction is only significant for one exposure to consolidation; partial exposure to a closing school.

The hypothesis that small schools have a lenient grading practice is addressed by Galloway et al. (2011). They reveal that small schools tend to overestimate student performance. As most closed schools in my sample are very small, this deserves to be addressed. In my data, the grading practice can be observed as the deviation between the grade set by the subject teacher, and performance on externally graded exams. Table 5 illustrates how the grading in small schools differs from the practice in larger units. The deviation between classroom grade and exam grade in a subject is regressed on a set of school size dummies. Dummy coefficients denote the deviation from schools with a cohort size of five or fewer students.

Both the magnitude and the significance of coefficients suggest that grading is more lenient in small schools. The constant term indicates that students in schools with five or fewer students score .38 of a grade higher on classwork than on the anonymous exam. In larger schools, almost half of this gap is closed. Differences appear to be highly non-linear. There is no statistical difference across lower school sizes than 10. The marginal effect of cohort size on grading practice is evident for variations from 10 to roughly 20 students. The grading at schools with a cohort size of 21-30 students does not appear to deviate from the grading at larger schools.

A thorough analysis of the effect of school size on grading practice is beyond the scope of this analysis. The demonstrated association cannot be taken as causal, but it identifies a pattern that corresponds with other findings in this study, and with relevant research. In most of my regressions, school size is not included as a control. The analysis aims to find the net effect of consolidation. A control for school size would target one of the mechanisms through which

Table 5: Grade setting and cohort size

	Grade deviation, classwork vs. exam
6-10 students	-0.004
	(.052)
11-20 students	-.129 ***
	(.047)
21-30 students	-.164 ***
	(.046)
31-40 students	-.148 ***
	(.046)
41-50 students	-.183 ***
	(.046)
51-60 students	-.158 ***
	(.046)
61-70 students	-.149 ***
	(.045)
71-80 students	-.155 ***
	(.045)
81-90 students	-.142 ***
	(.045)
91-100 students	-.145 ***
	(.045)
More than 100 students	-.186 ***
	(.044)
Constant term	.384 ***
	(.043)
Observations	71,377

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. The dependent variable is the deviation between classwork grades, awarded by the subject teacher, and anonymously graded written exams. Deviations are defined within subjects. The regression is based on graduates from 2009. The left out category is schools with a cohort size of 1-5 students

a consolidation may affect achievement, and hence change the interpretation of coefficients. Nonetheless, a second order polynomial on cohort size is included in Panel A in Table 6, as a robustness check.[9]

The main insight from Table 6 is the lack of significance for the effect of consolidation on the grade point average. Coefficients from the GPA regression have a similar magnitude as corresponding estimates from alternative outcomes. This offers further indication that the apparent reduction in GPA is caused by differences in school size. Coefficients from the GPA regression in this table are higher than without control for school size, which corresponds with the proven negative association between cohort size and the leniency in classwork grading.

Different outcomes are observed in different years, which implies that different regressions explore a different set of consolidations. The relatively low extent of overlap across samples is mainly due to the construction of outcome variables, and their availability in the data.[10] While GPA and exam scores are unavailable until the early 2000s, completed education is not observed after 2000. This is mainly due to the lag between graduation from lower secondary school and completed education. Panel B in Table 6 holds results for the sample that is common to the first four regressions in the analysis. These results leave no reason to believe that main conclusions are driven by different samples across regressions. Observed effects remain small and insignificant.

To the extent that the decision to close a school is based on its past performance, it would change the interpretation of coefficients.[11] In that case, attempts to estimate the effect of consolidation for a representative school could be subject to a positive bias. There is no reason to believe that this is the case in my analysis. Anecdotal evidence from newspapers and reports hardly point at other explanations for closure than the combination of depopulation and savings. If governments aimed at closing low-performing schools, they would nevertheless be unable to observe performance. National tests were not introduced in their current form until 2007, thus value-added measures of performance have not been available. The only sources for comparison of performance have been the grade point averages and, more likely, externally evaluated exam results. GPA and exam results prior to closure are explored in Table 8 in the appendix. Neither

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[9]In the regressions, school size occurs as the no. of graduates from a school in a given year, i.e., its cohort size.

[10]GPA is observed in 2001-2010, lower secondary exam grades are observed in 2002-2009, upper secondary school completion and upper secondary school track are observed in 1975-2006, and completed education is observed from 1975 to 2000.

[11]Engberg et al. (2012) explore a setting where past performance is a criterion for closure.

Table 6: Effect of closures. Family fixed effects

Panel A: Regressions including control for school size

	(1) Lower sec., GPA	(2) Lower sec., anon. exam	(3) Prob. of USS completion	(4) Prob. of acad. USS track
CLOSING * PARTLY EXPOSED	0.011	0.0168	-0.0108	0.0134
	(0.131)	(0.182)	(0.0936)	(0.0864)
CLOSING * FULLY EXPOSED	0.040	0.306	0.124	0.0118
	(0.184)	(0.233)	(0.156)	(0.115)
RECEIVING * PARTLY EXPOSED	0.0215	0.053	-0.012	-0.128**
	(0.094)	(0.113)	(0.0679)	(0.0622)
RECEIVING * FULLY EXPOSED	-0.072	-0.024	0.028	-0.041
	(0.142)	(0.177)	(0.113)	(0.0932)
SCHOOL SIZE	-0.008*	-0.004	-0.003	-0.001
	(0.004)	(0.00587)	(0.00235)	(0.00199)
SCHOOL SIZE^2	0.00003	-0.00001	0.000007	0.000003
	(0.00003)	(0.00004)	(0.000005)	(0.000005)
Observations	1,978	2,778	2,342	2,766

Panel B: Regressions with a common sample across outcomes

	(1) Lower sec., GPA	(2) Lower sec., anon. exam	(3) Prob. of USS completion	(4) Prob. of acad. USS track
CLOSING * PARTLY EXPOSED	-0.094	0.092	-0.134	0.203
	(0.147)	(0.267)	(0.118)	(0.148)
CLOSING * FULLY EXPOSED	0.050	0.618	0.162	0.523*
	(0.264)	(0.482)	(0.212)	(0.266)
RECEIVING * PARTLY EXPOSED	0.095	0.123	-0.095	0.061
	(0.127)	(0.233)	(0.102)	(0.128)
RECEIVING * FULLY EXPOSED	0.277	0.523	-0.081	0.343
	(0.235)	(0.428)	(0.189)	(0.237)
Observations	1,361	1,361	1,361	1,361

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. School size refers to the number of graduates in a given year. The regression also include unreported gender and birth order dummies, and second order polynomial on cohort.

GPA nor exam results indicate that closing schools are negatively selected on performance.

To sum up, the main insight from the regression analysis is the lack of a significant long-term effect of closure. Although most coefficients are negative, they tend to be insignificant across outcomes and exposures to closure. Nonetheless, consolidations appear to be detrimental to students' results in lower secondary school. All exposures to consolidation involves a significant reduction of the grade point average. As for results on written exams, the significance is specific to students with partial exposure to a closing school. It is reasonable that a change of teachers and peers involves coordination problems through the transition.

6 Concluding remarks

School consolidation in rural areas is a controversial issue in the public debate. Local resistance is often strong, and closures are claimed to harm both the local community and students' educational achievement. This paper focuses solely on how consolidations influence the latter. One main conclusion arises. Consolidation does not seem to significantly reduce performance of affected students, and it does not seem to have a positive effect. This holds for all variations of exposure.

Although the different regressions do not leave reason to believe that consolidation influences performance, the study offers some support for the opposition against closures. School consolidation is associated with a fall in the grade point average from lower secondary school. This measure offers the most immediate and observable evaluation of consolidation for affected families. Further estimation suggests that the apparent reduction in performance is caused by different grading practices across schools of different sizes. With regard to the opposition to consolidation, it is important to remember that consolidations have consequences that go beyond the academic performance of affected students. The maintenance of a school may be important for the viability of its region.

My analysis explores average effects for graduates from lower secondary school. This approach may hide important heterogeneous effects, e.g., related to school and student characteristics. One dimension of heterogeneity relates to students' position in the distribution of academic skills. Unfortunately, my sample size is too limited to provide reliable insight of such tendencies. Students' age and grade level at exposure is another possible source of heterogeneity.

It is not obvious that findings from consolidation of lower secondary schools apply to education at other levels, e.g., for primary school mergers. Unfortunately, I do not have sufficient data on primary school attendance. Consolidations of primary schools are particularly interesting, due to their vast extent. While the number of lower secondary schools in Norway decreased by six percent from 2003 to 2013, the corresponding reduction in primary schools was 18 percent.[12] Moreover, students may be more sensitive to changing environment and longer travel time at earlier ages. On the other hand, the few previous studies that exist explore primary school closures, and the results are largely consistent with mine.

School consolidation in Norway appears to be driven by demographic trends, rather than change in school policy. With a long-term trend of urbanization, many small communities have experienced depopulation. The selection of migrators from such areas has not been random, as addressed by Bütikofer, Polovkova and Salvanes (2014). This serves as a reminder that the remaining families hit by school closures may not be representative for the full population. On the other hand, explored families are likely to be representative for families who will be affected by future consolidations. The estimated effects should therefore be instructive for policy.

This study has called attention to many different mechanisms through which consolidation may affect achievement. Empirically, the analysis has solely focused on the net effect of consolidations. To the extent that different mechanisms cancel each other out, the lack of a significant net effect may hide the significance of specific mechanisms. This calls for further research on the effect of different mechanisms.

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A An alternative identification strategy analyzing the difference in difference between neighboring school districts

Neighboring school districts experience different exposure to consolidation. This makes it possible to use a difference-in-difference strategy to further explore the robustness of my findings. The identifying assumption is that affected and unaffected students from a given region would share a common trend in educational performance in the absence of a consolidation, i.e., that the counterfactual performance of exposed students can be extracted from the evolvement of unexposed students. If a change at consolidation is specific to treated students, it can thus be interpreted as a causal effect.

All postal addresses in Norway include a four-digit postal code. In the difference-in-difference approach, I match each closing school to the affected postal codes. Affected students reside at a postal code where students would attend the closed school in the absence of consolidation, i.e. a postal code where students attended the closing school prior to its closure. In addition to this direct exposure to closure, indirect exposure is observed as attendance to the school that receives students from the closing school. The "receiving" exposure refers to postal codes from which students enroll to the receiving school both after and prior to the relevant closure.

The composition of the control group is essential for the validity of the identifying assumption. Inclusion to the control group requires that a postal code shares its first three digits with a postal code with exposure to closure. For the purpose of this analysis, it is useful to specify a distinction between "postal code" and "postal area". I let "postal code" refer to the specific four-digit postal district, whereas "postal area" refers to areas that share the first three digits of the four-digit postal code. Postal codes with the first three digits in common are located in the same area, and likely to have similar demographic characteristics.

The composition of data is complicated by the lack of correspondence between postal codes and school districts. To the extent that a postal code holds families from several school districts, it may also hold families with different exposure to consolidation. Inclusion to the applied sample requires a common treatment within the postal code, i.e., that students attend the same school, or different schools with the same exposure to closure. In order for estimations not to confuse effects of different closures, inclusion to data is also conditional on a single school closure within the postal area. The restriction to areas with a single closure reduces the explored number of

schools to 41, compared to 76 in the fixed effects analysis. However, difference-in-difference estimation does not rely on different treatments within the family. This increases the number of identifying observations for each included consolidation.

Many postal codes change over time. Further complication is caused by changes in geographical postal units over the years. In particular, many previous postal codes have been divided into finer units. Changes in postal codes have been manually corrected for. Changing geographical units are included as they first appear in the data.

Families may respond to consolidation by leaving the affected neighborhood. This decision may be conducted prior to the actual closure. In this analysis, exposure is therefore based on families' residency five years prior to the consolidation. A five-year lag increases the probability that families who are responding to the closure are confused with families who would move regardless of the consolidation. To the extent that we are able to identify comparable postal districts, however, this should not influence results. If treated and untreated areas would share a similar mobility pattern in the absence of a closure, and a similar selection of migrators, the effect of consolidation can still be directly extracted as the difference in trends. It is also worth mentioning that different motivations for moving typically have similar implications for affected children's educational change.

Figure 1 shows the average upper secondary school completion rate by treatment status. This is a convenient measure for a visual inspection, due do the low noise associated with its measurement. There is no observable shift at consolidation for treated students, neither in absolute terms nor relative to unaffected students. This suggests that consolidations have little impact on the probability of completing upper secondary school, which is consistent with the pattern from other outcomes that are presented later. No trend change is observed for treated students at closure.

Table 7 offers some descriptives on postal areas that are affected by closure. Within postal areas, the population at postal codes that hold closed schools is significantly lower than the population at other postal codes.

Figure 1: Completion of upper secondary school by treatment status
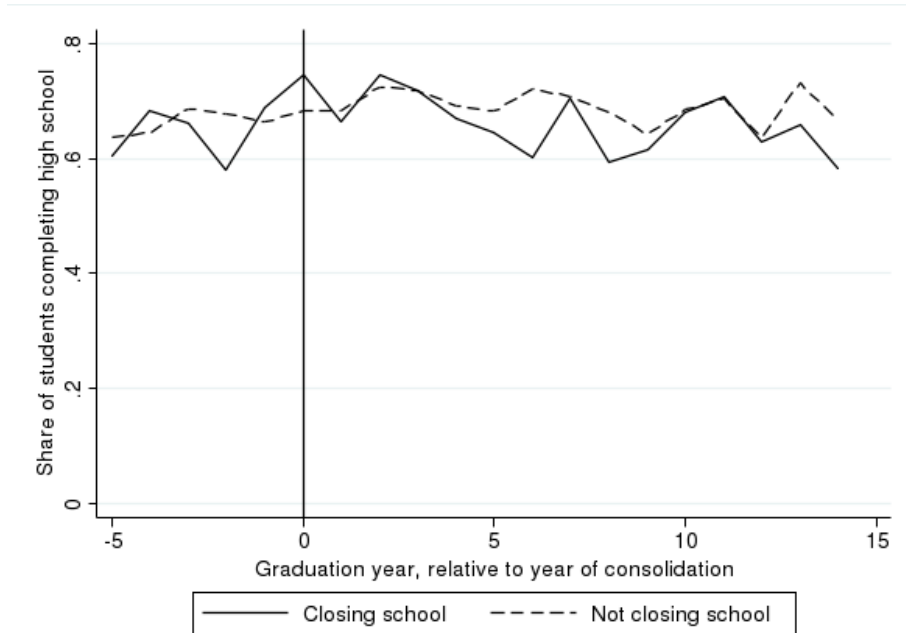
Upper secondary school completion, as opposed to drop-out or no upper secondary school education. As the timing of consolidations varies across schools, the timing occurs relative to the time for each consolidation. "Directly affected" refers to students who would attend the closing school in the absence of consolidation. With regard to "Not directly affected" students, the consolidation does not influence which school they attend. The group holds both unaffected students and students at receiving schools. Trends are not sensitive to the inclusion of receiving schools.

Table 7: Postal areas, summary statistics

	Mean (std.dev.)	Median	Min	Max	1987-2010
Population, closing postal codes	511 (795)	273	9	3,194	-25%
Population, receiving postal codes	844 (1092)	497	14	5,476	-19%
Population, unaffected postal codes	817 (1117)	363	10	5,593	-24%

Some municipalities have changed and merged over the period of the analysis. The change from 1987 to 2010 is calculated on basis of the subsample of postal codes that are consistent over these years. "Closing postal codes" refer to postal codes in which the school is closed. At "receiving postal codes", schools absorb students from closing schools. "Unaffected schools" constitute the control group, as schools at these postal codes are not affected by the consolidation.

A.1 Empirical strategy and results

If the identifying assumption holds, the causal effect of closure appears as the deviation in trends between the control group and students exposed to the closure. The employed difference-in-difference specification is the following:

$$PERFORMANCE_{iakct} = \beta_0$$
$$+\beta_1\, CLOSING_{ac} * SHORTLY\ AFTER_{at}$$
$$+\beta_2\, CLOSING_{ac} * AFTER_{at}$$
$$+\beta_3\, RECEIVING_{ac} * SHORTLY\ AFTER_{at}$$
$$+\beta_4\, RECEIVING_{ac} * AFTER_{at}$$
$$+\beta_5\, CLOSING_{ac} + \beta_6\, RECEIVING_{ac}$$
$$+\sum_j \beta_7 POSTAL\ AREA_a$$
$$+\sum_j \beta_8\, POSTAL\ AREA_a * AFTER_{at}$$
$$+\sum_j \beta_9\, POSTAL\ AREA_a * SHORTLY\ AFTER_{at}$$
$$+\beta_{10}\, f(BIRTH\ ORDER_i)$$
$$+\beta_{11}\, FEMALEi + \beta_{13}\, f(COHORT_k) + \epsilon_{iakct}$$

Subscript i denotes individuals; a denotes postal areas; k denotes cohorts; c denotes postal codes, and t denotes time. The following components are included:

CLOSING*SHORTLY AFTER: The effect of closure for students who transfer from the closing to the receiving school during lower secondary schooling. SHORTLY AFTER refers to graduation less than three years after consolidation, which implies that students were exposed while undergoing lower secondary education.

CLOSING*AFTER: The effect of closure for students who would attend the closing school in the absence of closure. AFTER refers to graduation at least three years after the relevant closure, which implies that students did their entire lower secondary education after consolidation.

RECEIVING*SHORTLY AFTER: The effect of closure for students who would attend the receiving school regardless of the consolidation, and graduate less than three years after consolidation.

RECEIVING*AFTER: The effect of closure for students who would attend the receiving

school regardless of the consolidation, and graduate at least three years after consolidation.

POSTAL AREA: The regressions include a dummy variable for each postal area, in order to capture regional variations in performance.

CLOSING: This dummy variable is one for postal codes where students attend a closing school prior to its closure. CLOSING controls for the possibility that the initial performance at closing schools deviates from that of other schools.

RECEIVING: This is a dummy variable with value one for students from postal codes with receiving schools.

AFTER*POSTAL AREA and SHORTLY AFTER*POSTAL AREA: These terms refer to area specific time trends.[13]

In addition to the reported variables, PERFORMANCE, COHORT, FEMALE and BIRTH ORDER occur as in the fixed effects regressions.

Table 8: Outcome by treatment status

		Lower sec., GPA	Lower sec., anon. exam	Prob. of upper sec. completion	Prob. of acad. upper sec. track	Level of educ. by age 26
Prior to closure	Not affected	4.00 (.81)	3.49 (1.02)	.684 (.465)	.382 (.486)	4.27 (1.52)
	Closing	3.95 (.80)	3.43 (1.03)	.656 (.475)	.358 (.479)	4.17 (1.50)
	Receiving	3.84 (.82)	3.36 (1.07)	.630 (.483)	.373 (.484)	4.21 (1.53)
After closure	Not affected	3.84 (.77)	3.45 (1.02)	.703 (.457)	.402 (.490)	4.33 (1.58)
	Closing	3.76 (.82)	3.30 (1.07)	.681 (.466)	.359 (.480)	4.31 (1.53)
	Receiving	3.88 (.82)	3.41 (1.03)	.707 (.455)	.356 (.479)	4.42 (1.51)

Average values of outcome variables, by exposure to consolidation. Standard deviations in parentheses.

Table 8 presents average outcomes by treatment status. Closure appears to be associated with reduced relative GPA and exam performance for students who would attend a closing school. For alternative outcomes, it is difficult to extract a consistent pattern that is specific to exposed students. Numbers are generally higher after consolidation, in accordance with the underlying trend of increasing enrollment to higher education.

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[13]The typical difference-in-difference set-up includes a dummy for observations after the explored change, to control for the trend that is common to treated and untreated observations. In this analysis, the timing of the treatment differs across the different postal areas. Control for a common change at consolidation would hence yield little insight. In order to capture potential underlying time trends that are common to different exposures to consolidation, trends need to be controlled for within postal areas. Regressions therefore include an interaction of postal area and the dummies that capture time relative to each closure.

Results based on the difference-in-difference regressions are presented in Table 9. The results confirm the absence of a consistent effect of consolidation. Most coefficients are insignificant, and it is hard to draw clear conclusions from the set of results. The lack of consistency across regressions reflects the absence of an underlying effect. The results leave no reason to conclude that consolidation affect performance. Neither students at closing nor receiving schools consistently change at consolidation. Moreover, there is no consistent pattern in the effect of graduation "shortly after" closure, as opposed to three or more years after.

Table 9: Effect of closures, Difference-in-differences

	(1) Lower sec., GPA	(2) Lower sec., anon. exam	(3) Prob. of upper sec. completion	(4) Prob. of acad. upper sec. track	(5) Level of educ. by age 26
CLOSING * SHORTLY AFTER (transition)	-0.0802 (0.0680)	0.288*** (0.105)	0.0260 (0.0402)	-0.00256 (0.0413)	-0.149 (0.181)
CLOSING * AFTER	-0.124** (0.0553)	0.0347 (0.0844)	0.0463 (0.0334)	-0.0878** (0.0343)	-0.0370 (0.144)
RECEIVING * SHORTLY AFTER (transition)	0.0175 (0.0775)	0.230* (0.124)	-0.00997 (0.0388)	-0.0796** (0.0399)	-0.199 (0.167)
RECEIVING * AFTER	0.0726 (0.0635)	0.123 (0.0947)	0.0246 (0.0317)	-0.108*** (0.0325)	-0.0147 (0.127)
Observations	7,907	6,227	11,429	11,431	6,807

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Regressions include different unreported controls, i.e., postal area specific performance and trends, birth order dummies, and a second order polynomial on birth cohort. (5) refers to the one-digit education code from NUS2000.

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03/17 March, **Kurt R. Brekke**, Tor Helge Holmås, Karin Monstad og Odd Rune Straume, "Competition and physician behaviour: Does the competitive environment affect the propensity to issue sickness certificates?"

04/17 March, **Mathias Ekström**, "Seasonal Social Preferences".

05/17 April, Orazio Attanasio, Agnes Kovacs, and **Krisztina Molnar**: "Euler Equations, Subjective Expectations and Income Shocks"

06/17 April, **Alexander W. Cappelen**, Karl Ove Moene, Siv-Elisabeth Skjelbred, and **Bertil Tungodden**, "The merit primacy effect"

07/17 May, **Jan Tore Klovland**, "Navigating through torpedo attacks and enemy raiders: Merchant shipping and freight rates during World War I"

08/17 May, **Alexander W. Cappelen**, Gary Charness, **Mathias Ekström**, Uri Gneezy, and **Bertil Tungodden: "**Exercise Improves Academic Performance"

09/17 June, **Astrid Kunze**, "The gender wage gap in developed countries"

10/17 June, **Kristina M. Bott, Alexander W. Cappelen, Erik Ø. Sørensen** and **Bertil Tungodden**, "You've got mail: A randomized field experiment on tax evasion"

11/17 August, Marco Pagano and **Luca Picariello**, "Talent Discovery, Layoff Risk and Unemployment Insurance"

12/17 August, Ingrid Kristine Folgerø, **Torfinn Harding** and Benjamin S. Westby, «Going fast or going green? Evidence from environmental speed limits in Norway"

13/17 August, **Chang-Koo Chi**, Pauli Murto, and Juuso Välimäki, "All-pay auctions with affiliated values"

14/17 August, **Helge Sandvig Thorsen**, "The effect of school consolidation on student achievement".